JYU DISSERTATIONS 681

Jussi Huuskonen

# Essays on the Role of Public Employment Services in Labour Market Matching

JYVÄSKYLÄ UNIVERSITY
SCHOOL OF BUSINESS AND ECONOMICS

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## ABSTRACT

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Problems in labour market matching can cause unemployment to remain high and vacancies unfilled. Public employment services (PES) provide job matching services and active labour market policies (ALMPs), which can promote labour market matching. By using comprehensive population-wide microdata from Statistics Finland and the Ministry of Economic Affairs and Employment (TEM), this dissertation provides new evidence on the effectiveness of PES.

The first article studies the impact of periodic interviews on unemployment duration. In 2017, a policy reform intensified the PES' practice of periodically interviewing unemployed jobseekers. We used a difference-in-differences approach that exploited regional variations in interview probabilities. The results show that more intensive interviews increased the transition rates to employment and participation in ALMPs. We found that employment effects were heterogeneous and strongest among jobseekers aged 25–34 and those with a low education level.

The second article studies the effects of vacancy referrals (VRs) on vacancy filling rates by using regional variation in the implementation of the 2014 reform. Using a difference-in-differences approach, we found that vacancy filling rates increased in areas where the number of VRs in relation to vacancies was increased the most. However, employment effects were negligible. One potential reason for this result is that VRs reduced the average quality and duration of post-unemployment jobs. We also found that the massive increase in the number of VRs reduced their average quality and effectiveness.

The third article examines the long-term effects of VRs and unemployment benefit sanctions on the outcomes of long-term unemployed jobseekers by using matching and panel data methods. The results showed that VRs increased employment probability. In turn, sanctions caused long-term unemployed individuals to exit the labour force and reduced their employment probability. This finding is related to incentive problems associated with the shift from unemployment benefits to other non-employment benefits. The study found evidence of an incentive trap: despite their significant employment effects, VRs and sanctions had minimal effects on unemployed jobseekers' disposable income.

Keywords: public employment services, active labour market policy, job search assistance, monitoring, vacancy referrals, employment, unemployment

# TIIVISTELMÄ (ABSTRACT IN FINNISH)

Huuskonen, Jussi
Tutkimuksia julkisten työvoimapalveluiden roolista työmarkkinoiden kohtaannossa
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Työmarkkinoiden kohtaanto-ongelmien seurauksena työttömyys voi pysyä korkealla tasolla ja avoimia työpaikkoja jäädä täyttymättä. Julkiset työvoimapalvelut tarjoavat työnvälityspalveluita sekä aktivointipalveluita, jotka voivat tehostaa kohtaantoa. Tämä väitöskirja tarjoaa uutta tietoa työvoimapolitiikan vaikutuksista. Tutkimuksissa käytetään Tilastokeskuksen ja Työ- ja elinkeinoministeriön kattavia mikroaineistoja.

Ensimmäinen artikkeli tutkii työttömille tehtävien haastattelujen vaikutusta työttömyyden kestoon. Vuoden 2017 politiikkauudistus lisäsi haastattelujen määrää voimakkaasti. Artikkelissa käytetään Difference-in-Differences -menetelmää, jossa hyödynnetään alueellista vaihtelua haastattelujen todennäköisyydessä. Tulosten mukaan tiheämmin toteutetut haastattelut lisäsivät ja nopeuttivat työttömien työllistymisiä sekä aktivointipalveluihin osallistumista. Työllisyysvaikutukset olivat voimakkaimpia 25-34 -vuotiailla, matalasti koulutetuilla sekä palvelualojen työttömillä työnhakijoilla.

Toinen artikkeli tutkii työtarjousten vaikutusta avointen työpaikkojen täyttymiseen. Tutkimus hyödyntää vuoden 2014 uudistuksen aiheuttamaa alueellista vaihtelua sekä Difference-in-Differences -menetelmää. Tulosten mukaan avointen työpaikkojen täyttyminen tehostui alueilla, joissa työtarjousten määrä suhteessa vakanssien määrään nousi eniten. Toisaalta työllisyysvaikutukset olivat olemattomia. Eräs mahdollinen selitys on, että työtarjoukset heikensivät työttömien vastaanottamien työpaikkojen laatua ja lyhensivät työsuhteiden kestoa. Lisäksi havaitsimme, että työtarjousten voimakas lisääminen heikensi niiden keskimääräistä laatua ja tehokkuutta.

Kolmas artikkeli tutkii työtarjousten ja työttömyysturvasanktioiden pitkän aikavälin vaikutuksia pitkäaikaistyöttömien työmarkkinatulemiin käyttäen kaltaistamis- ja paneeliaineistomenetelmiä. Tulosten mukaan työtarjoukset lisäsivät työllistymisen todennäköisyyttä. Sanktiot puolestaan lisäsivät työttömien siirtymiä työvoiman ulkopuolelle sekä vähensivät heidän työllistymistään. Tutkimuksessa havaitaan viitteitä kannustinloukuista: tilastollisesti merkitsevistä työllisyysvaikutuksista huolimatta työtarjouksilla ja sanktioilla oli vain hyvin pienet vaikutukset pitkäaikaistyöttömien käytettävissä oleviin tuloihin.

Avainsanat: julkiset työvoimapalvelut, aktiivinen työvoimapolitiikka, työnhaun tuki, valvonta, työtarjoukset, työllisyys, työttömyys

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As soon as I graduated in 2017, I got involved in a project for the Prime Minister’s Office. Together with Professor Jaakko Pehkonen and Kalle Tornberg, we investigated labour market matching in Finland. At that time, I ended up analysing large register-based micro-level datasets using Statistics Finland’s remote access system. The project also guided the direction of my dissertation.

After the project, I got the opportunity to start my doctoral studies and dissertation. Thanks to my funding organizations – Jyväskylä University School of Business and Economics (JSBE), the Yrjö Jahnsson Foundation, and OP Group Research Foundation – for making this all possible. I am very grateful for the financial support.

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Jyväskylä, July 2023
Jussi Huuskonen

## LIST OF INCLUDED ARTICLES

**Article 1**
Huuskonen, J. (2023) The Impact of Periodic Interviews on Unemployment Duration: Evidence from the 2017 Finnish Reform. *Forthcoming in LABOUR, 2023.* Available from http://doi.org/10.1111/labr.12245

**Article 2**
Huuskonen, J. The Impact of Vacancy Referrals on Vacancy Filling Rates: Evidence from Finland. (Unpublished)

**Article 3**
Huuskonen J. Back to Work: Sanctions or Vacancy Referrals for the Long-Term Unemployed? (Unpublished)

# CONTENTS

# 1 INTRODUCTION

## 1.1 Background

In the labour market, employers seek to hire workers with certain skills and qualifications, while jobseekers look for employment opportunities that match their skills and preferences. Labour market matching refers to the process by which jobseekers and employers find each other and make hiring decisions. Efficient labour market matching is important for the overall functioning of the labour market. When jobseekers and employers are able to find suitable matches quickly and efficiently, the labour market operates smoothly and the overall level of unemployment is reduced. On the other hand, when the matching process is slow or inefficient, unemployment can persist and increase. The process of matching jobseekers with employers can be complicated by factors such as information asymmetry, search costs and other frictions that hinder the efficient allocation of labour.

Labour market matching is generally analysed based on the Beveridge curve (see Elsby et al., 2015) or matching function (see Petrongolo & Pissarides, 2001). The Beveridge curve is a graphical representation of the relationship between the unemployment rate and the job vacancy rate in an economy. The Beveridge curve is typically a downward-sloping curve that shows an inverse relationship between the unemployment rate and the job vacancy rate. During periods of economic expansion, job vacancies tend to be plentiful, and the unemployment rate is low. Conversely, during periods of recession, job vacancies are scarce, and the unemployment rate is high.

However, the position of the Beveridge curve has shifted periodically in many developed economies (Elsby et al., 2015). Shifts in the Beveridge curve can be caused by a variety of factors. According to Pissarides (2000), higher unemployment income, higher taxes and higher mismatch shift the Beveridge curve out. Rise in long-term unemployment is also reported to reduce the matching efficiency of the labour market, explaining much of the outward shift in the US Beveridge curve (Kroft et al. 2016). People who experience long-term unemployment may face a range of negative consequences, including loss of skills and confidence, financial strain, social isolation, and even physical and mental health problems, which make finding a new job even more difficult (e.g. Pissarides, 1992; Ortego-Marti, 2017). Employers may also be hesitant to hire those who have been unemployed for a long time, which can further exacerbate the problem (e.g. Kroft et al., 2013).

Labour market matching can be influenced by government policies and public employment services (PES). Policy instruments and employment agencies can increase the rate of job matchings for given vacancies and unemployment, shifting the Beveridge curve inward. According to Launov and Wälde (2016), efficient operation of PES can considerably reduce unemployment. PES play an important role in labour market matching by providing job matching services and facilitating the exchange of information and job opportunities between jobseekers and employers. PES can help employers identify and recruit qualified candidates for their job openings. PES also provide active labour market programs (ALMPs) for jobseekers. ALMPs are a range of policies and programs to improve the employment prospects of jobseekers, including job search assistance (JSA), wage subsidies, job training and education programs (e.g. Alasalmi et al., 2020). These policies can affect the level of search in the labour market and the speed and efficiency of the matching process. By facilitating the efficient allocation of labour, PES can promote labour market matching, help reduce unemployment, and even promote economic growth.

JSA is an important PES tool. JSA refers to services and resources provided to jobseekers to help them find employment. Some examples of JSA include job search workshops, online job boards, job fairs and career events, vacancy referrals (VRs), and one-on-one career counselling and coaching. These services can help jobseekers identify job opportunities and increase their chances of finding employment. The existing evidence indicates that JSA and sanctioning schemes are the most effective ALMP measures in the short term (Vooren et al., 2019; Card et al., 2010, 2018; Kluve, 2010). JSA has been reported positive effects on re-employment particularly when combined with monitoring of job search (McGuinness et al., 2019). The articles in this dissertation focus on the following three PES tools: periodic interviews with unemployed jobseekers, VRs and unemployment benefit sanctions.[1]

[1] List of abbreviations:
ALMPs: Active labour market policies
JSA: Job search assistance
PES: Public employment services
VRs: Vacancy referrals

### 1.1.1 Periodic interviews with unemployed jobseekers

Periodic interviews with unemployed jobseekers are a service provided by PES to support jobseekers in their job search. These interviews are typically conducted at regular intervals, such as every 3-6 months (Gautier et al., 2018; Liljeberg & Söderström, 2017; Valtakari et al., 2019). During interviews, PES caseworkers typically check jobseekers' job search activities and assess the need for services, after which they may provide information about suitable job openings, training and other services. Frequent meetings between newly unemployed workers and caseworkers have been reported to have positive employment effects (e.g. Maibom et al., 2017).

In Finland, interviews last, on average, about 24 minutes (Valtakari et al., 2019). Approximately 74% of all interviews are conducted by telephone, whereas approximately 18% are face to face, with the remainder being conducted as distance meetings online (Valtakari et al., 2019). The concrete result of each interview is the creation (or updating) of an employment plan. Interviews are mandatory, and failure to attend them or the violation of the employment plan may lead to the interruption of an individual's unemployment benefits for 15–60 days (Sundvall & Mayer, 2018). This sanction period provides financial incentives for jobseekers to participate in interviews and pursue their employment plans. In 2017, the Finnish government reformed its policy to intensify the implementation of interviews. This reform was aimed at increasing job search activity, helping unemployed jobseekers find work more quickly, preventing long-term unemployment and accelerating the filling of vacancies (Valtakari et al., 2019).

### 1.1.2 Vacancy referrals (VRs)

VRs are a type of job placement service where PES prompt jobseekers to apply for specific job vacancies. Several studies report that VRs increase the transition rate from unemployment to employment (e.g. Bollens & Cockx, 2017; Van den Berg et al., 2019). For unemployed jobseekers, VRs are JSA and they can help jobseekers find employment more quickly and efficiently by matching them with job openings that are a good fit for their skills, experience and interests. VRs can also be a valuable service for employers seeking to fill job vacancies quickly and efficiently. Employers benefit by receiving a pool of pre-screened candidates. This can save employers' time and resources in the hiring process and increase the likelihood of finding a qualified candidate.

In Finland, VRs include monitoring, and a refusal to apply to an assigned vacancy can lead to a sanction. A sanction entails the suspension of unemployment benefits for 15–90 days. Misconduct can be noted by a PES caseworker or a potential employer. This sanction period provides financial incentives for jobseekers to apply for jobs. Certain reasons for refusals are considered valid, such as a too-long commute, a too-low wage, the wrong profession and an inability to work. According to Valtakari et al. (2014), over 50% of Finnish employers considered VRs important, and about 25% reported that they could use VRs in recruiting new workers. Small companies with fewer than five employees had, on average, more positive attitudes. In 2014, as a part of the Government structural policy programme, PES offices were guided to increase the number of VRs for unemployed jobseekers. The programme aimed to lower the structural unemployment rate, with the key elements of this being the rapid filling of vacant jobs and the shortening of unemployment periods.

### 1.1.3 Unemployment benefit sanctions

Unemployment benefit sanctions are penalties that can be imposed on individuals who receive unemployment benefits but fail to meet the requirements or obligations set by the government or unemployment agency. Sanctions can result in a reduction or complete exclusion of unemployment benefits for a specified period. The purpose of sanctions is to encourage unemployed individuals to comply with the requirements of the unemployment benefit system and actively seek employment. According to Van den Berg et al. (2022), sanctions are a key tool for incentivising unemployment benefit recipients to cooperate with PESs and take action to increase their chances of finding a job. Sanctions have been reported to increase job finding rates but also transitions from unemployment to outside the labour force (Abbring et al., 2005; Busk, 2016; Arni et al., 2013).

In Finland, the eligibility conditions for an unemployed individual to receive benefits and avoid sanctions are as follows: (i) register with PES as an unemployed person, (ii) actively search for a full-time job, (iii) apply for the jobs suggested by PES (via VRs), (iv) participate in the ALMPs offered by PES, and (v) participate in establishing and following a job search plan (Alasalmi et al., 2020). Violations of these criteria can lead to a sanction. Misconduct can be noted by a PES caseworker, a potential employer or ALMP programme staff. A sanction entails the suspension of unemployment benefits for 15–90 days. No warnings were issued in the 2010s. After repeated misconduct during a six-month period, entitlement to unemployment benefits is restored after spending at least 12 calendar weeks with a job, in an ALMP, as a full-time student or as a full-time entrepreneur. Otherwise, entitlement to unemployment benefits is restored only after five years. Unemployed individuals who receive sanctions may apply for other non-employment benefits. According to Busk (2016), sanction policies in Finland are average relative to other countries in Europe with respect to the sanction occurrence rate (10.2%) and the strictness of sanctions (100% reduction for eight weeks).

## 1.2 Labour market matching in Finland

In Finland, the Beveridge curve has moved further away from the origin in 2011-2020 (Figure 1). The unemployment rate and vacancy rate have been simultaneously at a high level. There are more job openings than ever before, but despite the large number of unemployed jobseekers, jobs cannot be filled. Employers have had recruiting problems, and unemployed jobseekers have had difficulties finding work. Unemployment periods have lengthened, and job openings are being filled more slowly (Pylkkänen, 2022). Unemployed jobseekers and job openings have been matching significantly worse than before (Pehkonen et al., 2018a).

Labour market mismatch is typically analysed from a regional and occupational perspective. Job openings and jobseekers may be in different areas and industries. Alasalmi (2022) uses an index developed by Şahin et al. (2014) to assess the development of matching problems in Finland from 2006 to 2022. According to the results, regional matching has improved: In 2020, the distribution of unemployed jobseekers between regions is clearly better for regional matching than in 2006. The trend of urbanization has concentrated jobs and workers in the same areas (see also Pehkonen et al., 2018c). On the other hand, the occupational matching has not weakened significantly either: In 2020, the occupations of unemployed jobseekers seemed to match the occupations of open job positions as well as in 2006. According to Economic Policy Council (2020), regional matching problems accounted for only about 2% of total unemployment and the proportion has decreased from 2011 to 2016. Occupational matching problems accounted for about 11% of unemployment and the proportion has remained relatively stable. Thus, although the overall labour market matching has deteriorated, the contribution of regional and occupational matching to matching problems has decreased in the 2010s.

What can explain the weakening of the labour market matching? The concept of labour shortage is a part of the problem. Larja and Peltonen (2023) classify occupations into labour shortage occupations, oversupply occupations and labour market mismatch occupations. Labour shortage means that there is no available labour for certain job positions. For example, jobseekers' occupation, skills, or ability do not match the job requirements. The biggest labour shortage is in social, health and welfare sector. Some occupations have over-supply, meaning that there are more jobseekers in relation to demand. Labour shortages and oversupply are related to structural changes in the economy: The number of industrial jobs has decreased and the number of jobs in the social and health sector has increased. In these occupations, matching could be improved by directing labour to shortage professions (e.g., retraining, adult education, international recruitment).

Several occupations face internal labour market mismatch, where there is simultaneously high vacancy rates and unemployment rates (Larja & Peltonen, 2023). Thus, there is both demand and supply for many occupations, but for some reason they do not meet, even though meeting would not require mobility between labour markets. According to Larja and Peltonen (2023), internal mismatch is strong in construction industry and food service and tourism industries. Typically, these occupations have a high proportion of non-standard employment relationships and a low level of earnings. According to Larja and Peltonen (2023), the outward shift of the Beveridge curve is largely related to the increase in the number of non-standard employment relations (e.g., temporary and part-time jobs) in the PES vacancy data. On the other hand, the share part-time employment has also increased (the Finnish Economic Policy Council, 2023). The outward shift of the Beveridge curve is more moderate when Statistics Finland's unemployment rate is used (panel B).[2]

A) PES unemployment rate B) Statistics Finland's unemployment rate

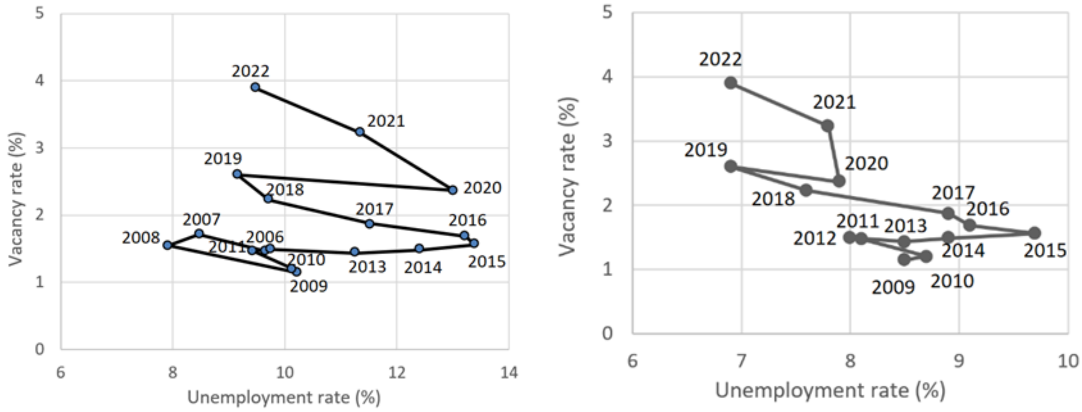


FIGURE 1 The Beveridge curve in Finland from 2006 to 2022.

Notes: Vacancy rate (at the end of the month) = Number of vacancies / total number of jobs; where the total number of jobs = number of vacancies + number of employed individuals. Vacancies refer to vacancies reported to PES Offices, which had not yet been filled on the reference dates. It is estimated that about 40% to 50% of all vacancies in Finland are notified to PES. Vacancy rates and PES unemployment rates: Employment Service Statistics [e-publication]. Helsinki: Ministry of Economic affairs and Employment [referred: 6.3.2023]. http://www.stat.fi/til/tyonv/meta_en.html
Statistics Finland's unemployment rates: Labour force survey [online publication]. Helsinki: Statistics Finland [Referenced: 6.3.2023]. https://stat.fi/en/statistics/tyti

Solutions to labour market mismatch problems within occupations are related to JSA and financial incentives, which are the central research topics of this dissertation. More efficient JSA for unemployed jobseekers, recruitment support for companies, and job placement technology (i.e. algorithms) have the potential to reduce matching problems. Financial incentives for employment are also important. In Finland, it has been reported that financial incentives have weakened in the 2010s (Pehkonen et al., 2018b). There are an alarmingly high number of people in a situation, where accepting a job or working additional hours does not increase net earnings at all. According to Puonti et al. (2022), in

[2] In Figure 1A, unemployment rate is based on the number of individuals who have registered as unemployed jobseekers in the PES offices. The Employment Service Statistics by PES includes persons receiving unemployment compensation, for which they have to register with unemployment services. An unemployed jobseeker can earn gross income up to 300 euros per month without it affecting their unemployment benefits. According to Statistics Finland's Labour Force Survey, a person is considered employed if they have worked for at least one hour during a week or if they are temporarily absent from work, for example, due to sick leave or vacation.

2021, more than 300,000 Finns were in an unemployment trap defined as a situation in which disposable income increases at most 25 percent when employed. Some key things that affect incentives are social security, taxation, and companies' ability to pay wages. The articles in this dissertation examine the effects of periodic interviews, vacancy referrals and unemployment benefit sanctions for the unemployed.

However, it should be noted that not all matching problems can be solved with JSA and monitoring. In the case of labour shortage occupations, education and training of the labour force is necessary. In addition to jobseekers' occupations, skills and knowledge are also important. Kyyrä and Pesola (2020) report that there seems to be a growing gap between the skills needed in new jobs and the skills possessed by unemployed jobseekers. It is also problematic that long-term unemployment, underemployment and disguised unemployment remain high in Finland (Kyyrä & Pesola, 2020). People who experience long-term unemployment may face a range of negative consequences, including loss of skills and confidence, financial strain, social isolation, and even physical and mental health problems, which make finding a new job even more difficult (e.g. Pissarides, 1992; Ortego-Marti, 2017).

## 1.3 Overview of the empirical essays

### 1.3.1 Research questions

This thesis contains three separate essays. Each essay studies topics related to unemployed jobseekers, job vacancies and PES. This thesis provides new evidence on the effectiveness of PES. Chapter 2 studies the effect of periodic interviews on unemployment duration and exit rates to employment, ALMPs and outside the labour force. Chapter 3 studies the impact of VRs on vacancy filling rates. Chapter 4 studies the effects of VRs and sanctions on labour market outcomes of long-term unemployed jobseekers. More precisely, the research questions of the thesis are:

- **Chapter 2:** How did the 2017 reform that intensified the PES's practice of periodically interviewing unemployed jobseekers affect unemployment duration? How did the intensifying of periodic interviews affect the transition rates from unemployment to employment, ALMPs and outside the labour force? Were the effects heterogeneous?
- **Chapter 3:** Did the 2014 reform that strongly increased the number of VRs in relation to vacancies affect vacancy filling rates? Did the massive increase in the number of VRs affect the probability that a vacancy will be filled? What is the role of vacancy and employer characteristics in vacancy filling rates?
- **Chapter 4:** How do unemployment benefit sanctions and VRs affect the labour market outcomes of long-term unemployed jobseekers in the long term? What are the effects on employment probability,

employment days and labour force participation? What are the effects on disposable income?

**Chapter 2** examines the effects of periodic interviews on unemployed jobseekers. Periodic interviews include JSA and monitoring of job search. JSA is documented to be one of the most effective means of promoting employment, particularly when combined with regular job search monitoring (e.g., Card et al., 2010, 2018; Kluve, 2010; Vooren et al., 2019; McGuinness et al., 2019). However, several studies have documented considerable displacement effects for those left without JSA (see Crepon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018). The evidence of displacement effects suggests that the effectiveness of JSA may be overestimated. This study contributes to the literature on JSA and monitoring by analysing the impact of nationwide reform in a way that takes local displacement effects on non-treated jobseekers into account. We studied the effects of a large-scale Finnish policy reform in 2017 that intensified the PES's practice of periodically interviewing unemployed jobseekers. Using regional variations in interview probabilities, we estimate the policy-relevant treatment effects on unemployment duration. We also examine the heterogeneity of the effects and consider possible mechanisms behind the effects.

**Chapter 3** examines the effects of VRs on vacancy filling rates. VRs are commonly used by PES to improve the matching of jobseekers and vacancies. The literature on VRs has focused on unemployed jobseekers searching for jobs, not firms searching for workers. Most studies on VRs have reported positive effects on the transition rates from unemployment to employment (e.g., van den Berg et al., 2019; Bollens and Cockx, 2017; Cheung et al., 2019). However, some studies have reported non-significant or even negative results (Van Belle et al., 2019; Engström et al., 2012). According to Engström et al. (2012), a large number of applications did not meet the qualification requirements for jobs. According to Van Belle et al. (2019), employers perceived referred jobseekers as being less motivated. We contribute to the literature on VRs by investigating how VRs affect the probability that a vacancy will be filled. In 2014, the number of VRs given by PES was massively increased as a part of the Government structural policy programme. Using regional variations caused by the reform, we investigate how VRs affect vacancy filling rates. We also analyse the role of vacancy and employer characteristics in vacancy filling rates.

**Chapter 4** examines the long-term effects VRs and unemployment benefit sanctions on the labour-market outcomes of long-term unemployed jobseekers. VRs are a key JSA tool that has been reported to reduce unemployment duration (Bollens & Cockx, 2017; Van den Berg et al., 2019). Unemployment benefit sanctions have also been reported to increase job finding rates (Abbring et al., 2005; Busk, 2016). However, several questions remain. First, previous studies have focused on short-term unemployed jobseekers who receive earnings-related unemployment benefits. This study focuses on long-term unemployed individuals, a group with the weakest employment prospects. Second, most previous studies estimated only short-term effects. This study estimates the long-term effects of sanctions and VRs on various labour-market outcomes. Third, few studies have estimated effects on wages and income. This study estimated the effects of sanctions and VRs on disposable income – that is, the net income that accounts for social security transfers.

### 1.3.2 Data and methods

All articles used comprehensive population-wide register data from Statistics Finland and the Employment Services Register of the Finnish Ministry of Economic Affairs and Employment (TEM). The FOLK database is maintained by Statistics Finland. The FOLK Basic dataset provides yearly panel data for the entire population of Finland. It contains individual-level information on demographic, educational and occupational and family characteristics. It also provides information on labour market status and income. The FOLK Period datasets contain data on employment relationships, unemployment periods and ALMP periods. TEM datasets contain Finnish PES administrative data on unemployed jobseekers and vacancy postings. The TEM URA dataset provides information on interviews and VRs by PES. The TEM Työnhaku (Job Search) dataset provides information on sanctions. All datasets contained individual identifiers, which made it possible to link the datasets. Data preparations and all regressions and figures were carried out in Stata (version 16.0).

TABLE 1 The datasets used in the articles

| Data module | Information |
|---|---|
| FOLK Basic data | Individual-level data on individuals' demographic, educational and occupational characteristics, and labour market status and income. The data are from the end of each year. |
| FOLK Period data | Individual-level data on employment relationships, unemployment periods and ALMP periods. |
| TEM Työnhakija (Jobseeker) | Individual-level data on jobseekers who have registered as jobseekers in the PES offices. |
| TEM Työnhaku (Job search) | Individual-level data on unemployment periods and sanctions. |
| TEM Työkunto (Working capacity) | Individual-level data on individuals' working capacity and disabilities. |
| TEM Työpaikka (Vacancy data) | Micro-level data on vacancy postings reported to PES. |
| TEM URA | Micro-level data on interviews and VRs. |

Notes: The specific instructions to obtain access to the data are available at https://tilastokeskus.fi/tup/mikroaineistot/hakumenettely_en.html
Descriptions of the datasets can be found in the Taika research data catalogue: https://taika.stat.fi/en/

**In Chapter 2**, we analyse the effects of periodic interviews by using regional variations in the implementation of the 2017 reform. We study the causal impact of intensifying interviews on unemployment duration using a difference-in-differences design with varying (non-binary) treatment intensities. Several studies have exploited regional variations in treatment intensity (e.g. Card, 1992; Angrist & Pischke, 2009; Ferracci et al., 2014; Frölich & Lechner, 2010; Räsänen & Mäkelä, 2021). We separately examine the effects of treatment intensity on the exit rates to employment, ALMPs and outside the labour force using the Cox proportional hazards model. The study sample consisted of the Finnish population that entered unemployment in the period covering 2015–2017. Our post-reform observations consisted of unemployment spells that began in January–February 2017 since the reform came into force at the beginning of 2017 and the regional differences were initially at their highest. The comparable pre-reform period consisted of unemployment spells that started in January and February of 2015–2016.

**In Chapter 3**, we used the TEM vacancy data that covered all vacancy postings that were announced to the PES from 2011 to 2015. The data include information on vacancy and employer characteristics, such as job type, work schedule type, job duration, required occupation, employer sector and employer size. The TEM URA dataset provided information on VRs given by Finnish PES. We analysed the effects of VRs by using regional variation in the implementation of the 2014 reform. We studied the effects of the increased number of VRs/Vacancy postings on vacancy filling rates using a difference-in-differences approach (Angrist & Pischke, 2009). In addition, we examined the effects on VR probability, VR duration and vacancy duration. Our baseline model compared the outcomes of vacancies in the top 15 (the treatment group) and the bottom 15 areas (the control group). We also estimated treatment intensity regressions using the vacancy data from all 67 travel-to-work areas.

**In Chapter 4**, to estimate VR and sanction effects, we applied a combination of matching and panel data methods (e.g. Burger et al., 2022; Caliendo & Tübbicke, 2020; Caliendo et al., 2008). The study sample was restricted to the long-term unemployed individuals who were unemployed and had no employment days, VRs or sanctions in 2011–2013. The individuals were followed until 2019. The identification of effects was based on the comparison of labour market outcomes between the treatment and control groups. We used propensity score matching (PSM) to create a matched sample. The purpose of PSM is to find non-treated individuals who are similar to treated individuals in terms of all relevant observed pre-treatment characteristics. The study compared the outcomes of individuals who had received a VR/sanction in 2014 to the outcomes of those who had not received any VRs or sanctions in 2014. The key outcome variables were employment, employment days, labour force participation and disposable income. Focusing on 2014 treatment events ensured that we had enough data on pre-trends and made it possible to examine long-term effects. Moreover, in 2014, a policy reform massively increased the number of VRs. The reform also led to stricter monitoring and increased imposition of sanctions. Because of the reform, VRs and sanctions were issued to individuals who would not have received them before 2014.

In every article, we show that the requirements for the difference-in-differences approach were fulfilled. The key identifying assumption is that the treatment and control groups had parallel trends in outcomes during the pre-treatment period (i.e. the common trend assumption). Without the treatment, trends would be identical; with the treatment, a deviation from this common trend is induced (Angrist & Pischke, 2009, 230). In addition, the identification requires that the composition of the treatment and control groups be stable before and after the treatment. To account for observable differences in the composition, the models included a large set of control variables. Area-fixed effects were included to capture such regional differences in labour-market conditions that were constant over time. Time-fixed effects were included to capture such differences in macroeconomic conditions that were constant across regions. In every article, we checked robustness of results, and estimated separate regressions for certain subgroups to examine treatment effect heterogeneity.

### 1.3.3 Main findings and contributions

**In Chapter 2**, we found that the intensification of interviews caused an increase in the transition rates to employment. However, the magnitude of the positive effect was smaller compared to studies that ignored displacement effects. In addition, our results show a strong effect of interviewing on participation in ALMPs. Helping unemployed jobseekers to exit more swiftly to ALMPs may increase their likelihood of employment in the future. Although the reform led to tighter monitoring and the imposition of sanctions increased, it appears to have not increased the total flow out of the labour force. We found that the employment effects were heterogeneous and strongest for jobseekers aged 25–34, jobseekers with a low education level or jobseekers whose field of education was services. According to the results, interviewing these groups is particularly beneficial. Interviews either boost their job search or make them accept lower-quality jobs. We found that treatment effects on ALMP hazards were particularly strong among jobseekers aged 55–62 and jobseekers with a high education level. We also considered possible mechanisms behind the treatment effects, including increased JSA, stricter monitoring and threat effects.

**In Chapter 3**, we found that vacancy filling rates increased in areas where the number of VRs in relation to vacancies was increased the most. In those areas, after the reform, a larger share of vacancies received VRs and vacancies received VRs sooner. Thus, VRs can help employers obtain a larger pool of applicants and cause applicants to apply sooner. However, employment effects were negligible. One potential reason for this result is that VRs reduced the average quality and duration of post-unemployment jobs. We also found that the massive increase in the number of VRs reduced their average quality and effectiveness. After the reform, a considerably lower share of VRs resulted in matches. Moreover, the share of cases in which the employer rejected an applicant who had received a VR increased considerably. Our results highlight that it is important that VRs are sent to jobseekers who meet the needs of employers. We also analysed the role of vacancy and employer characteristics in vacancy filling rates. Our results show that full-time, permanent and high-skill vacancies had the lowest vacancy filling rates. Thus, employers seem to be more demanding when hiring workers for such jobs, or applicants genuinely lack the necessary skills and competences.

**In Chapter 4**, we found that VRs increased employment probability by 51% (6.2 percentage points) over the following five years. Thus, VRs are an effective tool for helping long-term unemployed jobseekers and enhance the matching of unemployed jobseekers to job vacancies. In turn, sanctions caused long-term unemployed individuals to exit the labour force and reduced their employment probability. This finding is related to incentive problems associated with the shift from unemployment benefits to other non-employment benefits. Overall, this article demonstrates that it is difficult to simultaneously provide long-term unemployed jobseekers with both comprehensive social security and good incentives for employment. The results indicate that long-term unemployed jobseekers are likely to face incentive traps. Despite producing a clear increase in employment, VRs were not associated with much higher disposable income. Previous literature suggests that jobs accepted after receiving a VR may have lower wages and be less stable. Welfare subsidy cuts and taxation were other possible factors. The results indicate that sanctions have even smaller effects on disposable income than VRs do. This is mainly due to social security transfers; individuals outside the labour force can receive other non-employment benefits. The downside is that individuals outside the labour force do not have access to counselling, VRs or ALMPs by PES.

### 1.3.4 Concluding remarks and policy implications

This dissertation provides new empirical evidence on the effectiveness of PES. The articles focus on the following three PES tools: periodic interviews with unemployed jobseekers, VRs and unemployment benefit sanctions. The results have the following policy implications.

First, PES can affect labour market matching. According to the results, JSA combined with monitoring of job search produces desired results. The intensification of periodic interviews in 2017 had positive employment effects, and it also increased transition rates from unemployment to ALMPs. VRs for long-term unemployed jobseekers increased their employment probability. In addition, the massive increase in the number of VRs in 2014 increased vacancy filling rates. However, it should be noted that the magnitudes of the positive effects were smaller compared to studies that ignored displacement effects.

Second, the results show that the implementation of reforms involves challenges. The 2017 reform weakened the average quality of interviews, and the 2014 reform weakened the average quality and effectiveness of VRs. Regarding the interviews, the problems were related to the increased workload of the PES caseworkers. Moreover, targeting interviews can also be recommended because we found stronger employment effects for certain jobseeker groups. In turn, the massive increase in the number of VRs increased considerably the share of cases in which the employer did not approve the applicant who had received a VR. Our results highlight that it is important that VRs are sent to jobseekers who meet the needs of employers. Not all matching problems can be solved with JSA and monitoring. In the case of labour shortage occupations, education and training of the labour force is necessary, because the skills and knowledge of the jobseekers do not meet the requirements of the job.

Third, we documented that long-term unemployed jobseekers are likely to face incentive traps. VRs had positive employment effects but minimal effects on disposable income. This is related to the quality of post-unemployment jobs, taxation and social security cuts. In turn, sanctions caused long-term unemployed individuals to exit the labour force and reduced their employment probability but did not affect their disposable income. This finding is related to incentive problems associated with the shift from unemployment benefits to other non-employment benefits. The results demonstrate that it is complex to offer the long-term unemployed a comprehensive level of social security and good incentives for employment at the same time.

TABLE 2 Summary of articles

| Chapter | Topic | Data and methods | Main results |
|---|---|---|---|
| Ch 2 | -The effect of periodic interviews on unemployment duration: exit rates to employment, ALMPs and outside the labour force.<br>-The effects of the 2017 reform that intensified the PES's practice of periodically interviewing unemployed jobseekers. | -Unemployment spells that began in 2015-2017.<br>-The Cox proportional hazards model.<br>-Difference-in-differences method, treatment intensity regression, regional variations in interview probabilities. | -Increase in interview probability increased the monthly hazard rate of employment.<br>-Strong effect on participation in ALMPs.<br>-The flow out of labour force did not increase.<br>-Effects on employment hazards particularly high for jobseekers aged 25-34 and jobseekers with a low education level.<br>-Effects on ALMP hazards particularly strong for jobseekers aged 55-62 and for the highly educated. |
| Ch 3 | -The effect of VRs on vacancy filling rates.<br>-The effects of the 2014 reform that increased the number of VRs in relation to vacancies.<br>-The role of vacancy and employer characteristics in vacancy filling rates. | -Vacancy announcements reported to the PES in 2011-2015.<br>-Difference-in-differences method.<br>-Treatment group: Top 15 areas where the number of VRs in relation to vacancies increased the most.<br>-Control group: Bottom 15 areas. | -Vacancy filling rates increased in areas where the number of VRs in relation to vacancies was increased the most.<br>-Employment effects were negligible.<br>-The reform decreased the average quality and effectiveness of VRs.<br>-Full-time, permanent and high-skill vacancies had the lowest vacancy filling rates. |
| Ch 4 | -The long-term effects of unemployment benefit sanctions and VRs on the outcomes of long-term unemployed jobseekers: → employment probability, employment days, labour force participation, disposable income. | -FOLK data 2011-19: Individuals who were unemployed and had no employment days, VRs or sanctions in 2011-13.<br>-Propensity score matching and panel data methods.<br>-Treatment group: individuals who received a sanction/ VR in 2014.<br>-Control group: Individuals who did not receive any sanctions or VRs in 2014. | -VRs increased employment probability over the following five years.<br>-Sanctions caused long-term unemployed individuals to exit the labour force and reduced their employment probability: → shift to other non-employment benefits.<br>-Evidence of an incentive trap: VRs and sanctions had minimal effects on long-term unemployed jobseekers' disposable income. |

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# 2 THE IMPACT OF PERIODIC INTERVIEWS ON UNEMPLOYMENT DURATION: EVIDENCE FROM THE 2017 FINNISH REFORM[3]

**Abstract**

In 2017, a Finnish policy reform intensified the Public Employment Services' practice of periodically interviewing unemployed jobseekers. This study used high-quality administrative data to analyse the effect of interviews on unemployment duration. We used a difference-in-differences approach that exploited regional variations in treatment intensity. Our results show that a 10-percentage-point increase in interview probability increased the monthly hazard rate of employment by 3.1%, with the effect being strongest among jobseekers aged 25–34 and jobseekers with a low education level. Also, our results demonstrate a strong effect on participation in active labour market programmes.

**Keywords**: active labour market policy, unemployment duration, job search assistance, monitoring, treatment intensity

**JEL codes**: J64, J68

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## 2.1 Introduction

Job search assistance (JSA) and monitoring are important parts of the Public Employment Services (PES) and active labour market programmes (ALMPs). The existing evidence indicates positive effects of JSA on re-employment, particularly when combined with monitoring (Card et al., 2010, 2018; Kluve, 2010; Vooren et al., 2019). JSA seems to be one of the most effective means of promoting employment. However, several studies have documented considerable displacement effects for those left without JSA (see Crepon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018; Cheung et al., 2019). The evidence of displacement effects suggests that the effectiveness of JSA may be overestimated. The treatment evaluation literature typically compares participants' outcomes with those of non-participants, with the stable unit treatment value assumption (SUTVA) being a common assumption. The SUTVA states that an individual's outcome should not depend on other individuals' treatment statuses (Ferracci et al., 2014). This assumption might be violated for many reasons: the job-finding rates of non-treated individuals may decrease if treated individuals increase their search efforts. Neglecting these equilibrium effects can lead to biased estimates. If the SUTVA is violated, the proportion of individuals treated in the same area becomes relevant (Gautier et al., 2018).

This study contributes to the literature on JSA and monitoring by analysing the impact of nationwide reform in a way that takes local displacement effects on non-treated jobseekers into account. We studied the effects of a large-scale Finnish policy reform in 2017 that intensified the PES's practice of periodically interviewing unemployed jobseekers. These periodic interviews are a combination of JSA and job search monitoring. The reform was an exogenous shock that increased interview probabilities. It affected the entire country such that the intensity of treatment varied across areas. We used regional variations in interview probabilities to estimate the policy-relevant treatment effects on unemployment duration. The estimated effects were calculated as the sum of the positive treatment effects on the treated and the negative displacement effects on the non-treated.

Using administrative data containing comprehensive information on individuals' unemployment and employment periods, interviews, and background variables, we focused on a population of workers who became unemployed in January and February of 2015–2017. We used a difference-in-differences approach with varying treatment intensities and estimated the causal effect of the interviews on the exit rates to employment, ALMPs and outside the labour force. Our results show that the intensification of inter-views caused an increase in the transition rates to employment. A 10-percentage-point increase in the regional share of unemployed jobseekers interviewed during three months of consecutive unemployment increased the monthly exit rate to employment by approximately 3.1 percent. This positive effect is in line with the previous literature, but its magnitude is smaller compared to studies that ignored displacement effects. In addition, our results show a strong effect of interviewing on participation in ALMPs. Although the imposition of sanctions increased after the reform, it appears to have not increased the total flow out of the labour force. We also found evidence of heterogeneous effects. The treatment effects on employment hazards were particularly high for jobseekers aged 25–34, jobseekers with a low education level or jobseekers whose field of education was services. According to the results, interviewing these groups is particularly beneficial. We also found that treatment effects on ALMP hazards were particularly strong for jobseekers aged 55–62 and for the highly educated.

We performed several analyses to demonstrate the validity of the identification strategy. First, we conducted a formal test for the common trend assumption. Second, we showed that the areas that experienced either the highest or lowest treatment intensities exhibited parallel trends. Third, we revealed that these areas had similar economic and demographic conditions. Fourth, treatment intensity had low correlations with relevant regional characteristics.

We also considered possible mechanisms behind the treatment effects, including increased JSA, stricter monitoring and threat effects. The reform intensified interviews and increased their volume to support job searches and boost job search intensity. It also led to tighter monitoring of job searches, and the imposition of sanctions increased. The reform likely had considerable threat effects, also affecting unemployed jobseekers who were not interviewed. Moreover, the reform increased ALMP transitions and may have also increased the operating effectiveness of the PES.

This paper is organised as follows. In Section 2.2, we describe the Finnish system of periodically interviewing unemployed jobseekers and the reform that commenced in 2017. Section 2.3 presents the data and the empirical strategy. Section 2.4 provides descriptive analysis, including a formal test for the parallel trend assumption. Section 2.5 reports the results, and Section 2.6 concludes the paper.

## 2.2 Periodic Interviews in Finland and the 2017 Reform

In Finland, PES offices have conducted periodic interviews with unemployed jobseekers for several decades to support them in their job searches (Valtakari et al., 2019). In interviews, caseworkers check jobseekers' job search information and assess the need for services, after which they offer suitable jobs, training and other services to the jobseekers. According to Valtakari et al. (2019), approximately 74% of all interviews are conducted by telephone, whereas approximately 18% are face to face, with the remainder being conducted as distance meetings online. Face-to-face interviews are typically offered to jobseekers with the weakest job search capabilities. Interviews last, on average, about 24 minutes (Valtakari et al., 2019). The concrete result of each interview is the creation (or updating) of an employment plan. Interviews are mandatory, and failure to attend them or the violation of the employment plan may lead to the interruption of an individual's unemployment benefits for 15–60 days (Sundvall & Mayer, 2018). This sanction period provides financial incentives for jobseekers to participate in the interviews and pursue their employment plans.

In January 2017, the Finnish government reformed its policy to intensify the implementation of interviews.[4] This reform was aimed at increasing job search activity, helping unemployed jobseekers find work more quickly, preventing long-term unemployment and accelerating the filling of vacancies (Valtakari et al., 2019). According to the government's new policy, an interview must be organised for an individual whose unemployment has continued for three months and every three months thereafter, unless the interview is obviously unnecessary given the jobseeker's situation (Valtakari et al., 2019).[5] The former legislation defined the frequency of periodic interviews less precisely.

Figure 1 shows how the reform caused a large exogenous change in the scale of the interviews at the national level. Before the reform, less than 20% of unemployed jobseekers had been interviewed during the previous three months of consecutive unemployment. After the 2017 reform, the share of unemployed jobseekers interviewed increased to over 50%.

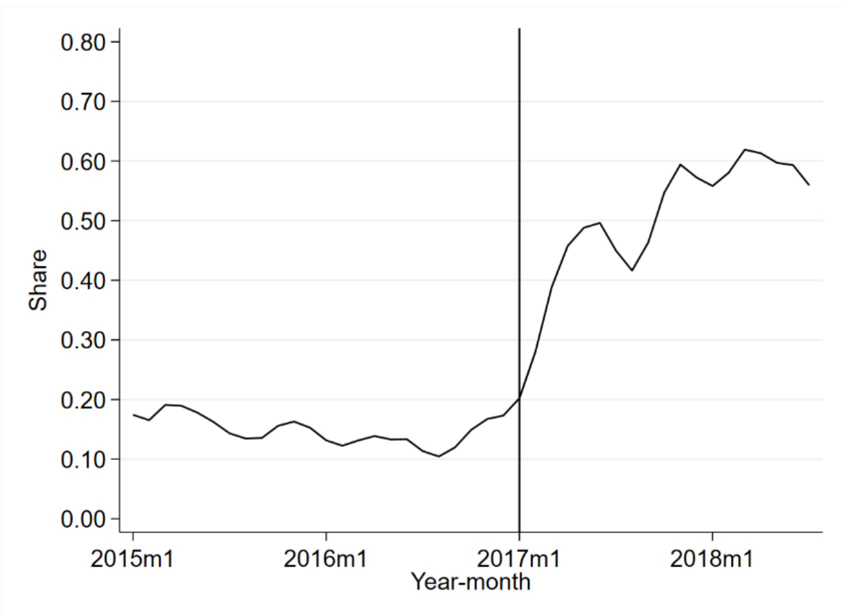


FIGURE 1 Share of unemployed jobseekers interviewed during the previous three months.

Notes: Cross-section on the 28th day of the month, unemployment spells of 90–365 days.

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[4] In 2016, the cost for periodic interviews was 49.7 million euros. The 2017 budget allocated an additional 17 million euros to improve the efficiency of employment services (Valtakari et al., 2019).

[5] For comparison, in Denmark, JSA typically consists of meetings with a caseworker every three months (Gautier et al., 2018). In Sweden, jobseekers meet a caseworker on average once per quarter (Liljeberg & Söderström, 2017).

In early 2017, large regional differences were evident in the changes in interview probabilities (see Table 1). We used this regional variation to study the effects of interviews on unemployment duration.[6] It is important to consider the reasons for the observed regional differences, primary among them being different management styles in different employment offices. In some offices, the quantitative implementation of periodic interviews was immediately established as a major goal, while in other offices, the number of interviews increased more slowly and gradually. According to Heikki Räisänen, the research director of the Ministry of Economic Affairs and Employment (MEE),[7] the most important factor was regions' policies: how important the goal of intensifying interviews had been considered and when an interview had been considered unnecessary. According to Valtakari et al. (2019), the resources related to the number of unemployed jobseekers were very similar in all regions. In Section 2.4.3, we show that the treatment intensity was exogenous to the relevant regional characteristics.

According to Valtakari et al. (2019), there was no clear indication that the intensification of interviews would have displaced resources from other PES activities (e.g. counselling services, competence development services and managing employer contacts). Rather, as Valtakari et al. (2019) reported, the intensification of interviews increased the personal workload of the PES caseworkers and weakened their well-being at work. The authors reported that many caseworkers felt incapable of conducting sufficiently high-quality interviews and offering relevant solutions to jobseekers. Thus, the reform probably weakened the quality of interviews and employment plans. According to Valtakari et al. (2019), there were differences between regional employment offices (REOs) in terms of how the reform affected the caseworkers' well-being and workload. The authors reported that REOs had office-specific differences in working methods and practices related to conducting interviews.

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[6] In 2017, two other labour market reforms came into force: The cost-competitiveness package reduced labour costs of Finnish companies, and the maximum duration of unemployment insurance was cut from 500 to 400 days (Economic Policy Council, 2017). These reforms did not cause regional variations because the related practices were consistent across the country before and after these reforms. However, the reforms may have contributed to the Finnish economy: The volume of GDP increased by 0.5% in 2015, 2.8% in 2016 and 3.2% in 2017 (See Statistics Finland, 2020. Official Statistics of Finland (OSF): Annual national accounts [e-publication]. ISSN = 1798-0623. http://www.stat.fi/til/vtp/2020/vtp_2020_2021-03-15_tie_001_en.html)

[7] Personal communication via email (7 October 2021).

## 2.3 Data and Methods

### 2.3.1 Data Sets

We used Finnish administrative data containing comprehensive individual-level information on unemployment spells, interviews and demographic background characteristics. The main data source was the Employment Services Register of the MEE. Since registration at an employment office is a requirement for receiving unemployment benefits, the database contains information on practically all unemployment spells. Each unemployment spell is combined with information on interview days, employment spells and individual background variables. Periodic interviews are recorded in the MEE's database as employment plans or their updates. Detailed information on the characteristics of individuals and employment spells was obtained from the FOLK database maintained by Statistics Finland. The FOLK data contain individual-level information on demographic, educational, occupational and family characteristics.

Our sample consisted of the Finnish population that entered unemployment in the period covering 2015–2017. We limited the analysis to jobseekers between 20 and 62 years of age. We excluded temporarily laid-off individuals and restricted the sample to new unemployment spells that had been preceded by an employment spell of at least 30 days. This ensured that the sample consisted of individuals whose status changed from employed to unemployed at time zero. Thus, we excluded individuals with long non-employment spells (e.g. individuals entering unemployment after a period in ALMPs or who were outside the labour force).

Our post-reform observations consisted of unemployment spells that began in January–February 2017 since the reform came into force at the beginning of 2017 and the regional differences were initially at their highest. Excluding unemployment spells that began later in 2017 ensured that our results were unaffected by the activation model, which was launched at the beginning of 2018.[8] Owing to the high seasonal variation in unemployment exit rates, a comparable pre-reform period consisted of unemployment spells that started in January and February of 2015–2016.

In Section 2.5, long unemployment spells were right-censored from 10 months onwards because the reform affected longer unemployment spells that started in 2016.[9] Therefore, we followed unemployment spells that started in 2015 until the end of 2015, unemployment spells that started in 2016 until the end of 2016, and unemployment spells that started in 2017 until the end of 2017. Thus,

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[8] The activation model reduced unemployment benefits for jobseekers who had not done enough paid employment, participated in employment services or earned enough as a self-employed person. About one-third of all benefit recipients faced sanctions in 2018 (see Economic Policy Council, 2019).

[9] Moreover, the right-censoring ensured that cutting the maximum duration of unemployment insurance in 2017 did not affect our results.

the pre-reform observations had low interview probabilities throughout their unemployment spells, whereas the post-reform observations had higher interview probabilities.

### 2.3.2 Methods

We analysed the effects of intensifying interviews by using regional variations in the implementation of the reform. The reform was followed by large regional variations in the implementation of interviews. Mainland Finland has 15 REOs, which are organised geographically, with each REO serving several municipalities (see Figure A1 in the Online Appendix). In early 2017, large regional differences were evident in the changes in interview probabilities. Table 1 shows the share of unemployed jobseekers interviewed in different REO areas in 2016 and 2017. The probability of being interviewed during three months of continuous unemployment increased from 2016 to 2017 in every area (see column 3). The change was highest in Central Finland (52.3 percentage points) and lowest in North Ostrobothnia (15.6 percentage points).

TABLE 1 Change in the share of unemployed jobseekers interviewed during three months of consecutive unemployment by REO area

| Area ID | REO area | Share interviewed 2016 (1) | Share interviewed 2017 (2) | Treatment intensity (3) |
|---|---|---|---|---|
| 10 | Central Finland | 8.3 | 60.6 | 52.3 |
| 14 | Kainuu | 22.1 | 72.4 | 50.3 |
| 3 | Satakunta | 12.5 | 52.4 | 40.0 |
| 5* | Pirkanmaa | 7.3 | 46.2 | 38.8 |
| 1 | Uusimaa | 8.2 | 46.7 | 38.5 |
| 8* | North-Savo | 8.2 | 44.6 | 36.4 |
| 7 | South-Savo | 10.7 | 42.8 | 32.1 |
| 4 | Hame | 16.8 | 45.2 | 28.4 |
| 2 | Varsinais-Suomi | 12.5 | 40.7 | 28.2 |
| 11 | South Ostrobothnia | 28.6 | 55.2 | 26.6 |
| 12 | Ostrobothnia | 15.0 | 39.6 | 24.6 |
| 15 | Lappi | 12.8 | 37.0 | 24.3 |
| 9 | North Karelia | 15.0 | 38.2 | 23.2 |
| 6 | Southeast Finland | 12.5 | 32.1 | 19.7 |
| 13* | North Ostrobothnia | 26.4 | 42.0 | 15.6 |

Notes: Share of unemployed jobseekers interviewed during the previous three months, an average of the cross-section on the 28th day of March–September. Unemployment spells of 90–365 days. Sources: MEE; our own calculations; see also Figure A1 in the Online Appendix. * The common trend assumption does not hold; see Section 2.4.1.

We studied the causal impact of intensifying interviews on unemployment duration using a difference-in-differences design with varying (non-binary) treatment intensities. Several studies have exploited regional variations in treatment intensity (e.g. Card, 1992; Angrist & Pischke, 2009; Ferracci et al., 2014; Frölich & Lechner, 2010; Räsänen & Mäkelä, 2021). In our study, treatment intensity was measured by the percentage-point change in the share of unemployed jobseekers interviewed during three months of continuous unemployment, from March–September 2016 to March–September 2017. Thus, the treatment intensity proxied the extent to which the probability of being interviewed increased in each area from 2016 to 2017. Like Räsänen and Mäkelä (2021), we assumed that interview shares were constant throughout the pre-treatment period. This assumption was supported as the interview shares were almost constant in 2015–2016, and the changes in interview shares in the pre-treatment period were small relative to the changes during the treatment period. The treatment intensity varied across the 15 REO areas, ranging from 16–52.

The outcome variable was unemployment duration, measured in months. An unemployment spell is defined as a sequence of time during which a person is an unemployed jobseeker in the MEE's register. We separately examined the effects of treatment intensity on the exit rates to employment, ALMPs and outside the labour force. The employment hazards included all transitions to employment relations that lasted for at least 30 days. The employment hazards take into account that jobseekers may be temporarily in ALMPs or outside the labour force. The 30-day condition guarantees that new employment relations did not end very soon. ALMPs include employment with wage subsidies, labour market training, coaching and work trials, rehabilitation work and self-motivated studies with unemployment benefits. Although individuals participating in ALMPS often receive unemployment benefits, they are no longer classified as unemployed by the MEE register.

The empirical hazard function for individual $i$ whose unemployment started in area $s$ in period $m$ is:

$$\theta_{ism}(t) = \lambda(t)\exp\{x_i\beta + \tau_m + \gamma_s + \delta * TreatIntensity_s * I(YEAR \geq 2017)\} \quad (1)$$

where $\lambda(t)$ is a time-varying baseline hazard function depending on the elapsed unemployment duration t estimated using the Cox proportional hazards model, $x$ is a vector of time-invariant individual characteristics measured at the start of the unemployment spell, $\tau_m$ are indicators for the time of unemployment entry (year-month) and $\gamma_s$ are indicators for the REO areas. $I(YEAR \geq 2017)$ is an indicator that the unemployment spell started after 1 January 2017, and TreatmentIntensity is the regionally varying treatment intensity.

The coefficient of the interaction term ($\delta$) shows the average effect of a one-percentage-point increase in treatment intensity on the monthly hazard rates. Using regional variation yields policy-relevant effects by automatically considering the displacement effects on non-treated individuals. Following Crepon et al. (2013), $P$ is the regional probability of treatment, and $T$ is the individual's treatment status. Assume that individuals assigned to the control group are never treated, so $T(0) = 0$. There are three potential outcomes of $y(P, T)$: $y(0, 0)$ is the potential outcome when no treatment takes place in the area, $y(1, 0)$ is the potential outcome when untreated in a treatment area, and $y(1, 1)$ is the potential outcome when treated. The displacement effect is defined as the externality imposed on a non-treated individual in a treated area. Following Crepon et al. (2013), the average displacement effect is $AE = E(y(1, 0) - y(0, 0))$. A simple comparison between a treatment group and a comparison group yields the treated in treated zone effect ($TTZ$), which overestimates the effectiveness of a policy instrument. Based on previous literature, assume a positive 'treated in treated zone' effect ($TTZ > 0$) and a negative displacement effect on the non-treated ($AE < 0$). Our model cannot disentangle the direct and displacement effects. However, our model does identify the policy-relevant treatment effect, which is the sum of a positive treatment effect on the treated and a negative displacement effect on the non-treated: $TT = TTZ + AE < TTZ$.

The key identifying assumption is that without the treatment, unemployment duration trends would be identical in all areas; with the treatment, a deviation from this common trend is induced (Angrist & Pischke, 2009, 230). We analyse the common trend assumption in Section 2.4.1. Heterogeneity across REO areas was captured by the area-fixed effects ($\gamma_s$), and the time-fixed effects ($\tau_m$) absorbed common shocks. Weak regional mobility of the Finnish labour force decreased the potential bias that might arise from spillover effects (Räsänen & Mäkelä, 2021).

To account for observable differences in the composition of different areas, the model includes a large set of covariates. We controlled for gender, age (five categories), education level (six categories), field of education (12 categories), previous occupation (six categories), number of unemployment months in the past two years (eight categories), disability, non-Finnish background and family status. All covariates were measured at the beginning of the unemployment period and were treated as time-invariant regressors within an unemployment spell. Also, we controlled for the regional unemployment rate, the regional output growth and the regional vacancy rate at the travel-to-work area level (at the end of year $y$-1). Travel-to-work areas (67) are defined by the Finnish Ministry of the Interior as entities formed from municipalities, the criteria for which are municipal cooperation, workers commuting and transport connections. A REO area may encompass several travel-to-work areas, while each area belongs to just one REO area.

## 2.4 Descriptive Analysis

In this section, we show that the requirements for the difference-in-differences approach were fulfilled. Our identification strategy required that the regions with different treatment intensities had parallel trends in outcomes during the pre-treatment period and that their composition was stable. Moreover, we examined whether the treatment intensity was exogenous to the relevant regional characteristics.

### 2.4.1 Common Trends

The key identifying assumption of our approach was that the exit rates would have followed the same parallel trends in all REOs, without the reform (i.e. the common trend assumption). We conducted a formal test for the common trend assumption. Using data from 2015 and 2016, we estimated a duration model with dummies for all REOs, a dummy for the year 2016, and interactions between the 2016 dummy and REO dummies.

TABLE 2 F-test results for interaction terms measuring common trends

| | Hazard to 30d employment | Hazard to ALMPs | Hazard to outside the labour force |
|---|---|---|---|
| All REOs | 48.63 | 50.91 | 9.95 |
| (N=39,049) | (0.000) | (0.000) | (0.766) |
| Without REOs 5, 8 and 13 | 17.05 | 13.57 | 7.15 |
| (N=29,545) | (0.106) | (0.258) | (0.787) |
| Without REOs 5, 8 and 13; Controls | 15.85 | 14.81 | 8.92 |
| (N=29,545) | (0.147) | (0.191) | (0.629) |

Notes: F-test results for D2016 x REO interactions. P-values are reported in parentheses. New unemployment spells that started in January and February of 2015–2016. Estimates for hazard rates from unemployment to employment, ALMPs and outside the labour force using data from 2015 and 2016 are reported in Tables A1 and A2 in the Online Appendix. The Cox proportional hazards model was used. Long unemployment spells were right-censored from 10 months onwards. The models included indicators for REO areas, a dummy for the year 2016, and D2016*REO interactions. The models in the last row included the same control variables as the models in Table 5. According to Table A1, the pre-trends in Pirkanmaa (REO 5), North Savo (REO 8) and North Ostrobothnia (REO 13) differed from those of the other REOs.

The joint tests of the coefficients of the interaction terms were significant (see Table 2 and Appendix Table A1). Thus, in certain REOs, the pre-trends differed from those in the other REOs. In the employment hazard model, the interaction term for North Ostrobothnia (REO 13) was significant at the 1% level. This area experienced considerably negative output growth of about -3% in 2015–2016 and very strong output growth of 7% in 2017, while the averages for other REOs were 0% in 2015–2016 and 2% in 2017. In the ALMP hazard model, the interaction terms were significant for North Savo (REO 8) and Pirkanmaa (REO 5) at the 5% level. In North Savo, ALMP hazards increased considerably more than in other areas from 2015 to 2016. For Pirkanmaa, the coefficient of the interaction term was negative. Pirkanmaa had extensive employment experiments in 2015 and 2017. According to Valtakari et al. (2019), in Pirkanmaa, the implementation of periodic interviews differed from other REOs, focusing more on customised and individual service.

Because of this, we excluded these three REOs from our analyses. After omitting these three REOs, the joint tests of the coefficients of the interaction terms were insignificant (see Table 2 and Online Appendix Table A2). Thus, the exit rates developed similarly in the remaining 12 REOs in 2015–2016.

Next, we examined the existence of common trends by dividing our data into three groups according to the magnitude of the treatment intensity: [19, 24.5), [24.5, 39) and [39, 53]. The first treatment group (the bottom three REOs) included 8,506 observations, the second (the middle six REOs) included 27,366 observations and the third (the top three REOs) included 7,005 observations. We were particularly interested in groups that experienced either the highest or lowest treatment intensities. Thus, Figures 2–4 provide descriptive evidence for the top and bottom three REOs.

Figure 2 shows that, before the reform, the interview probabilities were stable and slightly lower in the top three REOs than in the bottom three REOs. The figure also shows how abrupt the change was in the top three REOs in early 2017. In turn, in the bottom three REOs, the interview probability increased less and more gradually during 2017.

Figure 3 depicts the time series of the monthly exit rates from unemployment to employment, ALMPs and outside the labour force for unemployment spells in the top and bottom three REOs from 2015 to 2017. The average exit rates were similar for all outcomes of interest until the end of 2016, providing support for the parallel trend assumption. The reform affected all ongoing unemployment spells from January 2017 onwards, with treatment intensity being highest in the top REOs. Consistent with this, the exit rates from unemployment to ALMPs increased in the top three REO areas compared to the bottom three REOs in 2017. In 2017, the monthly exit rates to employment were slightly higher in the top three REOs than in the bottom three REOs, whereas they had been slightly lower before 2017.

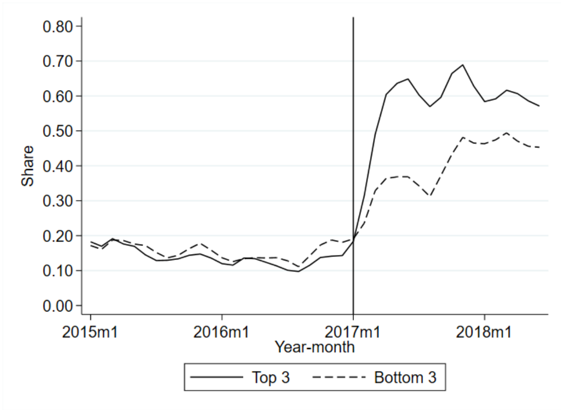
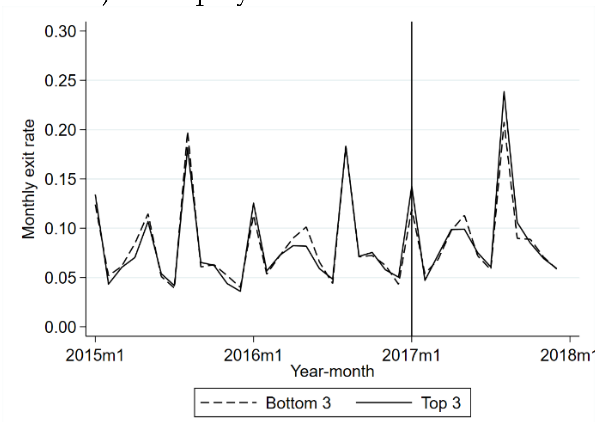


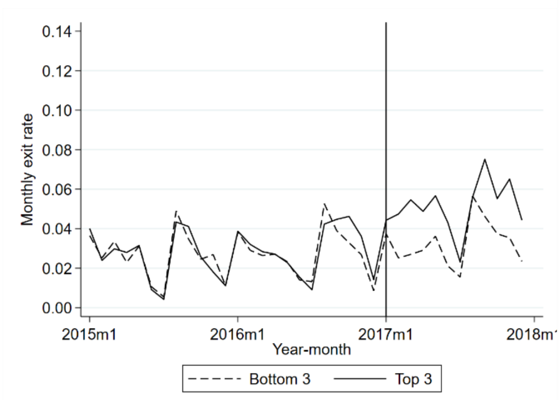

FIGURE 2 Share of unemployed jobseekers interviewed during the previous three months by treatment group.

Notes: Unemployment spells of 90–365 days. The top three REOs with the highest treatment intensities and the bottom three REOs with the lowest treatment intensities are shown in Table 1.

A) To employment




B) To ALMPs




C) To outside the labour force

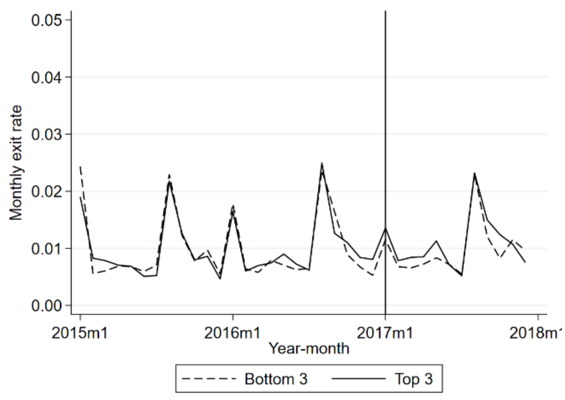



FIGURE 3 Monthly exit rates from unemployment to employment, ALMPs and outside the labour force by treatment group.

Notes: On the first day of the month, unemployment spells of 1–365 days, preceded by a work period of at least 30 days.

### 2.4.2 Empirical Hazard Rates

Our study sample consisted of new unemployment spells that started in January and February of 2015–2017. Figure 4 shows empirical hazard rates as a function of elapsed unemployment duration for the years 2015, 2016 and 2017. Three monthly exit rates are depicted for the top and bottom three REOs: to employment (Panels A and B), to ALMPs (Panels C and D) and to outside the labour force (Panels E and F).

Panels A and B show the total hazards to employment relations that last for at least 30 days. The panels take into account that jobseekers may be temporarily in ALMPs or outside the labour force. The 30-day condition guarantees that employment relations did not end very soon. The panels show that the probability of employment transition decreases with unemployment duration. For example, Kyyrä et al. (2019) and Busk (2016) reported similar results. Before the reform, the monthly hazards were about 10–22% for durations of less than five months and about 5–10% for durations over six months. After the reform, the exit rate to employment increased more in the top three REOs than in the bottom three REOs. Thus, Figure 4 suggests larger employment effects than Figure 3, which documented only direct employment transitions during a month for individuals who were unemployed on the first day of each month.

Panels C and D show that, before the reform, the monthly hazards to ALMPs were about 2–6%. After the reform, there was a large upward shift in ALMP hazards in the top three REOs. Table A3 in the Online Appendix shows that in the top three REOs, coaching and work trials as exit destinations particularly increased. Panels E and F report the exit rates to outside the labour force. They show that the probability of transition to outside the labour force was very low compared to the probability of employment and ALMP transitions. The exit rate to outside the labour force did not increase in the top REOs after the reform.

Thus, this descriptive evidence indicates that after the reform, higher treatment intensities were associated with higher transition rates from unemployment to employment and ALMPs. The comparison shows that the exit rates were similar in the top and bottom REOs in the pre-experiment period of 2015–2016. After the reform, the exit rates to employment and ALMPs increased more in the top three REOs than in the bottom three REOs. However, these differences in the raw empirical hazard rates cannot be interpreted as causal effects since they may have been driven by differences in observed and unobserved characteristics.

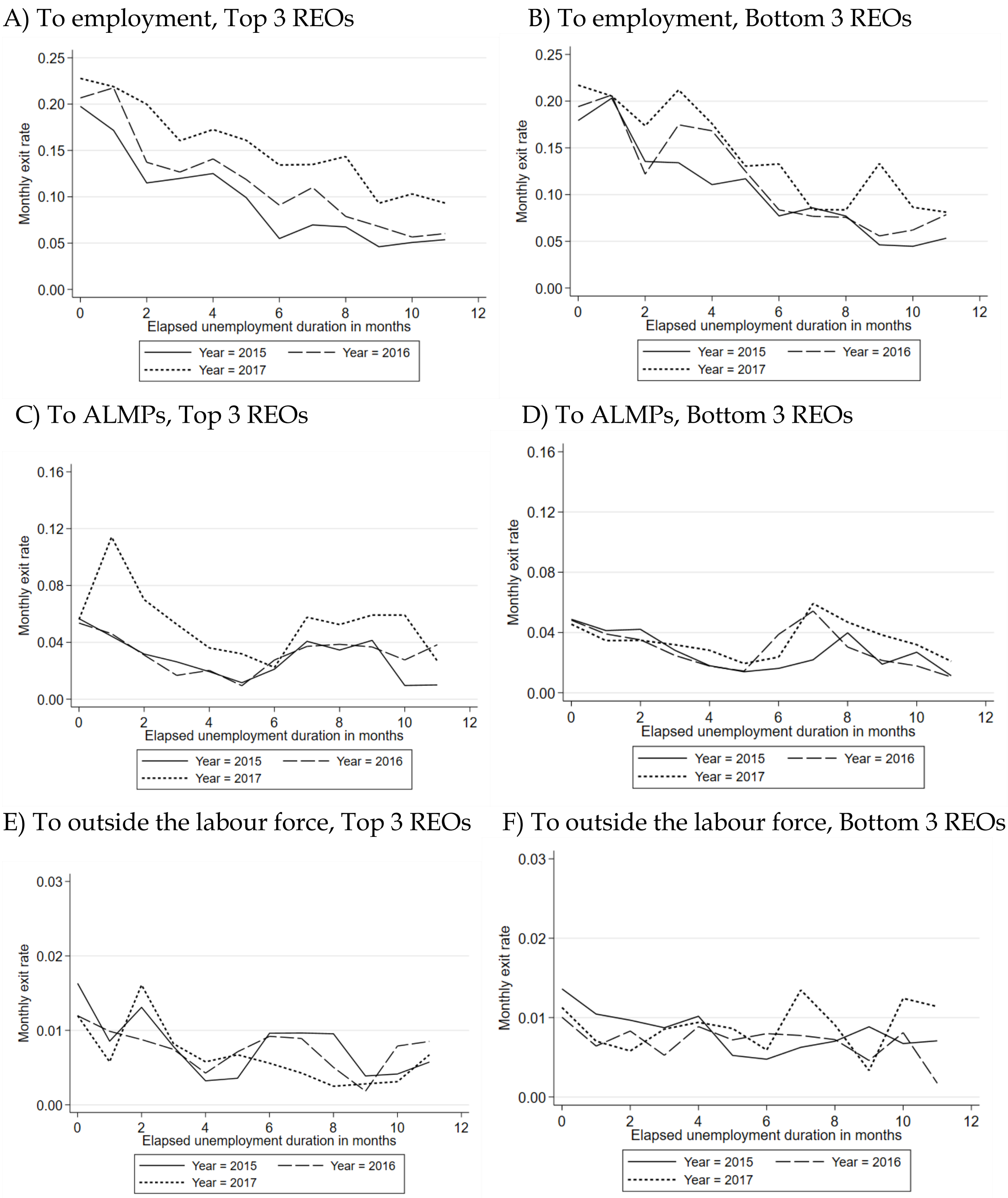



FIGURE 4 Empirical hazard rates by treatment group in 2015–2017.

Notes: The data consist of new unemployment spells that started in January and February of 2015–2017 and were preceded by a work period of at least 30 days. The top three REO areas with the highest treatment intensities and the bottom three REO areas with the lowest treatment intensities are shown in Table 1.

### 2.4.3 Regional and Individual Characteristics

The key identifying assumption of the empirical model is that regional differences in treatment intensity are exogenous to any relevant regional characteristics. To study the endogeneity issue, we, like Räsänen and Mäkelä (2021), computed correlations between the treatment intensity and pre-reform characteristics of the areas (see Table 3). The treatment intensity was not correlated with the regional pre-reform unemployment rate, employment rate, vacancy rate or economic growth. The correlation between the treatment intensity and pre-reform level of the interviews was weak. Moreover, we found weak correlations between treatment intensity and the fractions of young and old unemployed jobseekers. All of the correlations were statistically insignificant. This indicates that the pre-reform characteristics and economic performance of an area did not explain the magnitude of treatment intensity (i.e. how much interviews increased in this area).

TABLE 3 Correlations between treatment intensity and regional characteristics

| Treatment intensity | Correlation | P-value |
|---|---|---|
| Unemployment rate, 2015–2016 (labour force aged 15–74) | 0.14 | 0.508 |
| Employment rate, 2015–2016 (population aged 15–64) | –0.10 | 0.632 |
| Annual output growth, 2015–2016 | 0.11 | 0.299 |
| Vacancy rate, 2015–2016 | 0.01 | 0.950 |
| Fraction of jobseekers over 55, 2015–2016 | 0.20 | 0.351 |
| Fraction of jobseekers under 25, 2015–2016 | –0.14 | 0.523 |
| Pre-treatment level of interviews (interview share, 2015–2016) | –0.13 | 0.549 |

Notes: Correlations between treatment intensity and regional characteristics at the REO area level. Treatment intensity refers to a percentage-point change in interviews (2016–2017). The three REO areas that did not meet the common trend assumption were excluded from the data: Pirkanmaa (REO 5), North Savo (REO 8) and North-Ostrobothnia (REO 13).

Table 4 presents descriptive statistics on the main variables used in the empirical analysis. Column 1 presents the averages for the entire sample, while the other columns present the group averages for the three treatment groups. The top and bottom three REOs were similar in size and had very similar economic and demographic conditions. The only clear observable difference between the groups concerned treatment intensity. Before the reform, the average share of unemployed jobseekers interviewed during three months of continuous unemployment was 12%, whereas after the reform, it was 46%. The fraction of jobseekers interviewed increased by 48 percentage points in the top three REOs and 22 percentage points in the bottom three REOs. In the middle six REOs, the regional unemployment rate was lower, with more highly educated jobseekers and immigrants. These differences were driven by the Helsinki Metropolitan Area, which is located in the REO Uusimaa, one of the middle six REOs. However, since we used a difference-in-differences approach, it was more important that the changes in the composition of these groups were small (Uusitalo & Verho, 2010). Table A4 in the Online Appendix shows that the changes in group characteristics were small and similar across all groups.

TABLE 4 Summary statistics by treatment group

| Variable name | All 12 REOs (1) | Bottom 3 REOs (2) | Middle 6 REOs (3) | Top 3 REOs (4) |
|---|---|---|---|---|
| Interviewed during three months (%), 2016 | 12.3 | 13.2 | 12.2 | 11.8 |
| Interviewed during three months (%), 2017 | 45.6 | 35.2 | 45.3 | 59.5 |
| Treatment intensity, %-points | 33.3 | 22.0 | 33.1 | 47.7 |
| Regional unemployment rate (%) | 14.4 | 17.6 | 12.7 | 17.0 |
| Regional vacancy rate (%) | 0.95 | 0.78 | 1.00 | 0.97 |
| Regional economic growth (%) | 0.74 | 1.28 | 0.40 | 1.43 |
| Age | | | | |
| 20-24 | 0.12 | 0.13 | 0.12 | 0.12 |
| 25-34 | 0.27 | 0.25 | 0.28 | 0.25 |
| 35-44 | 0.21 | 0.20 | 0.21 | 0.19 |
| 45-54 | 0.21 | 0.22 | 0.21 | 0.22 |
| 55-62 | 0.19 | 0.21 | 0.17 | 0.21 |
| Female | 0.57 | 0.58 | 0.57 | 0.59 |
| Immigrant | 0.08 | 0.06 | 0.10 | 0.04 |
| Disability | 0.09 | 0.09 | 0.07 | 0.10 |
| Education level: | | | | |
| Secondary | 0.52 | 0.58 | 0.48 | 0.58 |
| Lowest tertiary | 0.08 | 0.08 | 0.08 | 0.08 |
| Lower tertiary | 0.14 | 0.15 | 0.14 | 0.13 |
| Master's degree or higher | 0.12 | 0.08 | 0.15 | 0.09 |
| Other, unknown | 0.14 | 0.12 | 0.15 | 0.12 |
| Number of observations | 42,877 | 8,506 | 27,366 | 7,005 |

Notes: Data contain new unemployment spells that started in January and February of 2015–2017 and were preceded by a work period of at least 30 days. Column 2: Unemployment spells in the bottom three REO areas with the lowest treatment intensities. Column 3: Unemployment spells in the middle six REO areas. Column 4: Unemployment spells in the top three REO areas with the highest treatment intensities. The REO areas are shown in Table 1.

## 2.5 Results

### 2.5.1 Homogeneous Effects

Table 5 reports estimates on transitions to employment (column 1), transitions to ALMPs (column 2), and transitions to outside the labour force (column 3). The first row in Table 5 shows coefficient estimates for Treatment Intensity x I(YEAR ⩾2017). The treatment effect on employment hazards was statistically significant at the 1% level: a 10-percentage-point increase in the interview probability increased the rate of transition to employment by 3.1% (= (exp(10 × 0.0031) - 1) x 100%). The estimate was based on data on actual employment spells, so it was unaffected by changes in the accuracy of PES unemployment records, and the 30-day condition ensured that new employment relations did not end very soon.

This positive employment effect is in line with the previous literature on JSA, but its magnitude is smaller compared to studies that ignored displacement effects. Maibom et al. (2017) found that frequent meetings between newly unemployed workers and caseworkers increased employment. Graversen and van Ours (2008) investigated how an intense activation programme in Denmark affected unemployed workers' job-finding rates. Their analysis showed that the re-employment rate in the treatment group was 30% higher than that in the control group. Gautier et al. (2018) evaluated the same activation programme, considering equilibrium effects. They found that participation in the activation programme increased the weekly rate of exit from unemployment by 17%. Thus, while displacement effects are important, the effects of JSA remain positive. It should also be noted that the Finnish reform took place during a boom rather than a recession. Cheung et al. (2019) found that displacement effects were smaller under good labour market conditions, with many job openings.

Our results showed a strong effect on the exit rate to ALMPs: a 10-percentage-point increase in treatment intensity increased hazards to ALMPs by 21%, the estimate being statistically significant at the 5% level (column 2). This is in line with Valtakari et al. (2019), who documented that the 2017 Finnish reform had a major impact on participation in ALMPs. Helping unemployed jobseekers to exit more swiftly to ALMPs may increase their likelihood of employment in the future. However, the effects are not necessarily immediate. According to Card et al. (2010), many ALMPs with insignificant or even negative impacts after a year have significantly positive impact estimates after two or three years.

TABLE 5 Baseline results by outcome

| | Hazard to employment (1) | Hazard to ALMPs (2) | Hazard to outside the labour force (3) |
|---|---|---|---|
| Treatment Intensity x I(YEAR $\geq$ 2017) | 0.0031*** | 0.0188** | −0.0098 |
| | (0.0011) | (0.0081) | (0.0061) |
| **Regional characteristics** | | | |
| Unemployment rate | −0.0094 | −0.0261** | 0.0059 |
| | (0.0058) | (0.0118) | (0.0139) |
| Output growth | −0.0032 | −0.0008 | −0.0035 |
| | (0.0026) | (0.0048) | (0.0055) |
| Vacancy rate | −0.0059 | −0.0236 | −0.0433 |
| | (0.0231) | (0.0508) | (0.0528) |
| **Age** (vs. 20-24) | | | |
| 25-34 | −0.204*** | −0.108*** | −0.564*** |
| | (0.021) | (0.040) | (0.062) |
| 35-44 | −0.364*** | −0.293*** | −0.776*** |
| | (0.038) | (0.040) | (0.098) |
| 45-54 | −0.485*** | −0.433*** | −0.590*** |
| | (0.061) | (0.052) | (0.090) |
| 55-62 | −0.937*** | −1.260*** | −0.551*** |
| | (0.076) | (0.073) | (0.093) |
| Immigrant | −0.272*** | 0.304*** | −0.004 |
| | (0.021) | (0.041) | (0.089) |
| Disability | −0.370*** | 0.236*** | 0.801*** |
| | (0.038) | (0.047) | (0.073) |
| **Education level** (vs. upper secondary) | | | |
| Lowest tertiary | 0.007 | 0.161*** | 0.088 |
| | (0.020) | (0.041) | (0.087) |
| Lower tertiary | 0.159*** | −0.061 | −0.049 |
| | (0.019) | (0.040) | (0.063) |
| Master's degree or higher | 0.190*** | −0.290*** | 0.025 |
| | (0.027) | (0.050) | (0.106) |
| Other, unknown | 0.056 | 0.045 | 0.063 |
| | (0.070) | (0.149) | (0.485) |
| N (unemployment spells) | 42,877 | 42,877 | 42,877 |

Notes: Estimates for hazard rates from unemployment to employment, ALMPs and outside the labour force. The Cox proportional hazards model was used. Long unemployment spells were right-censored from 10 months onwards. The model also includes indicators for REO areas (12), year-month indicators (6), indicators for the fields of education (12), previous occupation (6), family status (4) and the number of unemployment months during the previous two years (8). Standard errors were clustered at the travel-to-work area level (50 clusters). Significance levels: *** 1%, ** 5% and * 10%. See also Table A5 in the Online Appendix.

Manning (2009) found that a major change to the UK system of welfare support for the unemployed, with stricter enforcement of eligibility conditions, resulted in large flows out of claimant status but not into employment. Closely related, unemployment benefit sanctions have been reported to increase exits from unemployment outside the labour force (e.g. Arni et al., 2013; Busk, 2016). After the Finnish reform, the imposition of sanctions increased (see Table A10 in the Online Appendix). However, our results indicated that the Finnish reform did not increase the total flow out of the labour force (column 3 in Table 5).

The control variables that described statistical relationships provided estimates consistent with prior evidence (see Kyyrä et al., 2019; Busk, 2016; Svarer, 2011; Uusitalo & Verho, 2010). First, the probability of transitions decreases with age. Second, higher education is associated with higher employment hazards and lower ALMP hazards. Third, having a disability that affects the ability to work is associated with lower employment transition rates and higher transition rates to ALMPs and outside the labour force. Fourth, immigrants have a lower re-employment hazard rate than native Finns but a higher ALMP hazard rate. Fifth, many unemployment months in year y-2 were associated with lower employment hazards. Unemployment months in the preceding year seemed to matter less, probably because all the individuals in the sample had some recent work experience. Sixth, a high regional unemployment rate was associated with lower exit rates to ALMPs. The estimates for regional economic growth and the regional vacancy rate were not statistically significant.

### 2.5.2 Heterogeneous Effects

Table 6 reports the heterogeneous treatment effects for the various subgroups. Treatment effects on employment hazards were particularly high for jobseekers aged 25–34 (column 1): a 10-percentage-point increase in treatment intensity increased the rate of transition to employment by 5.8% ( = (exp(10 × 0.0056) - 1) x 100%). For the other age groups, treatment effects on employment hazards were lower and not statistically significant. Moreover, treatment effects on employment hazards were particularly high for jobseekers with a low education level (5.4%) and those whose field of education was services (8.1%). In turn, treatment effects on re-employment hazards were low for the highly educated and for immigrants. Treatment effects on employment hazards were significant for women (3.3%) but not for men. According to Bergemann and van den Berg (2008), the majority of studies on ALMPs have found more positive employment effects for women than for men.

TABLE 6 Results by subgroup and outcome

| | Hazard to employment (1) | Hazard to ALMPs (2) | Hazard outside the labour force (3) | Number of observations (4) |
|---|---|---|---|---|
| Baseline result | 0.0031*** | 0.0188** | −0.0098 | 42,877 |
| | (0.0011) | (0.0081) | (0.0061) | |
| Male | 0.0025 | 0.0159** | −0.0148* | 18,294 |
| | (0.0016) | (0.0075) | (0.0079) | |
| Female | 0.0032** | 0.0214** | −0.0070 | 24,583 |
| | (0.0014) | (0.0091) | (0.0066) | |
| Immigrant | −0.0023 | 0.0112 | 0.0143 | 3,479 |
| | (0.0043) | (0.0126) | (0.0221) | |
| **Age** | | | | |
| 20-24 | 0.0042 | 0.0103 | −0.0227** | 5,346 |
| | (0.0038) | (0.0066) | (0.0111) | |
| 25-34 | 0.0056*** | 0.0171 | −0.0091 | 11,566 |
| | (0.0018) | (0.0120) | (0.0105) | |
| 35-44 | −0.0017 | 0.0219* | 0.0015 | 8,793 |
| | (0.0022) | (0.0117) | (0.0232) | |
| 45-54 | 0.0028 | 0.0120 | 0.0050 | 9,194 |
| | (0.0025) | (0.0086) | (0.0111) | |
| 55-62 | 0.0021 | 0.0394*** | −0.0243 | 7,978 |
| | (0.0036) | (0.0095) | (0.0157) | |
| **Education level** | | | | |
| Secondary | 0.0053*** | 0.0132** | −0.0043 | 22,262 |
| | (0.0018) | (0.0052) | (0.0062) | |
| Lower tertiary | −0.0067** | 0.0289** | −0.0325** | 6,001 |
| | (0.0031) | (0.0115) | (0.0149) | |
| Master's degree or | −0.0018 | 0.0458** | − 0.0161 | 5,300 |
| higher | (0.0048) | (0.0193) | (0.0310) | |
| Field of education | 0.0078** | 0.0274*** | 0.00446 | 5,317 |
| services | (0.0037) | (0.0085) | (0.0113) | |

Notes: Coefficient estimates for $Treatment\ Intensity$ x $I(YEAR \geq 2017)$ from separate models for the various subgroups. The Cox proportional hazards model was used. Long unemployment spells were right-censored from 10 months onwards. The treatment effects of a one-percentage-point increase in treatment intensity can be calculated as follows: (exp($\delta$) - 1) x 100%. The models included the same control variables as the models in Table 5. Standard errors, reported in parentheses, were clustered at the travel-to-work area level (50 clusters). Significance levels: *** 1%, ** 5% and * 10%. Table A8 in the Online Appendix shows results with group-specific treatment intensities.

As shown in Table 6, we used the average treatment intensity in a given REO area. As outlined in the Online Appendix, we investigated the robustness of the results by providing additional estimations with group-specific treatment intensities. In Online Appendix Table A7, the estimation of group-specific interview probabilities is illustrated. The table shows which groups had the highest interview rates in 2015–2016, and the interaction terms show how interview rates changed in 2017. Before the reform, the interview rates were relatively higher among younger jobseekers, and highly educated jobseekers had lower interview rates. After the reform, interview rates increased particularly for the highly educated. In Table A8, we provide results with group-specific treatment intensities. Overall, the results were similar to those of Table 6. The results indicated that employment effects were strongest for individuals aged 25–34, women and individuals with a low education level. Thus, interviewing these groups was particularly beneficial. In turn, the smaller employment effects for older and highly educated workers were not driven by lower interview probabilities for these subgroups.

Column 2 in Table 6 reports the heterogeneous treatment effects on ALMP hazards, which were particularly high for jobseekers aged 55–62 (48%) and for the highly educated (34% and 58%). The results with group-specific treatment intensities were similar (see column 2 in Table A8). We documented that interview rates increased particularly for the highly educated. For them, the intensifying of interviews resulted in a large flow to ALMPs but not into employment. However, it should be noted that many ALMPs with insignificant or even negative impacts after a year have significantly positive impact estimates after two or three years (Card et al., 2010).

According to our results, high treatment intensities were not associated with higher hazards outside the labour force for any subgroup. We found evidence (significant at 5%) of negative treatment effects on hazards outside the labour force for individuals aged 20–24 and individuals with a lower tertiary education (column 3 in Table 6). This suggests that interviews may encourage some groups to continue their job searches and stay in the labour force.

### 2.5.3 Robustness Checks

We examined the robustness of our baseline results in various ways (see Table 7). First, we estimated the model without control variables (column 1), after which we gradually increased the number of control variables. Our results were robust to different specifications regarding control variables (see Table A6 in the Online Appendix). Second, we limited the analysis to unemployed jobseekers aged 25–55 because the eligibility criteria for unemployment benefits are stricter for individuals under 25 years of age, whereas the elderly unemployed have special provisions for unemployment benefits (Kyyrä & Pesola, 2020; Ilmakunnas & Ilmakunnas, 2015). The estimates (column 2) were slightly lower but also less precise because the sample size was smaller. Third, we used data from all 15 REOs and estimated the baseline model (column 3). The treatment effect on ALMP hazards was lower, likely because the two REOs had different pre-trends.

Fourth, to complement the main results and alleviate the concern about anticipation effects for the 2016 cohort, we performed the estimations again, using only the 2015 cohort as a control group (Table 7 Column 4). The results of this robustness check were similar, but the estimates were less precise because the sample size was smaller. The treatment effect on employment hazards was slightly stronger, while the effect on ALMPs hazards was weaker. Moreover, we found a significantly negative effect on the hazards to outside the labour force.

TABLE 7 Homogeneous results by outcome: sensitivity checks

| | Without control variables (1) | Jobseekers aged 25-55 (2) | All 15 REOs (3) | 2015 and 2017 cohorts (4) | Top 3 vs. Bottom 3 REOs (5) |
|---|---|---|---|---|---|
| **Results by outcome:** | | | | | |
| to employment | 0.0041*** | 0.0024** | 0.0029*** | 0.0048*** | 0.0761** |
| | (0.0012) | (0.0012) | (0.0010) | (0.0014) | (0.0315) |
| to ALMPs | 0.0183** | 0.0169* | 0.0145* | 0.0166* | 0.515** |
| | (0.0083) | (0.0101) | (0.0075) | (0.0092) | (0.222) |
| to outside the labour force | –0.0125** | –0.0059 | –0.0014 | –0.0143** | –0.0803 |
| | (0.0057) | (0.0075) | (0.0049) | (0.0072) | (0.1740) |
| **Controls:** | | | | | |
| Regional controls | No | Yes | Yes | Yes | Yes |
| Individual controls | No | Yes | Yes | Yes | Yes |
| REO area indicators | Yes | Yes | Yes | Yes | Yes |
| Year-quarter indicators | Yes | Yes | Yes | Yes | Yes |
| Clusters | 50 | 50 | 67 | 50 | 24 |
| N (unemployment spells) | 42,877 | 30,453 | 56,412 | 28,619 | 15,511 |

Notes: Columns 1–4: Coefficient estimates for $Treatment\ Intensity$ x $I(YEAR \geq 2017)$ from separate models: Hazard rates from unemployment to employment, to ALMPs and outside the labour force. The treatment effects of a one-percentage-point increase in treatment intensity can be calculated as follows: (exp($\delta$) – 1) x 100%. Column 5: Coefficient estimates for $Top3$ x $I(YEAR \geq 2017)$ using only the data from the top and bottom three REOs. The Cox proportional hazards model was used. Long unemployment spells were right-censored from 10 months onwards. The models in Columns 2–5 included the same control variables as the models in Table 5. Standard errors, reported in parentheses, were clustered at the travel-to-work area level. Significance levels: *** 1%, ** 5% and * 10%.

Fifth, while the variable of interest in the econometric analysis was a continuous measure of treatment intensity, column 5 reports the results of the duration analysis, in which the top three REOs were used as a treatment group and the bottom three REOs served as a comparison group. The results show that employment exit rates increased by 7.9% ((exp(0.0761) – 1)*100%) in the top three REOs compared to the bottom three REOs. The exits to ALMPs increased by 67.4% ((exp(0.515) – 1)*100%) in the top three REOs compared to the bottom three REOs.

Sixth, we considered the treatment effects on different outcomes (see Table A9 in the Online Appendix). We found evidence of positive treatment effects on annual employment months: A 10-percentage-point increase in treatment intensity increased the number of annual employment months and decreased the number of annual unemployment months by about 0.1 months (3 days). The effect on disposable income was not statistically significant.

Our analysis was based on the assumption that an increase in the number of interviews did not affect their quality. Although the government has provided financial support for the REOs to implement the reform, Valtakari et al. (2019) argued that the reform reduced the quality of the interviews. According to their study, the intensification of interviews increased the personal workload of the

PES caseworkers and weakened their well-being at work. According to Hainmueller et al. (2016), caseload influences the effectiveness of JSA because it determines how much time a caseworker can devote to each client. They found that unemployed jobseekers who were counselled in PES offices with lower caseloads were more successful in finding jobs.

Table A10 indicates that the average quality of the interviews may have deteriorated after the reform. In particular, the number of face-to-face meetings decreased considerably. According to Vehkasalo (2020), face-to-face counselling is more efficient than online or telephone counselling in reducing unemployment duration. The changes in the number of face-to-face meetings, vacancy referrals and wage support offers indicate that the quality of the interviews seems to have suffered more in the top three REOs, where the number of interviews increased the most. This suggests that our estimates might be biased downwards, meaning that the positive effects on the exit rates would be greater with a standardisation of interview quality.

### 2.5.4 Possible Mechanisms

We considered five possible channels behind the effects: (1) increased JSA, (2) stricter monitoring, (3) threat effects, (4) faster ALMP transitions and (5) enhanced operating effectiveness of the PES. Supporting material can be found in the Online Appendix.

First, to support job searches and boost job search intensity, the reform intensified the interviews and increased their volume (see Figure 1, and Table A10). According to Valtakari et al. (2019), many jobseekers found that the interviews were motivating and supported their job searches. Existing evidence indicates the positive effects of JSA on re-employment (e.g. Card et al., 2010, 2018; Kluve, 2010; Vooren et al., 2019). Altmann et al. (2018) reported that encouraging unemployed jobseekers and providing them with information about the importance of active job searches can increase their prospects of finding a job. Belot et al. (2019) discovered that an online tool that provides tailored advice to jobseekers can broaden their searches and thereby increase the number of job interviews for which they are selected. According to our results, interviewing low-educated and young jobseekers is particularly beneficial.

Second, the reform led to tighter monitoring of job searches, and the imposition of sanctions increased (see Table A10). According to the literature, combining JSA with regular job search monitoring and sanctions for non-compliance seems to generate the most favourable outcomes (e.g. McGuinness et al., 2019; McVicar, 2008; Hägglund, 2014). Gorter and Kalb (1996) noted that counselling and monitoring encourage people to submit more applications. Arni and Schiprowski (2019) found that unemployment duration decreased by 3% when job search requirements increased by one monthly job application. Like us, they found that the effects were heterogeneous and strongest among lower-skilled jobseekers. They reported that the number of imposed benefit sanctions rose by 12% per required monthly application. Unemployment benefit sanctions have been reported to increase exits from unemployment to employment but also outside the labour force (e.g. Lalive et al., 2005, Arni et al., 2013; Busk, 2016; Svarer, 2011). Moreover, previous research has reported that sanctioned individuals often accept jobs with shorter durations and lower earnings than do non-sanctioned individuals (e.g. van den Berg & Vikström, 2014; Arni et al., 2013).

Third, in addition to the direct treatment effect of the interviews, the reform likely had considerable threat effects, including a higher risk of being interviewed in the near future. The reform had been widely reported in the news beforehand. Previous studies have found that unemployed individuals are considerably more likely to find a job when facing the threat of having to participate in mandatory ALMPs (e.g. Rosholm & Svarer, 2008). Van den Berg et al. (2009) reported a positive ex-ante effect on search effort and a negative effect on the reservation wage. This means that threat effects can make individuals search harder and accept lower-quality jobs. Threat effects affect all jobseekers, including individuals who are not interviewed. However, threat effects were likely to be similar in all REO areas because jobseekers were barely aware of regional differences in interview probabilities. Figure 4 shows that, in 2017, employment hazards also increased in the bottom three REOs, which may be at least partly because of threat effects.

Fourth, the reform increased ALMP transitions (see Tables 5 and 6). Helping unemployed jobseekers to exit more swiftly to ALMPs may increase their likelihood of employment. In Finland, ALMPs include employment with wage subsidies, labour market training, coaching and work trials, rehabilitation work and self-motivated studies with unemployment benefits. In the top three REOs, coaching and work trials as exit destinations particularly increased (see Table A3). Enhanced services, including training programmes and JSA, seem to be the most effective in the short run, while private sector wage subsidies have the greatest effects in the long term (e.g. Vooren et al., 2019; Kluve, 2010; Sianesi, 2004). The so-called lock-in effects are relevant: Some ALMPs can take quite a long time, and during the period of programme participation, participants may put less effort into a job search (Vooren et al., 2019).

Fifth, the reform may have increased the operating effectiveness of the PES. Valtakari et al. (2019) reported that, before 2017, there were large regional differences in the implementation of interviews, and these differences were related to how unemployed jobseekers were employed. They also reported that, after the reform, regional differences in the implementation of interviews decreased, which seems to have reduced the regional differences in the matching efficiency. Launov and Wälde (2016) highlighted that reforming the PES can reduce unemployment. They compared the effects of reducing unemployment benefits and reorganising the PES's operations and found that the enhanced effectiveness of the PES explains more of the observed post-reform decline in unemployment than changing the monetary compensation scheme for unemployed workers.

## 2.6 Conclusions

In 2017, a large-scale policy reform in Finland increased the frequency of interviews with unemployed jobseekers at local public employment offices. This paper contributes to the existing JSA literature by providing quasi-experimental evidence on the effects of periodic interviews. We used a difference-in-differences approach that exploited regional variations in treatment intensity and considered possible displacement effects on non-treated jobseekers.

The analysis yielded four key findings. First, the interviews had a robust effect on employment transitions. A 10-percentage-point increase in the interview probability increased the monthly hazard rate of employment by approximately 3.1%. This positive employment effect is in line with the previous literature on JSA, but its magnitude is smaller compared to the studies that ignored displacement effects.

Second, our results showed a strong effect on the exit rate to ALMPs: a 10-percentage-point increase in treatment intensity increased hazards to ALMPs by 21%. Helping unemployed jobseekers to exit more swiftly to ALMPs may increase their likelihood of employment in the future. However, the effects are not necessarily immediate. Third, although the reform led to tighter monitoring and the imposition of sanctions increased, it appears to have not increased the total flow out of the labour force. According to the previous research, stricter monitoring and sanctions may increase transitions outside the labour force.

Fourth, we observed heterogeneous treatment effects for the various subgroups. Treatment effects on employment hazards were high for jobseekers aged 25–34 years and for jobseekers with a low education level. According to the results, interviewing these groups is particularly beneficial. We also found that treatment effects on ALMP hazards were particularly strong among jobseekers aged 55–62 and jobseekers with a high education level.

Possible channels behind these effects include increased JSA, stricter monitoring and threat effects. The reform intensified interviews and increased their volume to support job searches. It also led to tighter monitoring of job searches, and the imposition of sanctions increased. The reform likely had considerable threat effects, also affecting unemployed jobseekers who were not interviewed. Moreover, the reform increased ALMP transitions and may have also increased the operating effectiveness of the PES.

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# Appendix

## Appendix 1: REO Areas and common trends

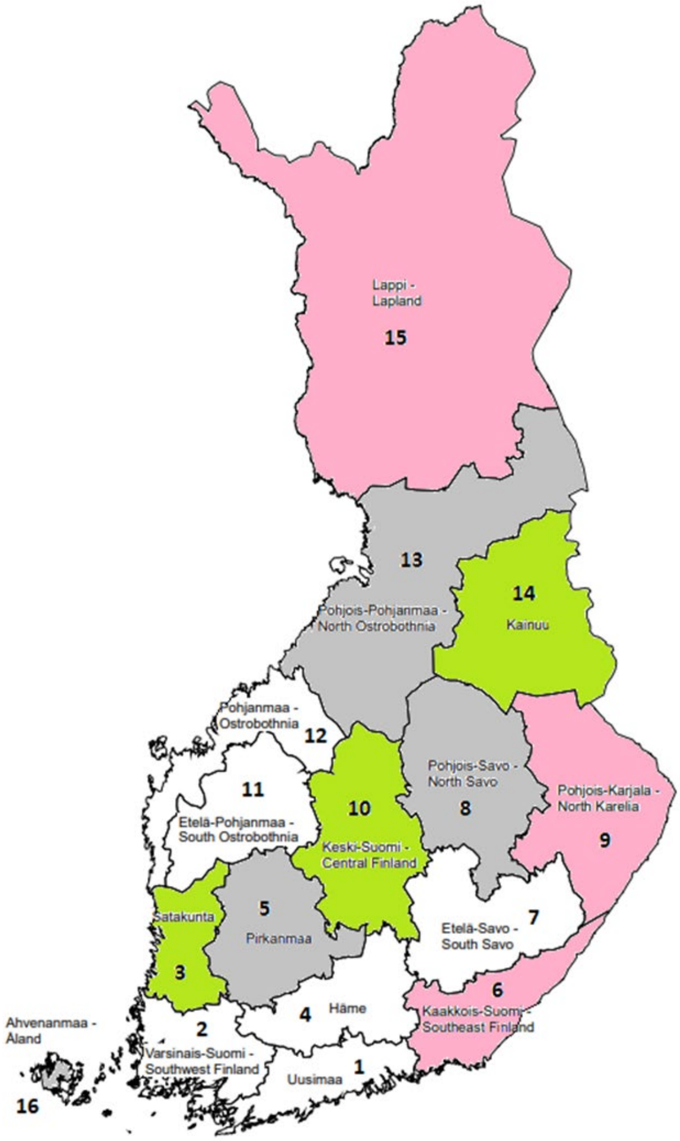


FIGURE A1 Administrative districts of the Ministry of Economic Affairs and Employment. Notes: The top three regional employment office (REO) areas where treatment intensity was highest (green): Central Finland (10), Kainuu (14) and Satakunta (3). The bottom three REO areas where treatment intensity was lowest (red): Southeast Finland (6), North Karelia (9) and Lappi (15). The REO areas North-Ostrobothnia (13), North Savo (8) and Pirkanmaa (5) did not meet the parallel trend assumption, See Table A1 and A2.

TABLE A1 Test for the parallel trend assumption

| | Hazard to employment (1) | Hazard to ALMPs (2) | Hazard to outside the labour force (3) |
|---|---|---|---|
| REO area dummies (15) | Yes | Yes | Yes |
| Year 2016 | 0.087 | -0.053 | -0.423* |
| **Interaction terms** (vs. D2016 x REO15) | | | |
| D2016 x REO1 | -0.007 | -0.037 | 0.362 |
| D2016 x REO2 | -0.061 | 0.149 | 0.503 |
| D2016 x REO3 | 0.083 | -0.093 | 0.316 |
| D2016 x REO4 | -0.020 | -0.024 | 0.474 |
| D2016 x REO5 | 0.068 | -0.333** | 0.426 |
| D2016 x REO6 | 0.005 | 0.164 | 0.535* |
| D2016 x REO7 | 0.106 | 0.386* | 0.259 |
| D2016 x REO8 | -0.107 | 0.397** | 0.029 |
| D2016 x REO9 | -0.095 | 0.087 | 0.030 |
| D2016 x REO10 | 0.132* | 0.072 | 0.131 |
| D2016 x REO11 | -0.011 | -0.027 | 0.228 |
| D2016 x REO12 | 0.120 | -0.075 | 0.311 |
| D2016 x REO13 | 0.232*** | -0.178 | 0.513* |
| D2016 x REO14 | -0.066 | 0.037 | 0.532 |
| Number of observations | 39,049 | 39,049 | 39,049 |
| F-test for the interactions | 48.63 | 50.91 | 9.95 |
| P-value | 0.000 | 0.000 | 0.766 |

Notes: Estimates for hazard rates from unemployment to employment, ALMPs, and outside the labour force. Data: New unemployment spells that started in January and February of 2015–2016. The Cox proportional hazards model was used. The models included indicators for REO areas (15), a dummy for the year 2016, and D2016*REO interactions (15). Significance levels: *** 1%, ** 5%, * 10%.

TABLE A2 Test for the parallel trend assumption, without REOs 5, 8 and 13

| | Hazard to employment (1) | Hazard to ALMPs (2) | Hazard to outside the labour force (3) |
|---|---|---|---|
| REO area dummies (15) | Yes | Yes | Yes |
| Year 2016 | 0.087 | -0.053 | -0.424* |
| **Interaction terms** (vs. D2016 x REO15) | | | |
| D2016 x REO1 | -0.008 | -0.037 | 0.363 |
| D2016 x REO2 | -0.061 | 0.149 | 0.503 |
| D2016 x REO3 | 0.083 | -0.094 | 0.315 |
| D2016 x REO4 | -0.020 | -0.24 | 0.475 |
| D2016 x REO6 | 0.005 | 0.164 | 0.535 |
| D2016 x REO7 | 0.106 | 0.396* | 0.259 |
| D2016 x REO9 | -0.95 | 0.086 | 0.030 |
| D2016 x REO10 | 0.133* | 0.072 | 0.131 |
| D2016 x REO11 | -0.011 | -0.026 | 0.229 |
| D2016 x REO12 | 0.119 | -0.074 | 0.312 |
| D2016 x REO14 | -0.066 | 0.037 | 0.532 |
| Number of observations | 29,545 | 29,545 | 29,545 |
| F-test for the interactions | 17.05 | 13.57 | 7.15 |
| P-value | 0.106 | 0.258 | 0.787 |

Notes: Estimates for hazard rates from unemployment to employment, ALMPs, and outside the labour force. Data: New unemployment spells that started in January and February of 2015–2016, without REOs Pirkanmaa (5), North Savo (8) and North Ostrobothnia (13). The Cox proportional hazards model was used. The models included indicators for REO areas (12), a dummy for the year 2016, and D2016*REO interactions (12). Significance levels: *** 1%, ** 5%, * 10%.

## Appendix 2: Empirical Hazard Rates and Exits from Unemployment

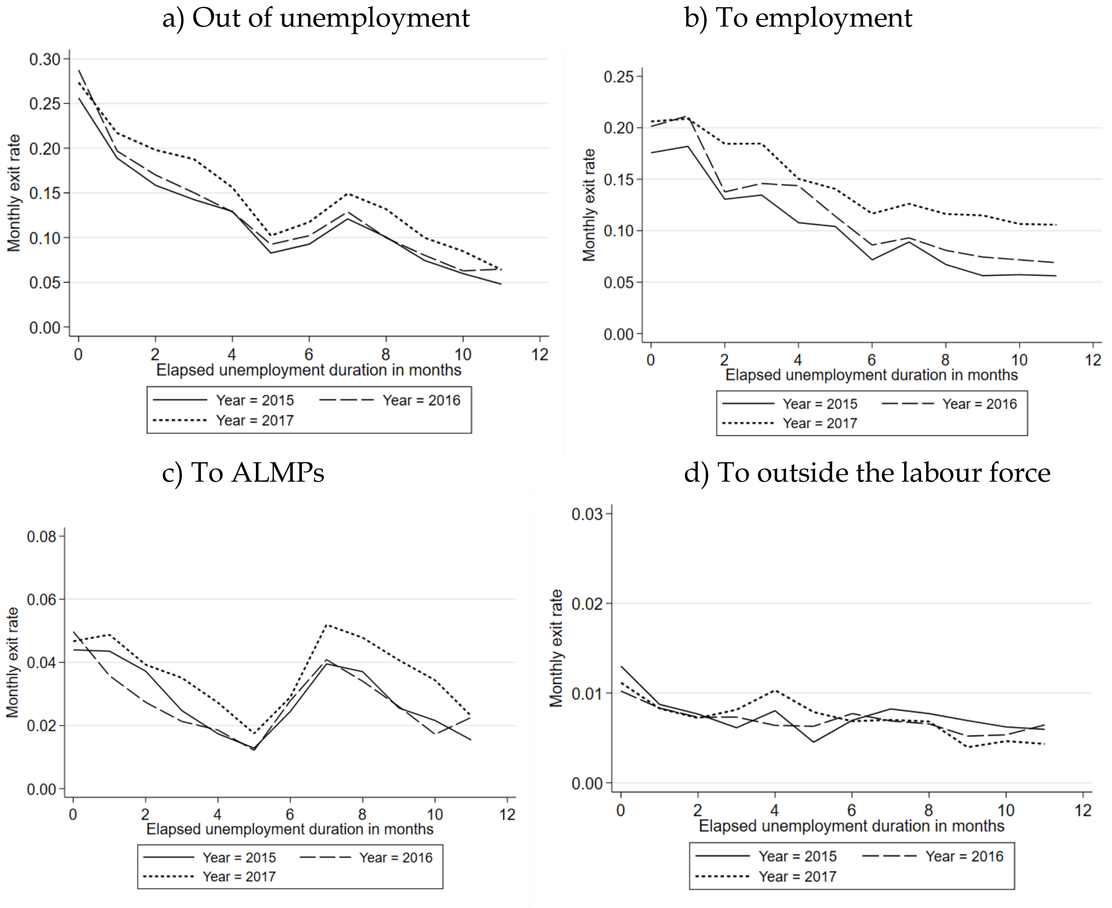


FIGURE A2 Empirical hazard rates out of unemployment and transitions to employment, ALMPs and outside the labour force. Notes: New unemployment spells that started in January and February of 2015–2017 and were preceded by a work period of at least 30 days. Long periods of unemployment were censored at 12 months.

Table A3 summarises the exits from unemployment during the first 365 days of the unemployment spells. In 2015-2016, about 50% of the unemployment spells ended directly in employment in the general labour market, about 16% ended in ALMPs, about 23% stayed unemployed and 11% exited to other destinations (column 1). In 2017, the share of the unemployment spells, which experienced no exits during the first 365 days, decreased by 5.7 percentage points (column 3). The share of exits to ALMPs increased by 2.5 percentage points and exits to employment increased by 0.9 percentage points. Thus, the reform seems associated with more exits from unemployment to employment and ALMPs.

ALMPs include employment with wage subsidies, labour market training, coaching and work trials, rehabilitation work and self-motivated studies with unemployment benefits. Although individuals participating ALMPS often receive UI benefits, these individuals are no longer classified as unemployed by the MEE register. The data show that after the reform, more unemployment spells ended in ALMPs. In the top three REOs, their share rose by 8.0 percentage points, while in the bottom three REOs, it rose by 1.6 percentage points. In the top three REOs, the portions of coaching and work trials as exit destinations particularly increased.

Valtakari et al. (2019) documented that the reform updated unemployment records by removing unemployed jobseekers from the data who had been employed or who had retired without employment offices knowing about this. In our sample, in 2015-2016, about 5% of unemployment spells ended because 'the job search was not renewed'. After the reform, this share increased by 2.6 percentage points, with no difference between the top and bottom three REOs. The share of exits outside the labour decreased more in the top REOs after the reform.

TABLE A3 Exits from unemployment during 365 days

| | All 2015-2016 (1) | All 2017 (2) | All Change (3) | Top 3 Change (4) | Bottom 3 Change (5) | DiD (6) |
|---|---|---|---|---|---|---|
| **Employment** | **50.2** | **51.1** | **0.9** | **1.2** | **-0.2** | **+1.4** |
| **ALMPs** | **15.6** | **18.1** | **2.5** | **8.0** | **1.6** | **+6.4** |
| Coaching & work trials | 5.6 | 7.6 | 2.0 | 8.8 | 0.2 | +8.6 |
| Labour market training | 4.3 | 4.6 | 0.3 | 0.4 | 0.4 | +0.0 |
| Wage subsidies | 2.5 | 2.9 | 0.4 | 0.5 | 0.3 | +0.2 |
| Self-motivated studies with UE benefits | 2.7 | 2.3 | -0.4 | -1.6 | -0.2 | -1.4 |
| Rehabilitating work | 0.4 | 0.7 | 0.3 | 0.0 | 0.9 | -0.9 |
| **Other** | **11.2** | **13.6** | **2.4** | **0.3** | **1.1** | **-0.8** |
| No job search renewal | 5.4 | 8.0 | 2.6 | 1.4 | 1.4 | +0.0 |
| Outside the labour force | 5.4 | 5.0 | -0.4 | -1.2 | -0.2 | -1.0 |
| Unknown | 0.5 | 0.6 | 0.1 | 0.2 | -0.2 | +0.4 |
| **Unemployed** | **22.9** | **17.2** | **-5.7** | **-9.6** | **-2.4** | **-7.2** |
| **Observations** | 29,545 | 13,332 | | | | |

Notes: New unemployment spells that started in January and February of 2015–2017. Each unemployment spell was preceded by a work period of at least 30 days. Columns 1–3: All unemployment spells. Column 4: Unemployment spells in the top three REO areas where treatment intensity was highest. Column 5: Unemployment spells in the bottom three REO areas, where treatment intensity was lowest. Column 6: Difference-in-differences (Δtop3 – Δbottom3).

## Appendix 3: Summary Statistics

TABLE A4 Summary statistics before and after the reform

| | Bottom 3 REOs Average | | Middle 6 REOs Average | | Top 3 REOs Average | |
|---|---|---|---|---|---|---|
| | 2015-16 | 2017 | 2015-16 | 2017 | 2015-16 | 2017 |
| Variable name | (1) | (2) | (3) | (4) | (5) | (6) |
| Regional unempl. rate (%) | 17.7 | 17.3 | 12.8 | 12.4 | 17.3 | 16.3 |
| Regional vacancy rate (%) | 0.72 | 0.92 | 0.97 | 1.05 | 0.83 | 1.30 |
| Reg. economic growth (%) | 0.83 | 2.28 | -0.40 | 2.16 | 1.38 | 1.54 |
| Age | | | | | | |
| 20-24 | 0.13 | 0.13 | 0.12 | 0.12 | 0.13 | 0.12 |
| 25-34 | 0.25 | 0.24 | 0.28 | 0.28 | 0.25 | 0.26 |
| 35-44 | 0.20 | 0.21 | 0.21 | 0.21 | 0.19 | 0.19 |
| 45-54 | 0.22 | 0.21 | 0.21 | 0.21 | 0.22 | 0.21 |
| 55-62 | 0.20 | 0.22 | 0.17 | 0.18 | 0.20 | 0.22 |
| Immigrant | 0.06 | 0.06 | 0.10 | 0.11 | 0.04 | 0.04 |
| Disability | 0.09 | 0.10 | 0.08 | 0.08 | 0.11 | 0.09 |
| Married | 0.37 | 0.34 | 0.36 | 0.34 | 0.38 | 0.36 |
| Family status | | | | | | |
| Male, no children under 3 | 0.39 | 0.41 | 0.40 | 0.39 | 0.38 | 0.37 |
| Male, children under 3 | 0.03 | 0.03 | 0.04 | 0.03 | 0.03 | 0.03 |
| Female, children under 3 | 0.04 | 0.03 | 0.03 | 0.03 | 0.04 | 0.04 |
| Female, no childr. under 3 | 0.54 | 0.54 | 0.53 | 0.55 | 0.55 | 0.56 |
| Educational level: | | | | | | |
| Secondary | 0.58 | 0.59 | 0.48 | 0.49 | 0.58 | 0.58 |
| Lowest tertiary | 0.08 | 0.08 | 0.08 | 0.08 | 0.09 | 0.08 |
| Lower tertiary | 0.14 | 0.13 | 0.14 | 0.15 | 0.13 | 0.14 |
| Master's degree or higher | 0.08 | 0.08 | 0.15 | 0.14 | 0.09 | 0.09 |
| Other, unknown | 0.12 | 0.13 | 0.15 | 0.14 | 0.12 | 0.11 |
| Number of observations | 5,850 | 2,656 | 18,791 | 8,575 | 4,904 | 2,101 |
| Field of education: | | | | | | |
| General | 0.04 | 0.04 | 0.07 | 0.07 | 0.04 | 0.04 |
| Education | 0.01 | 0.02 | 0.01 | 0.01 | 0.02 | 0.02 |
| Humanities and arts | 0.05 | 0.05 | 0.08 | 0.08 | 0.05 | 0.06 |
| Social studies | 0.02 | 0.01 | 0.03 | 0.03 | 0.01 | 0.02 |
| Trade, administration, law | 0.14 | 0.15 | 0.16 | 0.18 | 0.15 | 0.16 |
| Science | 0.01 | 0.01 | 0.02 | 0.02 | 0.02 | 0.02 |
| ICT | 0.02 | 0.03 | 0.04 | 0.04 | 0.03 | 0.02 |
| Technology | 0.25 | 0.25 | 0.19 | 0.18 | 0.23 | 0.22 |
| Agriculture and Forestry | 0.04 | 0.04 | 0.03 | 0.02 | 0.03 | 0.03 |
| Health & well-being | 0.17 | 0.15 | 0.11 | 0.13 | 0.16 | 0.17 |
| Services | 0.14 | 0.15 | 0.11 | 0.12 | 0.14 | 0.14 |
| Other, unknown | 0.11 | 0.12 | 0.15 | 0.13 | 0.12 | 0.10 |

TABLE A4 (Continued)

| Variable name | Bottom 3 REOs Average 2015-16 (1) | 2017 (2) | Middle 6 REOs Average 2015-16 (3) | 2017 (4) | Top 3 REOs Average 2015-16 (5) | 2017 (6) |
|---|---|---|---|---|---|---|
| Previous occupation | | | | | | |
| Lower officer | 0.39 | 0.39 | 0.38 | 0.39 | 0.38 | 0.39 |
| Senior officer | 0.11 | 0.10 | 0.18 | 0.17 | 0.11 | 0.11 |
| Industrial worker | 0.11 | 0.11 | 0.08 | 0.08 | 0.11 | 0.11 |
| Production worker | 0.10 | 0.12 | 0.09 | 0.08 | 0.11 | 0.10 |
| Distribution or service | 0.19 | 0.19 | 0.16 | 0.17 | 0.18 | 0.17 |
| Other | 0.11 | 0.10 | 0.11 | 0.11 | 0.11 | 0.13 |
| UE months in year y-1 | | | | | | |
| 0-3 | 0.71 | 0.72 | 0.78 | 0.76 | 0.71 | 0.68 |
| 4-6 | 0.17 | 0.16 | 0.13 | 0.12 | 0.17 | 0.17 |
| 7-9 | 0.09 | 0.10 | 0.07 | 0.09 | 0.10 | 0.12 |
| 10-12 | 0.03 | 0.02 | 0.03 | 0.03 | 0.03 | 0.03 |
| UE months in year y-2 | | | | | | |
| 0-3 | 0.66 | 0.63 | 0.73 | 0.68 | 0.65 | 0.59 |
| 4-6 | 0.13 | 0.14 | 0.10 | 0.12 | 0.12 | 0.14 |
| 7-9 | 0.09 | 0.10 | 0.08 | 0.08 | 0.10 | 0.11 |
| 10-12 | 0.12 | 0.13 | 0.09 | 0.12 | 0.13 | 0.16 |
| Number of observations | 5,850 | 2,656 | 18,791 | 8,575 | 4,904 | 2,101 |

Notes: Data contain new unemployment spells that started in January and February of 2015–2017 and were preceded by a work period of at least 30 days. Columns 1-2: Unemployment spells in the bottom three REO areas with the lowest treatment intensities. Columns 3-4: Unemployment spells in the middle six REO areas. Column 5-6: Unemployment spells in the top three REO areas with the highest treatment intensities. The REO areas are shown in Table 1.

## Appendix 4: Results

TABLE A5 Baseline results by outcome

| | Hazard to employment (1) | Hazard to ALMPs (2) | Hazard to outside the labour force (3) |
|---|---|---|---|
| Treatment Intensity x I(YEAR ≥ 2017) | 0.0031*** | 0.0188** | –0.0098 |
| | (0.0011) | (0.0081) | (0.0061) |
| **Regional characteristics** | | | |
| Unemployment rate | –0.0094 | –0.0261** | 0.0059 |
| | (0.0058) | (0.0118) | (0.0139) |
| Output growth | –0.0032 | –0.0008 | –0.0035 |
| | (0.0026) | (0.0048) | (0.0055) |
| Vacancy rate | –0.0059 | –0.0236 | –0.0433 |
| | (0.0231) | (0.0508) | (0.0528) |
| **Individual characteristics** | | | |
| Age (vs. 20-24) | | | |
| 25-34 | –0.204*** | –0.108*** | –0.564*** |
| | (0.021) | (0.040) | (0.062) |
| 35-44 | –0.364*** | –0.293*** | –0.776*** |
| | (0.038) | (0.040) | (0.098) |
| 45-54 | –0.485*** | –0.433*** | –0.590*** |
| | (0.061) | (0.052) | (0.090) |
| 55-62 | –0.937*** | –1.260*** | –0.551*** |
| | (0.076) | (0.073) | (0.093) |
| Immigrant | –0.272*** | 0.304*** | –0.004 |
| | (0.021) | (0.041) | (0.089) |
| Disability | –0.370*** | 0.236*** | 0.801*** |
| | (0.038) | (0.047) | (0.073) |
| **Education level** (vs. upper secondary) | | | |
| Lowest tertiary | 0.007 | 0.161*** | 0.088 |
| | (0.020) | (0.041) | (0.087) |
| Lower tertiary | 0.159*** | –0.061 | –0.049 |
| | (0.019) | (0.040) | (0.063) |
| Master's degree or higher | 0.190*** | –0.290*** | 0.025 |
| | (0.027) | (0.050) | (0.106) |
| Other, unknown | 0.056 | 0.045 | 0.063 |
| | (0.070) | (0.149) | (0.485) |
| **Family status** (vs. Male without children under 3) | | | |
| Male with children under 3 | .207*** | -.106 | .396*** |
| | (.0345) | (.0685) | (.124) |

TABLE A5 (Continued)

| | | | |
|---|---|---|---|
| Female with children under 3 | -.720*** | -.193*** | 2.68*** |
| | (.0429) | (.0672) | (.0631) |
| Female without children under 3 | .123*** | .153*** | .132*** |
| | (.0361) | (.0537) | (.0504) |
| **Previous occupation** (vs. lower level employees) | | | |
| Upper level employees | .0118 | -.0122 | -.133** |
| | (.0299) | (.0481) | (.0638) |
| Manufacturing workers | .107*** | -.0166 | .0401 |
| | (.0274) | (.0533) | (.0848) |
| Production workers | .0351 | .0497 | .1 |
| | (.0319) | (.0492) | (.0895) |
| Distribution and service workers | -.0629*** | .174*** | .113 |
| | (.0187) | (.0272) | (.0732) |
| Other workers | .0474*** | .0569 | .31*** |
| | (.0168) | (.0395) | (.095) |
| **Field of Education** (vs. Generic programmes) | | | |
| Education | .329*** | -.101 | -.465 |
| | (.0858) | (.0957) | (.334) |
| Arts and humanities | .073* | .0704 | -.475*** |
| | (.0438) | (.0704) | (.129) |
| Social sciences, journalism, information | .125*** | .0323 | -.525*** |
| | (.0455) | (.0825) | (.184) |
| Business, administration, law | .0689*** | -.0424 | -.209* |
| | (.0264) | (.0535) | (.117) |
| Natural sciences, mathematics, statistics | -.00401 | -.136 | -.424** |
| | (.0435) | (.102) | (.17) |
| ICT | -.179*** | .157** | -.147 |
| | (.0385) | (.0617) | (.15) |
| Engineering, manufacturing, construction | .0845*** | .16*** | -.0736 |
| | (.0323) | (.0539) | (.139) |
| Agriculture, forestry, fisheries | .192*** | -.113 | -.28* |
| | (.0447) | (.127) | (.149) |
| Health and welfare | .592*** | -.325*** | .258** |
| | (.0344) | (.0744) | (.109) |
| Services | .251*** | .0486 | -.103 |
| | (.0241) | (.0468) | (.134) |
| Unknown | -.183** | .0297 | -.273 |
| | (.0729) | (.167) | (.503) |

TABLE A5 (Continued)

| | | | |
|---|---|---|---|
| Unemployment months in year -1 (vs. 0-3) | | | |
| 4-6 | .124*** | -.0814* | -.0991 |
| | (.0199) | (.046) | (.0669) |
| 7-9 | .0315 | -.0513 | -.226*** |
| | (.0193) | (.0541) | (.0854) |
| 10-12 | .140*** | .232** | -.165 |
| | (.0475) | (.0977) | (.172) |
| Unemployment months in year -2 (vs. 0-3) | | | |
| 4-6 | -.0152 | -.0803* | -.208** |
| | (.0178) | (.0436) | (.0951) |
| 7-9 | -.153*** | -.171*** | -.312*** |
| | (.0253) | (.0429) | (.103) |
| 10-12 | -.533*** | -.27*** | -.557*** |
| | (.0501) | (.0476) | (.119) |
| N (unemployment spells) | 42,877 | 42,877 | 42,877 |

Notes: Estimates for hazard rates from unemployment to employment, ALMPs, and outside the labour force. The Cox proportional hazards model was used. The model also includes year-month indicators (6), and indicators for REO areas (15). Standard errors were clustered at the travel-to-work area level (50 clusters). Significance levels: *** 1%, ** 5% and * 10%. National classification of education.
https://www2.tilastokeskus.fi/en/luokitukset/koulutus/

TABLE A6 Homogeneous results by outcome: sensitivity checks

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Results by outcome:** | | | | |
| to employment | 0.0041*** | 0.0035*** | 0.0031*** | 0.0031*** |
| | (0.0012) | (0.0012) | (0.0011) | (0.0011) |
| to ALMPs | 0.0183** | 0.0176** | 0.0184** | 0.0188** |
| | (0.0083) | (0.0083) | (0.0079) | (0.0081) |
| to outside the labour force | –0.0125** | –0.0123** | –0.0105* | –0.0098 |
| | (0.0057) | (0.0058) | (0.0060) | (0.0061) |
| **Controls:** | | | | |
| Regional controls | - | Yes | Yes | Yes |
| Demographic and educational controls | - | - | Yes | Yes |
| Controls for previous occupation and unemployment history | - | - | - | Yes |
| REO area indicators (12) | Yes | Yes | Yes | Yes |
| Year-quarter indicators (6) | Yes | Yes | Yes | Yes |
| Clusters | 50 | 50 | 50 | 50 |
| N (unemployment spells) | 42,877 | 42,877 | 42,877 | 42,877 |

Notes: Coefficient estimates for $Treatment\ Intensity$ x $I(YEAR \geq 2017)$ from separate models: Hazard rates from unemployment to employment, to ALMPs, and to outside the labour force. The Cox proportional hazards model was used. The treatment effects of a one-percentage-point increase in treatment intensity can be calculated as follows: (exp($\delta$) – 1) x 100%. Standard errors, reported in parentheses, were clustered at the travel-to-work area level (50 clusters). Significance levels: *** 1%, ** 5% and * 10%.

## Appendix 5: Heterogeneous treatment effects using group-specific treatment intensity

TABLE A7 Group-specific interview probabilities

| Interview probability | Coefficient | s.e. |
|---|---|---|
| Constant term | 0.296*** | 0.016 |
| Male | -0.014*** | 0.001 |
| Immigrant | 0.024*** | 0.005 |
| Age (vs. 20-24) | | |
| 25 - 34 | -0.084*** | 0.011 |
| 35 - 44 | -0.114*** | 0.014 |
| 45 - 54 | -0.125*** | 0.014 |
| 55 - 62 | -0.164*** | 0.019 |
| Education level (vs. Upper secondary) | | |
| Lowest tertiary | -0.006** | 0.003 |
| Lower tertiary | -0.022*** | 0.003 |
| Master's degree or higher | -0.025*** | 0.006 |
| Unknown | -0.022*** | 0.005 |
| Field of Education (vs. Generic programmes) | | |
| Education | -0.036*** | 0.008 |
| Arts and humanities | -0.020*** | 0.007 |
| Social sciences, journalism, information | -0.009 | 0.007 |
| Business, administration, law | -0.015** | 0.006 |
| Natural sciences, mathematics, statistics | -0.014 | 0.009 |
| ICT | -0.001 | 0.006 |
| Engineering, manufacturing, construction | -0.013** | 0.005 |
| Agriculture, forestry, fisheries | -0.023*** | 0.007 |
| Health and welfare | -0.040*** | 0.008 |
| Services | -0.020*** | 0.005 |
| Unknown | 0.008 | 0.005 |
| REO area (vs. REO15) | | |
| 1 | -0.046*** | 0.008 |
| 2 | -0.005 | 0.006 |
| 3 | 0.006 | 0.010 |
| 4 | 0.046* | 0.023 |
| 6 | -0.022** | 0.010 |
| 7 | -0.028* | 0.015 |
| 9 | 0.065*** | 0.007 |
| 10 | -0.052*** | 0.005 |
| 11 | 0.229*** | 0.022 |
| 12 | 0.047*** | 0.007 |
| 14 | 0.124*** | 0.013 |
| Year (vs. 2015) | | |
| 2016 | -0.044*** | 0.005 |
| 2017 | 0.216*** | 0.021 |
| **Interaction terms** | | |
| D2017 x Male | -0.008*** | 0.003 |
| D2017 x Immigrant | -0.058*** | 0.005 |

TABLE A7 (Continued)

| | | |
|---|---|---|
| **Age interactions** (vs. D2017 x 20-24) | | |
| D2017 x Age 25-34 | 0.001 | 0.021 |
| D2017 x Age 35-44 | 0.005 | 0.029 |
| D2017 x Age 45-54 | 0.017 | 0.034 |
| D2017 x Age 55-62 | 0.021 | 0.030 |
| **Education level interactions** (vs. D2017x Upper secondary) | | |
| D2017 x Lowest tertiary | 0.005 | 0.009 |
| D2017 x Lower tertiary | 0.037*** | 0.006 |
| D2017 x Master's degree or higher | 0.030*** | 0.009 |
| D2017 x Unknown | 0.029** | 0.012 |
| **Field of education interactions** (vs. D2017 x Generic programmes) | | |
| D2017 x Education | 0.001 | 0.011 |
| D2017 x Arts and humanities | 0.002 | 0.005 |
| D2017 x Social sciences, journalism, information | 0.006 | 0.018 |
| D2017 x Business, administration, law | 0.025*** | 0.008 |
| D2017 x Natural sciences, mathematics, statistics | 0.018 | 0.017 |
| D2017 x ICT | 0.032** | 0.015 |
| D2017 x Engineering, manufacturing, construction | -0.001 | 0.007 |
| D2017 x Agriculture, forestry, fisheries | 0.008 | 0.010 |
| D2017 x Health and welfare | 0.029*** | 0.010 |
| D2017 x Services | 0.010 | 0.006 |
| D2017 x Unknown | -0.076*** | 0.010 |
| **REO area interactions** (vs. D2017 x REO15) | | |
| D2017 x REO1 | 0.142*** | 0.007 |
| D2017 x REO2 | 0.038 | 0.027 |
| D2017 x REO3 | 0.147*** | 0.012 |
| D2017 x REO4 | 0.027 | 0.035 |
| D2017 x REO6 | -0.034 | 0.021 |
| D2017 x REO7 | 0.060*** | 0.022 |
| D2017 x REO9 | -0.060*** | 0.010 |
| D2017 x REO10 | 0.265*** | 0.008 |
| D2017 x REO11 | -0.060*** | 0.012 |
| D2017 x REO12 | -0.025** | 0.011 |
| D2017 x REO14 | 0.230*** | 0.013 |
| Number of observations | 1,698,515 | |
| R2 | 0.154 | |
| Clusters | 50 | |

Notes: Estimates for the probability of being interviewed during three months of consecutive unemployment. The linear probability model was used. Standard errors were clustered at the travel-to-work area level (50 clusters). Significance levels: *** 1%, ** 5%, * 10%. The data contain unemployed jobseekers who were unemployed on the 28th day of March-September of 2015-2017. Cross-section on the 28th day of each month, unemployment spells of 90–365 days.

Table A7 shows the estimates for group-specific interview probabilities. It shows which groups had the highest interview rates in 2015–2016. The interaction terms show how interview rates changed in 2017. Before the reform, the interview rates were relatively higher among younger jobseekers. Highly educated jobseekers had lower interview rates. After the reform, interview rates increased particularly for the highly educated.

TABLE A8 Results with group-specific treatment intensity, by subgroup and outcome

| | Hazard to employment (1) | Hazard to ALMPs (2) | Hazard outside the labour force (3) | Number of observations (4) |
|---|---|---|---|---|
| Male | 0.0016 | 0.0115 | –0.0146* | 18,294 |
| | (0.0017) | (0.0085) | (0.0077) | |
| Female | 0.0031** | 0.0175* | –0.0073 | 24,583 |
| | (0.0015) | (0.0100) | (0.0064) | |
| Immigrant | 0.0025 | 0.0045 | 0.0105 | 3,479 |
| | (0.0048) | (0.0102) | (0.0201) | |
| Age | | | | |
| 20-24 | 0.0033 | 0.0085 | –0.0155 | 5,346 |
| | (0.0030) | (0.0062) | (0.0108) | |
| 25-34 | 0.0042** | 0.0130 | –0.0083 | 11,566 |
| | (0.0017) | (0.0133) | (0.0103) | |
| 35-44 | –0.0012 | 0.0166 | 0.0051 | 8,793 |
| | (0.0021) | (0.0130) | (0.0173) | |
| 45-54 | 0.0013 | 0.0020 | –0.0033 | 9,194 |
| | (0.0024) | (0.0090) | (0.0122) | |
| 55-62 | 0.0021 | 0.0401*** | –0.0258* | 7,978 |
| | (0.0034) | (0.0083) | (0.0149) | |
| Education level | | | | |
| Secondary | 0.0055*** | 0.0114** | –0.0061 | 22,262 |
| | (0.0018) | (0.0057) | (0.0057) | |
| Lower tertiary | –0.0073** | 0.0259** | –0.0287** | 6,001 |
| | (0.0030) | (0.0115) | (0.0140) | |
| Master's degree or higher | –0.0022 | 0.0380* | – 0.0076 | 5,300 |
| | (0.0041) | (0.0204) | (0.0270) | |
| Field of education services | 0.0058 | 0.0245*** | –0.0028 | 5,317 |
| | (0.0035) | (0.0095) | (0.0099) | |

Notes: Coefficient estimates for $Treatment\ Intensity$ x $I(YEAR \geq 2017)$ from separate models for the various subgroups. The treatment effects of a one-percentage-point increase in treatment intensity can be calculated as follows: (exp($\delta$) - 1) x 100%. The models included the same control variables as the models in Table 5. Standard errors, reported in parentheses, were clustered at the travel-to-work area level (50 clusters). Significance levels: *** 1%, ** 5% and * 10%. Group specific interview probabilities are reported in Table A7.

## Appendix 6: Treatment Effects on Different Outcomes

TABLE A9 Treatment effects on different outcomes in year y

| | Employment months (1) | Unemployment months (2) | Disposable income (3) |
|---|---|---|---|
| | Coeff. (s.e.) | Coeff. (s.e.) | Coeff. (s.e.) |
| Treatment effect | 0.0094** | -0.0083** | 26.29 |
| | (0.0039) | (0.0040) | (16.32) |
| REO area indicators (12) | Yes | Yes | Yes |
| Year-quarter indicators (6) | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes |
| Observations | 42,877 | 42,877 | 42,877 |
| Mean Y | 5.53 | 6.27 | 20,511 |
| SD Y | 4.35 | 4.04 | 10,195 |

Notes: Estimates for treatment effects on different outcomes in year y (OLS). Treatment effect of a one-percentage-point increase in treatment intensity. Dependent variables: Column 1: Number of employment months in year y. Column 2: Number of unemployment months in year y. Column 3: Disposable income in year y. Mean/SD Y denotes the mean and standard deviation of the dependent variable. Data contain new unemployment spells that started in January and February of 2015–2017. The controls included the variables in Table A5. Standard errors (in parentheses) were clustered at the travel-to-work area level (50 clusters). Significance levels: *** 1%, ** 5%, * 10%.

Table A9 reports the treatment effects (OLS) for various labour market outcomes: number of employment and unemployment months during year $y$ and disposable income for year $y$. We found evidence of small positive treatment effects on employment months: A 10-percentage-point increase in treatment intensity increased employment months and decreased unemployment months by 0.1 months (3 days). The effect on disposable income was not statistically significant (columns 5–6).

## Appendix 7: Quality of Interviews

TABLE A10 The quality of interviews

| | Top 3 REOs | | Middle 6 REOs | | Bottom 3 REOs | |
|---|---|---|---|---|---|---|
| | 2016 | 2017 (%) | 2016 | 2017 (%) | 2016 | 2017 (%) |
| Employment plans | 40,863 | +195 | 170,313 | +189 | 52,062 | +105 |
| Face-to-face meetings | 13,051 | -45 | 70,537 | -43 | 19,940 | -43 |
| Vacancy referrals | 30,489 | +8 | 194,883 | +13 | 31,164 | -8 |
| Wage support offers | 2,526 | -17 | 20,629 | -6 | 4,757 | -4 |
| Sanctions | 20,883 | +13 | 93,539 | +17 | 23,972 | +13 |

Notes: Total annual amounts in 2016 and percentage changes in the total annual amounts from 2016 to 2017, by treatment group.

The dataset does not contain information on the quality of individual interviews (e.g.. duration). However, we can evaluate the quality of the interviews indirectly by using data on the total amounts of face-to-face meetings, vacancy referrals, wage support offers and sanctions. Table A10 indicates that the average quality of the interviews may have deteriorated after the reform. In 2017, the number of new employment plans (or their updates) increased sharply, whereas the number of face-to-face meetings decreased considerably. According to Vehkasalo (2020), face-to-face counselling is more efficient than online and telephone counselling in reducing unemployment duration. The number of wage support offers decreased, whereas the number of vacancy referrals and sanctions increased but much less than the number of the interviews.

Regionally, it seems that the quality of interviews suffered more in the top three REOs, where their amount was increased the most. Compared to the bottom three REOs, the number of face-to-face meetings and wage support offers decreased more in the top three REOs. The number of vacancy referrals increased in the top REOs but much less than the number of the interviews. The imposition of sanctions increased by 13% in the top and bottom three REOs.

## Online Appendix References


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# 3 THE IMPACT OF VACANCY REFERRALS ON VACANCY FILLING RATES: EVIDENCE FROM FINLAND[10]

**Abstract**
The Public Employment Services' (PES) vacancy referrals (VRs) are reported to be an effective active labour market policy tool that increases transition rates from unemployment to employment. The literature on VRs has focused on unemployed jobseekers searching for jobs, not firms searching for workers. We investigated how a reform that increased the number of VRs affected vacancy filling rates. We analysed extensive and detailed Finnish PES vacancy data, which included all vacancy announcements reported to the PES from 2011 to 2015. After the reform, the number of VRs in relation to vacancies increased considerably in some travel-to-work areas, while the change was minimal in other areas. Using a difference-in-differences approach, we found that vacancy filling rates increased in areas where the number of VRs was increased the most. However, despite the positive effects on vacancy filling rates, employment effects were negligible. One potential reason for this result is that VRs reduced the average quality and duration of post-unemployment jobs. We also found that the reform decreased the average quality and effectiveness of VRs.

**Keywords:** vacancies, employment services, vacancy referrals, active labour market policy, labour market matching

**JEL codes**: J63, J64, J68

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## 3.1 Introduction

Since the 2010s, Finland has had a large number of vacant jobs and high unemployment simultaneously, reflecting problems in labour market matching. Employers have had recruiting problems, and unemployed jobseekers have had difficulties finding work. This study examines the role of public employment services (PES) in vacancy filling rates. A vacancy referral (VR) is an official instruction from a PES caseworker to a jobseeker to apply for a specific vacant job. Such VRs are commonly used by PES to improve the matching of jobseekers and vacancies, and they are one of the most important active labour market policy (ALMP) tools. For jobseekers, VRs are job search assistance (JSA), but they also include monitoring because a refusal to apply for an assigned vacancy can lead to a sanction.

The previous studies on VRs have focused on their effects on unemployed jobseekers. The existing evidence indicates positive effects on the part of JSA on re-employment, particularly when combined with regular job search monitoring (e.g., Card et al., 2010, 2018; Kluve, 2010; Vooren et al., 2019; McGuinness et al., 2019). Most studies on VRs have reported positive effects on the transition rates from unemployment to employment (e.g., van den Berg et al., 2019; Bollens and Cockx, 2017; Cheung et al., 2019). However, some studies have reported non-significant or even negative results (Van Belle et al., 2019; Engström et al., 2012). According to Engström et al. (2012), a large number of applications did not meet the qualification requirements for the jobs. According to Van Belle et al. (2019), employers perceived referred jobseekers as being less motivated. Moreover, several studies show that JSA creates substantial displacement effects, leading to higher unemployment for non-treated jobseekers (e.g., Crepon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018). Thus, overall employment effects may be overestimated in many studies.

The previous literature on the effects of PES practices on vacancy filling rates is scant. Some studies suggest that PES and intensive mediation can improve labour market matching by reducing vacancy duration (e.g., Ropper, 1988; van Ours, 1994; Lindeboom et al., 1994). Nivalainen (2014) found that the introduction of a PES online service reduced vacancy durations. Related to employers' search for workers, Horton (2017) found that, in online labour markets, algorithmically recommending workers to employers can substantially increase the fill rates for vacant jobs. The recommendations can identify and deliver a larger number of potential applicants for a job opening. This should also be the goal of VRs. Moreover, VRs are also a tool with which to monitor unemployed jobseekers and boost their job search intensity.

We contribute to the literature on VRs by investigating how VRs affect the probability that a vacancy will be filled. In 2014, the number of VRs given by PES was massively increased as a part of the Government structural policy programme. Using extensive and detailed Finnish PES vacancy data, which included all vacancy announcements reported to PES from 2011 to 2015, we found that the reform increased the number of VRs in relation to vacancies considerably in some travel-to-work areas, while the change was minimal in some other areas. Based on this quasi-experimental setting, we restricted the data to vacancy postings in the top 15 (treatment group) and bottom 15 areas (control group). Using a difference-in-differences approach, we found that vacancy filling rates increased in areas in which the number of VRs in relation to vacancies was increased the most. Our data analysis shows that pre-trends had been similar in the top and bottom areas.

We also analysed the role of vacancy and employer characteristics in vacancy filling rates. Our results show that full-time, permanent and high-skill vacancies had the lowest vacancy filling rates. Thus, employers seem to be more demanding when hiring workers for such jobs, or applicants genuinely lack the necessary skills and competences.

Despite the positive effects on vacancy filling rates, employment effects were negligible. One potential reason for this result is that VRs reduced the average quality and duration of post-unemployment jobs. Jobs accepted after receiving VRs have been documented to be less permanent than jobs obtained without VRs (e.g., Van den Berg et al., 2019).

We also document that the increase in the number of VRs reduced their average quality and effectiveness. After the reform, the share of cases in which the employer rejected an applicant who had received a VR increased considerably. Our results highlight that it is important that VRs are sent to jobseekers who meet the needs of employers.

The study is organized as follows. Section 3.2 presents the relevant literature. In Section 3.3, we describe Finnish VRs and the 2014 reform. Section 3.4 discusses the data and methods. Section 3.5 reports the results, and Section 3.6 concludes the article.

## 3.2 Relevant literature

### 3.2.1 Previous empirical literature

Most of the literature on PES practices and ALMPs has focused on their effects on unemployed jobseekers searching for jobs, not employers searching for workers. The previous literature on the effects of PES practices on vacancy filling rates is scant. Several studies suggest that PES and intensive mediation can improve labour market matching by reducing vacancy duration (Ropper, 1988; van Ours, 1994; Lindeboom et al., 1994). Nivalainen (2014) investigated how the introduction of a PES online service affected the duration of employer search. According to the results, it shortened the average duration of vacancies.

Related to employer search, Horton (2017) found that algorithmically recommending workers to employers can substantially increase hiring. The study reported that, in an online labour market, the algorithmic recommendations increased the overall fill rate in technical job openings by 20%.

According to the study, algorithmic recommendations were most effective for job openings that generally receive fewer applicants. The recommendations can identify and deliver more and higher-quality applicants for a job opening. According to Gürtzgen et al. (2021), mere online recruiting (without algorithms) raises the number of applicants but also the share of unsuitable candidates per vacancy.

The previous literature on VRs has focused on their employment effects. The majority of studies have reported that VRs increase the transition rate from unemployment to employment (e.g., Bollens and Cockx, 2017; Van den Berg et al., 2019; Cheung et al., 2019). Bollens and Cockx (2017) studied the effects of three VR types: (1) caseworkers' referrals by phone or by e-mail; (2) automatic referrals via software, without caseworker intervention, and (3) referrals transmitted in a meeting with a caseworker. All three instruments were found to increase the transition rates to employment, but VRs sent by caseworkers were more effective than automatic referrals.

There are several explanations for the positive employment effects, including higher job search intensity and lower reservation wages. First, VRs can suggest vacant jobs that individuals would not have otherwise been aware of. Counselling and monitoring can encourage people to submit more applications (Gorter and Kalb, 1996). Also, VRs can help referred jobseekers apply to the most relevant jobs earlier (Cheung et al., 2019). According to Van Ours and Ridder (1992), applicants who contacted the firm within two weeks filled almost 80 percent of vacancies. Belot et al. (2019) found that an online tool that provided tailored advice to jobseekers can broaden their searches and thereby increase the number of job interviews for which they were selected. In a best-case scenario, an open vacancy and an applicant are a good match.

Second, VRs include monitoring, and a refusal to apply for an assigned vacancy can lead to a sanction. Thus, VRs have threat effects, providing incentives to search for work more actively to avoid sanctions (e.g., Rosholm and Svarer, 2008). Threat effects affect all jobseekers, including non-sanctioned individuals (e.g., Lalive et al., 2005). According to Arni and Schiprowski (2019), unemployment duration decreased by 3% when job search requirements increased by one monthly job application. They also report that the number of imposed benefit sanctions rose by 12% per required monthly application. VRs and sanctions are reported to affect the quality of post-unemployment jobs. According to Van den Berg et al. (2019), jobs accepted after receiving VRs paid lower wages and were less stable than jobs found without VRs. Imposed sanctions are reported to increase job-finding rates, but they also increase transitions out of the labour force (e.g., Lalive et al., 2005; Busk, 2016). As compared to non-sanctioned individuals, sanctioned individuals are reported to accept lower-quality jobs: jobs with shorter durations, lower hourly wages and fewer working hours per week (e.g., Van den Berg and Vikström, 2014; Arni et al., 2013).

Some studies have reported that VRs have non-significant or even negative effects. Engström et al. (2012) reported that one-third of VRs did not result in job applications and VRs did not have a significant impact on unemployment duration. According to them, only about 60% of the applications were considered realistic by employers, meaning that many applications did not meet the qualification requirements for the jobs. Van Belle et al. (2019) found that VRs had substantial adverse effects on employment probability. According to them, employers perceived referred jobseekers as being less motivated than jobseekers who applied without VRs. Moreover, several studies show that JSA creates substantial displacement effects, leading to higher unemployment for the non-treated (see Crepon et al., 2013; Ferracci et al., 2014; Gautier et al., 2018). This makes the positive effect estimates seem questionable in studies that did not take displacement effects into account.

### 3.2.2 Theoretical framework for VRs and vacancy filling rates

A firm is defined as having a vacancy if it is looking to hire a worker (Burdett and Cunningham, 1998). Generally, the duration of a vacancy is defined as the difference between the date an employer began searching for a new worker and the date a new worker was hired. More specifically, the vacancy duration can be decomposed into an application period and a selection period (van Ours and Ridder, 1993). The employer must pay the cost of advertising and screen the applicants. Advertising generates a positive arrival rate for candidates, $\delta(t)$. By varying the advertising expenditure, an employer can affect the arrival rate. However, the actual arrival rate of applicants depends on many factors, such as the total number of jobseekers in the area, the total number of other vacancies, and job-specific factors (Burdett and Cunningham, 1998).

Following Burdett and Cunningham (1998), we assume that each applicant is the realization of a random draw from a distribution of abilities. Applicants must be interviewed and screened, which creates costs for the employer. After interviewing a candidate, the employer will be able to determine the candidate's ability level x and thus the expected profit generated. The optimal policy is to accept the first applicant with an ability level greater than the reservation ability (Burdett and Cunningham, 1998). The reservation ability level $R(t)$ is influenced by several factors (e.g., the costs of advertising and screening, the expected gain from hiring an individual and market conditions). Following Burdett and Cunningham (1998), we define the conditional probability of hiring $\lambda(t)$ as follows:

$$\lambda(t) = \delta(t)\, Pr(x > R(t) \mid \text{no applicant previously hired}) \quad (1)$$

where $\delta(t)$ is the arrival rate of applicants, $x$ is the applicant's ability level and $R(t)$ is the reservation ability level. Next, we consider how an increase in the number of VRs to vacancies affects vacancy filling rates. Not all vacancies receive any applications. VRs can increase the number of applicants and make them apply sooner. Thus, undoubtedly, VRs increase the jobseeker's search effort and the arrival rate of applicants $\delta(t)$. If $\delta(t)$ is low without VRs, a larger pool of applicants should increase the probability of hiring. However, an adverse consequence of increased VRs can be that the average ability level of applicants decreases. Moreover, some applicants may send applications simply to avoid a sanction and do not actually want the job in question. As a result, some employers may receive unmanageable numbers of applications from unqualified and poorly motivated candidates. Thus, both the quantity and quality of the application pool matter, and the resulting conditional probability of hiring an applicant can increase or decrease. One key question is whether the applicants have the qualities and competences that the employer desires.

## 3.3 Institutional background: VRs in Finland

A VR is an official instruction from a PES caseworker for a jobseeker to apply for a specific vacant job. VRs include monitoring, and a refusal to apply to an assigned vacancy can lead to a sanction. Misconduct can be noted by a PES caseworker or a potential employer. A sanction entails the suspension of unemployment benefits for 15–90 days. This sanction period provides financial incentives for jobseekers to apply for jobs. Certain reasons for refusals are considered valid, such as a too-long commute, a too-low wage, the wrong profession and an inability to work.[11]

Young individuals and those with secondary education receive relatively more VRs than older and more highly educated individuals. In terms of occupational groups, VRs are most common in industrial work, healthcare and social care, and service work. A suggested job can be in a different municipality than where the jobseeker lives. In addition, suggested jobs quite often represent a different occupational category than the jobseeker's occupation in the PES register data. In addition to unemployed jobseekers, those in employment, education and ALMPs also receive VRs. The probability of receiving a VR increases with the duration of unemployment (Räisänen and Järvelä, 2014).

According to Valtakari et al. (2014), over 50% of employers considered VRs important, and about 25% reported that they could use VRs in recruiting new workers. Small companies with fewer than five employees had, on average, more positive attitudes.

In 2014, the number of VRs given by PES was increased as a part of the Government structural policy programme.[12] The programme aimed to lower the structural unemployment rate, with the key elements of this being the rapid

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[11] The duration of the daily commute exceeds three hours, the salary for full-time work is less than 1,134€/month, the wage paid for part-time work after deduction of travel costs is lower than the unemployment benefit or the job does not match the jobseeker's education and work experience. See FINLEX Työttömyysturvalaki 30.12.2002/1290; 8.6.2012/288 https://www.finlex.fi/fi/laki/ajantasa/2002/20021290#O1L2aP5

[12] Hallituksen päätös rakennepoliittisen ohjelman toimeenpanosta 29.11.2013. https://valtioneuvosto.fi/documents/10184/1043916/rakennepoliitisen-ohjelman-toimeenpano.pdf/6e77c257-6ae9-4166-a6e7-bd7dedc29b52

filling of vacant jobs and the shortening of unemployment periods. A key objective was to enhance PES, and PES offices were guided to increase the number of VRs for unemployed jobseekers. According to the programme, VRs should be made immediately at the beginning of the unemployment period, as well as regularly as unemployment lengthens, to all unemployed people who have sufficient labour market skills. According to the new policy, VRs should be made for a wider variety of job opportunities. After three months of being unemployed, vacancies were also offered from outside the unemployed person's professional field. In addition, the program instructed PES caseworkers to offer vacant jobs from beyond an 80-kilometre radius from the unemployed person's home. In 2015, legislation came into force stipulating that such a job must be accepted if the daily commute via public transport did not exceed three hours for full-time work and two hours for part-time work.

The reform was an official directive to increase the use of VRs, but it did not specify any specific level of increase. Figure 1 shows that the number of VRs in relation to open vacancies increased considerably after the reform. At the national level, the number of VRs in relation to vacancy postings increased from 0.7 to over 1.4 (see Table 1). The reform also increased the share of vacancy postings that received at least one VR. The data analysis shows large regional variations in the implementation: in some areas, the number of VRs increased greatly, while increases in some other areas were much more moderate (see Table 2). We used this regional variation to study the effects of VRs on vacancy filling rates.

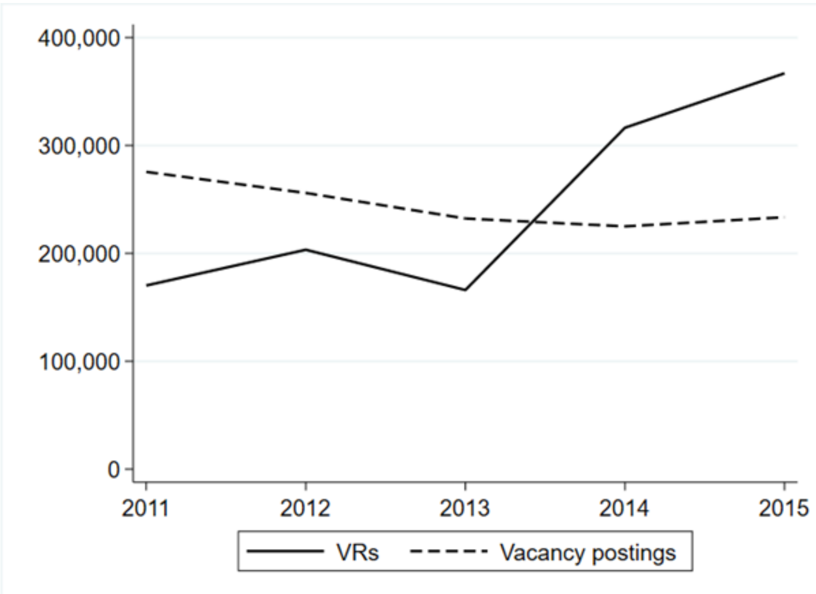


FIGURE 1 Vacancy referrals and vacancy postings in 2011–2015.

TABLE 1 Vacancy referrals, by year

| | VRs | VRs/ vacancy postings | VRs/ vacancies | Vacancy postings with at least 1 VR | Vacancy postings with ≥2 VRs |
|---|---|---|---|---|---|
| 2011 | 170,239 | 0.62 | 0.33 | 0.26 | 0.11 |
| 2012 | 203,362 | 0.79 | 0.42 | 0.30 | 0.13 |
| 2013 | 166,042 | 0.73 | 0.38 | 0.27 | 0.12 |
| 2014 | 316,476 | 1.41 | 0.71 | 0.39 | 0.22 |
| 2015 | 366,884 | 1.57 | 0.78 | 0.39 | 0.23 |

Notes: VRs by year. Number of VRs, number of VRs in relation to vacancy postings, number of VRs in relation to vacancies, the fraction of vacancy postings with at least one VR and the fraction of vacancy postings with two or more VRs. A single vacancy posting may include several vacancies.

## 3.4 Data and methods

### 3.4.1 Datasets

We used the Finnish PES administrative data containing information on vacancy postings. Our data covered all vacancy postings that were announced to the PES from 2011 to 2015. The data include information on vacancy and employer characteristics, such as job type, work schedule type, job duration, required occupation, employer sector and employer size. In Finland, a large proportion of all vacancies is reported to PES: it is estimated that about 40–50% of all vacancies are reported to the PES (Finnish Labour Review, 2020).

The TEM URA Job Offers dataset provided information on VRs given by Finnish PES, and the TEM Job Search datasets provided information on sanctions. The FOLK dataset provided yearly panel data for the entire population of Finland. It contains individual-level information on demographic, educational and occupational characteristics. All datasets contained individual identifiers, which made it possible to link the datasets. Each vacancy posting was combined with information on all the VRs that were assigned to it. Moreover, each VR was linked to the related vacancy posting and individual, through which we obtained information on vacancy and individual background variables. The data were limited to the period of 2011–2015 because comprehensive data on VRs were only available from 2011 onwards and the number of VRs decreased again after 2015.

Previously, the PES vacancy dataset has been analysed by Nivalainen (2014). In that study, vacancy duration was defined as the period between the start and end dates of a vacancy posting at PES. It was noted that vacancy duration is a proxy for the duration of employer search because (a) employers may use other methods before registering their vacancies with PES and (b) employers may continue their search processes after the end date. It should be noted that most vacancy postings in the PES data had fixed application periods that were announced in the vacancy posting. For them, the end of the application period (i.e., the end of the vacancy duration) does not necessarily indicate whether the vacancy was filled or not. In our study, we pay special attention to whether vacancies were filled or not. Thus, instead of vacancy duration, our focus was on vacancy filling rates. It should be noted that a single vacancy posting may include one or more vacancies. In this study, we define a vacancy posting as filled when at least one individual was hired.

TABLE 2 The number of VRs in relation to vacancy postings by area

| Area ID | Area | VRs/vacancy postings, 2011-2013 | VRs/vacancy postings 2014-2015 | Change | Vacancy postings, 2011-2015 |
|---|---|---|---|---|---|
| 52 | Riihimäen | 0.61 | 2.83 | 2.22 | 19,535 |
| 146 | Järviseudun | 0.91 | 2.96 | 2.05 | 5,408 |
| 63 | Etelä-Pirkanmaan | 0.47 | 2.43 | 1.96 | 13,367 |
| 53 | Forssan | 0.26 | 1.98 | 1.72 | 11,855 |
| 142 | Seinäjoen | 1.01 | 2.69 | 1.68 | 50,376 |
| 69 | Ylä-Pirkanmaan | 0.45 | 2.08 | 1.62 | 7,486 |
| 61 | Luoteis-Pirkanmaan | 0.76 | 2.24 | 1.49 | 5,532 |
| 144 | Kuusiokuntien | 1.94 | 3.38 | 1.44 | 5,816 |
| 44 | Pohjois-Satakunnan | 0.28 | 1.65 | 1.37 | 6,349 |
| 14 | Raaseporin | 0.90 | 2.22 | 1.33 | 13,558 |
| 68 | Lounais-Pirkanmaan | 1.32 | 2.63 | 1.31 | 9,072 |
| 114 | Varkauden | 0.96 | 2.13 | 1.17 | 13,591 |
| 112 | Kuopion | 1.21 | 2.28 | 1.07 | 78,265 |
| 173 | Oulunkaaren | 0.65 | 1.71 | 1.06 | 6,547 |
| 51 | Hämeenlinnan | 0.68 | 1.73 | 1.05 | 43,856 |
| | … | … | … | … | … |
| 82 | Kotkan-Haminan | 0.90 | 1.24 | 0.35 | 27,536 |
| 153 | Sydösterbotten | 1.25 | 1.58 | 0.34 | 4,624 |
| 161 | Kaustisen | 0.46 | 0.79 | 0.32 | 4,962 |
| 25 | Loimaan | 0.48 | 0.79 | 0.31 | 11,015 |
| 21 | Åboland-Turunmaan | 0.14 | 0.45 | 0.31 | 7,006 |
| 178 | Koillismaan | 0.35 | 0.64 | 0.30 | 8,632 |
| 125 | Pielisen Karjalan | 0.64 | 0.86 | 0.22 | 8,095 |
| 101 | Mikkelin | 0.25 | 0.43 | 0.17 | 29,097 |
| 122 | Joensuun | 0.53 | 0.70 | 0.17 | 48,375 |
| 103 | Savonlinnan | 0.31 | 0.48 | 0.16 | 16,272 |
| 113 | Koillis-Savon | 1.22 | 1.37 | 0.16 | 1,830 |
| 124 | Keski-Karjalan | 0.68 | 0.72 | 0.04 | 4,549 |
| 181 | Kehys-Kainuun | 0.82 | 0.86 | 0.04 | 6,349 |
| 193 | Torniolaakson | 0.97 | 0.95 | -0.02 | 2,089 |
| 162 | Kokkolan | 1.09 | 0.96 | -0.13 | 19,939 |

Notes: The number of VRs in relation to vacancy postings by area in the top 15 and bottom 15 areas. The largest areas with the most vacancy postings were neither the top nor the bottom areas. The change in VRs/V was 0.66 in the Helsinki area and 0.60 in the Turku area. The Tampere area (1.59) was not included in the top 15 areas because the bottom 15 areas did not include any comparable area (224,659 vacancy postings in the Tampere area from 2011 to 2015). See Table A1.1 in the Online Appendix for all areas.

### 3.4.2 Methods

We analysed the effects of VRs by using regional variation in the implementation of the reform. The reform was followed by large regional variations in the implementation of VRs. Travel-to-work areas (67) have been defined by the Finnish Ministry of the Interior as entities formed from municipalities, with the criteria being municipal cooperation, workers commuting and transport connections. A travel-to-work area may encompass several municipalities, while each municipality belongs to just one travel-to-work area. We calculated how much the number of VRs in relation to vacancy postings changed in each area after the reform (2014–2015) as compared to the level before the reform (2011–2013). Table 2 shows large regional variations: certain areas saw a greatly increased number of VRs, while in certain other areas, this changed only slightly. We limited the data to the vacancy postings in the top 15 (treatment group) and bottom 15 areas (control group). After the reform, the number of VRs in relation to vacancy postings increased by over 1.05 in the top 15 areas, while it increased at most 0.35 in the bottom 15 areas. Our analysis data consist of 247,140 vacancy postings. The top areas included 141,281 vacancy postings, and the bottom areas 105,859 vacancy postings.

We studied the effects of the increased number of VRs/V on vacancy filling rates using a difference-in-differences approach (Angrist and Pischke, 2009). Our data included many years, so our model allowed for effects before, during and after the reform. Our model compared the outcomes of vacancies in the top 15 (the treatment group) and the bottom 15 areas (the control group). To estimate the average treatment effects, we used the following equation:

$$Y_{ist} = \sum_{j \neq -2} \alpha_j \boldsymbol{I}\,[t = j] + \mu_r + \lambda_s + X_{ist}\delta + \ v_{ist} \qquad (2)$$

where $Y_{ist}$ is the outcome for vacancy posting $i$ in period $s$ at event time $t$. Similarly to Horton (2017), we focus on treatment effects on the probability that a vacancy is filled. The vacancy filling probability was measured as a dummy variable that was equal to 1 for vacancy postings that were filled. In addition, we examined the effects on VR probability, VR duration and vacancy duration. VR probability was measured as a dummy variable that was equal to 1 for vacancy postings that received at least one VR. VR duration, which was measured in days, was the duration before a vacancy posting received its first VR. VR duration was measured only for those vacancy postings that received at least one VR. Vacancy duration, which was measured in days, was defined as a sequence of times during which a vacancy posting was open for applications in PES.

The terms on the right-hand side of equation (2) are indicators for the event time, $\mu_r$ are area-fixed effects, $\lambda_s$ are period-fixed effects (year-month), $X_{ist}$ is a vector of control variables and $v_{ist}$ is an unobserved error term. The event time $t$ was indexed relative to the reform year, 2014. The post-reform observations consisted of vacancy postings from 2014 to 2015 because the reform came into force at the beginning of 2014. The estimated leads ($\alpha_{-3}, \alpha_{-1}$) show the anticipatory effects, and the estimated lags ($\alpha_0$, $\alpha_1$) show the post-treatment effects. The event time dummy for $t$ = -2 was omitted, so the coefficients for the other event time dummies measured the effect of the reform relative to the year 2012.

The control variables $X_{ist}$ included vacancy and employer characteristics and regional controls. Regional macro-variables included the monthly unemployment rate, monthly vacancy rate and annual output growth rate. These were measured at the regional level during the period when the vacancy announcement was posted. Vacancy-level characteristics included variables such as a vacancy's job type (four categories), job duration (five categories), work schedule type (seven categories), required occupation (nine categories) and a dummy for having a fixed application period. Employer controls included an employer's sector (three categories), number of personnel (ten categories) and industrial classification (17 categories).

The area-fixed effects (i.e., area dummies) were included to capture such regional differences in labour-market conditions that were constant over time. The period-fixed effects (i.e., year-month dummies) were included to capture such differences in macroeconomic conditions that were constant across regions. Standard errors were clustered at the regional level (30 clusters) to account for unobservable within-region variation in outcomes. The estimation was implemented using the EVENTDD programme for Stata (Clarke and Tapia-Schythe, 2021).

The identification requires that the treatment and control groups have parallel pre-trends in outcomes. In addition, the identification requires that the composition of the treatment and control groups be stable. These issues were examined in the next subsection (3.4.3). For robustness, we estimated additional models. Because vacancy filling rates have been better documented for vacancies with non-fixed application periods, we estimated separate regressions for vacancies with fixed application periods and vacancies with non-fixed application periods. In addition, we estimated treatment intensity regressions using the vacancy data from all 67 travel-to-work areas.

### 3.4.3 Descriptive analysis

The identification requires that the treatment and control groups have parallel pre-trends in outcomes. Figure 2 depicts the time series of VR probability, VR duration, vacancy filling rates and vacancy duration in the top and bottom areas from 2011 to 2015. Thus, it shows the pre- and post-treatment trends for the treatment and control groups. Figure 2a shows that the top 15 areas had anticipatory effects on VR probability in 2013. The reform was prepared in 2013, and it was decided then that the number of VRs would be immediately increased, beginning in 2014. Apparently, the top regions began to increase the number of VRs in 2013. Because of this, the event time dummy for $t$ = -2 was omitted, instead of $t$ = -1, in equation 2. The pre-treatment trends were similar for all outcomes of interest in the years 2011–2012, providing support for the parallel trend assumption. The significance of pre-treatment coefficients is evaluated in Section 3.5.1.

Appendix Figure A1.2 shows that the number of VRs in relation to vacancy postings remained at a low level in the bottom areas after the reform, while in the top areas, it increased considerably. Correspondingly, VR probability increased, and VR duration decreased in the top areas compared to the bottom areas (Figure 2). Simultaneously, vacancy filling rates increased in the top areas as compared to the bottom areas. In 2015, vacancy filling rates were higher in the top areas than in the bottom areas, while they had been lower before 2014. Vacancy durations showed rising trends in both the top and bottom areas.

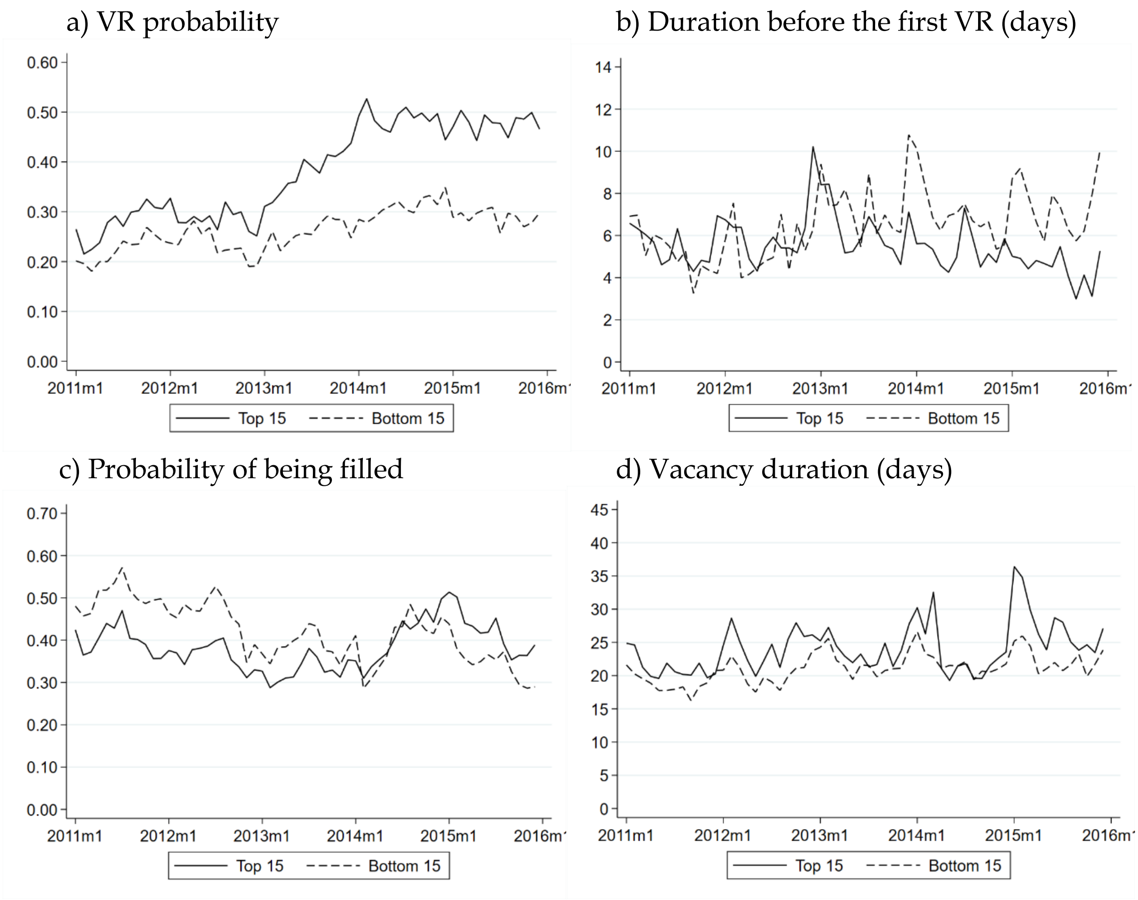


FIGURE 2 Vacancy outcomes in the top and bottom 15 areas in 2011-2015

In addition, the identification requires that the composition of the treatment and control groups be stable. Appendix Tables A3.1–A3.7 show that the characteristics of all vacancies and the characteristics of vacancies and individuals that received VRs were similar in both groups before and after the reform. Also, the changes in characteristics were small and similar in the top and bottom areas.

Table 3 shows that, after the reform, the number of VRs/vacancy postings increased from 0.68 to 2.27 in the top areas. In the bottom areas, the corresponding number increased from 0.57 to 0.78. After the reform, more vacancies received VRs, and they were given sooner in the top areas as compared to the bottom areas. After the reform, the fraction of vacancies that received at least one VR increased from 31% to 48% in the top areas, while it increased from 25% to 30% in the bottom areas. The first VRs were typically made soon after a vacancy was announced to PES. In the top areas, the average duration before the first VR was 5.6 days before the reform and 4.9 after it. In the bottom areas, this value increased from 5.3 to 7.2 after the reform.

According to Table 3, the enhanced VR use in the top areas may have increased vacancy filling rates. After the reform, the vacancy filling rate increased from 38% to 41% in the top areas, while it decreased from 48% to 37% in the bottom areas. One reason for these small numbers is that the data do not include accurate information on vacancy fillings after the application period. Some vacancies can take a very long time to be filled, and many vacancies remain unfilled. According to Maunu (2016), more than 25% of employers experienced recruitment problems from 2013 to 2014. Average vacancy durations increased by about 3 days in both the top and bottom areas.

TABLE 3 Vacancy postings and VRs in the top and bottom areas

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| **Number of vacancy postings** | 61,306 | 52,229 | 45,546 | 39,817 |
| Fixed application period | 0.73 | 0.77 | 0.62 | 0.69 |
| Non-fixed application period | 0.27 | 0.23 | 0.38 | 0.31 |
| VRs | 41,803 | 118,734 | 25,912 | 30,988 |
| VRs/Vacancy postings | 0.68 | 2.27 | 0.57 | 0.78 |
| Vacancies with at least one VR | 0.31 | 0.48 | 0.25 | 0.30 |
| **Average durations (days):** | | | | |
| First VR | 5.6 | 4.9 | 5.3 | 7.2 |
| Vacancy duration | 22.7 | 25.6 | 19.4 | 22.3 |
| **Vacancy outcomes:** | | | | |
| Vacancy filled | 0.38 | 0.41 | 0.48 | 0.37 |
| Enough applicants | 0.03 | 0.01 | 0.03 | 0.02 |
| Vacancy cancelled | 0.06 | 0.05 | 0.07 | 0.05 |
| Application period ended | 0.53 | 0.52 | 0.43 | 0.56 |

Notes: Vacancies and VRs in the top 15 and bottom 15 areas before and after the reform. The areas are defined in Table 2.

Table 4 shows descriptive statistics for the main variables used in the empirical analysis for the top and bottom areas. The top areas had, on average, lower regional unemployment rates and lower output growth rates as compared to the bottom areas from 2011 to 2015. The vacancy rates were quite similar in both groups. The top areas had a higher share of vacancies with fixed application periods. Otherwise, the vacancy characteristics were similar in both groups. About 85% of vacancies had wage work as the job type, about 67% were full-time, about 50% had a job duration over 12 months, and about 74% were in the private sector. The most required occupation was service and sales workers (about 30%).

TABLE 4 Summary statistics by treatment status

| | **Top 15 areas** | **Bottom 15 areas** |
|---|---|---|
| **Regional controls:** | | |
| Unemployment rate | 8.85 | 11.20 |
| Vacancy rate | 1.18 | 1.00 |
| Economic growth rate | 0.03 | 1.30 |
| **Vacancy and employer characteristics:** | | |
| Fixed application period | 0.75 | 0.65 |
| **Job type** | | |
| Wage work | 0.86 | 0.84 |
| Commission paid | 0.05 | 0.05 |
| Entrepreneur | 0.03 | 0.03 |
| Rotation leave substitute | 0.06 | 0.08 |
| **Work schedule type** | | |
| Full-time work | 0.67 | 0.66 |
| Shift work | 0.13 | 0.12 |
| Evening work | 0.11 | 0.11 |
| Part-time work | 0.08 | 0.10 |
| **Job duration** | | |
| Below 1 month | 0.05 | 0.06 |
| 1–3 months | 0.13 | 0.14 |
| 3–6 months | 0.15 | 0.16 |
| 6–12 months | 0.14 | 0.15 |
| Over 12 months | 0.52 | 0.50 |
| **Employer's sector** | | |
| Public | 0.26 | 0.27 |
| Private | 0.74 | 0.73 |
| **Required occupation** | | |
| Service and sales workers | 0.29 | 0.31 |
| Managers | 0.01 | 0.01 |
| Professionals | 0.16 | 0.16 |
| Technicians | 0.20 | 0.20 |
| Clerical support workers | 0.04 | 0.04 |
| Agricultural, forestry and fishery workers | 0.01 | 0.02 |
| Building, craft and related trades workers | 0.10 | 0.09 |
| Plant and machine operators, assemblers, drivers | 0.06 | 0.06 |
| Elementary occupations | 0.13 | 0.12 |
| **Number of observations (vacancy postings)** | 141,281 | 105,859 |

Notes: The characteristics of vacancy postings in the top 15 and bottom 15 areas from 2011 to 2015. Classification of occupations 2010 by Statistics Finland. https://www2.tilastokeskus.fi/en/luokitukset/ammatti/

## 3.5 Results

### 3.5.1 Main results

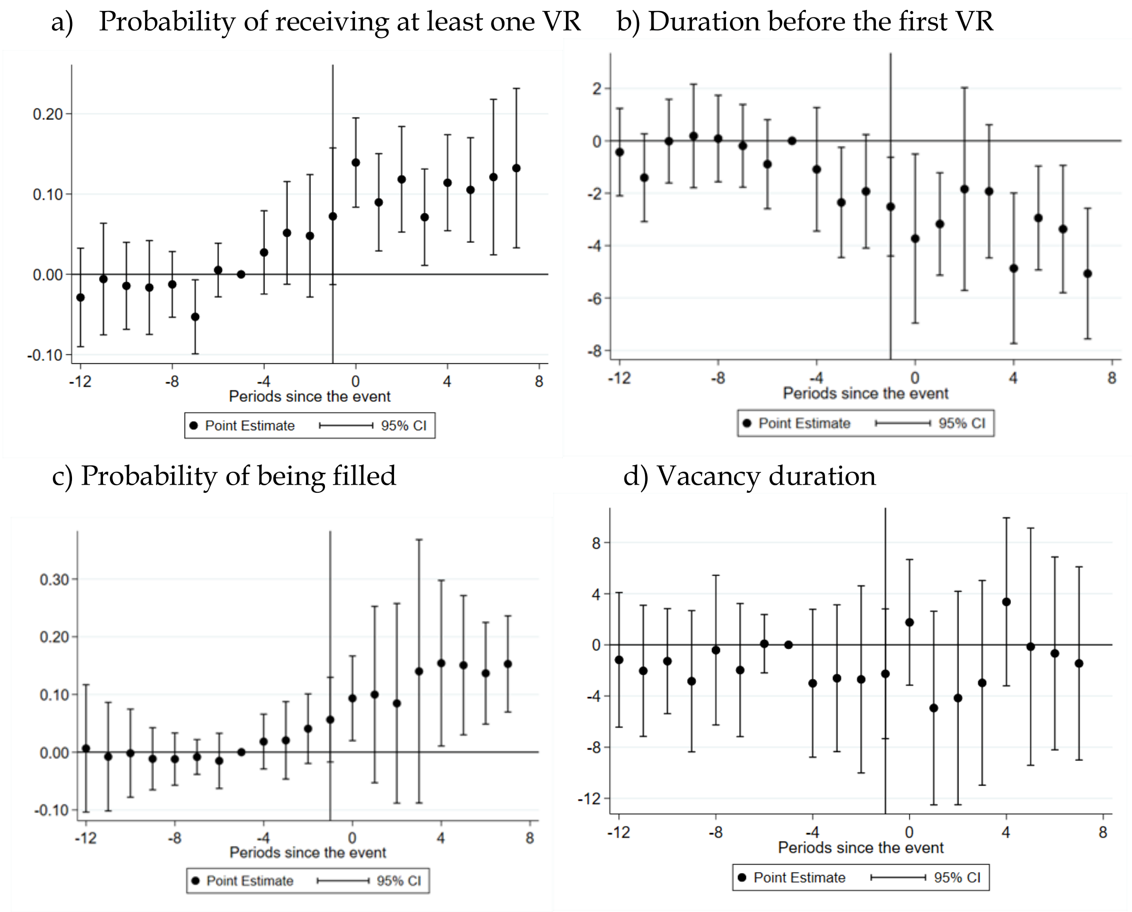


FIGURE 3 Estimation results with quarterly event time indicators.

Notes: The outcomes of vacancies in the top 15 areas (the treatment group) versus the bottom 15 areas (the control group). Estimated coefficients and 95% confidence intervals. Event time 0 corresponds to the first quarter of the reform year, 2014. The reference period was event time t = -5 (the fourth quarter of 2012).

Figure 3 and Table 5 report the effect estimates on VR probability, VR duration, probability of being filled and vacancy duration. VR probability and VR duration measure the difference in treatment between the top 15 and bottom 15 areas. Figure 3 shows estimation results with quarterly event time indicators, allowing for a more detailed assessment of the statistical significance of pre-trends than yearly event time indicators. According to the figure, the pre-trends were similar from 2011 to 2012, but there were anticipation effects in the top areas in 2013.

In Table 5, 2012 was set as the reference year, and the first row shows the coefficient estimates for the interaction term Top15 * year 2011 (event time t = -3). The estimates were statistically non-significant for all outcomes, showing that pre-trends before 2013 were similar in the top and bottom areas. VR probability and VR duration are interesting outcomes from the employer's point of view. After the reform, in 2014 (event time 0), the probability of a vacancy posting receiving at least one VR increased by 12.5 percentage points in the top 15 areas as compared to the bottom 15 areas. Moreover, vacancies in the top areas received VRs sooner after the reform: the duration before the first VR decreased, on average, by three days in the top areas as compared to the bottom areas.

TABLE 5 Estimation results by outcome

| | **VR probability** (1) | **Duration before first VR** (2) | **Probability of being filled** (3) | **Vacancy duration** (4) |
|---|---|---|---|---|
| Top15 * year 2011 | 0.002 | -0.21 | 0.006 | -1.02 |
| | (0.020) | (0.46) | (0.031) | (0.97) |
| Top15 * year 2013 | 0.066* | -1.72** | 0.040 | -1.93 |
| | (0.033) | (0.64) | (0.033) | (1.62) |
| Top15 * year 2014 | 0.125*** | -2.50** | 0.111 | -1.80 |
| | (0.030) | (0.99) | (0.073) | (1.23) |
| Top15 * year 2015 | 0.135*** | -3.77*** | 0.158*** | 1.16 |
| | (0.037) | (0.79) | (0.054) | (2.57) |
| Year-month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 247,140 | 80,223 | 247,140 | 247,140 |
| R2 | 0.177 | 0.072 | 0.372 | 0.090 |
| Mean Y | 0.325 | 5.78 | 0.400 | 22.68 |

Notes: The data include vacancy postings in the top 15 and bottom 15 areas from 2011 to 2015. Standard errors (in parentheses) were clustered at the travel-to-work area level (30 clusters). Significance levels: *** 1%, ** 5% and * 10% (all two-sided tests). The models include regional controls (monthly unemployment rate, monthly vacancy rate and annual economic growth rate), vacancy controls (vacancy's job type, work schedule type, job duration, required occupation and a dummy for having a fixed application period) and employer controls (sector, number of personnel and industrial classification). The coefficient estimates for the control variables are reported in Appendix Table A2.1.

The key outcome variable was the probability that a vacancy is filled. According to the results, vacancy filling rates increased in the top areas after the reform (column 3 in Table 5). The estimate for 2015 indicates that, in 2015, the probability of a vacancy being filled increased by 15.8 percentage points in the top 15 areas as compared to the bottom 15 areas. For 2014, the treatment effect was 11 percentage points, but the estimate was not statistically significant. The results suggest that, to a certain extent, it is possible to improve the filling of vacancies by increasing VRs considerably. On the other hand, the result for 2014 indicates that even a substantial increase in VRs does not necessarily contribute to the filling of vacancies. It should be noted that a considerable fraction of VRs was directed to vacancies that had already received VRs. After the reform, the number of cases in which multiple VRs were made for a single vacancy increased (see Online Appendix Figure A1.2f).

Most vacancies had a fixed vacancy duration. Because we found no significant effect on vacancy duration (Figure 3d and column 4 in Table 5), the increased vacancy filling rates in the top areas were not because of longer application periods. A confounding factor could be a considerable change in the number of vacancies per vacancy announcement. However, the number of vacancies in relation to vacancy postings and the share of vacancy postings with multiple vacancies increased only slightly and similarly in the top and bottom areas after the reform (Figure A1.3).

The coefficient estimates for the control variables are reported in Online Appendix Table A2.1. The estimates for regional controls were not statistically significant at the 5% level. However, according to the previous literature, they should be controlled. Russo et al. (2006) argued that, in tight labour markets, vacancies attract fewer applicants, and firms' search cost per applicant rises. In turn, vacancies attract more applicants if the unemployment-vacancy ratio is high (Abbring and van Ours, 1993).

The results show that certain vacancy characteristics were associated with VR probability (Column 1 in Online Appendix Table A2.1). Full-time work vacancies had higher VR probabilities as compared to evening work or part-time work vacancies. Wage work and rotation-leave-substitute positions received VRs with a considerably higher probability than commission pay and entrepreneur vacancies. Vacancies with job durations between 1 and 12 months received more VRs than vacancies for which the job duration was below 1 month or over 12 months. Vacancies for which the required occupation was managers, professionals or technicians received VRs with the lowest probability. Other vacancies, requiring less education, received VRs with a higher probability. Vacancies involving sales or manufacturing received VRs with the highest probability. Comparing columns 1 and 2 shows that a higher probability of receiving VRs was associated with a shorter duration of receiving VRs and vice versa. For example, VR duration was shorter for full-time work vacancies, wage work and rotation-leave-substitute vacancies and vacancies in sales. The duration before the first VR was longer for entrepreneurial and commission pay vacancies, vacancies with job durations over 12 months and vacancies whose required occupation was manager or professional.

Certain vacancy characteristics were associated with higher or lower vacancy filling rates. Temporary vacancies with short job durations were more likely to be filled than more permanent vacancies. Vacancies for part-time work and evening work were more likely to be filled than those for full-time work. This may reflect the fact that employers are more demanding when hiring workers for full-time and permanent vacancies. Vacancies with professional or technician as the required occupation had the lowest vacancy filling rates. Entrepreneurial jobs were considerably less likely be filled, and rotation-leave-substitute positions more likely to be filled, than wage work jobs. Vacancies with a fixed application period had a considerably lower probability of being filled than vacancies with a non-fixed application period. The outcome for many vacancies with a fixed application period was that the application period ended (Table A1.2). The share of vacancies with a fixed application period increased in both the top and bottom areas in 2014–2015 as compared to 2011–2012.

Certain vacancy characteristics were connected to vacancy duration. Vacancy duration was shorter for vacancies with a fixed application period. For vacancies with a non-fixed application period, the end of the vacancy duration typically meant that a vacancy was filled. Vacancy duration was longer for vacancies with longer job durations. Vacancy duration was particularly long for entrepreneurial vacancies and particularly short for rotation-leave-substitute vacancies. In addition, major differences in vacancy duration were evident between industries.

Previous studies report that non-manual vacancies have lower filling rates than manual vacancies, emphasising the role of skill shortages (Adams et al., 2000; Andrews et al., 2008). Faster hiring has been found to go hand in hand with lower hiring standards (Carrillo-Tudela et al., 2020). Correspondingly, vacancy durations are documented to increase with the required educational level and prior experience (van Ours and Ridder, 1991; Adams et al., 2000; Barron et al., 1997). According to Brenčič (2009), employment protection legislation makes it costly for employers to terminate a permanent employment contract, meaning that lowering hiring requirements can cause high firing costs if an under-qualified worker is hired on a permanent contract. Our data did not include information on wages, but some studies report that vacancy duration is negatively correlated with starting wage (Mueller et al., 2020). It is likely that vacancies with higher salaries attract more applicants of a higher quality, with other factors being constant.

### 3.5.2 Robustness

To account for observable differences in the composition of different areas, the models included a large set of covariates. We controlled for regional macro-variables, as well as vacancy and employer characteristics. We examined the robustness of the results by estimating models without control variables and then gradually increasing control variables. Our results were robust to different control variable specifications.

While the baseline regressions included the vacancy postings in the top and bottom 15 areas, we estimated the same models for the top and bottom 20, 25 and 30 areas (Appendix Tables A2.2–A2.5). The results were similar, but naturally, the magnitudes of the treatment effects were smaller when the top and bottom groups included more areas.

As a robustness check, we estimated the models separately for vacancies with non-fixed application periods and vacancies with fixed application periods. Vacancy fillings have been comprehensively documented for vacancies with non-fixed application periods. For them, the end of the vacancy duration typically means that the vacancy has been filled. According to the results, most estimates were small and statistically insignificant for the vacancies with non-fixed application periods (Appendix Table A2.6). Their probability of receiving VRs did not increase in the top areas as compared to the bottom areas after the reform, and the effect on the probability of being filled was very low. However, the results show that the duration before the first VR and vacancy duration decreased slightly in the top areas as compared to the bottom areas.

Most vacancies had fixed application periods, and the reform particularly affected them (Appendix Table A2.7). For these vacancies, the probability of receiving at least one VR increased considerably in the top areas as compared to the bottom areas after the reform. The increase was 16 percentage points in 2014 and 17 percentage points in 2015 as compared to the 2012 level. In addition, vacancies in the top areas received VRs sooner after the reform: in 2014-2015, the duration before the first VR decreased by about 3 days in the top areas as compared to the bottom areas (column 2). The results indicate that vacancy filling rates increased in the top areas as compared to the bottom areas after the reform (column 3). The coefficient estimates are more positive than in the baseline analysis, but the estimate for 2014 remains statistically insignificant. The effect on vacancy duration was insignificant for vacancies with fixed application periods.

### 3.5.3 Treatment intensity regressions

We estimated treatment intensity regressions using the vacancy data from all 67 travel-to-work areas (Table 6). The treatment intensity measure was the regional change in the number of VRs in relation to vacancy postings from the 2011–2013 period to the 2014–2015 period. The minimum value was -0.13, and the maximum value was 2.22. Treatment intensities for all areas are reported in Appendix Table A1.1. According to the results, a higher treatment intensity was statistically significantly related to a higher probability of vacancies being filled and longer vacancy durations after the reform. In line with the baseline results, on average, VR probability increased, and the duration before the first VR decreased in areas with high treatment intensity after the reform.

Replicating treatment intensity regressions for vacancies with fixed application periods yielded similar results (see Appendix Table A2.8). However, because the vacancy duration was fixed for these vacancies, the increased filling rates in high-treatment-intensity areas may have been at least partially due to the increased vacancy durations.

Replicating treatment intensity regressions for vacancies with non-fixed application periods yielded results showing that higher treatment intensity was related to shorter vacancy durations (see Appendix Table A2.9). Because the treatment effect on the probability of a vacancy being filled was approximately zero, vacancies with non-fixed application periods were filled sooner in areas with high treatment intensity after the reform.

Thus, the results support the robustness of the main results. However, high treatment intensity was also related to longer vacancy durations for vacancies with fixed application periods, which complicates the interpretation of the results.

TABLE 6 Treatment intensity regression results

| | VR probability (1) | Duration before the first VR (2) | Probability of being filled (3) | Vacancy duration (4) |
|---|---|---|---|---|
| Treatment intensity x Dpost | 0.101*** | -2.09*** | 0.106*** | 5.69** |
| | (0.011) | (0.36) | (0.031) | (2.83) |
| Year/month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (67) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 1,214,765 | 390,831 | 1,214,765 | 1,214,765 |
| R2 | 0.142 | 0.036 | 0.374 | 0.085 |
| Mean Y | 0.322 | 5.85 | 0.297 | 20.90 |

Notes: Treatment intensity regression results for all vacancies in all areas from 2011 to 2015. The data included vacancy postings in 67 travel-to-work areas from 2011 to 2015. Treatment intensity is the regional change in the number of VRs in relation to vacancy postings from the 2011–2013 period to the 2014–2015 period. The minimum value was -0.13, and the maximum value was 2.22. Treatment intensities for all areas are reported in Appendix Table A1.1. Significance levels are as follows: *** 1%, ** 5% and * 10%. Standard errors (in parentheses) were clustered at the travel-to-work-area level (67 clusters). The models included year-month indicators (60), indicators for areas (67) and control variables. The control variables were the same as in Table 5.

In Table 7, treatment intensity regressions were estimated using the Cox proportional hazards model. The Cox regression is a useful method when complete information on vacancy fillings is not available for all vacancies, as it allows for censoring. Censoring occurs when a vacancy's vacancy duration ends, and the vacancy in question remains unfilled. In the Cox model, the baseline hazard may vary, and parameters describe the relationship between covariates and a vacancy's hazard of being filled at any given time t. As a proportional hazards model, a unit increase in a covariate is multiplicative with respect to the hazard rate. For example, an increase in treatment intensity leads to proportional scaling of the baseline hazard.

Table 7 reports the estimates for the hazard of a vacancy receiving a VR (column 1) and being filled (column 2). The results indicate a strong effect on the hazard rate of being filled. Specifically, a 1-unit-increase in treatment intensity (i.e., the rate of VRs/vacancy postings) increased the daily hazard of a vacancy being filled by 26% (calculated as (exp(0.229) - 1) x 100%) in 2014 and 38% in 2015. A 1-unit-increase in treatment intensity increased the VR hazard by 40% in 2014 and 64% in 2015. The coefficient estimates for Treatment Intensity x 2011 were not statistically significant, indicating similar pre-trends before 2013 in areas with high and low treatment intensities. The treatment effects for 2013 were statistically significant at the 5% level, suggesting anticipation effects in the top areas in 2013. The coefficients of the control variables support the results reported in Section 5.1 (see Table A2.10).

TABLE 7 Results of Cox proportional hazards model with treatment intensity

| | Hazard of receiving VR<br>(1) | Hazard of being filled<br>(2) |
|---|---|---|
| Treatment intensity x 2011 | -0.012 | -0.012 |
| | (0.062) | (0.047) |
| Treatment intensity x 2013 | 0.304** | 0.144** |
| | (0.127) | (0.063 |
| Treatment intensity x 2014 | 0.336*** | 0.229** |
| | (0.065) | (0.102) |
| Treatment intensity x 2015 | 0.494*** | 0.321*** |
| | (0.091) | (0.104) |
| Year-month indicators (60) | Yes | Yes |
| Area indicators | Yes | Yes |
| Control variables | Yes | Yes |
| Number of vacancy postings | 1,214,765 | 1,146,557 |

Notes: Treatment intensity regression results for all vacancies in all areas from 2011 to 2015. The Cox proportional hazards model was used. Long vacancy durations were right-censored from 180 days onwards (95% of vacancies had vacancy duration of less than 70 days). The duration of the first VR was adjusted to be at least one day. The data included vacancy postings in 67 travel-to-work areas from 2011 to 2015. Treatment intensity refers to the regional change in the number of VRs in relation to vacancy postings from the 2011–2013 period to the 2014–2015 period. The minimum value was -0.13, and the maximum value was 2.22. Appendix Table A1.1 provides the treatment intensities for all areas. The models included year-month indicators (60), indicators for areas (67) and control variables. Standard errors were clustered at the travel-to-work area level (67 clusters). Significance levels: *** 1%, ** 5% and * 10%. The coefficients of the control variables are reported in Table A2.10 in the Online Appendix.

### 3.5.4 Quality of VRs

The massive increase in the number of VRs may have affected their average quality. According to Hainmueller et al. (2016), caseload influences the effectiveness of JSA because it determines how much time a caseworker can devote to each client. They found that unemployed jobseekers who had been counselled in PES offices with lower caseloads were more successful in finding jobs.

Appendix Table A1.4 indicates that the effectiveness of VRs decreased after the reform. It seems to have suffered more in the top areas, where the number of VRs was increased the most. After the reform, a smaller share of VRs resulted in matches as compared to the years 2011–2012. In the top areas, the share of cases in which the individual who received the VR was hired decreased from 12% to 4%. One reason for the decreased VR matching rate may be that, after the reform, more VRs were given for the same vacancies. If there are many VRs per vacant job, only one VR can result in the vacancy being filled because only one of the applicants can be hired.

Simultaneously, the share of cases in which an employer did not approve the applicant increased from 6 percentage points to 18 percentage points in the top 15 areas. According to Maunu (2016), the main problem for employers recruiting new workers was that applicants had insufficient work experience or education. In addition to decreased VR quality, one reason for the increased rejection rate could be a negative change in the quality of applications: if jobseekers are pushed to apply against their will, they may send empty or low-quality job applications. Appendix Table A1.4 also shows that the cases in which the jobseeker did not contact the employer increased. In the bottom areas, the corresponding changes were in the same direction as but considerably smaller than the changes in the top areas.

Appendix Tables A3.1–A3.7 show vacancy and jobseeker characteristics in the top and bottom 15 areas before and after the reform. They show characteristics separately for all vacancies and the vacancies that received VRs. In addition, the tables show the characteristics of individuals who received VRs. Table A3.1 shows that vacancy characteristics were similar in the top and bottom areas before and after the reform. Table A3.2 shows that VRs were made in the same proportion to certain types of vacancies in the top and bottom areas. VRs were mainly made for vacancies for which the job type was wage work or rotation-leave-substitute. After the reform, VRs increased for vacancies with part-time work and shift work as compared to full-time work. About 50% of vacancies had job durations of over 12 months. Such vacancies received relatively fewer VRs, and vacancies with job durations below six months received relatively more VRs.

Appendix Table A3.3 shows that, after the reform, younger individuals received more and older individuals fewer VRs, both in the top and bottom areas. Individuals with low education levels received the majority of VRs. Moreover, it was quite common that VRs were directed to jobseekers who had different previous occupations than the referred vacancy (Table A3.4). It was also quite common that individuals received VRs to vacancies located in different municipalities than their municipalities of residence. After the reform, the proportion of these cases did not change in the top areas, but the absolute number increased.

The occupational distribution of vacancy postings was similar in the top and bottom areas (Appendix Table A3.5). The most common occupation of vacancies was service and sales workers (about 30%). Appendix Table A3.6 shows that such vacancies received a correspondingly large share of VRs. In turn, vacancies requiring professionals or technicians received relatively less VRs than their share of all vacancies.

### 3.5.5 Other labour market outcomes

In Figure 4, we examine whether the reform was followed by changes in other labour market outcomes. According to the results, trends in employment, unemployment and labour force participation rates were similar in the top and bottom areas both before and after the reform. Thus, even though the reform increased the number of VRs and vacancy filling rates, employment was not improved. The reform led to the tighter monitoring of job searches, and the number of individuals who received sanctions increased more in the top areas than in the bottom areas after the reform. Sanctions may have pushed some unemployed jobseekers out of the labour force, and the small gap in the labour force participation rates between the top and bottom areas narrowed slightly after the reform.

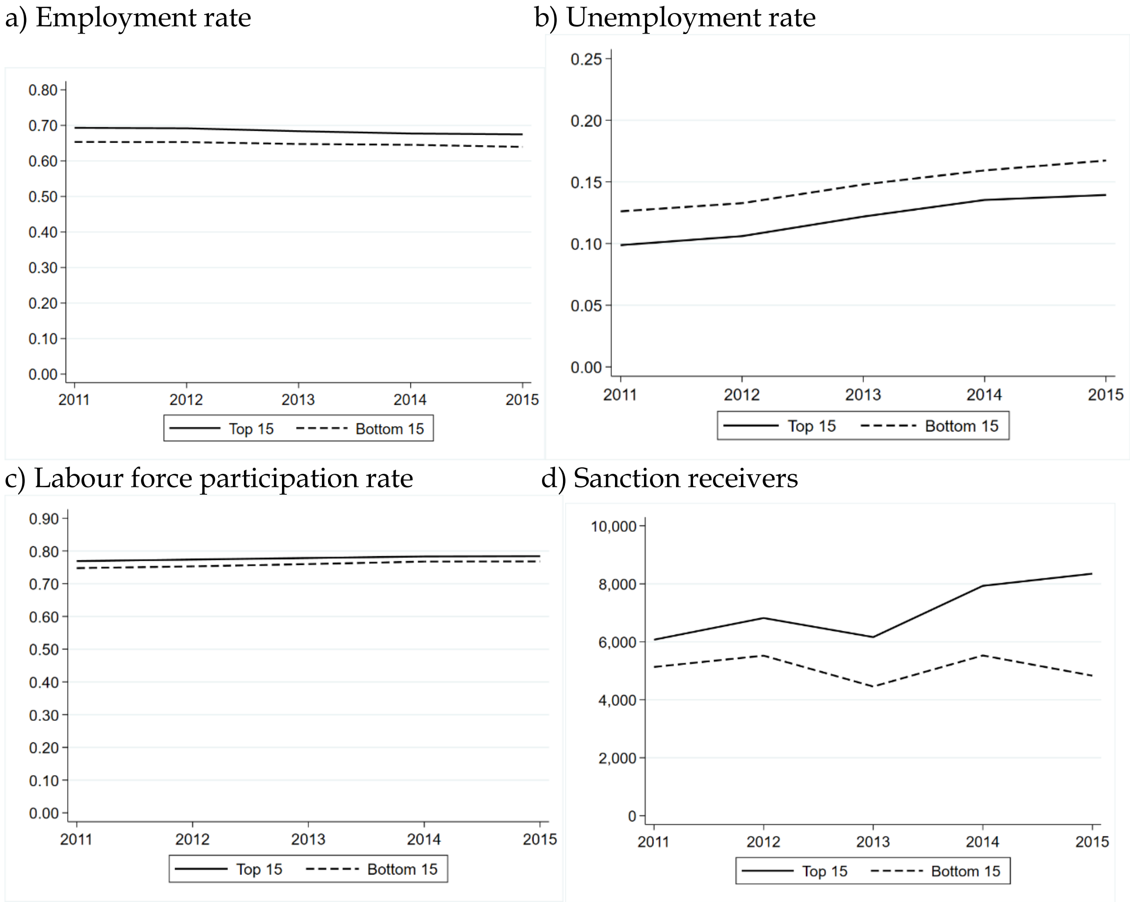


FIGURE 4 Employment rates, unemployment rates, labour force participation rates and the number of sanction receivers in the top 15 and bottom 15 areas from 2011 to 2015.

Notes: Individuals aged 18–64 at the end of each year.

Some Finnish reports have examined VRs and the 2014 reform. According to Räisänen and Järvelä (2014), the 2014 reform did not seem to affect the average unemployment duration. They reported that unemployment durations decreased for young individuals but increased for individuals over 50. Because they also report that the main increase in the number of VRs hit younger individuals, the results may be a sign of substantial displacement effects: the reform increased young jobseekers' employment probabilities at the cost of older jobseekers. Valtakari et al. (2014) noted that maintaining employers' positive attitudes towards VRs requires that VRs are sent to jobseekers who meet the needs of employers. According to Räisänen (2016), the total efficiency of VRs declined in 2015, and because of that, VRs should be used more selectively. After 2015, the number of VRs in relation to vacancy postings decreased.

It is worth considering why employment effects seem to be negligible despite the positive effect on vacancy filling rates. There are a few possible explanations. First, a large quantitative increase in VRs may have reduced the average quality of post-unemployment jobs. According to the previous literature, the jobs accepted after receiving VRs are less permanent than jobs found without VRs (e.g., Van den Berg et al., 2019). Related to this, the number of dismissals from filled vacancies may have increased if VRs pushed jobseekers into sub-optimal jobs. In some cases, longer vacancy duration can mean fairer recruitment practices or that more suitable jobseekers are hired (Adams et al., 2000). For these reasons, increased vacancy filling rates do not necessarily lead to increased employment rates.

Second, if the vacancies were filled by individuals who had already been employed, the increased vacancy filling rates are not reflected in employment or unemployment rates. Typically, most newly recruited workers had been employed in other jobs, and only a minority had been unemployed (Bagger et al., 2022; Maunu, 2016). The share of VRs for employed individuals did not increase after the reform, but the absolute number of such cases increased in the top areas (Table A3.3).

Third, the quantitative increase of VRs to PES vacancies may have caused displacement effects for the vacancies that were not announced to PES. Our data cover all vacancy postings that were announced to PES from 2011 to 2015. However, not all private sector vacancies are reported to PES. The increased filling rates of PES vacancies could have caused displacement effects for private sector vacancies that were not announced to PES. In this case, VRs boosted vacancy filling rates of the PES vacancies but at the cost of other vacancies.

Fourth, the PES data have imperfect information on the filling of vacancies that have fixed application periods. The increased vacancy filling rates in the top areas indicate that fillings occurred sooner, but some vacancies may have been filled later, after the application periods. It would be valuable to determine whether PES offices could inquire about the filling of vacancies after application periods end. Such data would better enable the evaluation of the quality and success of VRs and other PES policies related to vacancies and labour market matching.

## 3.6 Conclusions

The literature on VRs has focused on unemployed jobseekers searching for jobs, not firms searching for workers (e.g., Bollens and Cockx, 2017; Van den Berg et al., 2019; Van Belle et al., 2019). We investigated how a reform that increased the number of VRs affected vacancy filling rates. Using extensive and detailed Finnish PES vacancy data, we found that the reform increased the number of VRs in relation to vacancies considerably in some travel-to-work areas, while the change was minimal in other areas. Using a difference-in-differences approach, we found that vacancy filling rates increased in areas where the number of VRs increased the most. In those areas, after the reform, vacancy postings received, on average, more VRs; a larger share of vacancies received VRs and vacancies received VRs sooner. Thus, VRs can help employers obtain a larger pool of applicants than could be obtained via traditional means and cause applicants to apply sooner than they otherwise would. VRs are likely to be most effective for job openings that generally receive few applications.

However, despite the positive effects on vacancy filling rates, employment effects were negligible. One potential explanation is that VRs reduced the average quality and duration of post-unemployment jobs. Jobs accepted after receiving VRs have been documented to be less permanent (e.g., Van den Berg et al., 2019). Also, the number of resignations may have increased, particularly if VRs and the tighter monitoring of job searches forced unemployed jobseekers into sub-optimal jobs (e.g., Van den Berg and Vikström, 2014).

The massive increase in the number of VRs reduced their average quality and effectiveness. After the reform, a considerably lower share of VRs resulted in matches. Also, the share of cases in which the employer rejected an applicant who had received a VR increased considerably. This is in line with results indicating that caseload affects the performance of PES (Hainmueller et al., 2016). Most VRs were given to young individuals and individuals with low education levels, and many employers have reported that applicants had insufficient work experience or education (Maunu, 2016). Moreover, many referred jobseekers had previous occupations that were different from that of the referred vacancy. If unemployed jobseekers are pushed to apply against their will, they may send empty or low-quality job applications (e.g., Engström et al., 2012). It is important that VRs are sent to jobseekers who meet the needs of employers. If VRs cause some employers to receive unmanageable numbers of applications from unqualified candidates, employers may start to avoid VR applicants or even move to recruiting channels other than PES.

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# Appendix

## Appendix 1: Descriptive analysis

TABLE A1.1 Increase in VRs by travel-to-work area

| Area ID | Area | VRs/vacancy postings, 2011-2013 | VRs/vacancy postings, 2014-2015 | Change | Vacancy postings, 2011-2015 |
|---|---|---|---|---|---|
| 162 | Kokkolan | 1.09 | 0.96 | -0.13 | 19,939 |
| 193 | Torniolaakson | 0.97 | 0.95 | -0.02 | 2,089 |
| 181 | Kehys-Kainuun | 0.82 | 0.86 | 0.04 | 6,349 |
| 124 | Keski-Karjalan | 0.68 | 0.72 | 0.04 | 4,549 |
| 113 | Koillis-Savon | 1.22 | 1.37 | 0.16 | 1,830 |
| 103 | Savonlinnan | 0.31 | 0.48 | 0.16 | 16,272 |
| 122 | Joensuun | 0.53 | 0.70 | 0.17 | 48,375 |
| 101 | Mikkelin | 0.25 | 0.43 | 0.17 | 29,097 |
| 125 | Pielisen Karjalan | 0.64 | 0.86 | 0.22 | 8,095 |
| 178 | Koillismaan | 0.35 | 0.64 | 0.30 | 8,632 |
| 21 | Åboland-Turunmaan | 0.14 | 0.45 | 0.31 | 7,006 |
| 25 | Loimaan | 0.48 | 0.79 | 0.31 | 11,015 |
| 161 | Kaustisen | 0.46 | 0.79 | 0.32 | 4,962 |
| 153 | Sydösterbotten | 1.25 | 1.58 | 0.34 | 4,624 |
| 82 | Kotkan-Haminan | 0.90 | 1.24 | 0.35 | 27,536 |
| 197 | Pohjois-Lapin | 0.54 | 0.95 | 0.41 | 12,350 |
| 138 | Saarijärven-Viitasaaren | 0.38 | 0.81 | 0.43 | 8,526 |
| 135 | Äänekosken | 0.43 | 0.87 | 0.44 | 6,398 |
| 41 | Rauman | 0.40 | 0.85 | 0.45 | 31,374 |
| 151 | Kyrönmaan | 0.97 | 1.45 | 0.48 | 2,602 |
| 182 | Kajaanin | 0.54 | 1.06 | 0.52 | 25,118 |
| 22 | Salon | 0.23 | 0.76 | 0.52 | 22,208 |
| 71 | Lahden | 1.39 | 1.91 | 0.52 | 81,557 |
| 196 | Tunturi-Lapin | 0.28 | 0.82 | 0.54 | 12,483 |
| 132 | Joutsan | 0.11 | 0.65 | 0.54 | 1,706 |
| 115 | Sisä-Savon | 0.84 | 1.41 | 0.57 | 4,743 |
| 93 | Imatran | 0.48 | 1.07 | 0.58 | 11,386 |
| 154 | Jakobstadsregionen | 0.59 | 1.18 | 0.58 | 11,299 |
| 23 | Turun | 0.38 | 0.98 | 0.60 | 157,798 |
| 111 | Ylä-Savon | 1.44 | 2.04 | 0.60 | 19,265 |
| 16 | Loviisan | 0.16 | 0.76 | 0.60 | 6,566 |
| 194 | Itä-Lapin | 0.34 | 0.95 | 0.61 | 5,734 |
| 91 | Lappeenrannan | 0.71 | 1.32 | 0.61 | 33,720 |
| 133 | Keuruun | 0.57 | 1.20 | 0.63 | 3,364 |
| 43 | Porin | 0.37 | 1.02 | 0.65 | 51,833 |
| 11 | Helsingin | 0.82 | 1.48 | 0.66 | 895,709 |
| 24 | Vakka-Suomen | 0.96 | 1.62 | 0.66 | 16,017 |
| 174 | Raahen | 0.72 | 1.39 | 0.67 | 12,161 |

TABLE A1.1 (Continued)

| Area ID | Area | VRs/vacancy postings, 2011-2013 | VRs/vacancy postings, 2014-2015 | Change | Vacancy postings, 2011-2015 |
|---|---|---|---|---|---|
| 177 | Ylivieskan | 0.33 | 1.01 | 0.68 | 13,812 |
| 131 | Jyväskylän | 0.21 | 0.90 | 0.69 | 84,109 |
| 175 | Haapavesi-Siikalatvan | 0.31 | 1.01 | 0.70 | 4,744 |
| 134 | Jämsän | 0.17 | 0.90 | 0.72 | 7,140 |
| 15 | Porvoon | 0.18 | 0.94 | 0.76 | 25,009 |
| 176 | Nivala-Haapajärven | 0.49 | 1.27 | 0.78 | 6,630 |
| 105 | Pieksämäen | 0.73 | 1.53 | 0.80 | 10,880 |
| 141 | Suupohjan | 1.18 | 1.98 | 0.81 | 6,395 |
| 192 | Kemi-Tornion | 1.04 | 1.85 | 0.82 | 19,721 |
| 152 | Vaasan | 0.87 | 1.71 | 0.84 | 46,083 |
| 81 | Kouvolan | 1.28 | 2.17 | 0.90 | 31,924 |
| 191 | Rovaniemen | 0.63 | 1.54 | 0.91 | 29,863 |
| 171 | Oulun | 0.53 | 1.52 | 0.98 | 105,901 |
| 51 | Hämeenlinnan | 0.68 | 1.73 | 1.05 | 43,856 |
| 173 | Oulunkaaren | 0.65 | 1.71 | 1.06 | 6,547 |
| 112 | Kuopion | 1.21 | 2.28 | 1.07 | 78,265 |
| 114 | Varkauden | 0.96 | 2.13 | 1.17 | 13,591 |
| 68 | Lounais-Pirkanmaan | 1.32 | 2.63 | 1.31 | 9,072 |
| 14 | Raaseporin | 0.90 | 2.22 | 1.33 | 13,558 |
| 44 | Pohjois-Satakunnan | 0.28 | 1.65 | 1.37 | 6,349 |
| 144 | Kuusiokuntien | 1.94 | 3.38 | 1.44 | 5,816 |
| 61 | Luoteis-Pirkanmaan | 0.76 | 2.24 | 1.49 | 5,532 |
| 64 | Tampereen | 0.61 | 2.19 | 1.58 | 224,659 |
| 69 | Ylä-Pirkanmaan | 0.45 | 2.08 | 1.62 | 7,486 |
| 142 | Seinäjoen | 1.01 | 2.69 | 1.68 | 50,376 |
| 53 | Forssan | 0.26 | 1.98 | 1.72 | 11,844 |
| 63 | Etelä-Pirkanmaan | 0.47 | 2.43 | 1.96 | 13,367 |
| 146 | Järviseudun | 0.91 | 2.96 | 2.05 | 5,408 |
| 52 | Riihimäen | 0.61 | 2.83 | 2.22 | 19,535 |

Notes: The number of VRs in relation to vacancy postings by area.

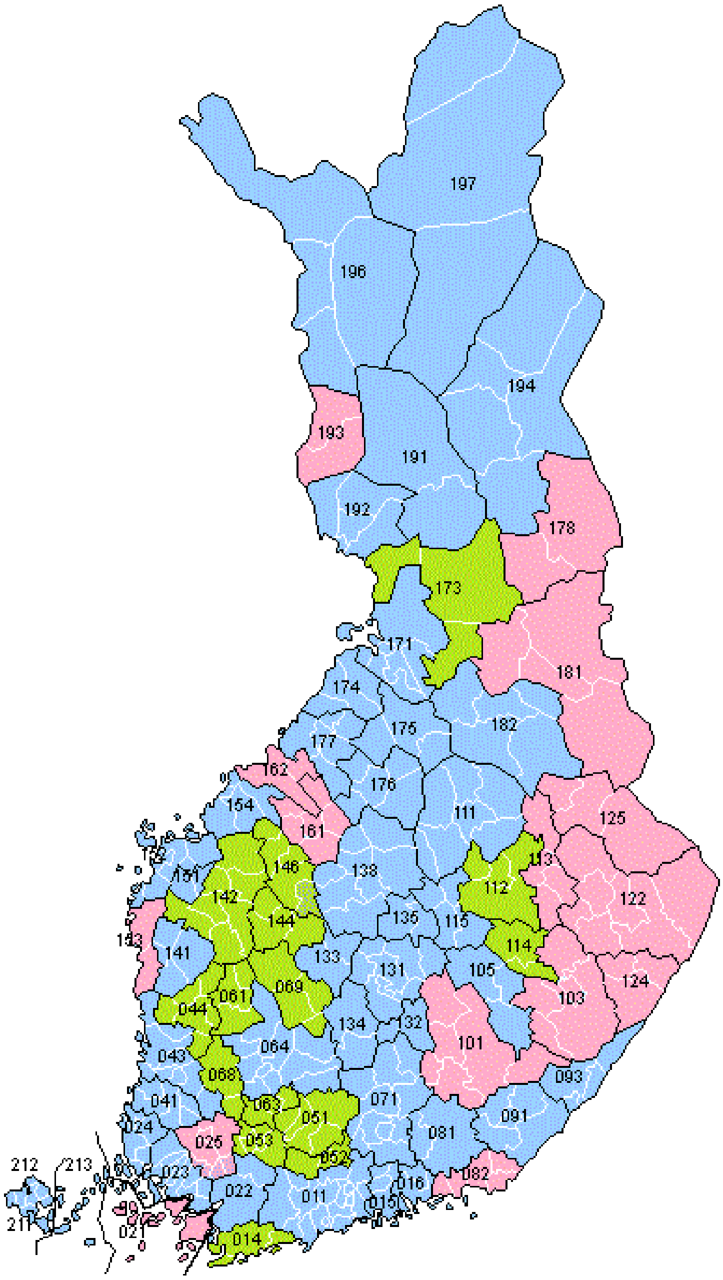


FIGURE A1.1 The top 15 and bottom 15 areas. Notes: Tampere area was not included in the top 15 areas because the bottom 15 areas did not include any comparable area (224,659 vacancy postings in Tampere area in 2011-2015). Sources: Statistics Finland, own calculations.

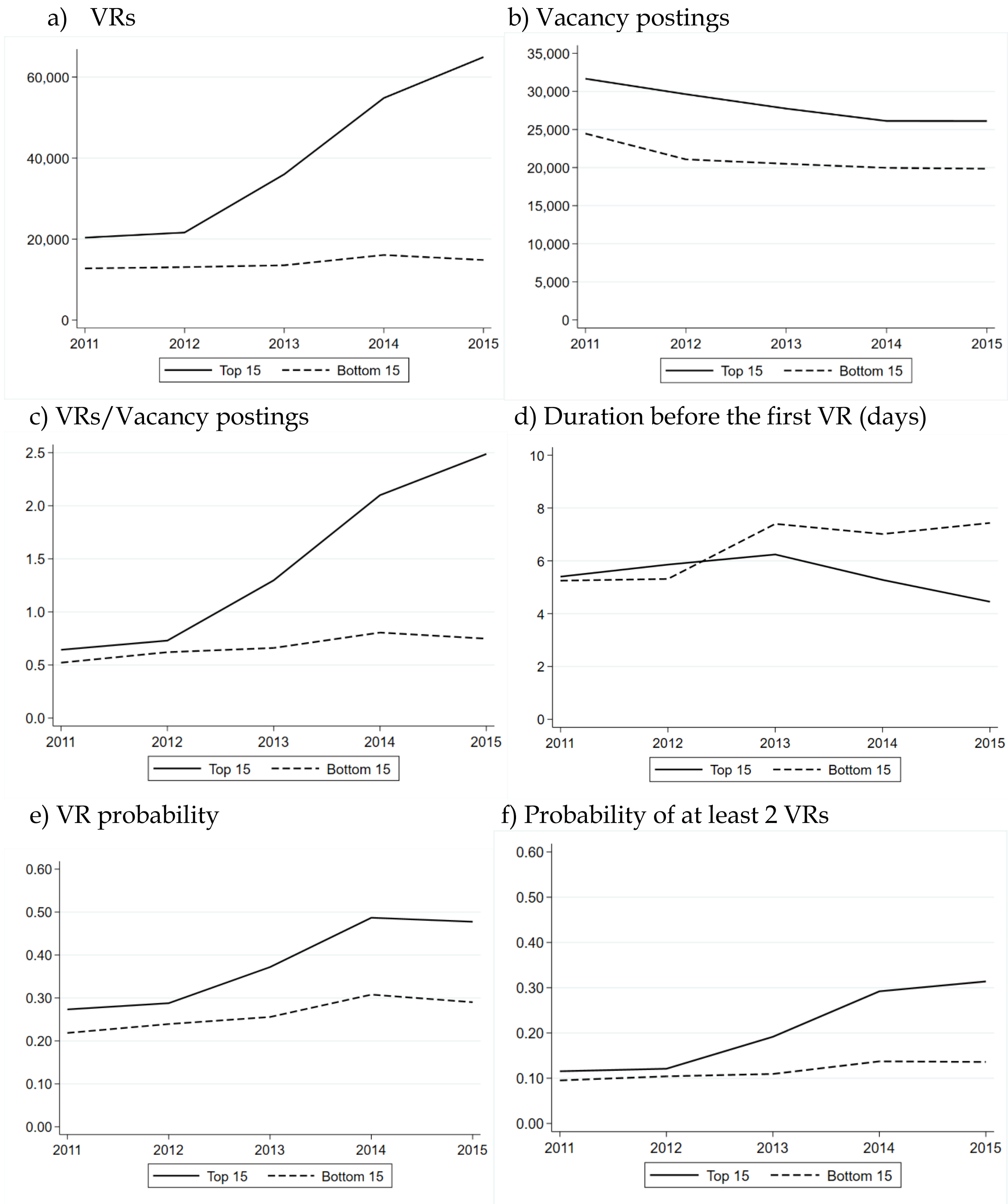


FIGURE A1.2 Vacancy Referrals and Vacancy Postings in the top 15 and bottom 15 areas in 2011-2015.

a) Vacancies/vacancy postings b) Vacancy postings with 2 or more vacancies

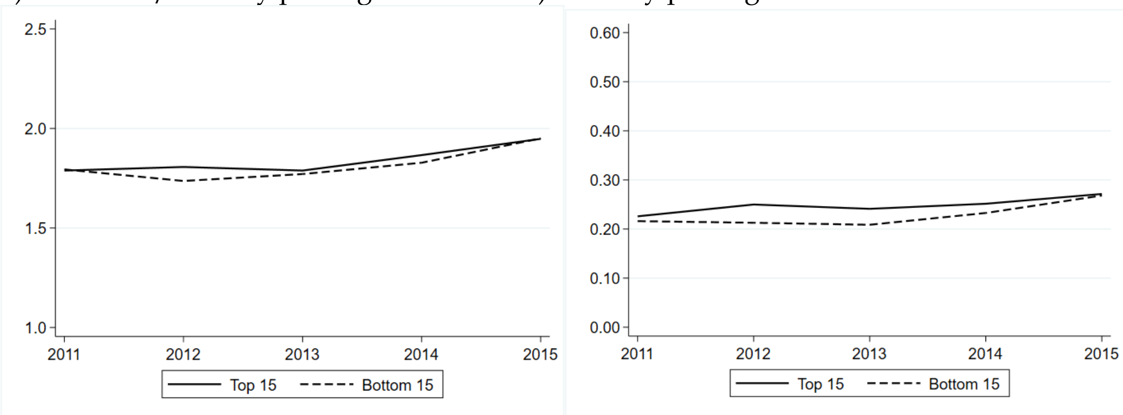


FIGURE A1.3 The number of vacancies in vacancy postings in the top 15 and bottom 15 areas in 2011-2015.

a) Sanction receivers b) Sanction receivers/Unemployed jobseekers

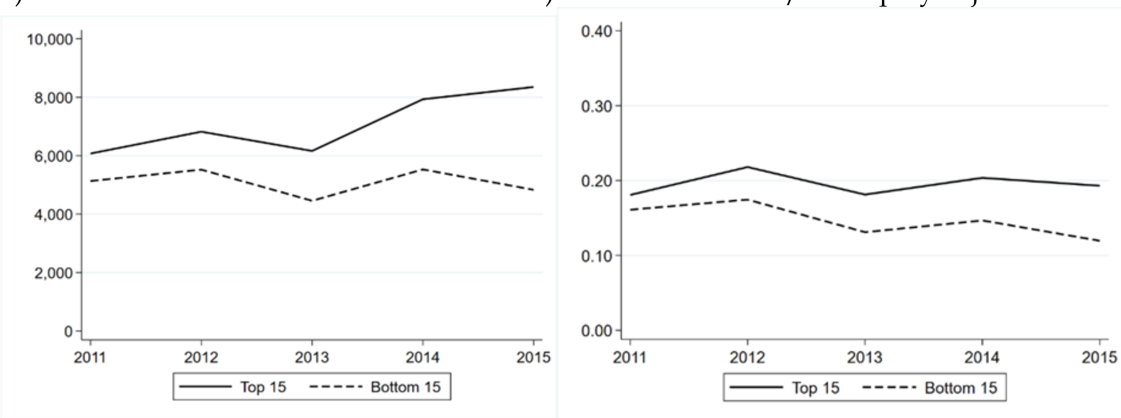


FIGURE A1.4 Sanctions in the top 15 and bottom 15 areas in 2011-2015.

a) All vacancies b) Vacancies with non-fixed application periods

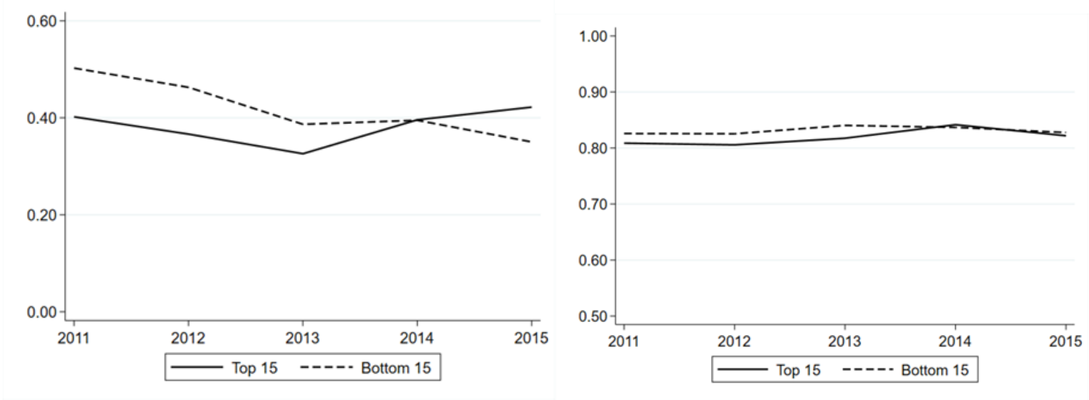


FIGURE A1.5 Vacancy filling rates in the top 15 and bottom 15 areas in 2011-2015.

a) All vacancies b) Vacancies with non-fixed application periods

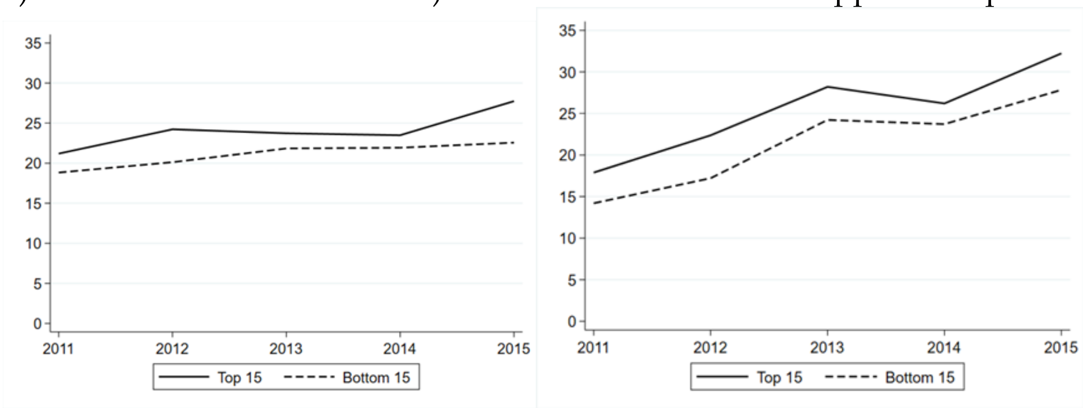


FIGURE A1.6 Vacancy durations in the top 15 and bottom 15 areas in 2011-2015.

TABLE A1.2 Vacancies, VRs and vacancy outcomes in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of vacancy postings | 61,306 | 52,229 | 45,546 | 39,817 |
| Fixed application period | 0.73 | 0.77 | 0.62 | 0.69 |
| Non-fixed application period | 0.27 | 0.23 | 0.38 | 0.31 |
| VRs | 41,803 | 118,734 | 25,912 | 30,988 |
| VRs/Vacancy postings | 0.68 | 2.27 | 0.57 | 0.78 |
| Vacancies with at least one VR | 0.31 | 0.48 | 0.25 | 0.30 |
| Duration before first VR (days) | 5.6 | 4.9 | 5.3 | 7.2 |
| Vacancy duration | 22.7 | 25.6 | 19.4 | 22.3 |
| Filled vacancies | 0.38 | 0.41 | 0.48 | 0.37 |
| **Vacancy outcomes** | | | | |
| Vacancy filled via VR | 0.12 | 0.10 | 0.15 | 0.13 |
| Filled by another PES's jobseeker | 0.14 | 0.18 | 0.18 | 0.13 |
| Vacancy filled online | 0.06 | 0.06 | 0.05 | 0.07 |
| Vacancy filled otherwise | 0.06 | 0.07 | 0.10 | 0.06 |
| Enough applicants | 0.03 | 0.01 | 0.03 | 0.02 |
| Vacancy cancelled | 0.06 | 0.05 | 0.07 | 0.05 |
| Application period ended | 0.53 | 0.52 | 0.43 | 0.56 |

Notes: Data sources: TEM Vacancies, TEM URA Vacancy referrals.

TABLE A1.3 Vacancy durations, application periods and selection periods in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of vacancy postings | 61,306 | 52,229 | 45,546 | 39,817 |
| Average vacancy duration | 23 | 26 | 19 | 22 |
| Vacancies with fixed application period | 44,747 | 40,314 | 28,264 | 27,645 |
| Average duration of the fixed application period | 19 | 19 | 21 | 21 |

Notes: Data sources: TEM Vacancies.

TABLE A1.4 Outcomes of VRs in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of VRs | 41,803 | 118,734 | 25,912 | 30,988 |
| **Outcomes** | | | | |
| Match: the individual who received the VR was hired | 0.12 | 0.04 | 0.13 | 0.10 |
| The vacancy was filled by another applicant | 0.12 | 0.10 | 0.14 | 0.14 |
| The employer did not approve the applicant | 0.06 | 0.18 | 0.07 | 0.11 |
| The individual did not accept the VR | 0.02 | 0.02 | 0.02 | 0.03 |
| The individual did not contact the employer | 0.02 | 0.05 | 0.02 | 0.04 |
| The VR did not lead to a contract | 0.03 | 0.04 | 0.03 | 0.04 |
| Vacancy was cancelled | 0.01 | 0.01 | 0.01 | 0.01 |
| Unknown | 0.62 | 0.54 | 0.57 | 0.50 |

Notes: Data sources: TEM URA Vacancy referrals.

## Appendix 2: Estimation results

TABLE A2.1 Estimation results

| | VR probability (1) | Duration before first VR (2) | Probability of being filled (3) | Vacancy duration (4) |
|---|---|---|---|---|
| Top 15 * year 2011 | .002 | -.230 | .006 | -1.06 |
| | (.02) | (.458) | (.031) | (.958) |
| Top 15 * year 2013 | .066* | -1.73** | .041 | -1.98 |
| | (.0326) | (.639) | (.033) | (1.61) |
| Top 15 * year 2014 | .125*** | -2.51** | .112 | -1.79 |
| | (.03) | (1.01) | (.072) | (1.23) |
| Top 15 * year 2015 | .135*** | -3.78*** | .158*** | 1.16 |
| | (.0372) | (.802) | (.054) | (2.56) |
| **Regional controls** | | | | |
| Unemployment rate | -.0027 | -.098 | -.011 | .194 |
| | (.00797) | (.243) | (.012) | (.677) |
| Vacancy rate | -.0014 | -.289* | .028 | .617 |
| | (.0061) | (.161) | (.021) | (.574) |
| Economic growth | -.0003 | .0173 | .004 | .178* |
| | (.0008) | (.018) | (.002) | (.0901) |
| **Vacancy controls** | | | | |
| Fixed application period | -.011 | -1.88*** | -.535*** | -6.55*** |
| | (.0124) | (.498) | (.0281) | (1.7) |
| **Work schedule type (vs. full-time work)** | | | | |
| 2-shift work | -.139*** | 3.63*** | -.028* | -.165 |
| | (.0193) | (1.00) | (.015) | (.754) |
| 3-shift work | .014* | .402 | .0115** | .166 |
| | (.008) | (.264) | (.005) | (.485) |
| 4-5 shift work | .0499** | .591* | .0149 | 1.67** |
| | (.019) | (.329) | (.015) | (.788) |
| Evening work | -.072*** | 1.09*** | .0416*** | -.0945 |
| | (.012) | (.392) | (.006) | (.47) |
| Part-time work | -.052*** | .278 | .0247*** | -1.55*** |
| | (.009) | (.252) | (.008) | (.389) |
| Other | -.114*** | 1.93** | .0619*** | -2.65 |
| | (.019) | (.798) | (.0136) | (1.6) |
| **Job duration (vs. below 1 month)** | | | | |
| 1-3 months | .0544*** | 1.57** | -.031*** | 6.47*** |
| | (.0098) | (.652) | (.009) | (1.14) |
| 3-6 months | .0663*** | 2.00*** | -.0529*** | 7.77*** |
| | (.010) | (.689) | (.008) | (1.37) |
| 6-12 months | .0516*** | 1.74** | -.061*** | 7.32*** |
| | (.0126) | (.651) | (.010) | (1.37) |
| Over 12 months | .0143 | 2.72*** | -.071*** | 9.20*** |
| | (.0136) | (.654) | (.00997) | (1.44) |

| | | | | |
|---|---|---|---|---|
| **Number of personnel (vs. 0-4)** | | | | |
| 5-9 | .015* | -.0449 | .023** | 2.55** |
| | (.0085) | (.198) | (.0085) | (1.18) |
| 10-19 | .0024 | -.865*** | .010 | -2.49*** |
| | (.0146) | (.299) | (.010) | (.574) |
| 20-49 | -.0259 | -.573** | .0016 | -3.33*** |
| | (.0201) | (.27) | (.0079) | (.454) |
| 50-99 | .0188*** | -.297 | .0172* | -2.11** |
| | (.0062) | (.403) | (.0100) | (.88) |
| 100-199 | -.0253** | -1.12*** | -.0131 | -3.61*** |
| | (.0119) | (.254) | (.015) | (.819) |
| 200-499 | -.0181 | -.462 | .012 | -2.33*** |
| | (.0122) | (.316) | (.0162) | (.60) |
| 500-999 | -.0105 | -.95*** | -.011 | -2.63*** |
| | (.0105) | (.341) | (.0144) | (.704) |
| Over 1000 | -.0145 | -.117 | -.0384** | -1.54 |
| | (.0109) | (.22) | (.0183) | (1.08) |
| Unknown | -.0173** | -.485*** | .0153 | -1.88*** |
| | (.0074) | (.158) | (.0098) | (.386) |
| **Sector (vs. public)** | | | | |
| Private | .023 | .466 | .0243 | .578 |
| | (.0156) | (.304) | (.0145) | (.46) |
| Other | .12*** | -1.29 | .0675*** | -1.96 |
| | (.0238) | (.838) | (.021) | (1.39) |
| **Job type: (vs. wage work)** | | | | |
| Commission pay | -.26*** | 9.37*** | -.0068 | .999 |
| | (.0176) | (1.39) | (.0266) | (1.31) |
| Entrepreneur | -.338*** | 40.2*** | -.142*** | 35.00*** |
| | (.0219) | (7.39) | (.0152) | (5.76) |
| Rotation leave substitute | .265*** | -3.56*** | .214*** | -21.1*** |
| | (.0698) | (.912) | (.0111) | (1.52) |
| **Required occupation (vs. Service & sales)** | | | | |
| Managers | -.187*** | 1.370 | -.0287 | -.0911 |
| | (.0161) | (1.17) | (.0217) | (.627) |
| Professionals | -.144*** | .978*** | -.0478*** | 2.23*** |
| | (.0135) | (.234) | (.0099) | (.486) |
| Technicians | -.092*** | .265 | -.0296*** | -.16 |
| | (.00541) | (.198) | (.00807) | (.35) |
| Clerical support | .0483*** | -1.66*** | .0157 | -1.08** |
| | (.012) | (.181) | (.00952) | (.504) |
| Agricultural, forestry | .0738*** | -1.24** | .035** | -1.73 |
| | (.017) | (.536) | (.015) | (1.23) |
| Craft | .0849*** | -1.11*** | -.005 | -.171 |
| | (.0232) | (.278) | (.0088) | (.745) |
| Plant &machine oper., drivers | .096*** | -1.92*** | .0243*** | -2.1*** |
| | (.02) | (.345) | (.00597) | (.663) |
| Other occupations | .0755*** | -.621*** | .0255*** | 1.58** |
| | (.0101) | (.202) | (.00597) | (.72) |

| | | | | |
|---|---|---|---|---|
| **Firm's industry (vs Sales)** | | | | |
| Agriculture, forestry, fishing | -.042** | 2.64*** | .0145 | 5.09*** |
| | (.02) | (.538) | (.0206) | (1.16) |
| Manufacturing | -.011 | 1.35*** | -.00895 | 3.19*** |
| | (.0186) | (.281) | (.0126) | (.918) |
| Construction | -.043*** | 1.22* | -.0349*** | 3.97*** |
| | (.013) | (.642) | (.0113) | (.991) |
| Transportation, storage | -.03** | 1.93*** | .00687 | 4.69*** |
| | (.0131) | (.317) | (.0139) | (.966) |
| Accommodation, food service | -.0475*** | 3.93*** | -.00557 | 10.9*** |
| | (.0089) | (.451) | (.0112) | (.951) |
| Information, communication | -.079*** | 3.60** | -.0463*** | 5.11** |
| | (.0165) | (1.55) | (.0128) | (2.14) |
| Financial, insurance | -.0469** | .599 | -.0251 | -.422 |
| | (.017) | (.419) | (.015) | (1.14) |
| Real estate activities | -.0329* | 2.33*** | .0318 | 2.57 |
| | (.0168) | (.645) | (.0188) | (1.97) |
| Professional, scientific, tech. | -.129*** | 2.63*** | -.0549* | -.829 |
| | (.0128) | (.444) | (.0302) | (1.34) |
| Administrative, support serv. | -.158*** | 2.56*** | -.0639*** | -.851 |
| | (.0216) | (.332) | (.0223) | (1.24) |
| Public administration, defence | -.0651** | 1.75*** | -.0073 | 2.7*** |
| | (.0242) | (.314) | (.0198) | (.545) |
| Education | -.0987*** | .829** | -.0068 | .86 |
| | (.0231) | (.384) | (.0126) | (.713) |
| Human health, social work | -.0908*** | 1.49*** | .021 | 3.37*** |
| | (.0136) | (.268) | (.0154) | (.724) |
| Arts, entertainment, recreation | -.0492*** | 1.52*** | -.0311** | 6.32*** |
| | (.0148) | (.521) | (.0127) | (1.11) |
| Other service activities | -.0533*** | 2.71*** | -.00394 | 7.67** |
| | (.0141) | (.801) | (.0107) | (3.36) |
| Other | -.0751*** | 2.46*** | .0255 | 2.74** |
| | (.0176) | (.576) | (.0179) | (1.27) |
| Constant | .337*** | 7.54*** | .796*** | 18** |
| | (.0856) | (2.29) | (.0964) | (6.68) |
| Year/month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Number of observations | 247,140 | 80,223 | 247,140 | 247,140 |
| R2 | 0.174 | 0.069 | 0.372 | 0.099 |
| Mean Y | 0.325 | 5.78 | 0.40 | 22.68 |

Notes: Regional controls: Monthly unemployment rates and vacancy rates in each area in the first day of each month. Regional annual economic growth rates in each area in each year. The model also includes indicators for areas (30) and year-month indicators (60), as well as indicators for required occupation (11) and firm's industrial classification (16). Standard errors are clustered at the travel-to-work area level (30 clusters). Significance levels: *** 1%, ** 5% and * 10%.

TABLE A2.2 Robustness of results; Outcome: VR probability

| | Top 15 vs. bottom 15 (1) | Top 20 vs. bottom 20 (2) | Top 25 vs. bottom 25 (3) | Top 30 vs. bottom 30 (4) |
|---|---|---|---|---|
| Top15 * year 2011 | 0.002 | -0.009 | -0.003 | 0.001 |
| | (0.020) | (0.019) | (0.015) | (0.014) |
| Top15 * year 2013 | 0.066* | 0.024 | 0.034 | 0.032* |
| | (0.033) | (0.025) | (0.021) | (0.019) |
| Top15 * year 2014 | 0.125*** | 0.074*** | 0.084*** | 0.073*** |
| | (0.030) | (0.024) | (0.023) | (0.018) |
| Top15 * year 2015 | 0.135*** | 0.113*** | 0.096*** | 0.091*** |
| | (0.037) | (0.029) | (0.027) | (0.022) |
| Year-month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 247,140 | 477,255 | 581,629 | 736,354 |
| R2 | 0.177 | 0.171 | 0.173 | 0.171 |
| Mean Y | 0.325 | 0.328 | 0.328 | 0.314 |

Notes: The data include vacancy postings in the top and bottom areas from 2011 to 2015. Standard errors (in parentheses) were clustered at the travel-to-work area level. Significance levels: *** 1%, ** 5% and * 10% (all two-sided tests). The control variables were the same as in Table A2.1.

TABLE A2.3 Robustness of results; Outcome: Duration before the first VR

| | Top 15 vs. bottom 15 (1) | Top 20 vs. bottom 20 (2) | Top 25 vs. bottom 25 (3) | Top 30 vs. bottom 30 (4) |
|---|---|---|---|---|
| Top15 * year 2011 | -0.21 | -0.30 | -0.30 | -0.23 |
| | (0.46) | (0.41) | (0.30) | (0.30) |
| Top15 * year 2013 | -1.72** | -1.02* | -0.78* | -0.49 |
| | (0.64) | (0.58) | (0.42) | (0.37) |
| Top15 * year 2014 | -2.50** | -2.29** | -1.93*** | -1.32** |
| | (0.99) | (0.85) | (0.61) | (0.52) |
| Top15 * year 2015 | -3.77*** | -3.68*** | -2.83*** | -2.30*** |
| | (0.79) | (0.71) | (0.64) | (0.53) |
| Year-month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 80,223 | 156,760 | 190,969 | 230,990 |
| R2 | 0.072 | 0.051 | 0.048 | 0.045 |
| Mean Y | 5.78 | 5.89 | 5.80 | 5.86 |

Notes: The data include vacancy postings in the top and bottom areas from 2011 to 2015. Standard errors (in parentheses) were clustered at the travel-to-work area level. Significance levels: *** 1%, ** 5% and * 10% (all two-sided tests). The control variables were the same as in Table A2.1.

TABLE A2.4 Robustness of results; Outcome: Probability of being filled

| | Top 15 vs. bottom 15 (1) | Top 20 vs. bottom 20 (2) | Top 25 vs. bottom 25 (3) | Top 30 vs. bottom 30 (4) |
|---|---|---|---|---|
| Top15 * year 2011 | 0.006 | -0.009 | -0.005 | -0.014 |
| | (0.031) | (0.018) | (0.017) | (0.014) |
| Top15 * year 2013 | 0.041 | 0.023 | 0.021 | 0.013 |
| | (0.033) | (0.030) | (0.023) | (0.019) |
| Top15 * year 2014 | 0.112 | 0.059 | 0.051 | 0.041 |
| | (0.072) | (0.046) | (0.036) | (0.031) |
| Top15 * year 2015 | 0.158*** | 0.146** | 0.121** | 0.097* |
| | (0.054) | (0.064) | (0.058) | (0.053) |
| Year-month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 247,140 | 477,255 | 581,629 | 736,354 |
| R2 | 0.372 | 0.382 | 0.376 | 0.382 |
| Mean Y | 0.400 | 0.358 | 0.353 | 0.342 |

Notes: The data include vacancy postings in the top and bottom areas from 2011 to 2015. Standard errors (in parentheses) were clustered at the travel-to-work area level. Significance levels: *** 1%, ** 5% and * 10% (all two-sided tests). The control variables were the same as in Table A2.1.

TABLE A2.5 Robustness of results: Outcome: Vacancy duration

| | Top 15 vs. bottom 15 (1) | Top 20 vs. bottom 20 (2) | Top 25 vs. bottom 25 (3) | Top 30 vs. bottom 30 (4) |
|---|---|---|---|---|
| Top15 * year 2011 | -1.02 | -0.12 | -0.51 | -0.52 |
| | (0.97) | (0.84) | (0.79) | (0.71) |
| Top15 * year 2013 | -1.93 | -1.41 | -1.29 | -0.96 |
| | (1.62) | (0.93) | (0.79) | (0.69) |
| Top15 * year 2014 | -1.80 | -1.42 | -1.07 | -1.09* |
| | (1.23) | (0.88) | (0.76) | (0.63) |
| Top15 * year 2015 | 1.16 | 5.77 | 5.20 | 4.97 |
| | (2.57) | (4.59) | (4.73) | (4.31) |
| Year-month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 247,140 | 477,255 | 581,629 | 736,354 |
| R2 | 0.090 | 0.094 | 0.089 | 0.090 |
| Mean Y | 22.68 | 22.41 | 22.18 | 21.63 |

Notes: The data include vacancy postings in the top and bottom areas from 2011 to 2015. Standard errors (in parentheses) were clustered at the travel-to-work area level. Significance levels: *** 1%, ** 5% and * 10% (all two-sided tests). The control variables were the same as in Table A2.1.

TABLE A2.6 Estimation results for vacancies with non-fixed application periods

| | VR probability (1) | Duration before first VR (2) | Probability of being filled (3) | Vacancy duration (4) |
|---|---|---|---|---|
| Top 15 * year 2011 | -0.033 | 0.50 | -0.001 | -0.70 |
| | (0.026) | (0.65) | (0.023) | (1.16) |
| Top 15 * year 2013 | 0.017 | -1.38 | 0.005 | -1.77 |
| | (0.050) | (0.97) | (0.013) | (1.17) |
| Top 15 * year 2014 | 0.036 | -0.64 | 0.030* | -2.85* |
| | (0.066) | (1.52) | (0.018) | (1.43) |
| Top 15 * year 2015 | 0.026 | -2.59** | 0.019 | -0.18 |
| | (0.065) | (1.01) | (0.016) | (1.88) |
| Year/month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Clusters | 30 | 30 | 30 | 30 |
| Number of vacancy postings | 71,933 | 30,067 | 71,933 | 71,933 |
| R2 | 0.179 | 0.141 | 0.080 | 0.216 |
| Mean Y | 0.418 | 5.92 | 0.824 | 22.57 |

Notes: The data include vacancy postings in the top and bottom 15 areas from 2011 to 2015. Standard errors (in parentheses) were clustered at the travel-to-work area level. Significance levels: *** 1%, ** 5% and * 10% (all two-sided tests). The control variables were the same as in Table A2.1.

TABLE A2.7 Estimation results for vacancies with fixed application periods

| | **VR probability** (1) | **Duration before first VR** (2) | **Probability of being filled** (3) | **Vacancy duration** (4) |
|---|---|---|---|---|
| Top 15 * year 2011 | 0.013 | -0.89 | 0.003 | -1.46 |
| | (0.023) | (0.62) | (0.049) | (1.13) |
| Top 15 * year 2013 | 0.089*** | -1.41* | 0.075 | -0.63 |
| | (0.032) | (0.81) | (0.047) | (1.64) |
| Top 15 * year 2014 | 0.162*** | -3.16*** | 0.156 | 0.17 |
| | (0.024) | (1.14) | (0.096) | (1.19) |
| Top 15 * year 2015 | 0.173*** | -3.36*** | 0.216*** | 3.68 |
| | (0.035) | (0.78) | (0.074) | (3.08) |
| Year/month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Clusters | 30 | 30 | 30 | 30 |
| Number of vacancy postings | 175,207 | 50,156 | 175,207 | 175,207 |
| R2 | 0.176 | 0.050 | 0.129 | 0.069 |
| Mean Y | 0.286 | 5.69 | 0.225 | 22.72 |

Notes: The data include vacancy postings in the top and bottom 15 areas from 2011 to 2015. Standard errors (in parentheses) were clustered at the travel-to-work area level. Significance levels: *** 1%, ** 5% and * 10% (all two-sided tests). The control variables were the same as in Table A2.1.

TABLE A2.8 Treatment intensity regression results for vacancies with fixed application periods

| | VR probability (1) | Duration before first VR (2) | Probability of being filled (3) | Vacancy duration (4) |
|---|---|---|---|---|
| Treatment intensity x Dpost | 0.106*** | -1.90*** | 0.138*** | 8.46** |
| | (0.010) | (0.26) | (0.040) | (3.52) |
| Year/month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (67) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 982,466 | 290,191 | 982,466 | 982,466 |
| R2 | 0.131 | 0.044 | 0.088 | 0.063 |
| Mean Y | 0.295 | 5.71 | 0.170 | 20.25 |

Notes: This table replicates the treatment intensity regressions in Table 6 using only vacancies with fixed application periods. The data included vacancy postings in 67 travel-to-work areas in 2011-2015. Treatment intensity is the regional change in the number of VRs in relation to vacancy postings from the 2011-2013 period to the 2014-2015 period. The minimum value was -0.13 and the maximum value was 2.22. Treatment intensities for all areas are reported in Table A1.1. Significance levels: *** 1%, ** 5% and * 10%. Standard errors were clustered at the travel-to-work area level (67 clusters). The models included year-month indicators (60), indicators for areas (67) and control variables. The control variables were the same as in Table A2.1.

TABLE A2.9 Treatment intensity regression results for vacancies with non-fixed application periods

| | VR probability (1) | Duration before first VR (2) | Probability of being filled (3) | Vacancy duration (4) |
|---|---|---|---|---|
| Treatment intensity x Dpost | 0.085** | -2.47*** | 0.001 | -2.59** |
| | (0.033) | (0.83) | (0.009) | (1.18) |
| Year/month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (67) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 232,299 | 100,640 | 232,299 | 232,299 |
| R2 | 0.169 | 0.064 | 0.081 | 0.169 |
| Mean Y | 0.433 | 6.27 | 0.836 | 23.62 |

Notes: This table replicates the treatment intensity regressions in Table 6 using only vacancies with non-fixed application periods. The data included vacancy postings in 67 travel-to-work areas in 2011-2015. Treatment intensity is the regional change in the number of VRs in relation to vacancy postings from the 2011-2013 period to the 2014-2015 period. Significance levels: *** 1%, ** 5% and * 10%. Standard errors were clustered at the travel-to-work area level (67 clusters). The models included year-month indicators (60), indicators for areas (67) and control variables. The control variables were the same as in Table A2.1.

TABLE A2.10 Results of Cox proportional hazards model with treatment intensity

| | Hazard of receiving VR (1) | Hazard of being filled (2) |
|---|---|---|
| Top 15 * year 2011 | -.0122 | -.0117 |
| | (.0616) | (.0465) |
| Top 15 * year 2013 | .304** | .144** |
| | (.127) | (.0626) |
| Top 15 * year 2014 | .336*** | .229** |
| | (.0647) | (.102) |
| Top 15 * year 2015 | .494*** | .321*** |
| | (.091) | (.104) |
| **Regional controls** | | |
| Unemployment rate | -.0406* | -.074** |
| | (.021) | (.0351) |
| Vacancy rate | .0047 | .00806 |
| | (.0168) | (.0137) |
| Economic growth | -.00341 | -.000253 |
| | (.00359) | (.00239) |
| **Vacancy controls** | | |
| Fixed application period | -.0268 | -1.03*** |
| | (.0241) | (.051) |
| **Work schedule type (vs. full-time work)** | | |
| 2-shift work | -.78*** | .00711 |
| | (.0511) | (.0328) |
| 3-shift work | .0452 | -.00724 |
| | (.0311) | (.0116) |
| 4-5 shift work | .122*** | -.0702*** |
| | (.0459) | (.0261) |
| Evening work | -.194*** | .0383 |
| | (.0344) | (.0256) |
| Part-time work | -.17*** | .135*** |
| | (.0143) | (.0166) |
| Other | -.313*** | .221*** |
| | (.0451) | (.059) |
| **Job duration (vs. below 1 month)** | | |
| 1-3 months | .175*** | -.276*** |
| | (.0356) | (.0684) |
| 3-6 months | .274*** | -.388*** |
| | (.033) | (.0713) |
| 6-12 months | .257*** | -.44*** |
| | (.032) | (.0626) |
| Over 12 months | .178*** | -.548*** |
| | (.0387) | (.062) |
| **Number of personnel (vs. 0-4)** | | |
| 5-9 | .05*** | .0263 |
| | (.0126) | (.05) |
| 10-19 | -.0468** | .191** |
| | (.0187) | (.0811) |
| 20-49 | -.0787*** | .104** |
| | (.0288) | (.0445) |

| | | |
|---|---|---|
| 50-99 | .0322* | .0915 |
| | (.0174) | (.0618) |
| 100-199 | -.066 | .026 |
| | (.0421) | (.04) |
| 200-499 | -.0515** | .106*** |
| | (.0239) | (.0365) |
| 500-999 | -.0308 | -.00383 |
| | (.0334) | (.0398) |
| Over 1000 | -.107*** | -.183*** |
| | (.0218) | (.0622) |
| Unknown | -.0734*** | .0561* |
| | (.0244) | (.0304) |
| **Sector (vs. public)** | | |
| Private | .0816** | .0464 |
| | (.0338) | (.0751) |
| Other | .34*** | .12 |
| | (.0454) | (.0753) |
| **Job type: (vs. wage work)** | | |
| Commission pay | -1.68*** | -.112*** |
| | (.195) | (.0318) |
| Entrepreneur | -2.07*** | -1.04*** |
| | (.179) | (.0863) |
| Rotation leave substitute | .883*** | 1.01*** |
| | (.0733) | (.0828) |
| **Required occupation (vs. Service & sales)** | | |
| Managers | -.793*** | -.183*** |
| | (.0759) | (.0478) |
| Professionals | -.607*** | -.269*** |
| | (.0404) | (.037) |
| Technicians | -.472*** | -.0876*** |
| | (.0289) | (.0182) |
| Clerical support | .15*** | .179*** |
| | (.0214) | (.0379) |
| Agricultural, forestry | .292*** | .0337 |
| | (.0495) | (.0292) |
| Craft | .235*** | -.0924*** |
| | (.0216) | (.0273) |
| Plant &machine oper., drivers | .281*** | .0755 |
| | (.0321) | (.048) |
| Other occupations | .313*** | .0372* |
| | (.0682) | (.0204) |
| **Firm's industry (vs Sales)** | | |
| Agriculture, forestry, fishing | -.213*** | -.223*** |
| | (.0288) | (.0513) |
| Manufacturing | -.141*** | -.23*** |
| | (.0372) | (.0505) |
| Construction | -.195*** | -.245*** |
| | (.0254) | (.0521) |
| Transportation, storage | -.189*** | -.1*** |
| | (.0254) | (.0243) |
| Accommodation, food service | -.214*** | -.432*** |

| | | |
|---|---|---|
| | (.0142) | (.0435) |
| Information, communication | -.336*** | -.331*** |
| | (.0444) | (.0502) |
| Financial, insurance | -.171** | -.295*** |
| | (.0762) | (.081) |
| Real estate activities | -.134*** | -.0935* |
| | (.0456) | (.055) |
| Professional, scientific, tech. | -.565*** | -.232*** |
| | (.0843) | (.0758) |
| Administrative, support serv. | -.58*** | -.222*** |
| | (.0656) | (.0743) |
| Public administration, defence | -.287*** | -.367*** |
| | (.0651) | (.0941) |
| Education | -.301*** | -.222** |
| | (.0362) | (.0949) |
| Human health, social work | -.249*** | -.224*** |
| | (.026) | (.0748) |
| Arts, entertainment, recreation | -.14*** | -.321*** |
| | (.0505) | (.0268) |
| Other service activities | -.249*** | -.282*** |
| | (.0717) | (.0596) |
| Other | -.419*** | .037 |
| | (.0335) | (.0254) |
| Year/month indicators (60) | Yes | Yes |
| Area indicators (67) | Yes | Yes |
| Number of observations | 1,214,765 | 1,146,557 |

Notes: Treatment intensity regression results for all vacancies in all areas from 2011 to 2015. The data included vacancy postings in 67 travel-to-work areas from 2011 to 2015. Treatment intensity refers to the regional change in the number of VRs in relation to vacancy postings from the 2011–2013 period to the 2014–2015 period. The minimum value was -0.13, and the maximum value was 2.22. Appendix Table A1.1 provides the treatment intensities for all areas. The Cox proportional hazards model was used. Long vacancy durations were right-censored from 180 days onwards. The duration of the first VR was adjusted to be at least one day. Standard errors were clustered at the travel-to-work area level (67 clusters). Significance levels: *** 1%, ** 5% and * 10%. Regional controls: Monthly unemployment rates and vacancy rates in each area in the first day of each month. Regional annual economic growth rates in each area in each year.

TABLE A2.11 Results of Cox proportional hazards model: hazard of a vacancy being filled

| | Top 15 vs. bottom 15 (1) | Top 20 vs. bottom 20 (2) | Top 25 vs. bottom 25 (3) | Top 30 vs. bottom 30 (4) |
|---|---|---|---|---|
| Top * 2011 | 0.061 | -0.013 | -0.003 | -0.043 |
| | (0.098) | (0.069) | (0.056) | (0.052) |
| Top * 2013 | 0.292*** | 0.142 | 0.139* | 0.107 |
| | (0.085) | (0.098) | (0.077) | (0.066) |
| Top * 2014 | 0.500*** | 0.272* | 0.248** | 0.223** |
| | (0.174) | (0.146) | (0.114) | (0.101) |
| Top * 2015 | 0.519*** | 0.300** | 0.255** | 0.190* |
| | (0.133) | (0.149) | (0.112) | (0.099) |
| Year-month indicators (60) | Yes | Yes | Yes | Yes |
| Area indicators (30) | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |
| Number of vacancy postings | 225,869 | 444,499 | 542,776 | 687,203 |

Notes: The data include vacancy postings in the top and bottom areas from 2011 to 2015. The Cox proportional hazards model was used. Long vacancy durations were right-censored from 180 days onwards. The duration of the first VR was adjusted to be at least one day. Standard errors (in parentheses) were clustered at the travel-to-work area level (67 clusters). Significance levels: *** 1%, ** 5% and * 10% (all two-sided tests). The control variables were the same as in Table A2.1.

## Appendix 3: Characteristics of vacancies and jobseekers

TABLE A3.1 Characteristics of all vacancies in the top 15 and bottom 15 areas

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of vacancy postings | 61,306 | 52,229 | 45,546 | 39,817 |
| Job type | | | | |
| Wage work | 0.87 | 0.86 | 0.88 | 0.83 |
| Commission pay | 0.04 | 0.05 | 0.03 | 0.05 |
| Entrepreneur | 0.03 | 0.03 | 0.02 | 0.03 |
| Rotation leave substitute | 0.06 | 0.06 | 0.07 | 0.09 |
| Working schedule type | | | | |
| Full-time work | 0.68 | 0.66 | 0.67 | 0.66 |
| 2-shift work | 0.01 | 0.01 | 0.01 | 0.01 |
| 3-shift work | 0.07 | 0.08 | 0.07 | 0.07 |
| 4-5 shift work | 0.05 | 0.04 | 0.04 | 0.04 |
| Part-time work | 0.08 | 0.09 | 0.10 | 0.09 |
| Evening work | 0.10 | 0.13 | 0.10 | 0.12 |
| Job duration | | | | |
| Below 1 month | 0.05 | 0.04 | 0.07 | 0.04 |
| 1-3 months | 0.13 | 0.14 | 0.14 | 0.14 |
| 3-6 months | 0.15 | 0.16 | 0.15 | 0.17 |
| 6-12 months | 0.14 | 0.14 | 0.15 | 0.15 |
| Over 12 months | 0.53 | 0.52 | 0.49 | 0.50 |
| Number of personnel | | | | |
| 0-4 | 0.21 | 0.16 | 0.18 | 0.15 |
| 5-9 | 0.07 | 0.09 | 0.07 | 0.07 |
| 10-19 | 0.08 | 0.07 | 0.08 | 0.07 |
| 20-49 | 0.09 | 0.11 | 0.10 | 0.08 |
| 50-99 | 0.05 | 0.05 | 0.05 | 0.05 |
| 100-199 | 0.05 | 0.07 | 0.07 | 0.11 |
| 200-499 | 0.06 | 0.09 | 0.07 | 0.09 |
| 500-999 | 0.04 | 0.06 | 0.04 | 0.06 |
| over 1000 | 0.09 | 0.14 | 0.11 | 0.16 |
| Unknown | 0.26 | 0.15 | 0.23 | 0.16 |
| Sector | | | | |
| Public | 0.26 | 0.24 | 0.28 | 0.26 |
| Private | 0.72 | 0.73 | 0.69 | 0.72 |
| Other | 0.02 | 0.02 | 0.02 | 0.02 |

Notes: Data sources: TEM Vacancies.

TABLE A3.2 Characteristics of vacancies which received VRs in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of VRs | 41,803 | 118,734 | 25,912 | 30,988 |
| Job type | | | | |
| Wage work | 0.93 | 0.97 | 0.91 | 0.91 |
| Commission pay | 0.00 | 0.01 | 0.00 | 0.00 |
| Rotation leave substitute | 0.07 | 0.02 | 0.08 | 0.08 |
| Work schedule type | | | | |
| Full-time work | 0.69 | 0.63 | 0.67 | 0.65 |
| 2-shift work | 0.09 | 0.11 | 0.09 | 0.10 |
| 3-shift work | 0.05 | 0.06 | 0.05 | 0.05 |
| Part-time work | 0.16 | 0.19 | 0.17 | 0.19 |
| Job duration | | | | |
| Below 1 month | 0.06 | 0.03 | 0.07 | 0.05 |
| 1-3 months | 0.18 | 0.19 | 0.19 | 0.18 |
| 3-6 months | 0.20 | 0.19 | 0.20 | 0.21 |
| 6-12 months | 0.14 | 0.14 | 0.13 | 0.15 |
| Over 12 months | 0.43 | 0.44 | 0.41 | 0.41 |

Notes: Data sources: TEM Vacancies, TEM URA Vacancy referrals.

TABLE A3.3 Characteristics of individuals who received VRs in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of VRs | 41,803 | 118,734 | 25,912 | 30,988 |
| Age: | | | | |
| Under 20 | 0.04 | 0.03 | 0.03 | 0.04 |
| 20-29 | 0.37 | 0.42 | 0.33 | 0.40 |
| 30-39 | 0.21 | 0.21 | 0.20 | 0.20 |
| 40-49 | 0.20 | 0.17 | 0.22 | 0.17 |
| 50-59 | 0.16 | 0.14 | 0.20 | 0.16 |
| Over 60 | 0.02 | 0.02 | 0.02 | 0.02 |
| Mean age | 35.6 | 34.2 | 37.0 | 35.0 |
| Male | 0.49 | 0.49 | 0.44 | 0.50 |
| Immigrant | 0.03 | 0.03 | 0.05 | 0.08 |
| Language | | | | |
| Finnish | 0.95 | 0.94 | 0.90 | 0.87 |
| Swedish | 0.02 | 0.02 | 0.04 | 0.05 |
| Other | 0.03 | 0.04 | 0.05 | 0.08 |
| Education level | | | | |
| Upper secondary level | 0.65 | 0.68 | 0.66 | 0.66 |
| Short-cycle tertiary education | 0.07 | 0.06 | 0.07 | 0.06 |
| Bachelor's or equivalent level | 0.09 | 0.11 | 0.10 | 0.11 |
| Master's or equivalent level | 0.03 | 0.04 | 0.03 | 0.04 |
| Unknown | 0.15 | 0.11 | 0.14 | 0.13 |
| Labour market status | | | | |
| Employed | 0.09 | 0.09 | 0.10 | 0.10 |
| Unemployed | 0.81 | 0.85 | 0.81 | 0.82 |
| Laid off | 0.02 | 0.01 | 0.02 | 0.01 |
| In ALMPs | 0.03 | 0.01 | 0.02 | 0.03 |
| Outside the labour force | 0.04 | 0.02 | 0.04 | 0.02 |
| Unknown | 0.02 | 0.01 | 0.02 | 0.02 |

Notes: Data sources: TEM URA Vacancy referrals, Folk Basic data.

TABLE A3.4 Locations and occupations of vacancies and individuals who received VRs in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of VRs | 41,803 | 118,734 | 25,912 | 30,988 |
| VR's vacancy and jobseeker were in the same travel-to-work area (Area ID) | 0.72 | 0.74 | 0.80 | 0.78 |
| VR's vacancy and jobseeker were in the same municipality | 0.56 | 0.56 | 0.63 | 0.61 |
| VR's vacancy and jobseeker had the same occupation class (first number) | 0.49 | 0.54 | 0.52 | 0.55 |

Notes: Data sources: TEM Vacancies, TEM Jobseekers, TEM URA Vacancy referrals. Classification of Occupations 2010. https://www2.tilastokeskus.fi/en/luokitukset/ammatti/

TABLE A3.5 Characteristics of all vacancies in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of vacancy postings | 61,306 | 52,229 | 45,546 | 39,817 |
| Required occupation | | | | |
| Managers | 0.01 | 0.01 | 0.01 | 0.01 |
| Professionals | 0.16 | 0.15 | 0.16 | 0.16 |
| Technicians | 0.20 | 0.19 | 0.19 | 0.21 |
| Clerical support workers | 0.04 | 0.03 | 0.04 | 0.04 |
| Service and sales | 0.27 | 0.31 | 0.31 | 0.31 |
| Agricultural, forestry | 0.02 | 0.01 | 0.02 | 0.01 |
| Craft & related trades | 0.10 | 0.10 | 0.08 | 0.09 |
| Plant & machine operators, assemblers, drivers | 0.07 | 0.06 | 0.06 | 0.05 |
| Elementary occupations | 0.11 | 0.11 | 0.11 | 0.10 |
| Employer's field of activity | | | | |
| Agriculture, forestry and fishing A | 0.02 | 0.01 | 0.03 | 0.02 |
| Manufacturing C | 0.07 | 0.05 | 0.07 | 0.06 |
| Water supply, sewerage, waste managements and remediation E | 0.00 | 0.01 | 0.00 | 0.01 |
| Construction F | 0.03 | 0.03 | 0.04 | 0.03 |
| Wholesale and retail trade; repair of motor vehicles G | 0.09 | 0.10 | 0.09 | 0.11 |
| Transportation and storage H | 0.03 | 0.02 | 0.03 | 0.02 |
| Accommodation and food service I | 0.04 | 0.03 | 0.04 | 0.04 |
| Information and communication J | 0.01 | 0.01 | 0.01 | 0.01 |
| Financial and insurance K | 0.01 | 0.01 | 0.01 | 0.01 |
| Real estate activities L | 0.02 | 0.01 | 0.03 | 0.01 |
| Professional, scientific, technical M | 0.08 | 0.07 | 0.07 | 0.06 |
| Administrative, support service N | 0.20 | 0.27 | 0.13 | 0.21 |
| Public administration and defence, social security O | 0.15 | 0.17 | 0.18 | 0.19 |
| Education P | 0.04 | 0.03 | 0.05 | 0.04 |
| Human health and social work Q | 0.13 | 0.11 | 0.14 | 0.11 |
| Arts, entertainment and recreation R | 0.01 | 0.01 | 0.02 | 0.01 |
| Other service activities S | 0.05 | 0.03 | 0.04 | 0.03 |
| Households as employers T | 0.02 | 0.01 | 0.02 | 0.02 |

Notes: Data sources: TEM Vacancies. Classification of Occupations 2010. https://www2.tilastokeskus.fi/en/luokitukset/ammatti/ Standard industrial Classification TOL 2008. https://www.stat.fi/en/luokitukset/toimiala/

TABLE A3.6 Characteristics of vacancies which received VRs in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of VRs | 41,803 | 118,734 | 25,912 | 30,988 |
| Vacancy's required occupation: | | | | |
| Professionals (Science, engineering, health, teaching, business, IT, legal) | 0.06 | 0.07 | 0.06 | 0.06 |
| Technicians and associate professionals (Science, engineering, health, business, legal, IT) | 0.10 | 0.10 | 0.12 | 0.12 |
| Clerical support workers | 0.07 | 0.06 | 0.06 | 0.05 |
| Service and sales workers | 0.28 | 0.32 | 0.31 | 0.31 |
| Skilled agricultural, forestry and fishery workers | 0.02 | 0.02 | 0.04 | 0.02 |
| Craft and related trades workers | 0.17 | 0.16 | 0.15 | 0.16 |
| Plant and machine operators, assemblers, drivers | 0.12 | 0.10 | 0.11 | 0.12 |
| Elementary occupations | 0.17 | 0.17 | 0.16 | 0.15 |
| Employer's field of activity | | | | |
| Agriculture, forestry and fishing A | 0.02 | 0.01 | 0.04 | 0.03 |
| Manufacturing C | 0.12 | 0.08 | 0.13 | 0.11 |
| Water supply, sewerage, waste managements, remediation E | 0.01 | 0.01 | 0.01 | 0.01 |
| Construction F | 0.07 | 0.04 | 0.06 | 0.05 |
| Wholesale and retail trade; repair of motor vehicles G | 0.15 | 0.15 | 0.12 | 0.12 |
| Transportation and storage H | 0.04 | 0.03 | 0.06 | 0.04 |
| Accommodation and food service I | 0.04 | 0.04 | 0.05 | 0.05 |
| Information and communication J | 0.01 | 0.01 | 0.01 | 0.01 |
| Financial and insurance K | 0.01 | 0.01 | 0.01 | 0.01 |
| Real estate activities L | 0.02 | 0.01 | 0.01 | 0.01 |
| Professional, scientific, technical M | 0.03 | 0.04 | 0.03 | 0.04 |
| Administrative, support service N | 0.18 | 0.29 | 0.10 | 0.19 |
| Public administration and defence, social security O | 0.13 | 0.13 | 0.15 | 0.15 |
| Education P | 0.03 | 0.01 | 0.03 | 0.02 |
| Human health and social work Q | 0.09 | 0.09 | 0.11 | 0.09 |
| Arts, entertainment and recreation R | 0.01 | 0.01 | 0.01 | 0.01 |
| Other service activities S | 0.03 | 0.02 | 0.03 | 0.03 |
| Activities of households T | 0.01 | 0.01 | 0.02 | 0.02 |

Notes: Data sources: TEM Vacancies, TEM URA Vacancy referrals. Classification of Occupations 2010. https://www2.tilastokeskus.fi/en/luokitukset/ammatti/
Standard industrial Classification TOL 2008. https://www.stat.fi/en/luokitukset/toimiala/

TABLE A3.7 Characteristics of jobseekers who received VRs in the top 15 and bottom 15 areas.

| | Top 15 2011-2012 | Top 15 2014-2015 | Bottom 15 2011-2012 | Bottom 15 2014-2015 |
|---|---|---|---|---|
| Number of VRs | 41,803 | 118,734 | 25,912 | 30,988 |
| Jobseeker's most recent occupation | | | | |
| Professionals | 0.07 | 0.08 | 0.06 | 0.08 |
| Technicians | 0.09 | 0.10 | 0.10 | 0.11 |
| Clerical support workers | 0.07 | 0.06 | 0.07 | 0.05 |
| Service and sales workers | 0.21 | 0.26 | 0.24 | 0.25 |
| Agricultural, forestry, fishery | 0.02 | 0.02 | 0.03 | 0.02 |
| Craft & related trades | 0.22 | 0.23 | 0.18 | 0.22 |
| Plant & machine operators, assemblers, drivers | 0.08 | 0.07 | 0.08 | 0.08 |
| Elementary occupations | 0.09 | 0.08 | 0.09 | 0.08 |
| Unknown | 0.15 | 0.09 | 0.14 | 0.11 |
| Jobseeker's field of education | | | | |
| Generic programmes | 0.05 | 0.04 | 0.04 | 0.05 |
| Education | 0.01 | 0.01 | 0.01 | 0.02 |
| Arts and humanities | 0.04 | 0.05 | 0.04 | 0.04 |
| Social sciences, journalism, and information | 0.01 | 0.01 | 0.01 | 0.01 |
| Business, administration and law | 0.17 | 0.17 | 0.16 | 0.14 |
| Natural sciences, mathematics, and statistics | 0.01 | 0.01 | 0.01 | 0.01 |
| ICT | 0.03 | 0.03 | 0.02 | 0.03 |
| Engineering, manufacturing, and construction | 0.34 | 0.33 | 0.31 | 0.33 |
| Agriculture, forestry, fisheries, and veterinary | 0.03 | 0.03 | 0.04 | 0.04 |
| Health and welfare | 0.13 | 0.15 | 0.17 | 0.16 |
| Services | 0.18 | 0.17 | 0.20 | 0.17 |

Notes: Data sources: TEM URA Vacancy referrals, Folk basic data, TEM jobseekers. Classification of Occupations 2010. https://www2.tilastokeskus.fi/en/luokitukset/ammatti/

# 4 BACK TO WORK: SANCTIONS OR VACANCY REFERRALS FOR THE LONG-TERM UNEMPLOYED?[13]

**Abstract**
This article examines the long-term effects of public employment services' vacancy referrals (VRs) and unemployment benefit sanctions on the labour-market outcomes of long-term unemployed jobseekers. The study used rich micro-level register data from Finland and applied a combination of matching and panel data methods. The results showed that VRs are an effective tool for helping long-term unemployed jobseekers. A VR increased employment probability by 51% (6.2 percentage points) over the following five years. Sanctions caused long-term unemployed individuals to exit the labour force and reduced their employment probability. This finding is related to incentive problems associated with the shift from unemployment benefits to other non-employment benefits. The study found evidence of an incentive trap: despite their significant employment effects, VRs and sanctions had minimal effects on long-term unemployed jobseekers' disposable income. Thus, this article demonstrates that it is complex to simultaneously provide long-term unemployed jobseekers with both comprehensive social security and good incentives for employment.

**Keywords:** long-term unemployment, employment services, unemployment policy, sanctions, vacancy referrals

**JEL codes:** J21, J64, J68

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## 4.1 Introduction

In the aftermath of the Great Recession in the 2010s, short-term and long-term unemployment increased in many countries. Unemployment, particularly long-term unemployment, is harmful to both individuals and economies, as workers lose skills, and human capital depreciation makes unemployment more persistent (e.g. Pissarides 1992; Ortego-Marti 2017). The rise in long-term unemployment is reported to have reduced the overall matching efficiency of the labour market, which explains much of the outward shift in the Beveridge curve after the Great Recession (Kroft et al. 2016).

The concept of negative duration dependence means that exit rates from unemployment fall with unemployment duration. In other words, the longer one has been unemployed, the less likely one is to find a job. Firms' rankings of applicants based on unemployment duration are a significant factor in explaining the observed negative duration dependence (Fernandez-Blanco and Preugschat 2018). Employers seem to perceive the long-term unemployed as indicative of less productive workers (Eriksson and Lagerström 2006; Kroft et al. 2013). The negative duration dependence can also be explained by lower search effort among long-term unemployed jobseekers (Kroft et al. 2016).

This article examines the potential of public employment services (PESs) in reducing long-term unemployment. I study the effects of vacancy referrals (VRs) and unemployment benefit sanctions on long-term unemployed jobseekers. Multiple studies have reported that job search assistance (JSA) and sanctioning schemes are the most effective active labour market policy (ALMP) measures in the short term (Vooren et al. 2019; Card et al. 2010, 2018). A VR is an official instruction from a PES caseworker for a jobseeker to apply for a specific vacant job. VRs are a key JSA tool that has been reported to reduce unemployment duration (Bollens and Cockx 2017; Van den Berg et al. 2019). Unemployment benefit sanctions have also been reported to increase job finding rates (Abbring et al. 2005; Busk 2016).

However, several questions remain, to whose resolution this article will contribute in multiple ways. First, previous studies have focused on short-term unemployed jobseekers who receive earnings-related unemployment benefits. According to Svarer (2011), the effects of sanctions differ for various types of unemployed individuals. Sanction effects seem to depend on the type of social benefit (e.g. Busk 2016). This study focused on long-term unemployed individuals, a group with the weakest employment prospects. Sanctions may not be effective for them because after receiving a sanction, they can apply for other types of social security transfers instead of unemployment benefits. In contrast, at least certain ALMPs have been reported to have more positive employment effects for the long-term unemployed jobseekers (Gerfin et al. 2005; Caliendo et al. 2008). VRs may be particularly effective for long-term unemployed individuals because their job search intensity is likely to be very low. On the other hand, VRs may not be effective for them if employers avoid individuals with long unemployment histories.

Second, most previous studies estimated only short-term effects. Positive short-term effects do not guarantee that the effects will be positive in the longer term. This study used comprehensive administrative register data from Finland, which made it possible to study a long follow-up period and estimate the long-term effects of sanctions and VRs on various labour-market outcomes. Estimating long-term effects gives information on the persistence of treatment effects and show how they evolve over time. It is particularly interesting if we observe permanent changes in long-term unemployed individuals' labour market outcomes.

Third, few studies have estimated effects on wages and income. This study estimated the effects of sanctions and VRs on disposable income – that is, the net income that accounts for social security transfers.

The identification of effects was based on the comparison of labour market outcomes between the treatment and control groups. I used propensity score matching (PSM) to create a matched sample. Multiple studies have used similar methods (e.g. Burger et al. 2022; Caliendo and Tübbicke 2020; Caliendo et al. 2008). The results showed that VRs are an effective tool for helping long-term unemployed jobseekers. A VR increased employment probability by 51% (6.2 percentage points) over the following five years. Moreover, I found that sanctions caused long-term unemployed individuals to exit the labour force and reduced their employment probability. This finding is related to incentive problems associated with the shift from unemployment benefits to other non-employment benefits (e.g. Boeri and Edwards 1998). My results show evidence of an incentive trap: Despite their significant employment effects, VRs and sanctions had minimal effects on long-term unemployed individuals' disposable incomes. Thus, this article demonstrates that it is difficult to simultaneously provide long-term unemployed jobseekers with both comprehensive social security and good incentives for employment.

This article is organised as follows: Section 4.2 presents the relevant literature. In Section 4.3, I provide a brief introduction to the Finnish unemployment-benefit system, benefit sanctions and VRs. Section 4.4 discusses the data and methodology. Section 4.5 reports the results, and Section 4.6 concludes the article.

## 4.2 Relevant literature

### 4.2.1 Unemployment benefit sanctions

The purpose of unemployment benefits is to provide financial opportunities for unemployed jobseekers to apply for jobs and to compensate for the financial losses caused by unemployment. Unemployment benefits entail a moral hazard effect by reducing the income gap between employment and continuing unemployment. Thus, high unemployment benefits can reduce job search intensity and raise reservation wages, which, in turn, prolong unemployment and reduce employment probability (Mortensen 1977). Job search requirements, monitoring and sanctions for non-compliance can reduce this moral hazard by decreasing the value of remaining unemployed and thus leading to higher job search intensity and lower reservation wages (Arni and Schiprowski 2019). Optimal policy has been argued to involve monitoring and benefit sanctions (Boone et al. 2007; McGuinness et al. 2019). McVicar (2008) showed that periods of zero monitoring increased the average unemployment duration and reduced the hazard rate for job entry. According to Van den Berg et al. (2022), sanctions are a key tool for incentivising unemployment benefit recipients to cooperate with PESs and take action to increase their chances of finding a job.

Sanctions are temporary unemployment benefit exclusions or cuts that are imposed on unemployed jobseekers when job search efforts are deemed insufficient and when ALMP programmes or suitable job offers are rejected. Sanctions have been reported to increase the job-finding rates of the unemployed (Abbring et al. 2005; Lalive et al. 2005; Busk 2016). However, sanctions have also been reported to increase transitions from unemployment to outside the labour force (Arni et al. 2013; Busk 2016). Moreover, sanctions seem to lower the quality of post-unemployment jobs, with some studies reporting that sanctioned individuals accept jobs with shorter durations and lower earnings than non-sanctioned individuals do (Van den Berg and Vikström 2014; Arni et al. 2013). Van den Berg and Vikström (2014) found that sanctioned individuals more often move to part-time jobs and jobs with lower occupational levels.

Some studies distinguish between the sanction imposition effect (the ex-post effect) and the threat effect (the ex-ante effect), arguing that both effects matter (Boone et al. 2009; Lalive et al. 2005; Arni et al. 2013). The threat effect means that the mere possibility of sanctions motivates individuals to actively search for work. The threat of a sanction affects all jobseekers, including non-sanctioned individuals. Identifying threat effects requires that the data contain a policy change in the monitoring regime. Some studies based on such data have suggested that the threat effect may be substantial (e.g. Boone et al. 2009).

Previous studies have indicated that threat effects are heterogeneous and weaker for the long-term unemployed. Although Rosholm and Svarer (2008) found strong and positive threat effects in Denmark, this effect was not observed for the long-term unemployed. Tuomala (2011) studied a Finnish activation reform that included unemployment benefit sanctions for those who refused to participate in ALMPs during a new activation period. According to the results, the reform had no effect on long-term unemployed jobseekers' employment probability.

At worst, sanctions do more harm than good. Van den Berg et al. (2022) analysed the effects of strict sanctions on young welfare recipients whose institutional setting includes sanctions that cancel benefits for three months. According to their results, the sanctions increased the job entry rate but also increased exits from the labour force and decreased wages. They also noted that sanctions can have negative side effects for the sanctioned individuals' quality of life, such as difficulties in paying rent and debt problems. According to Machin and Marie (2006), benefit cuts, sanctions and a tougher benefit regime can have the unintended consequence of increasing crime.

### 4.2.2 Vacancy referrals (VRs)

A VR is an official instruction from a PES caseworker for a jobseeker to apply for a specific vacant job. VRs are commonly used by PESs to improve the matching of jobseekers and vacancies. From the unemployed jobseekers' perspective, VRs are JSA but they also include monitoring because a refusal to apply to an assigned vacancy can lead to a sanction.

The existing evidence shows that JSA has positive effects on re-employment, particularly when combined with monitoring (Card et al. 2010, 2018; Kluve 2010; Vooren et al. 2019; McGuinness et al. 2019). VRs passed on by caseworkers to jobseekers constitute a key part of JSA. The majority of studies report that VRs increase the transition rate from unemployment to employment (e.g. Bollens and Cockx 2017; Van den Berg et al. 2019).

VRs function according to various mechanisms. First, they may directly enhance job search by suggesting potential jobs for which to apply. VRs can help referred jobseekers apply to the most relevant jobs sooner. According to Gorter and Kalb (1996), counselling and monitoring can encourage people to submit more applications. Belot et al. (2019) reported that JSA can broaden jobseekers' searches and thereby increase the number of job interviews for which they were selected.

Second, VRs have threat effects, providing incentives to search for work more actively to avoid sanctions. Previous studies have found that unemployed individuals are likelier to find a job when facing the threat of having to participate in mandatory ALMPs (Rosholm and Svarer 2008; Graversen and Larsen 2013). According to Van den Berg et al. (2019), jobs accepted after receiving VRs had lower wages and were less stable than jobs found without VRs. This suggests that some individuals increase their search activity and lower their reservation wages after receiving a VR.

However, some studies have reported non-significant or even negative results. According to Engström et al. (2012), one-third of the VRs did not result in job applications, and VRs did not have a significant impact on unemployment duration. Van Belle et al. (2019) reported VRs' adverse effects on employment probability. According to them, employers perceived referred jobseekers as being less motivated.

## 4.3 Institutional background

### 4.3.1 Unemployment benefits in Finland in the 2010s

Finland has three types of unemployment benefits: (i) unemployment insurance (UI) allowance, (ii) basic unemployment allowance (BUA) and (iii) labour market support (LMS). The long-term unemployed individuals discussed in this article all received LMS in the pre-treatment period. UI allowance and BUA require an employment history of at least 26 weeks (18h/week) over the last 28 months prior to unemployment (the employment condition). Eligibility for UI allowance also requires a six-month membership in an unemployment insurance fund. During the observation period of this study, the maximum duration of UI allowances and BUAs was 500 business days (Busk et al., 2021). Jobseekers who do not meet the employment condition or who have exhausted their UI allowance or BUA are entitled to LMS.

The UI allowance is based on prior earnings, whereas LMS (and BUA) entail a flat daily rate. In 2013, the average UI allowance was 1,441€/month, whereas the LMS was 746€/month (KELA 2014). The share of the unemployed receiving BUA is low, and the amount received is the same as for the LMS.

Individuals who do not qualify for LMS (e.g. individuals outside the labour force) can apply for basic social assistance (SA). One can also receive a general housing allowance (HA). SA is a means-tested benefit, so its amount depends on the income and assets of all family members. HA also depends on the income of all family members. Individuals can receive unemployment benefits and SA/HA simultaneously if their income is sufficiently low. In 2013, the average SA for a single person was 450€/month, and the average HA was 286€/month (THL 2015). In addition, it is possible to receive a child supplement for all benefits. Small-scale work, such as part-time work (maximum 300€/month), is allowed while receiving unemployment benefits.

### 4.3.2 Sanctions in Finland

Sanctions are intended to encourage unemployed individuals to either find a job or participate in ALMP programmes. The eligibility conditions for an unemployed individual to receive benefits and avoid sanctions are as follows: (i) register with PESs as an unemployed person, (ii) actively search for a full-time job, (iii) apply for the jobs suggested by PESs (via VRs), (iv) participate in the ALMPs offered by PESs, and (v) participate in establishing and following a job search plan (Alasalmi et al. 2020). Violations of these criteria can lead to a sanction. Misconduct can be noted by a PES caseworker, a potential employer or ALMP programme staff. No warnings were issued in the 2010s.

In Finland, a sanction entails the suspension of unemployment benefits for 15–90 days (see Table 1). After repeated misconduct during a six-month period, entitlement to unemployment benefits is restored after spending at least 12 calendar weeks (approximately 90 days) with a job, in an ALMP, as a full-time student or as a full-time entrepreneur. Otherwise, entitlement to unemployment benefits is restored only after five years. All sanctions entail a 100% reduction in benefits. Unemployed individuals who receive sanctions may apply for other benefits, such as SA and/or a general HA. According to Busk (2016), sanction policies in Finland are average relative to other countries in Europe, at least with respect to the sanction occurrence rate (10.2%) and the strictness of sanctions (100% reduction for eight weeks).

TABLE 1 Reasons for and duration of unemployment benefit sanctions

| Reason for a benefit sanction | Duration |
|---|---|
| Refusal of work | 60/90 days |
| Refusal of work whose duration is less than two weeks | 30 days |
| Refusal or dropping out of ALMPs | 60 days |
| Refusal to participate in creating or inspecting a job search plan | 15/30 days (+ until plan updated) |
| Neglect of job search plan agreements | 60 days |
| Repeated misconduct (during six months) | 12 weeks of work or ALMPs or five years |

Notes: This information can be found in Unemployment Security Act (Työttömyysturvalaki 1290/2002), see FINLEX (2022).

### 4.3.3 VRs in Finland

A VR is an official instruction from a PES caseworker for a jobseeker to apply for a specific vacant job. VRs include monitoring, and refusal to apply to an assigned vacancy can lead to a sanction. Certain reasons for refusals are considered valid, such as too-long commute, too-low wage, wrong profession and inability to work. [14]Young individuals and those with secondary education receive relatively more VRs than older and more highly educated individuals. A suggested job can be in a different municipality than where the jobseeker lives. In addition, suggested jobs quite often represent a different occupational category than the jobseeker's previous occupation. (Räisänen and Järvelä, 2014).

In 2014, the number of VRs by the PES was increased as a part of the Government structural policy programme (see Valtioneuvosto 2013). PES offices were guided to increase the number of VRs for unemployed jobseekers. According to the new policy, VRs should be made for a wider variety of job opportunities. After three months of being unemployed, vacancies would be offered also from outside the unemployed person's professional field.

[14] The duration of the daily commute exceeds three hours, the salary for full-time work is less than 1,134€/month, the wage paid for part-time work after deduction of travel costs is lower than the unemployment benefit or the job does not match education and work experience. See Työttömyysturvalaki 1290/2002 and 288/2012 (FINLEX 2022).

## 4.4 Data and methods

### 4.4.1 Data and study sample

This study used population-wide register data from Statistics Finland and the Ministry of Economic Affairs and Employment (TEM). The FOLK Basic database contains individual-level data on the population permanently living in Finland on the last day of each year, providing information on income, labour market status and demographic variables. The FOLK Employment database contains data on employment periods. The TEM Job Search and TEM URA Job Offers datasets provide information on sanctions and VRs by Finnish PESs. All datasets contained individual identifiers, which made it possible to link the datasets.

I limited the data to the years 2011–2019 because comprehensive data on VRs were only available from 2011 onwards. Focusing on 2014 treatment events ensured that we had enough data on pre-trends and made it possible to examine long-term effects. Moreover, in 2014, the number of VRs was massively increased as a part of the Government structural policy programme. The programme also instructed to implement stricter monitoring, which led to increased imposition of sanctions. Compared to 2013, in 2014 the number of VRs increased by 91% and the number of sanctions increased by 27% (see Online Appendix Table A14). Because of the reform, VRs and sanctions were issued to individuals who would not have received them before 2014.

I restricted the analysis to the individuals who were unemployed and had no employment days in 2011–2013. [15] I limited the sample to those individuals who did not receive any VRs or sanctions in 2011–2013. From the jobseeker's point of view, receiving a VR/sanction is unexpected after three years of unemployment without being given any VRs or sanctions. In addition, I limited the sample to the individuals who were between 30 and 50 years of age in 2013. This was done because unemployment benefit eligibility criteria for individuals under 25 years of age are stricter, whereas older workers may be eligible for early retirement schemes (e.g. Kyyrä and Pesola 2020). These restrictions ensured that the individuals in the sample were similar and shared common pre-trends.

In 2014, 344 of these 6,850 individuals received VRs, 565 received sanctions, 42 received both VRs and sanctions, and 5,899 did not receive any VRs or sanctions. In the VR analysis, the treatment group consisted of 386 individuals who had received VRs in 2014. Thus, the VR treatment group also included the 42 individuals who had received both a VR and a sanction because refusal to apply to an assigned vacancy can lead to a sanction. The data showed that in

[15] The definition of long-term unemployed varies. According to Statistics Finland, a jobseeker is a long-term unemployed person after being unemployed continuously for over a year. In some studies (e.g. Kroft et al. 2016), the long-term unemployed are unemployed workers whose unemployment duration is more than 26 weeks. In Busk (2016), those who have been unemployed for more than 500 days are considered long-term unemployed individuals.

most cases, VRs preceded sanctions. The VR control group consisted of the individuals who had not received any VRs or sanctions in 2014.

In the sanction analysis, the treatment group consisted of the 565 individuals who had received sanctions but no VRs in 2014, while the control group consisted of the individuals who had not received any sanctions or VRs in 2014. The individuals were followed until 2019. This process yielded 56,603 observations for the VR analysis and 58,252 observations for the sanction analysis.

### 4.4.2 Matching

To estimate treatment effects, the only difference between the treatment and control groups should be whether they received the treatment. Potential confounding variables influence both treatment selection and outcome variables, which may bias the estimated treatment effects. Table 2 shows that particularly in the VR analysis, the unmatched control group differed from the treatment group. The characteristics of the treatment group were more favourable than those of the control group: a larger share of jobseekers with high education levels and a smaller share of individuals with disabilities. In the sanction analysis, the treatment group had a lower share of highly educated individuals.

To solve the selection problem, I used PSM to create balanced control groups. The purpose of PSM is to find non-treated individuals who are similar to treated individuals in terms of all relevant observed pre-treatment characteristics. Multiple studies have used similar matching methods in the context of evaluating ALMP effects (e.g., Burger et al. 2022; Caliendo and Tübbicke 2020; Gerfin et al. 2005; Caliendo et al. 2008). I used detailed register data to identify untreated control groups with a nearly identical likelihood of being treated based on individual characteristics and area of residence. All individuals had similar unemployment histories because they had been unemployed for at least three years without any employment days.

I used single nearest-neighbour matching without replacement, which means that an individual from the control group was chosen as a matching partner for a treated individual who was closest in terms of propensity score. According to Imbens and Wooldridge (2009), using only a single match leads to the most credible inference with the least bias. It is recommended to include only the variables that influence the participation decision and the outcome variable simultaneously. The matching variables I used were age, prior income, dummies for gender, high education level, immigrant status, disability, having children under 7, participation in labour market training and regional employment office (REO) area dummies.[16] I used options for overlap and common support, which ensured the existence of potential matches in the control group. The PSMATCH2 programme by Leuven and Sianesi (2003) was used to implement the PSM method.

After implementing the PSM, I tested the covariate balance between the treated and non-treated groups. Table 2 shows that the matched control group

[16] Mainland Finland has 15 REOs.

was similar to the treatment group in both the VR analysis and sanction analysis. The balance statistics on bias and variance ratios indicated that the matched samples were well balanced. See Tables A1–A6 in the Online Appendix for more detailed descriptive statistics on matching quality (see also Rubin, 2001).

TABLE 2 Descriptive statistics for unmatched and matched samples in 2013 (treated vs. control observations)

| | **(1) Treated (Unmatched) Mean** | **(2) Untreated (Unmatched) Mean** | **(3) Treated (Matched) Mean** | **(4) Untreated (Matched) Mean** |
|---|---|---|---|---|
| **a) VR analysis** | | | | |
| Age | 41.59 | 41.97 | 41.51 | 41.66 |
| Male | 0.66 | 0.67 | 0.65 | 0.64 |
| Highly-educated | 0.27 | 0.15*** | 0.28 | 0.28 |
| Immigrant | 0.15 | 0.11** | 0.16 | 0.16 |
| Disability | 0.14 | 0.38*** | 0.14 | 0.14 |
| Children under 7 | 0.15 | 0.11** | 0.15 | 0.14 |
| In ALMPs in 2013 | 0.11 | 0.04*** | 0.12 | 0.11 |
| Disposable income in 2013 | 12513 | 12368 | 12759 | 13032 |
| Disposable income in 2012 | 12405 | 12140 | 12721 | 12954 |
| Disposable income in 2011 | 11968 | 11224** | 12249 | 12057 |
| Taxable income in 2013 | 9611 | 9347* | 9599 | 9612 |
| Taxable income in 2012 | 9931 | 9272*** | 9966 | 9883 |
| Taxable income in 2011 | 9977 | 8216*** | 9395 | 8914 |
| Number of observations | 386 | 5889 | 345 | 345 |
| **b) Sanction analysis** | | | | |
| Age | 40.76 | 41.97*** | 40.63 | 40.93 |
| Male | 0.65 | 0.67 | 0.64 | 0.66 |
| Highly-educated | 0.09 | 0.15*** | 0.10 | 0.09 |
| Immigrant | 0.10 | 0.11 | 0.11 | 0.11 |
| Disability | 0.36 | 0.38 | 0.38 | 0.38 |
| Children under 7 | 0.08 | 0.11* | 0.09 | 0.07 |
| In ALMPs in 2013 | 0.03 | 0.05 | 0.03 | 0.04 |
| Disposable income in 2013 | 12482 | 12368 | 12669 | 12588 |
| Disposable income in 2012 | 12217 | 12140 | 12515 | 12534 |
| Disposable income in 2011 | 11272 | 11224 | 11586 | 11740 |
| Taxable income in 2013 | 8901 | 9347*** | 9143 | 9099 |
| Taxable income in 2012 | 8928 | 9272** | 9024 | 9045 |
| Taxable income in 2011 | 7764 | 8216** | 7889 | 7895 |
| Number of observations | 565 | 5899 | 478 | 478 |

Notes: p-value tests for the significance of difference in means between treated and non-treated: *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). The matching variables included dummies for 15 REO areas. See Online Appendix Tables A1–A6 for more detailed descriptive statistics.

### 4.4.3 Matched sample model

To estimate VR and sanction effects on employment, labour force participation and income, this study employed an approach similar to Böckerman et al. (2019). I compared the outcomes of individuals who had received a VR/sanction in 2014 to the outcomes of those who had not received any VRs or sanctions in 2014. The data restrictions ensured that the treated individuals were observed every year for three years before and five years after the first imposition of a VR or a sanction. To estimate the average treatment effects on the treated individuals, I used the following equation:

$$Y_{ist} = \sum_{j \neq -1} \alpha_j \boldsymbol{I}\,[t = j] + \sum_{k} \beta_k \boldsymbol{I}\,[age_{is} = k] + \mu_r + \lambda_s + X_{ist}\delta + v_{ist} \qquad (1)$$

The outcome is denoted by $Y_{ist}$ for individual $i$ in year $s$ at event time $t$. The key outcome variables were employment, employment days, labour force participation and disposable income. Employment was measured as a dummy variable equal to 1 for individuals who were employed during the year. Employment days referred to an individual's employment days during the year. Labour force status was measured during the last week of the year. An individual was classified as being in the labour force if the individual was employed or unemployed during the last week of the year. The preferred income measure was total annual disposable income, as it is the net income that accounts for social security transfers and taxation.[17]

The terms on the right-hand side of the equation above are dummies for event time and each year of age, $\mu_r$ are REO area fixed effects, $\lambda_s$ are year fixed effects, $X_{ist}$ are controls and $v_{ist}$ is an unobserved error term. The event time $t$ was indexed relative to the year of the first VR/sanction. The event time dummy for $t$ = -1 was omitted, and the coefficients for the other event time dummies measured the effect of a VR/sanction relative to one year before the VR/sanction. Year fixed effects were included to capture differences in macroeconomic conditions, and area fixed effects were included to capture regional differences in labour-market conditions. Standard errors were clustered at the individual level to account for unobservable, within-person variation in outcomes. The estimation was implemented using the EVENTDD programme (Clarke and Tapia-Schythe 2021).

The identification of the effects requires, first, that the treatment and control groups have parallel trends in outcomes. In our setting, pre-trends in employment and labour force participation were identical because the sample consisted only of individuals who were unemployed without any employment days in 2011–2013. Matching based on prior income ensured that pre-trends in

[17] Disposable income = (gross income, including wage income, entrepreneurial income, property income and income transfers received) – (paid income transfers, such as taxes). Income measures were adjusted to 2010 euro value by using the consumer price index.

income were also similar.[18] Second, the approach also requires that there be no anticipatory effects. This was likely to hold in our setting because the individuals in the sample had been unemployed for three years without receiving any VRs or sanctions. Third, the composition of the treatment and control groups should be stable. This held because the same individuals were followed. The matching ensured that the individuals in both groups were similar in all relevant observed pre-treatment characteristics.

## 4.5 Results and Discussion

### 4.5.1 VR effects

The results showed that VRs increased employment probability and the number of employment days (Figure 1). The employment effect was long lasting, and the estimate increased over time (Figure 1a). The effect on labour force participation was also positive, but the estimate was statistically significant at the 5% level only in one post-treatment year (Figure 1b). Pre-trends in employment and labour force participation were identical because the sample consisted only of individuals who were unemployed without any employment days in 2011–2013.

Table 3 reports the average effect estimates for the outcomes in years 0–5 after a VR – that is, the results of the model in which the event time dummies from 0 to 5 were replaced with a single post-treatment dummy. Jobseekers who received a VR had a 6.2 percentage points higher probability of finding employment compared to the matched control group. Receiving a VR was associated with an increase in annual employment days by approximately 20 and with a 4.1 percentage point increase in the probability of staying in the labour force.

The positive employment effect of VRs is in line with previous studies. According to Van den Berg et al. (2019), receiving a VR leads to an increase of approximately 74% in the relative probability of finding a job for individuals who had been unemployed for more than 1.5 years. Bollens and Cockx (2017) reported that VRs sent by caseworkers more than triple the transition rate to employment, while automatic referrals double this rate. They mentioned that the treatment effects were so large because they were measured in proportional terms, and the individuals analysed in their data exhibited very low transition rates to employment in the absence of the treatment. According to the results, individuals who received a VR had, on average, 51% (= 0.062/0.121) higher employment probability and 5.4% (= 0.041/0.758) higher labour force participation rate compared to the matched control group in years 0–5 after receiving a VR.

[18] Figures A3–A7 in the Online Appendix illustrate the pre- and post-treatment trends in outcomes for the matched control and treatment groups.

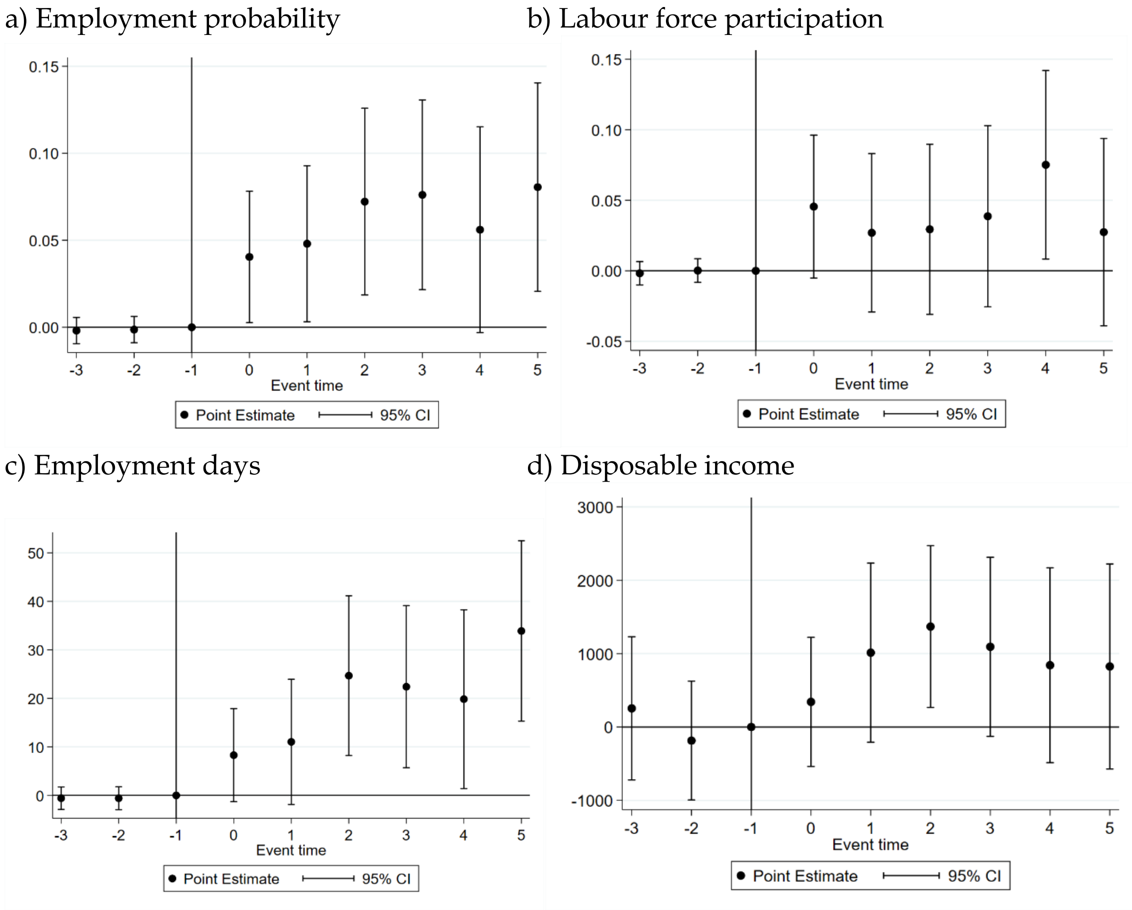


FIGURE 1 Impact of VRs on labour-market outcomes

Notes: Estimated coefficients and 95% confidence intervals. The reference year was t = -1. Estimations were based on the matched sample of treated and untreated individuals. The full results of each regression are shown in Online Appendix Table A7.

The results showed that long-term unemployment likely involves incentive traps. Despite their strong employment effects, VRs did not have very strong effects on annual disposable income (Figure 1d). The average effect on annual disposable income (+914 euros) was significant only at the 10% level. At the 95% confidence interval (two-sided tests), we could rule out positive effects larger than 1,942 euros (15% of average annual disposable income). Annual taxable income was, on average, 1,211 euros higher for VR recipients than for the control group.[19] The insignificant effect of VRs on disposable income can mean that higher earnings were mitigated by welfare subsidy cuts and progressive taxation of labour earnings. According to Van den Berg et al. (2019), jobs taken after receiving a VR have lower wages and are less stable than jobs found without receiving a VR. Although it is possible that a lower-quality job can lead to better-quality jobs in

[19] Taxable income includes earnings from employment and taxable social benefits. Income measures were adjusted to 2010 euro value by using consumer price index. See Table A13 and Figure A1 in the Online Appendix.

the future, the results showed no significant effects of VRs on disposable income even five years after receiving a VR.

There are a few potential explanations behind the positive employment effects. First, VRs may directly enhance job search by suggesting potential jobs to apply for. Second, VRs have threat effects, providing incentives to search for work more actively to avoid sanctions. Third, the potential reduction in job quality following VRs suggests that VRs may lead to some individuals lowering their reservation wages. The positive long-term effects may reflect that for the long-term unemployed jobseekers, even a short employment period may greatly improve employment prospects in the future. According to Eriksson and Rooth (2014), subsequent work experience eliminates the negative influence of past long-term unemployment.

### 4.5.2 Sanction effects

According to the results, sanctions pushed long-term unemployed jobseekers not towards work but rather out of the labour force. Labour force participation decreased sharply after the imposition of a sanction, and receiving a sanction was associated with a lower labour force participation rate for up to five years (Figure 2b). The point estimate of -0.101 indicated that sanctioned jobseekers exited the labour force with, on average, a 10 percentage points higher probability for five years after being sanctioned (Table 3). The average effect on employment probability was negative (-3.1 percentage points), and sanctions decreased annual employment days by approximately 9 days on average.

This study found no significant differences in annual disposable income between sanctioned individuals and the matched control group (Figure 2d and Table 3). At the 95% confidence interval (two-sided tests), we could rule out negative effects larger than -929 euros per year (7.5% of average annual disposable income). The results in Online Appendix Table A13 indicate that a sanction reduced annual taxable income by approximately 970 euros on average. This could be explained by social security transfers: Whereas long-term unemployed individuals receive LMS, individuals outside the labour force can get basic SA. LMS is taxable income, and SA is a tax-free benefit. As the effect on disposable income was, on average, very small, it seems that many sanctioned individuals exited the labour force and received SA instead of unemployment benefits.

The negative effect on labour force participation is in line with the previous literature, but the negative effect on long-term unemployed jobseekers' employment probability is a new finding. Many previous studies have found positive sanction-imposition effects, although the results seem to depend on the type of social benefit and institutional background (Busk 2016; Van den Berg et al. 2004). Abbring et al. (2005) reported that for UI benefit recipients, the effect of sanctions on the re-employment rate was 58–67%. According to the results, individuals who were sanctioned had, on average, 31% (= –0.031/0.099) lower employment probability and 14% (=–0.101/0.709) lower labour force participation rate compared to the matched control group in years 0–5 after receiving a sanction. It should be noted that most previous studies have estimated only short-term effects on the exit rates from unemployment, leaving the longer-term effects unclear. One exception is Van den Berg et al. (2019), who found evidence of lower sanction effects over the longer term. According to their results, the impact of sanctions on employment was significantly positive in the first three months after the imposition (+40%), but after that the estimate was not significantly different from zero.

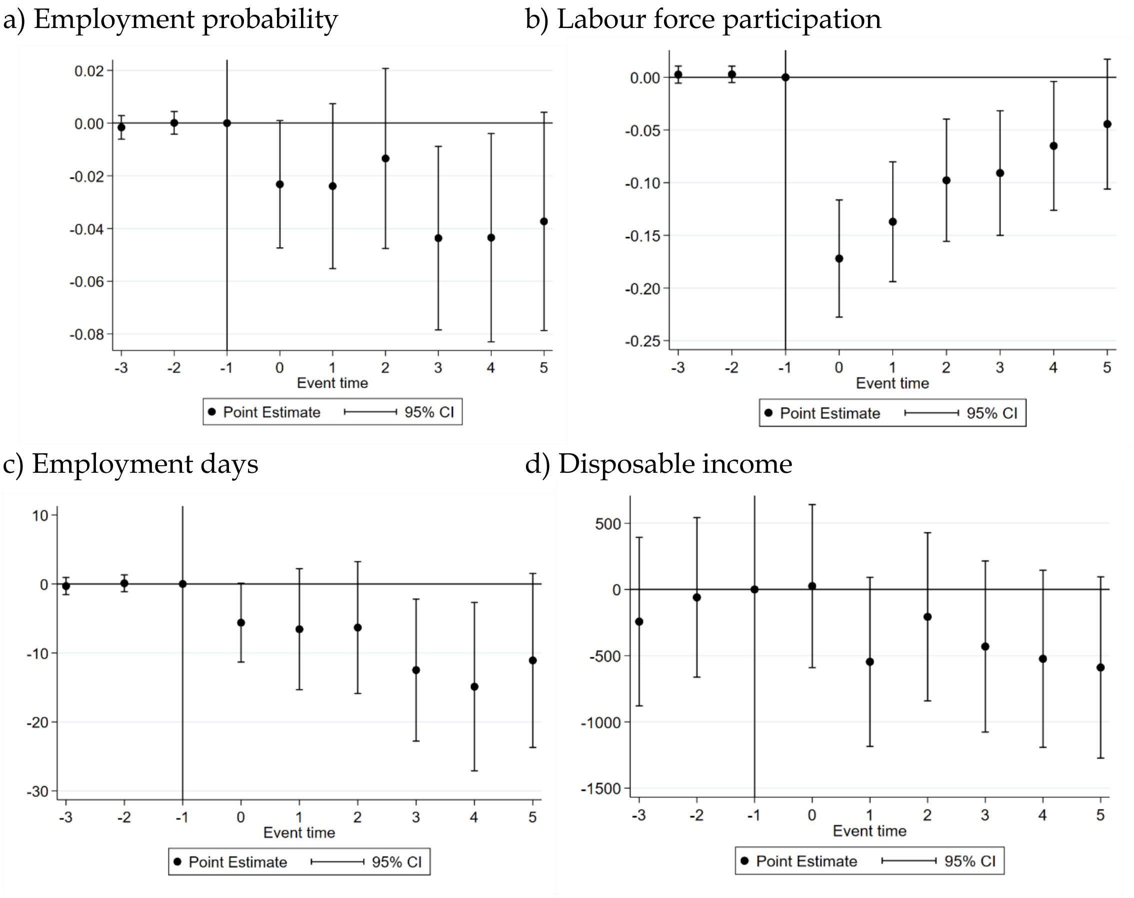


FIGURE 2 Impact of sanctions on labour market outcomes

Notes: Estimated coefficients and 95% confidence intervals. The reference year was t = -1. Estimations were based on the matched sample of treated and untreated individuals. The full results of each regression are shown in Online Appendix Table A8.

Moreover, previous studies have focused on short-term unemployed individuals receiving earnings-related unemployment benefits. For such individuals, sanctions seem to decrease the value of staying unemployed, which leads to higher job finding rates. In contrast, for long-term unemployed individuals, finding employment is difficult. The long-term unemployed are highly dependent on government support policies. Sanctions may force them to apply for basic SA and general HA, and some may stay out of the labour force permanently. In such cases, they do not receive counselling or VRs from PES caseworkers and do not have access to ALMPs. As individuals outside the labour force are not required to apply for jobs, lower labour force participation is likely to reduce the economy's job search intensity. The finding that sanctions increase SA reliance and reduce the number of working days entails more expenses and less tax-based income for the public sector.

In addition, sanctions can have negative side effects on the quality of life of sanctioned individuals, such as difficulties in paying rent and debt problems (Van den Berg et al. 2022). Sanctions may even increase crime (Machin and Marie 2006). Hämäläinen et al. (2009) noted that many long-term unemployed individuals have multiple problems, such as illness and over-indebtedness. They emphasised the role of PES caseworkers in encouraging and motivating the long-term unemployed in their job searches. Instead of sanctions, other forms of intervention are needed, such as guidance, support, rehabilitation and ALMPs.

TABLE 3 Average effects during years 0–5 after treatment (matched sample)

| | **VR**<br>**(1)** | **Sanction**<br>**(2)** |
|---|---|---|
| Employment probability | 0.062*** | –0.031*** |
| | (0.018) | (0.012) |
| | $\bar{Y}$ = 0.101 | $\bar{Y}$ = 0.056 |
| Labour force participation | 0.041** | –0.101*** |
| | (0.018) | (0.018) |
| | $\bar{Y}$ = 0.852 | $\bar{Y}$ = 0.772 |
| Employment days | 20.01*** | –9.49*** |
| | (5.677) | (3.647) |
| | $\bar{Y}$ = 29.11 | $\bar{Y}$ = 15.40 |
| Disposable income | 913.9* | –378.5 |
| | (524.8) | (280.8) |
| | $\bar{Y}$ = 12945 | $\bar{Y}$ = 12341 |
| Observations | 6208 | 8604 |

Notes: Average effect estimates in years 0–5 after a vacancy referral/sanction. *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) were clustered at the individual level. Moreover, the table reports sample averages of $Y$. Each coefficient estimate reports the result of a separate regression. The regressions included dummy variables for calendar year, regional employment office area and age in years. They also included the following control variables (dummies): male, high education level, disability, immigrant, children under 7 and participation in ALMPs. Estimations were based on the matched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value. The estimated pre-treatment effects t = -3 and t = -2 were all insignificant. See also Online Appendix Tables A7 and A8.

### 4.5.3 Robustness

For robustness, I estimated additional models (Table 4). First, I estimated models that included individual fixed effects to account for any time-invariant differences across individuals (Columns 1 and 3). Overall, the results were similar to those of the baseline model, which supports the robustness of the main results. The only notable difference was that VRs seemed to have a stronger positive effect on disposable income.

Second, despite the combination of PSM and fixed effects regression, there may persist concerns that VRs and sanctions are not random. To examine the magnitude of the selection bias, I repeated the baseline analysis without limiting the sample via the PSM method (Columns 2 and 4). The results for the unmatched samples were similar to the baseline results, suggesting that treatment selection bias was quite low.

TABLE 4 Robustness: Average effects during years 0–5 after treatment

| | **VR analysis** | | **Sanction analysis** | |
|---|---|---|---|---|
| | Individual FE (1) | Unmatched sample (2) | Individual FE (3) | Unmatched sample (4) |
| Employment probability | 0.064*** | 0.077*** | –0.031** | –0.031*** |
| | (0.018) | (0.014) | (0.012) | (0.007) |
| Labour force participation | 0.040** | 0.063*** | –0.103*** | –0.099*** |
| | (0.019) | (0.013) | (0.019) | (0.013) |
| Employment days | 20.47*** | 23.57*** | –9.50** | –9.57*** |
| | (5.668) | (4.46) | (3.696) | (2.16) |
| Disposable Income | 1140.3*** | 775.6* | –461.5** | –68.6 |
| | (438.9) | (412.1) | (215.2) | (196.0) |
| Individual fixed effects | Yes | No | Yes | No |
| Observations | 6208 | 56603 | 8604 | 58252 |

Notes: Average effect estimates in years 0–5 after a VR/sanction. *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) were clustered at the individual level. Each coefficient estimate reports the result from a separate regression. The regressions included dummy variables for calendar year, REO area and age in years. They also included control variables (dummies): male, highly educated, disability, immigrant, children under 7 and in ALMPs. Estimations were based on the matched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value. Models 1 and 3 included individual fixed effects. Models 2 and 4 repeated the baseline analysis for the unmatched sample. See also Online Appendix Tables A9–A12.

One possible concern is that jobseekers receiving VRs may have a profile that fits the needs of employers, which could partially explain the positive employment effects of VRs. However, long-term unemployed individuals are not employers' preferred candidates for new employees. Moreover, the 2014 reform increased the annual number of VRs by 91%, which means that VRs were given to individuals who would not have received them before 2014 (see Online Appendix Table A14). According to the new policy, VRs should be made for a wider variety of job opportunities, also from outside the unemployed person's professional field. According to results by Räisänen (2016), VRs were sent with insufficient selectivity after the reform. The reform also increased the imposition of sanctions. The data restrictions and the use of matching methods ensured that the treated and untreated individuals were similar in all relevant observed pretreatment characteristics.

What about the ex-ante threat effects of sanctions and VRs? Significant threat effects of sanctions would mean that the estimates would be downward biased. Similarly, significant threat effects of VRs would mean that VRs would have even stronger positive effects. However, the individuals in the study sample had been unemployed without any employment spells, VRs or sanctions for three years. For them, the ex-ante threat effect of being sanctioned or receiving a VR was likely to be very small. Moreover, previous literature suggests that threat effects are weak for long-term unemployed individuals (e.g. Rosholm and Svarer 2008; Tuomala 2011). In turn, after receiving a VR, the threat effect of being sanctioned is likely to be substantial because refusing to apply to an assigned vacancy can lead to a sanction.

### 4.5.4 Extensions

As an extension, I estimated separate regressions for certain subgroups to examine treatment effect heterogeneity. I examined whether the effects of VRs and sanctions differed in relation to gender or age. The results showed that VRs had stronger positive employment effects on females and younger individuals (Table 5). VRs' positive effect on labour force participation was strongest for jobseekers over 40 years. The subgroup analysis showed that sanctions had a statistically significant negative employment effect on males and individuals under 40 years, while the effects were insignificant for females and older individuals. Sanctions decreased labour force participation the most for males and older jobseekers.

In addition to the positive effects, VRs may also have undesirable side effects. Van den Berg et al. (2019) studied VRs in Germany, where minimum job search requirements do not apply during sickness periods. They found that VRs increased the probability of reporting sickness. Similarly, Hofmann (2014) reported an increased transition rate into short-term sick leave among unemployed individuals who had received VRs. I estimated the effects of VRs and sanctions on the probability of having a diagnosis for jobseekers who did not have a diagnosis in 2013. Neither VRs nor sanctions had statistically significant effects on the probability of having a diagnosis (see Figure A2 in the Online Appendix). It should be noted that the diagnosis category is documented only for those jobseekers whose chances of getting a job or staying in work have been significantly reduced due to a diagnosed injury, illness or disability. Cases of mild health issues are not documented. Thus, the results did not show whether the same phenomenon exists in Finland.

TABLE 5 Average effects by subgroup during years 0–5 after treatment

| | Male (1) | Female (2) | Under 40 years (3) | Over 40 years (4) |
|---|---|---|---|---|
| **a) VR analysis** | | | | |
| Employment probability | 0.048** | 0.085** | 0.067** | 0.055** |
| | (0.021) | (0.035) | (0.031) | (0.022) |
| Labour force participation | 0.022 | 0.066* | –0.005 | 0.068*** |
| | (0.023) | (0.034) | (0.030) | (0.025) |
| Employment days | 15.61** | 26.39** | 24.58** | 15.05** |
| | (6.58) | (11.04) | (10.09) | (6.86) |
| Disposable Income | 1227.9** | 996.2 | 1177.1** | 1104.9* |
| | (485.9) | (920.2) | (581.8) | (636.8) |
| Observations | 3986 | 2222 | 2600 | 3608 |
| **b) Sanction analysis** | | | | |
| Employment probability | –0.041*** | –0.016 | –0.047** | –0.017 |
| | (0.014) | (0.022) | (0.019) | (0.015) |
| Labour force participation | –0.143*** | –0.035 | –0.087*** | –0.120*** |
| | (0.023) | (0.033) | (0.026) | (0.027) |
| Employment days | –13.91*** | –2.33 | –13.35** | –6.11 |
| | (4.39) | (6.63) | (5.78) | (4.71) |
| Disposable Income | –452.4* | –532.1 | –794.2** | –150.5 |
| | (244.0) | (418.4) | (330.0) | (282.0) |
| Observations | 5625 | 2979 | 4095 | 4509 |

Notes: Average effect estimates in years 0–5 after receiving a sanction. *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) are clustered at the individual level. Each coefficient estimate reports the result from a separate regression. The regressions included individual fixed effects and dummy variables for calendar year, regional employment office area and age in years. Estimations were based on the matched sample of the treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value. The estimated pre-treatment effects t = -3 and t = -2 are all insignificant.

## 4.6 Conclusions

This article has investigated the effects of VRs and unemployment benefit sanctions on the labour market outcomes of long-term unemployed jobseekers in Finland. The study was based on population-based register data from the 2011–2019 period. It used matching methods to identify control groups of untreated individuals with similar characteristics as the treated individuals. The setting ensured that the treatment and control groups had parallel pre-trends in outcomes. This study found that VRs and sanctions have long-lasting effects on long-term unemployed jobseekers.

First, this study found that VRs increased employment probability. This finding is consistent with previous literature reporting VRs' positive employment effects. A new finding is that the employment effect was statistically significant even five years after receiving a VR. Using VRs, PESs have the possibility to reduce long-term unemployment and enhance the matching of the unemployed to job vacancies.

Second, this study found that sanctions increased exits from unemployment to outside the labour force and decreased employment probability. The negative effect on labour force participation is in line with the previous literature, but the negative effect on long-term unemployed jobseekers' employment probability is a new finding. This finding is related to incentive problems associated with the shift from unemployment benefits to other non-employment benefits. Sanctions are not an effective measure for this group. Thus, the results indicate that VRs are more fruitful than benefit sanctions in promoting long-term unemployed jobseekers' employment prospects.

Third, this article demonstrates that it is difficult to simultaneously provide long-term unemployed jobseekers with both comprehensive social security and good incentives for employment. The results indicate that long-term unemployed jobseekers are likely to face incentive traps. Despite producing a clear increase in employment, VRs were not associated with much higher disposable income. Previous literature suggests that jobs accepted after receiving a VR may have lower wages and be less stable. Welfare subsidy cuts and taxation were other possible factors. Although these factors reduce the income of individuals who have found work, they improve public finances. The results indicate that sanctions have even smaller effects on disposable income than VRs do. This is mainly due to social security transfers; individuals outside the labour force can receive other non-employment benefits. The downside is that individuals outside the labour force do not have access to counselling, VRs or ALMPs by PESs. The negative employment effect entails more expenses and less tax-based income for the public sector.

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# Appendix

## Appendix 1: Matching results

TABLE A1 Descriptive statistics by group for 2011–2019

| | **VR analysis** | | **Sanction analysis** | |
|---|---|---|---|---|
| | Unmatched sample | Matched sample | Unmatched sample | Matched sample |
| Observations | 56,603 | 6,208 | 58,252 | 8,604 |
| Age | 44.0 | 43.6 | 43.9 | 42.8 |
| Male | 0.67 | 0.64 | 0.67 | 0.65 |
| Highly educated | 0.16 | 0.28 | 0.14 | 0.10 |
| Immigrant | 0.11 | 0.16 | 0.11 | 0.11 |
| Disability | 0.37 | 0.15 | 0.38 | 0.39 |
| Married | 0.23 | 0.25 | 0.22 | 0.20 |
| Children under 7 | 0.09 | 0.11 | 0.08 | 0.07 |
| Urban area | 0.88 | 0.90 | 0.88 | 0.88 |
| In ALMPs | 0.09 | 0.13 | 0.08 | 0.07 |
| Employed | 0.07 | 0.10 | 0.06 | 0.06 |
| Employment days | 19.0 | 29.1 | 17.3 | 15.4 |
| In labour force | 0.82 | 0.85 | 0.81 | 0.77 |
| Unemployment months | 8.8 | 8.7 | 8.8 | 8.5 |
| Disposable income | 13,209 | 14,086 | 13,148 | 13,424 |
| Taxable income | 10,189 | 11,010 | 10,018 | 9,626 |

Notes: Individuals who were unemployed without any employment days, VRs or sanctions in 2011–2013. In the VR analysis, the sample was limited to individuals who did not receive any sanctions in 2014. In the sanction analysis, the sample was limited to individuals who did not receive any VRs in 2014.

TABLE A2 Probit results for receiving treatment

| | VR analysis | | Sanction analysis | |
|---|---|---|---|---|
| Variable | (1) Coeff. | (2) Std. Err. | (3) Coeff. | (4) Std. Err. |
| Age | –0.0068 | 0.0049 | –0.0213*** | 0.0041 |
| Male | –0.0368 | 0.0631 | –0.0525 | 0.0552 |
| Highly educated | 0.2339*** | 0.0683 | –0.1824** | 0.0776 |
| Immigrant | 0.0068 | 0.0860 | 0.0195 | 0.0838 |
| Disability | –0.5776*** | 0.0723 | –0.0312 | 0.0527 |
| Children under 7 | –0.0145 | 0.0900 | –0.1847** | 0.0894 |
| In ALMPs in 2013 | 0.4375*** | 0.0998 | –0.1957 | 0.1308 |
| Disposable income in 2013 | 0.0039 | 0.0092 | –0.0006 | 0.0095 |
| Disposable income in 2012 | –0.0056 | 0.0123 | 0.0072 | 0.0112 |
| Disposable income in 2011 | –0.0006 | 0.0087 | 0.0043 | 0.0079 |
| Taxable income in 2013 | –0.0137 | 0.0149 | –0.0318* | 0.0190 |
| Taxable income in 2012 | 0.0138 | 0.0137 | –0.0071 | 0.0155 |
| Taxable income in 2011 | 0.0130 | 0.0087 | –0.0054 | 0.0101 |
| 15 indicators for region of residence | Yes | | Yes | |
| Number of individuals on common support in 2013 | 5,409 | | 5,498 | |
| Log-likelihood | -1,173.1 | | -1,553.5 | |
| Pseudo R2 | 0.086 | | 0.044 | |

Notes: Standard errors are clustered at the individual level. Statistical significances are denoted by *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Prior income was measured in 1,000 euros (adjusted to 2010 euro value).

TABLE A3 VR analysis, descriptive statistics for unmatched sample in 2013, treated vs. control groups

| Variable | (1) Treated, mean | (2) Control, mean | (3) % bias | (4) t-test | (5) p-value | (6) V(T)/ V(C) |
|---|---|---|---|---|---|---|
| Age | 41.59 | 41.97 | -6.4 | -1.23 | 0.221 | 1.05 |
| Male | 0.655 | 0.671 | -3.4 | -0.65 | 0.516 | . |
| Highly educated | 0.269 | 0.149 | 30.0 | 6.34 | 0.000 | . |
| Immigrant | 0.150 | 0.109 | 12.2 | 2.47 | 0.013 | . |
| Disability | 0.142 | 0.376 | -55.3 | -9.31 | 0.000 | . |
| Children under 7 | 0.148 | 0.110 | 11.4 | 2.29 | 0.022 | . |
| In ALMPs in 2013 | 0.111 | 0.045 | 25.0 | 5.90 | 0.000 | . |
| Disposable income in 2013 | 12,513 | 12,368 | 2.3 | 0.50 | 0.619 | 1.67 |
| Disposable income in 2012 | 12,406 | 12,140 | 4.7 | 0.92 | 0.359 | 1.15 |
| Disposable income in 2011 | 11,969 | 11,224 | 11.0 | 2.31 | 0.021 | 1.61 |
| Taxable income in 2013 | 9,612 | 9,347 | 9.4 | 1.89 | 0.059 | 1.43 |
| Taxable income in 2012 | 9,931 | 9,272 | 18.6 | 3.80 | 0.000 | 1.56 |
| Taxable income in 2011 | 9,377 | 8,216 | 23.7 | 4.61 | 0.000 | 1.38 |
| Number of observations | 386 | 5,889 | | | | |
| **Overall covariate balance** | | | | | | |
| LR-test of the joint insignificance of variables | | 220.3 (p = 0.000) | | | | |
| Mean [median] absolute bias | | 13.0 [10.5] | | | | |
| Rubin's B ("bias") | | 88.6 | | | | |
| Rubin's R ("ratio of variances") | | 0.60 | | | | |

Notes: Data included dummies for region of residence (REO) in 2013 (15 REO areas). V(T)/V(C) indicates the variance ratio (for continuous covariates) of the treated over the non-treated individuals. The ratio should be equal to 1 for perfect balance. According to Rubin (2001), B < 25 and 0.5 < R < 2 indicate sufficiently balanced samples.

TABLE A4 VR analysis, descriptive statistics for matched sample in 2013, treated vs. control groups.

| Variable | (1) Treated, mean | (2) Control, mean | (3) % bias | (4) t-test | (5) p-value | (6) V(T)/ V(C) |
|---|---|---|---|---|---|---|
| Age | 41.50 | 41.65 | -2.5 | -0.32 | 0.748 | 1.02 |
| Male | 0.646 | 0.638 | 1.8 | 0.24 | 0.812 | . |
| Highly educated | 0.281 | 0.278 | 0.7 | 0.08 | 0.933 | . |
| Immigrant | 0.157 | 0.162 | -1.7 | -0.21 | 0.836 | . |
| Disability | 0.145 | 0.139 | 1.4 | 0.22 | 0.828 | . |
| Children under 7 | 0.148 | 0.142 | 1.7 | 0.22 | 0.829 | . |
| In ALMPs in 2013 | 0.119 | 0.113 | 2.1 | 0.24 | 0.812 | . |
| Disposable income in 2013 | 12,759 | 13,033 | -4.2 | -0.52 | 0.604 | 1.21 |
| Disposable income in 2012 | 12,721 | 12,954 | -4.1 | -0.49 | 0.623 | 0.84 |
| Disposable income in 2011 | 12,249 | 12,057 | 2.8 | 0.36 | 0.722 | 1.39 |
| Taxable income in 2013 | 9,600 | 9,612 | -0.5 | -0.06 | 0.953 | 1.37 |
| Taxable income in 2012 | 9,966 | 9,883 | 2.4 | 0.28 | 0.778 | 1.03 |
| Taxable income in 2011 | 9,395 | 8,914 | 9.8 | 1.25 | 0.213 | 1.18 |
| Number of observations | 345 | 345 | | | | |
| **Overall covariate balance** | | | | | | |
| LR-test of the joint insignificance of variables | | 9.21 (p = 0.999) | | | | |
| Mean [median] absolute bias | | 3.0 [2.4] | | | | |
| Rubin's B ("bias") | | 23.0 | | | | |
| Rubin's R ("ratio of variances") | | 1.31 | | | | |

Notes: Data included dummies for region of residence (REO) in 2013 (15 REO areas). V(T)/V(C) indicates the variance ratio (for continuous covariates) of the treated over the non-treated individuals. The ratio should be equal to 1 for perfect balance. According to Rubin (2001), B < 25 and 0.5 < R < 2 indicate sufficiently balanced samples.

TABLE A5 Sanction analysis, descriptive statistics for unmatched sample in 2013, treated vs. control groups.

| Variable | (1) Treated, mean | (2) Control, mean | (3) % bias | (4) t-test | (5) p-value | (6) V(T)/ V(C) |
|---|---|---|---|---|---|---|
| Age | 40.76 | 41.98 | -19.8 | -4.6 | 0.000 | 1.11 |
| Male | 0.655 | 0.671 | -3.5 | -0.8 | 0.423 | . |
| Highly educated | 0.092 | 0.149 | -17.5 | -3.68 | 0.000 | . |
| Immigrant | 0.097 | 0.109 | -3.9 | -0.88 | 0.381 | . |
| Disability | 0.356 | 0.376 | -4.2 | -0.95 | 0.342 | . |
| Children under 7 | 0.085 | 0.109 | -8.3 | -1.81 | 0.070 | . |
| In ALMPs in 2013 | 0.035 | 0.045 | -4.8 | -1.04 | 0.300 | . |
| Disposable income in 2013 | 12,482 | 12,368 | 2.2 | 0.48 | 0.632 | 0.84 |
| Disposable income in 2012 | 12,217 | 12,140 | 1.4 | 0.32 | 0.747 | 0.91 |
| Disposable income in 2011 | 11,272 | 11,224 | 0.9 | 0.18 | 0.856 | 0.74 |
| Taxable income in 2013 | 8,901 | 9,347 | -19.3 | -3.81 | 0.000 | 0.63 |
| Taxable income in 2012 | 8,928 | 9,272 | -12.1 | -2.37 | 0.018 | 0.65 |
| Taxable income in 2011 | 7,764 | 8,215 | -11.5 | -2.17 | 0.030 | 0.52 |
| Number of observations | 565 | 5,899 | | | | |
| **Overall covariate balance** | | | | | | |
| LR-test of the joint insignificance of variables | | 141.34 (p = 0.000) | | | | |
| Mean [median] absolute bias | | 8.4 [5.3] | | | | |
| Rubin's B ("bias") | | 58.0 | | | | |
| Rubin's R ("ratio of variances") | | 0.89 | | | | |

Notes: Data included dummies for region of residence (REO) in 2013 (15 REO areas). V(T)/V(C) indicates the variance ratio (for continuous covariates) of the treated over the non-treated individuals. The ratio should be equal to 1 for perfect balance. According to Rubin (2001), $B < 25$ and $0.5 < R < 2$ indicate sufficiently balanced samples.

TABLE A6 Sanction analysis, descriptive statistics for matched sample in 2013, treated vs. control groups.

| Variable | (1) Treated, mean | (2) Control, mean | (3) % bias | (4) t-test | (5) p-value | (6) V(T)/V(C) |
|---|---|---|---|---|---|---|
| Age | 40.63 | 40,92 | -4.8 | -0.73 | 0.467 | 1.05 |
| Male | 0.644 | 0.663 | -4.0 | -0.61 | 0.541 | . |
| Highly educated | 0.105 | 0.088 | 5.0 | 0.88 | 0.381 | . |
| Immigrant | 0.109 | 0.111 | -0.7 | -0.10 | 0.918 | . |
| Disability | 0.381 | 0.383 | -0.4 | -0.07 | 0.947 | . |
| Children under 7 | 0.090 | 0.073 | 5.5 | 0.94 | 0.345 | . |
| In ALMPs in 2013 | 0.033 | 0.038 | -2.1 | -0.35 | 0.727 | . |
| Disposable income in 2013 | 12,669 | 12,588 | 1.5 | 0.24 | 0.808 | 0.95 |
| Disposable income in 2012 | 12,515 | 12,534 | -0.3 | -0.05 | 0.957 | 0.97 |
| Disposable income in 2011 | 11,586 | 11,740 | -2.7 | -0.43 | 0.665 | 0.80 |
| Taxable income in 2013 | 9,143 | 9,100 | 2.1 | 0.45 | 0.651 | 1.24 |
| Taxable income in 2012 | 9,025 | 9,045 | -0.8 | -0.14 | 0.892 | 1.08 |
| Taxable income in 2011 | 7,890 | 7,895 | -0.2 | -0.03 | 0.976 | 1.09 |
| Number of observations | 478 | 478 | | | | |
| **Overall covariate balance** | | | | | | |
| LR-test of the joint insignificance of variables | | 12.57 (p = 0.987) | | | | |
| Mean [median] absolute bias | | 2.8 [2.2] | | | | |
| Rubin's B ("bias") | | 22.5 | | | | |
| Rubin's R ("ratio of variances") | | 1.67 | | | | |

Notes: Data included dummies for region of residence (REO) in 2013 (15 REO areas). V(T)/V(C) indicates the variance ratio (for continuous covariates) of the treated over the non-treated individuals. The ratio should be equal to 1 for perfect balance. According to Rubin (2001), B < 25 and 0.5 < R < 2 indicate sufficiently balanced samples.

## Appendix 2: Additional estimation tables and figures

TABLE A7 Results of VR analysis (matched sample)

| | Employment (1) | Employment days (2) | Labour force participation (3) | Disposable income (4) |
|---|---|---|---|---|
| Treated: 3 years before T | –0.002 | –0.580 | –0.002 | 254.4 |
| | (0.004) | (1.178) | (0.004) | (496.9) |
| Treated: 2 years before T | –0.001 | –0.587 | 0.000 | –185.2 |
| | (0.004) | (1.197) | (0.004) | (412.5) |
| Treated: treatment year | 0.040** | 8.293* | 0.046* | 342.4 |
| | (0.019) | (4.884) | (0.026) | (447.9) |
| Treated: 1 year after T | 0.048** | 11.040* | 0.027 | 1013.1 |
| | (0.023) | (6.575) | (0.029) | (621.5) |
| Treated: 2 years after T | 0.072** | 24.68*** | 0.029 | 1368.8** |
| | (0.027) | (8.388) | (0.031) | (561.3) |
| Treated: 3 years after T | 0.076*** | 22.400*** | 0.039* | 1093.5* |
| | (0.028) | (8.509) | (0.033) | (621.6) |
| Treated: 4 years after T | 0.056* | 19.840** | 0.075** | 842.1 |
| | (0.030) | (9.387) | (0.034) | (676.2) |
| Treated: 5 years after T | 0.081*** | 33.890*** | 0.027 | 825.3 |
| | (0.031) | (9.468) | (0.034) | (711.7) |
| Male | –0.036** | –9.899** | 0.023* | –2830.8*** |
| | (0.014) | (4.470) | (0.014) | (491.6) |
| Highly educated | 0.024* | 8.236* | 0.044*** | –922.4 |
| | (0.015) | (4.695) | (0.013) | (587.6) |
| Immigrant | 0.016 | 6.489** | –0.004 | 734.3 |
| | (0.019) | (5.988) | (0.016) | (615.9) |
| Disability | –0.022 | –6.593 | –0.056*** | –1209.9** |
| | (0.015) | (4.630) | (0.019) | (491.5) |
| Children under 7 | 0.033* | 10.29* | –0.022 | 3398.9*** |
| | (0.019) | (6.156) | (0.018) | (560.3) |
| In ALMPs | –0.037*** | –13.650*** | –0.057*** | –1091.4*** |
| | (0.012) | (3.354) | (0.016) | (298.0) |
| Observations | 6,208 | 6,208 | 6,208 | 6,208 |
| Mean outcome | 0.101 | 29.11 | 0.852 | 12,945 |
| R2 | 0.112 | 0.114 | 0.127 | 0.101 |

Notes: *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) are clustered at the individual level. The regressions included dummy variables for calendar year, REO area and age in years. Estimations were based on the matched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value.

TABLE A8 Results of Sanction analysis (matched sample).

| | Employment<br>(1) | Employment days<br>(2) | Labour force participation<br>(3) | Disposable income<br>(4) |
|---|---|---|---|---|
| Treated: 3 years before | –0.002 | –0.306 | 0.002 | –243.0 |
| | (0.002) | (0.640) | (0.004) | (323.9) |
| Treated: 2 years before | 0.000 | 0.099 | 0.002 | –60.11 |
| | (0.002) | (0.621) | (0.004) | (306.7) |
| Treated: treatment year | –0.023* | –5.614* | –0.172*** | 25.25 |
| | (0.012) | (2.911) | (0.028) | (313.5) |
| Treated: 1 year after | –0.024 | –6.561 | –0.137*** | –546.3* |
| | (0.016) | (4.467) | (0.029) | (325.2) |
| Treated: 2 years after | –0.013 | –6.318 | –0.098*** | –206.9 |
| | (0.017) | (4.876) | (0.029) | (323.6) |
| Treated: 3 years after | –0.044** | –12.48** | –0.091*** | –431.1 |
| | (0.018) | (5.243) | (0.030) | (328.8) |
| Treated: 4 years after | –0.043** | –14.89** | –0.065** | –523.7 |
| | (0.020) | (6.213) | (0.031) | (340.4) |
| Treated: 5 years after | –0.037* | –11.08* | –0.044 | –589.4* |
| | (0.021) | (6.427) | (0.031) | (348.5) |
| Male | –0.006 | –1.080 | 0.016 | –2,369*** |
| | (0.009) | (2.786) | (0.013) | (314.3) |
| Highly educated | 0.0392** | 10.89** | 0.045** | –278.9 |
| | (0.016) | (4.739) | (0.021) | (514.6) |
| Immigrant | –0.003 | 0.0195 | 0.007 | 704.2 |
| | (0.015) | (4.658) | (0.019) | (606.3) |
| Disability | –0.028*** | –8.126*** | –0.040*** | 25.11 |
| | (0.008) | (2.412) | (0.013) | (253.1) |
| Children under 7 | 0.043** | 11.59** | –0.037* | 3,690*** |
| | (0.018) | (5.199) | (0.021) | (584.4) |
| In ALMPs | 0.022 | 3.386 | –0.045** | –566.6 |
| | (0.015) | (4.051) | (0.020) | (390.7) |
| Observations | 8,604 | 8,604 | 8,604 | 8,604 |
| Mean outcome | 0.056 | 15.40 | 0.772 | 12,341 |
| R2 | 0.064 | 0.065 | 0.189 | 0.139 |

Notes: *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) are clustered at the individual level. The regressions included dummy variables for calendar year, REO area and age in years. Estimations were based on the matched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value.

TABLE A9 Results of VR analysis (unmatched sample).

| | Employment<br>(1) | Employment days<br>(2) | Labour force participation<br>(3) | Disposable income<br>(4) |
|---|---|---|---|---|
| Treated: 3 years before | –0.013*** | –3.905*** | –0.005* | 330.7 |
| | (0.002) | (0.629) | (0.003) | (369.1) |
| Treated: 2 years before | –0.013*** | –3.803*** | –0.005* | –170.5 |
| | (0.002) | (0.632) | (0.003) | (270.0) |
| Treated: treatment year | 0.033** | 6.927* | 0.045*** | 161.6 |
| | (0.015) | (3.751) | (0.017) | (332.9) |
| Treated: 1 year after | 0.037** | 8.941* | 0.062*** | 799.5* |
| | (0.017) | (4.863) | (0.019) | (438.9) |
| Treated: 2 years after | 0.085*** | 27.65*** | 0.073*** | 926.2** |
| | (0.020) | (6.283) | (0.021) | (426.9) |
| Treated: 3 years after | 0.105*** | 30.29*** | 0.047** | 913.1* |
| | (0.021) | (6.415) | (0.022) | (488.7) |
| Treated: 4 years after | 0.104*** | 32.06*** | 0.078*** | 902.3* |
| | (0.022) | (6.898) | (0.023) | (530.5) |
| Treated: 5 years after | 0.098*** | 35.54*** | 0.072*** | 951.2* |
| | (0.022) | (7.301) | (0.023) | (542.3) |
| Male | –0.016*** | –4.621*** | 0.031*** | –1,994*** |
| | (0.004) | (1.187) | (0.005) | (137.0) |
| Highly educated | 0.050*** | 14.98*** | 0.037*** | 232.4 |
| | (0.006) | (1.875) | (0.006) | (197.2) |
| Immigrant | 0.005 | 1.598 | –0.018** | 449.6** |
| | (0.007) | (2.042) | (0.007) | (221.3) |
| Disability | –0.031*** | –9.016*** | –0.050*** | 154.1 |
| | (0.003) | (1.010) | (0.005) | (108.5) |
| Children under 7 | 0.029*** | 8.906*** | –0.017** | 3,328*** |
| | (0.006) | (1.955) | (0.007) | (233.0) |
| In ALMPs | 0.006 | –2.911** | –0.049*** | –204.2 |
| | (0.006) | (1.458) | (0.007) | (153.6) |
| Observations | 56,603 | 56,603 | 56,603 | 56,603 |
| Mean outcome | 0.068 | 18.98 | 0.819 | 12,138 |
| R2 | 0.072 | 0.071 | 0.144 | 0.097 |

Notes: *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) are clustered at the individual level. The regressions included dummy variables for calendar year, REO area and age in years. Estimations were based on the unmatched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value.

TABLE A10 Results of Sanction analysis (unmatched sample).

| | Employment<br><br>(1) | Employment days<br>(2) | Labour force participation<br>(3) | Disposable income<br>(4) |
|---|---|---|---|---|
| Treated: 3 years before | 0.002 | 0.714 | 0.013*** | 111.6 |
| | (0.002) | (0.453) | (0.002) | (204.9) |
| Treated: 2 years before | 0.003** | 0.891* | 0.012*** | 180.6 |
| | (0.002) | (0.461) | (0.002) | (201.5) |
| Treated: treatment year | –0.019*** | –5.399*** | –0.194*** | 302.6 |
| | (0.007) | (1.556) | (0.021) | (215.6) |
| Treated: 1 year after | –0.027*** | –7.526*** | –0.116*** | 169.5 |
| | (0.010) | (2.700) | (0.021) | (231.3) |
| Treated: 2 years after | -0.016 | –6.947** | –0.075*** | 151.9 |
| | (0.011) | (2.983) | (0.021) | (255.7) |
| Treated: 3 years after | –0.041*** | –11.68*** | –0.079*** | -173.9 |
| | (0.011) | (3.066) | (0.021) | (264.1) |
| Treated: 4 years after | –0.045*** | –13.59*** | –0.071*** | –373.1* |
| | (0.012) | (3.586) | (0.021) | (221.3) |
| Treated: 5 years after | –0.040*** | –12.21*** | –0.060*** | –495.5** |
| | (0.013) | (3.909) | (0.021) | (215.2) |
| Male | –0.015*** | –4.237*** | 0.028*** | –2,036*** |
| | (0.004) | (1.125) | (0.005) | (129.4) |
| Highly educated | 0.053*** | 15.48*** | 0.033*** | 238.7 |
| | (0.006) | (1.843) | (0.007) | (185.7) |
| Immigrant | 0.002 | 0.728 | –0.015** | 292.9 |
| | (0.006) | (1.986) | (0.007) | (220.4) |
| Disability | –0.030*** | –8.708*** | –0.052*** | 199.9* |
| | (0.003) | (0.950) | (0.005) | (103.8) |
| Children under 7 | 0.026*** | 7.908*** | –0.017** | 3,332*** |
| | (0.006) | (1.899) | (0.007) | (233.6) |
| In ALMPs | 0.012** | –0.691 | –0.049*** | –123.9 |
| | (0.006) | (1.459) | (0.007) | (157.1) |
| Observations | 58,252 | 58,252 | 58,252 | 58,252 |
| Mean outcome | 0.064 | 17.30 | 0.810 | 12,083 |
| R2 | 0.068 | 0.062 | 0.151 | 0.102 |

Notes: *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) are clustered at the individual level. The regressions included dummy variables for calendar year, REO area and age in years. Estimations were based on the unmatched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value.

TABLE A11 Results of VR analysis with individual fixed effects (matched sample)

| | Employment<br>(1) | Employment days<br>(2) | Labour force participation<br>(3) | Disposable income<br>(4) |
|---|---|---|---|---|
| Treated: 3 years before | –0.002 | –0.663 | –0.004 | 426.9 |
| | (0.005) | (1.458) | (0.004) | (338.7) |
| Treated: 2 years before | 0.001 | 0.180 | –0.002 | 43.58 |
| | (0.004) | (1.183) | (0.004) | (225.0) |
| Treated: treatment year | 0.041** | 8.380* | 0.045* | 497.6 |
| | (0.019) | (4.861) | (0.026) | (350.1) |
| Treated: 1 year after | 0.048** | 10.90 | 0.026 | 1,253** |
| | (0.023) | (6.627) | (0.029) | (526.3) |
| Treated: 2 years after | 0.074*** | 25.08*** | 0.028 | 1,614*** |
| | (0.027) | (8.389) | (0.031) | (490.2) |
| Treated: 3 years after | 0.079*** | 23.09*** | 0.037 | 1,374** |
| | (0.027) | (8.398) | (0.033) | (544.9) |
| Treated: 4 years after | 0.061** | 21.24** | 0.075** | 1,060* |
| | (0.030) | (9.390) | (0.034) | (618.5) |
| Treated: 5 years after | 0.084*** | 34.68*** | 0.029 | 1,055 |
| | (0.031) | (9.497) | (0.034) | (649.8) |
| Observations | 6,208 | 6,208 | 6,208 | 6,208 |
| Mean outcome | 0.101 | 29.11 | 0.852 | 12,945 |
| R2 | 0.134 | 0.142 | 0.142 | 0.057 |

Notes: *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) are clustered at the individual level. The regressions included individual fixed effects and dummy variables for calendar year, REO area and age in years. Estimations were based on the matched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value.

TABLE A12 Results of Sanction analysis with individual fixed effects (matched sample).

| | Employment<br>(1) | Employment days<br>(2) | Labour force participation<br>(3) | Disposable income<br>(4) |
|---|---|---|---|---|
| Treated: 3 years before | 0.000 | 0.336 | 0.002 | –240.9 |
| | (0.002) | (0.547) | (0.004) | (241.3) |
| Treated: 2 years before | 0.000 | 0.0931 | 0.001 | –99.26 |
| | (0.001) | (0.411) | (0.003) | (174.3) |
| Treated: treatment year | –0.022* | –5.453* | –0.174*** | 13.51 |
| | (0.012) | (2.895) | (0.028) | (187.8) |
| Treated: 1 year after | –0.024 | –6.605 | –0.139*** | –603.9** |
| | (0.016) | (4.467) | (0.029) | (234.5) |
| Treated: 2 years after | –0.013 | -6.343 | –0.099*** | –317.3 |
| | (0.018) | (4.885) | (0.029) | (260.1) |
| Treated: 3 years after | –0.045** | –12.72** | –0.093*** | –560.9** |
| | (0.018) | (5.298) | (0.031) | (278.5) |
| Treated: 4 years after | –0.044** | –15.06** | –0.066** | –622.1** |
| | (0.021) | (6.325) | (0.032) | (310.8) |
| Treated: 5 years after | –0.037* | –10.98* | –0.045 | –693.8** |
| | (0.021) | (6.481) | (0.032) | (351.6) |
| Observations | 8,604 | 8,604 | 8,604 | 8,604 |
| Mean outcome | 0.056 | 15.40 | 0.772 | 12,341 |
| R2 | 0.067 | 0.072 | 0.218 | 0.060 |

Notes: *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) were clustered at the individual level. The regressions included individual fixed effects and dummy variables for calendar year, REO area and age in years. Estimations were based on the matched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value.

TABLE A13 Average income effects during years 0-5 after treatment.

| | VR<br>(1) | Sanction<br>(2) |
|---|---|---|
| Taxable income | 1211.2*** | –969.8*** |
| | (421.4) | (222.3) |
| Ln Taxable income | 0.065** | –0.116*** |
| | (0.028) | (0.021) |
| Disposable Income | 913.9* | –378.5 |
| | (524.8) | (280.8) |
| Ln Disposable income | 0.023 | –0.025 |
| | (0.029) | (0.022) |
| Observations | 6,208 | 8,604 |

Notes: Average effect estimates in years 0-5 after a VR/sanction. *significant at 10%; **significant at 5%; ***significant at 1% (all two-sided tests). Standard errors (in parentheses) are clustered at the individual level. Each coefficient estimate reports the result of a separate regression. The regressions included dummy variables for calendar year, REO area and age in years. They also included the following control variables (dummies): male, high education level, disability, immigrant, children under 7 and participation in ALMPs. Estimations were based on the matched sample of treated and untreated individuals. Annual disposable incomes were adjusted to 2010 euro value. The estimated pre-treatment effects t = -3 and t = -2 were all insignificant.

a) Impact of VRs                                        b) Impact of sanctions

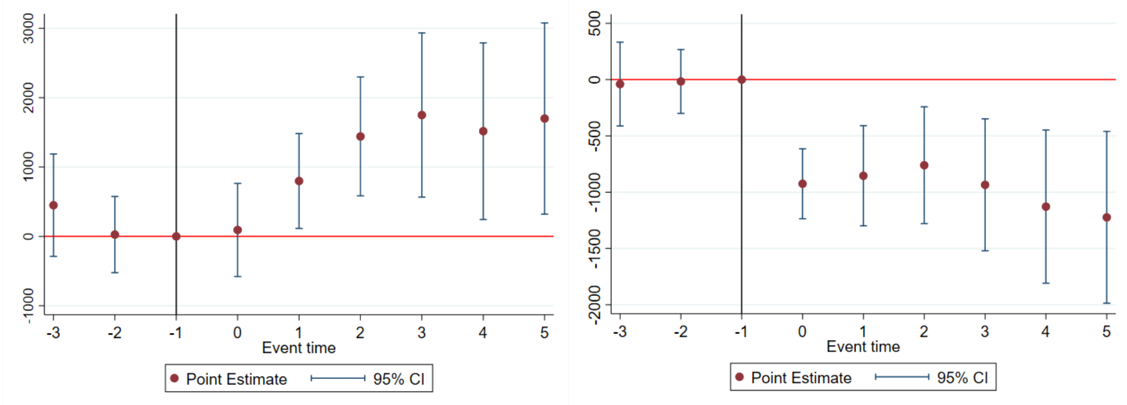


FIGURE A1 Impact of VRs and sanctions on annual taxable income. Notes: Estimated coefficients and 95% confidence intervals. The reference year was t = -1. Estimations were based on the matched sample of treated and untreated individuals. See also Table A13.

a) Impact of VRs                                        b) Impact of sanctions

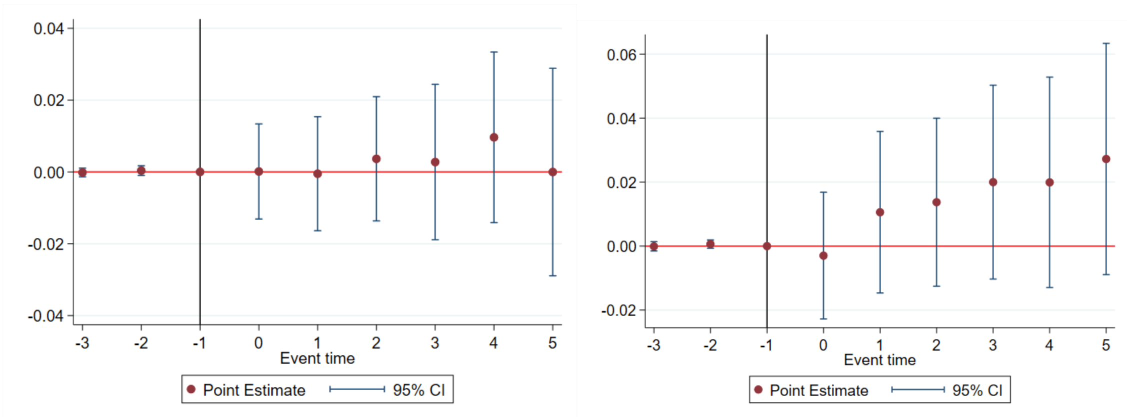


FIGURE A2 Impact of VRs and sanctions on the probability of having a diagnosis. Notes: Estimated coefficients and 95% confidence intervals. The reference year was t = -1. Estimations were based on the matched sample of treated and untreated individuals. The data contained only those individuals who did not have a diagnosis in 2013.

## Appendix 3: Additional figures and information

a) VR data

b) Sanction data

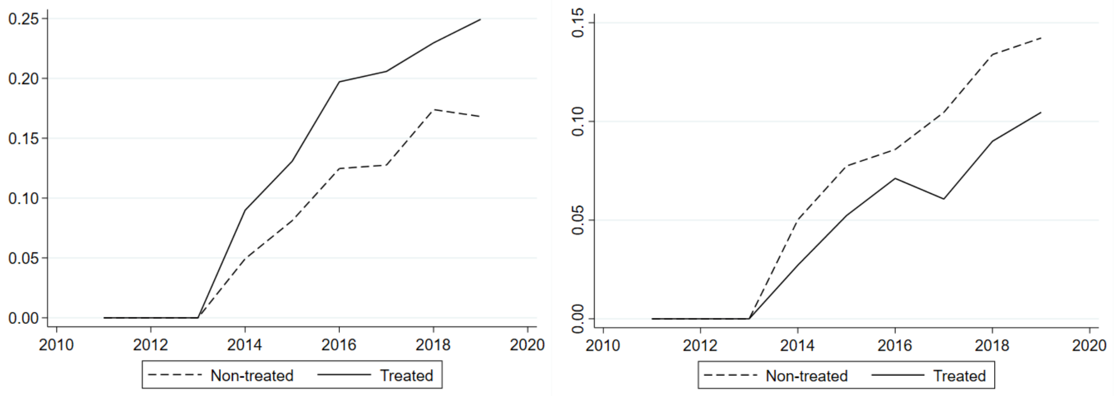


FIGURE A3 Development of employment probability by treatment status. Notes: Matched sample.

a) VR data

b) Sanction data

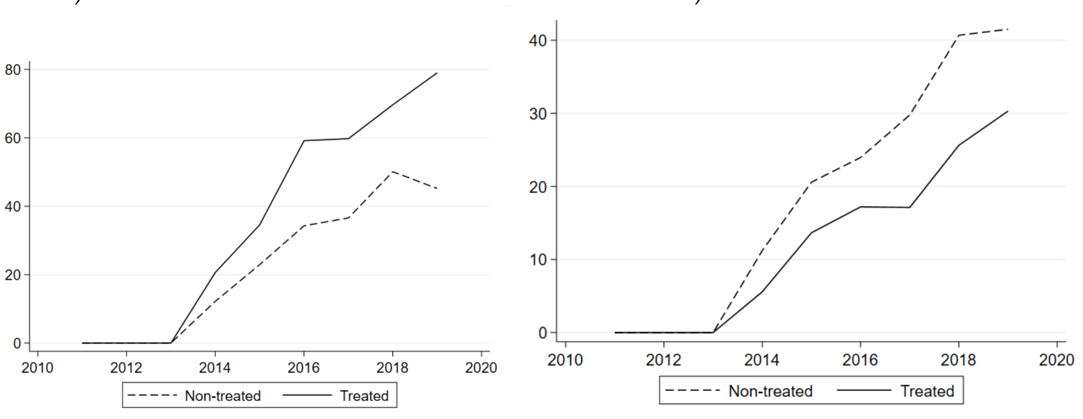


FIGURE A4 Development of annual employment days by treatment status.
Notes: Matched sample.

a) VR data b) Sanction data

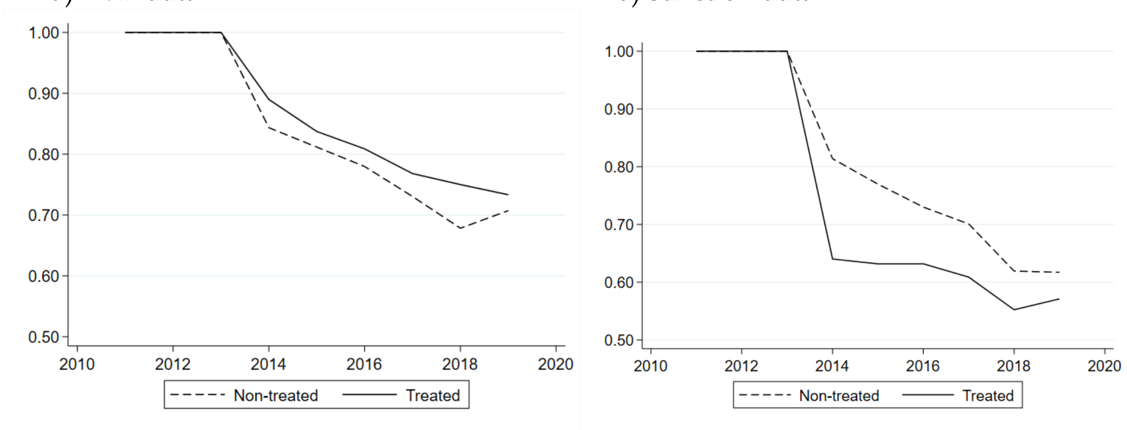


FIGURE A5 Development of labour force participation by treatment status.
Notes: Matched sample.

a) VR data b) Sanction data

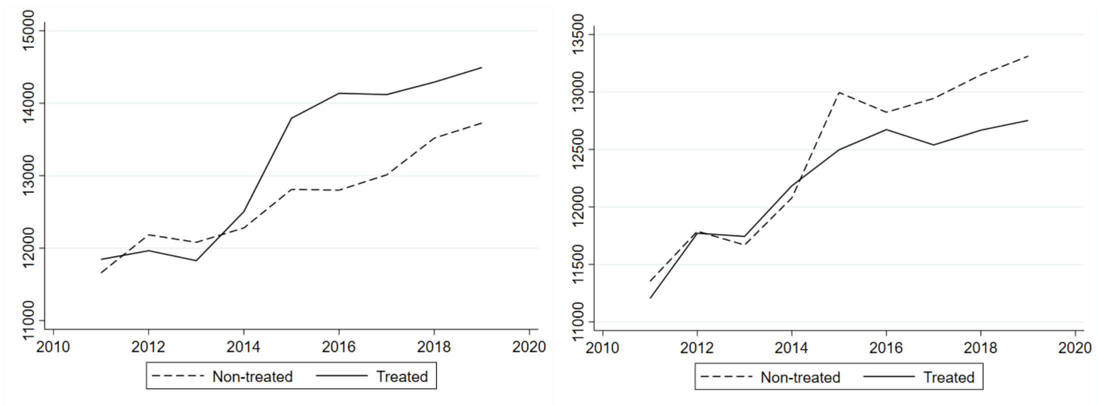


FIGURE A6 Development of annual disposable income by treatment status (deflated).
Notes: Matched sample.

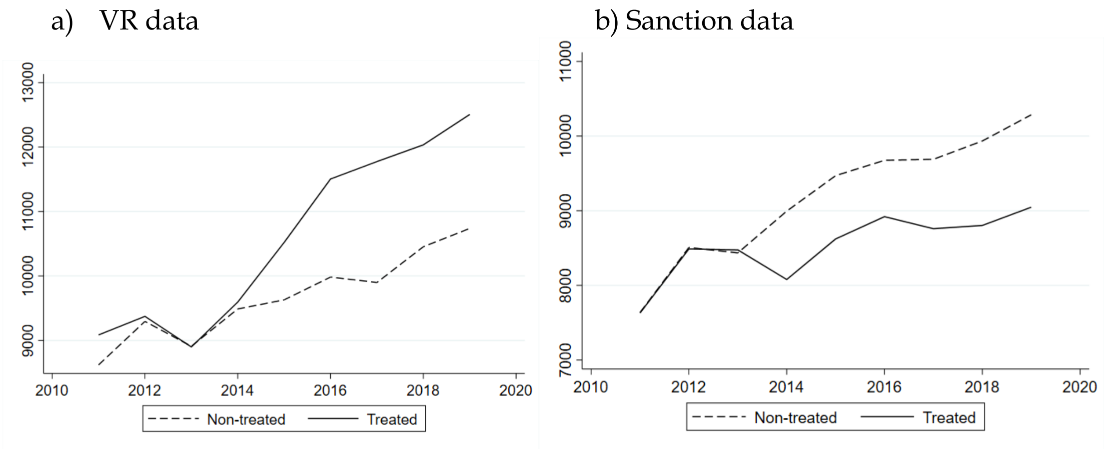


FIGURE A7 Development of annual taxable income by treatment status (deflated). Notes: Matched sample.

TABLE A14 Unemployed jobseekers, vacancy referrals and sanctions, by year

| Year | Unemployed jobseekers | Long-term unemployed (>104 weeks) | VRs | Sanctions |
|---|---|---|---|---|
| 2011 | 243,900 | 23,000 | 170,239 | 77,839 |
| 2012 | 253,200 | 28,700 | 203,362 | 83,032 |
| 2013 | 294,100 | 32,800 | 166,042 | 77,331 |
| 2014 | 325,700 | 39,800 | 316,476 | 98,236 |
| 2015 | 351,900 | 48,300 | 366,884 | 78,597 |
| 2016 | 348,800 | 56,800 | 341,487 | 69,370 |
| 2017 | 303,400 | 52,900 | 359,433 | 79,578 |
| 2018 | 255,900 | 39,400 | 253,238 | 77,766 |
| 2019 | 240,400 | 30,500 | 209,536 | 82,334 |

Notes: Number of unemployed jobseekers, VRs and unemployment benefit sanctions in Finland in 2011–2019. Sources: VRs and sanctions from TEM URA micro data. Unemployed jobseekers from Finnish Labour Review 4/2021 by the Ministry of Economic Affairs and Employment.

## YHTEENVETO (SUMMARY IN FINNISH)

### Tutkimuksia julkisten työvoimapalveluiden roolista työmarkkinoiden kohtaannossa

Työmarkkinoilla on kohtaanto-ongelma, kun avoimia työpaikkoja ja työttömiä työnhakijoita on tarjolla, mutta työnantajat eivät löydä sopivia työntekijöitä eivätkä työnhakijat sopivia työpaikkoja. Kohtaanto-ongelmien seurauksena työttömyys voi pysyä korkealla tasolla ja avoimia työpaikkoja jäädä täyttymättä. Julkiset työvoimapalvelut tarjoavat työnvälityspalveluita sekä aktivointipalveluita, jotka voivat tehostaa kohtaantoa. Tämä väitöskirja tarjoaa uutta empiiristä tietoa julkisten työvoimapalveluiden vaikutuksista. Väitöskirja koostuu johdantoluvusta sekä kolmesta artikkelista. Johdantoluku käy läpi olennaisen tutkimuskirjallisuuden sekä esittelee väitöskirjan tutkimuskysymykset, aineistot, menetelmät ja keskeisimmät tulokset. Artikkelit keskittyvät kolmeen työvoimapolitiikan välineeseen: työttömien määräaikaishaastatteluihin, työtarjouksiin sekä työttömyysturvasanktioihin. Tutkimuksissa on käytetty Tilastokeskuksen ja Työ- ja elinkeinoministeriön kattavia mikroaineistoja.

Ensimmäinen artikkeli (Luku 2) tutkii työttömille tehtävien haastattelujen vaikutusta työttömyyden kestoon. Vuoden 2017 politiikkauudistus lisäsi haastattelujen määrää voimakkaasti. Artikkelissa käytetään Difference-in-Differences -menetelmää, jossa hyödynnetään alueellista vaihtelua haastattelujen todennäköisyydessä. Tulosten mukaan tiheämmin toteutetut haastattelut lisäsivät ja nopeuttivat työttömien työllistymisiä sekä aktivointipalveluihin osallistumista. Toisaalta havaitut positiiviset työllisyysvaikutukset olivat suuruusluokaltaan pienempiä kuin tutkimuksissa, jotka eivät ole huomioineet syrjäyttämisvaikutuksia ilman työnhaun tukea jääneiden työttömien työllistymiseen. Työllisyysvaikutukset olivat voimakkaimpia 25-34 -vuotiailla sekä matalasti koulutetuilla ja palvelualojen työnhakijoilla. Mahdolliset vaikutuskanavat liittyvät lisääntyneeseen työnhaun tukeen, tiukempaan valvontaan sekä uhkavaikutuksiin.

Toinen artikkeli (Luku 3) tutkii työtarjousten vaikutusta avointen työpaikkojen täyttymiseen. Tutkimuksessa hyödynnetään vuoden 2014 uudistuksen aiheuttamaa alueellista vaihtelua sekä Difference-in-Differences -menetelmää. Tulosten mukaan avointen työpaikkojen täyttyminen tehostui alueilla, joissa työtarjousten määrä suhteessa vakanssien määrään nousi eniten. Toisaalta tutkimuksessa havaitaan, että työtarjousten voimakas lisääminen heikensi niiden keskimääräistä laatua ja tehokkuutta. Lisäksi työllisyysvaikutukset vaikuttivat pieniltä. Yksi mahdollinen selitys on, että työtarjoukset heikensivät työttömien vastaanottamien työpaikkojen keskimääräistä laatua ja lyhensivät työsuhteiden kestoa.

Kolmas artikkeli (Luku 4) tutkii työtarjousten ja työttömyysturvasanktioiden pitkän aikavälin vaikutuksia pitkäaikaistyöttömien työmarkkinatulemiin. Pitkäaikaistyöttömyyden lisääntymisen on todettu olevan eräs merkittävä kohtaannon heikentymiseen liittyvä tekijä. Tutkimuksessa käytetään kaltaistamis- ja paneeliaineistomenetelmiä. Tulosten mukaan työtarjoukset lisäsivät työllistymisen todennäköisyyttä. Sanktiot puolestaan lisäsivät työttömien siirtymiä työvoiman ulkopuolelle sekä vähensivät heidän työllistymistään. Tutkimuksessa havaitaan viitteitä kannustinloukuista: tilastollisesti merkitsevistä työllisyysvaikutuksista huolimatta työtarjouksilla ja sanktioilla oli vain hyvin pienet vaikutukset pitkäaikaistyöttömien käytettävissä oleviin tuloihin.

Artikkelien pohjalta voidaan tehdä seuraavia politiikkajohtopäätöksiä. Ensinnäkin julkisilla työvoimapalveluilla voidaan vaikuttaa työmarkkinoiden kohtaantoon. Tulosten mukaan työnhaun tuki yhdistettynä työnhaun valvontaan tuottaa positiivisia tuloksia. Haastattelujen tehostaminen vuonna 2017 tuotti positiivisia työllisyysvaikutuksia sekä tehosti työttömien siirtymistä aktivointipalveluihin. Työtarjousten määrän lisääminen vuonna 2014 lisäsi avointen työpaikkojen täyttymistä. Pitkäaikaistyöttömille tehdyt työtarjoukset lisäsivät heidän työllistymisen todennäköisyyttään. Toisaalta on myös hyvä ottaa huomioon, että työnhaun tuella ja valvonnalla ei voida ratkaista kaikkia kohtaanto-ongelmia. Erityisesti työvoimapulasta kärsivien alojen kohdalla tarvitaan työvoiman kouluttamista, sillä ongelmana on, että työnhakijoiden ammattitaito ja osaaminen eivät vastaa työn vaatimuksia.

Toisena johtopäätöksenä voidaan todeta, että uudistuksilla voi olla negatiivisia sivuvaikutuksia. Vuoden 2017 uudistus heikensi haastattelujen keskimääräistä laatua, ja vuoden 2014 uudistus heikensi työtarjousten keskimääräistä laatua ja tehokkuutta. Haastattelujen osalta on raportoitu, että vuoden 2017 uudistus lisäsi TE-toimistojen työntekijöiden työtaakkaa huomattavasti. Työtarjousten merkittävän lisäyksen seurauksena aiempaa huomattavasti suurempi osa työtarjouksen saaneista työnhakijoista ei täyttänyt työnantajan vaatimuksia. Työtarjouksia kannattaisi kohdentaa pääasiassa sellaisille työnhakijoille, jotka vastaavat työnantajan toiveita.

Kolmanneksi havaittiin viitteitä siitä, että monet pitkäaikaistyöttömät ovat kannustinloukussa. Työtarjouksilla havaittiin positiivisia työllisyysvaikutuksia, mutta vain hyvin pienet vaikutukset käytettävissä oleviin tuloihin. Sanktiot puolestaan lisäsivät työttömien siirtymiä työvoiman ulkopuolelle sekä vähensivät heidän työllistymistään, mutta eivät vaikuttaneet käytettävissä oleviin tuloihin. Nämä havainnot liittyvät todennäköisesti sosiaaliturvan ja verotuksen kannustinongelmiin sekä työsuhteiden laatuun. Pitkäaikaistyöttömyyden vähentämisen haasteena on, että pitkäaikaistyöttömille on hankala tarjota samanaikaisesti kattava sosiaaliturvan taso sekä hyvät työllistymisen kannusteet.