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Regulating Labor Immigration: The Effects of Lifting Labor Market Testing

352

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ABSTRACT

Labor market testing (LMT) requires firms to demonstrate there are no local work-ers available before hiring an immigrant. We examine the effect of removing LMT requirements for non-EU workers in Finland utilizing regional and temporal vari-ation in occupations exempted from LMT. We combine individual and firm-level administrative data with hand-collected information on local changes in labor market testing rules and apply a staggered difference-in-differences research design. We find that removing the LMT requirement increases the inflow of non-EU workers to treated occupation-region cells. This is mainly driven by non-EU individuals already in Finland. Five years post-treatment, the negative earnings effect is 2 % at the occupation-region level and 4% for incumbent workers at the individual level, more pronounced in low-wage and service-oriented occupations and among older workers. In low-paying occupations, the earnings effect is largely attributable to decreased working hours and to a suppressed wage drift for stayers. However, we also observe a positive employment effect at the individual level for workers in the upper segment of the wage distribution. At the firm level, LMT removal increases the number of non-EU employees while having no effect on profitability.

JEL Codes: J20, J38, J61, J68

Keywords:
labor market testing, immigration, labor supply, wages, shortage list

TIIVISTELMÄ

Työperäisen maahanmuuton sääntely: tutkimus saatavuusharkinnan poiston vaikutuksista

Saatavuusharkinta vaatii yrityksiä osoittamaan, että paikallista työvoimaa ei ole saatavilla ennen EU/ETA-alueen ulkopuolelta tulevan maahanmuuttajan palkkaamista. Tutkimme saatavuusharkinnan poistamisen vaikutuksia Suomessa hyödyntäen alueellista ja ajallista vaihtelua saatavuusharkinnasta vapautetuissa ammateissa. Yhdistämme yksilö- ja yritystason rekisteriaineistoja käsin kerättyyn tietoon paikallisista muutoksista saatavuusharkinnasta vapautetuissa ammateissa ja käytämme erot eroissa -tutkimusasetelmaa. Havaitsemme, että saatavuusharkinnan poistaminen lisää EU:n ulkopuolelta tulevien työntekijöiden määrää niissä ammatti-alue -yksiköissä, joista saatavuusharkinta on poistettu. Tämä johtuu kuitenkin pääasiassa EU:n ulkopuolisista henkilöistä, jotka ovat olleet jo Suomessa. Viisi vuotta saatavuusharkinnan poiston jälkeen negatiivinen vaikutus vuosituloihin on 2% ammatti-aluetasolla ja 4% jo aiemmin ammatissa työskennelleille yksilötasolla. Negatiivinen palkkavaikutus on selvempi matalapalkkaisissa ja palvelualojen ammateissa sekä ikääntyneiden työntekijöiden keskuudessa. Matalapalkkaisissa ammateissa ansiovaikutus johtuu osin lyhennetyistä työajoista ja jatkajien palkkaliukumien pienemisestä. Havaitsemme kuitenkin yksilötasolla myös positiivisen työllisyysvaikutuksen korkeapalkkaisempien ammattien työntekijöille. Myös yritystasolla saatavuusharkinnan poisto lisää EU:n ulkopuolisten työntekijöiden määrää, mutta sillä ei ole vaikutusta yritysten voittoihin.

JEL koodit: J20, J38, J61, J68

Avainsanat:
saatavuusharkinta, maahanmuutto, työn tarjonta, palkat, työvoimapulalista

1 Introduction

The regulation of labor immigration remains a contentious issue across the Global North. Labor market testing (LMT) constitutes a core component of employer-driven immigration policies in many EU countries, with analogous requirements in U.S. visa programs. LMT requires employers to demonstrate that no suitable local workers are available before hiring a foreign worker. The policy aims to balance the protection of vulnerable workers' wages and employment with the need to address labor shortages. Despite its widespread use, empirical research on LMT's actual effects is limited, particularly for non-specialist, low-skilled occupations. This study is among the first to estimate the causal effects of LMT policies in these contexts.

A standard labor supply model posits that an increasing supply of immigrant labor may exert downward pressure on wages and limit employment opportunities for both native and non-native incumbent workers (see, e.g., Borjas 1999, 2003). More recent economic frameworks extend this view, showing that wage effects can be negative or positive depending on whether immigrants and incumbent workers are complements or substitutes, as well as the ease with which individuals transition to new jobs and tasks (see, e.g., Peri and Sparber 2009; Manacorda et al. 2012; Cattaneo et al. 2015; Peri 2016; Foged and Peri 2016a). Additionally, individuals may respond to immigration by adjusting their choices related to education, labor force participation, or occupation, particularly during the early stages of their careers (Llull, 2018).

This paper demonstrates that labor migration policies have heterogeneous effects on native and incumbent migrant workers. Our analysis uses a quasi-experimental design leveraging the phased removal of LMT across occupations and regions in Finland, where the policy predominantly targets non-specialist, low-skilled occupations. We employ a staggered differences-in-differences approach using comprehensive population-wide register data from a Nordic welfare state, enabling a detailed examination of outcomes at the occupation-region, individual, and firm levels. Furthermore, we extend the analysis to explore the effects of LMT exemptions on wage bargaining and government transfers.

Our occupation-region-level analysis reveals that abolishing LMT requirements significantly increases the inflow and stock of non-EU workers in the affected regions and occupations.[1] The majority of this growth comes from immigrants with limited or no work authorization already residing in the country. Five years post-treatment, we observe a negative effect of €647 (approximately -2%) on the average annual earnings of native workers in the occupation-region unit. In contrast, the average earnings of non-EU

[1] We refer to all workers from outside of the EU/EEA area as non-EU workers.

workers in the same occupation-region remain unchanged. Negative earnings effects at the occupation-region level are observed exclusively in low-paid occupations, defined as those in the lowest quartile of the salary distribution. This negative effect is driven by reduced working hours and is more pronounced among older workers.

At the individual level, our analysis identifies a negative wage effect of €1,121 (approximately -4%) for incumbent workers five years after the policy change. Unlike the occupation-region-level findings, this individual-level impact affects both non-EU and EU workers and is not limited to the lowest quartile. As with the occupation-region-level results, the negative effect is stronger among older workers. Especially for workers in the the lowest salary quartile of occupations, the effect is largely attributable to reduced working hours. We also detect a negative and statistically significant impact on average hourly wages, a pattern not observed at the occupation-region level.

While we observe a 1.7% positive employment effect for natives five years after the policy change, the employment effect for incumbent immigrants remains, on average, negligible. When an occupation-region faces a negative wage shock, treated individuals could potentially mitigate the effect by switching their region, occupation, or position, thereby experiencing a smaller earnings effect than the treated occupation-region. However, we find no evidence of substantial transitions, including movements into higher-paid occupations. Although the positive employment effect suggests some degree of individual-level adaptation, we do not observe a reversal of the negative earnings effect for natives. This contrasts with findings by Cattaneo et al. (2015), Foged and Peri (2016a), and D'Amuri and Peri (2014), which indicate that immigration can enable native workers to advance into more complex roles and higher positions within the wage distribution.

Our firm-level analysis shows that removing LMT prompts prompted firms to expand their workforce, leading to a decline in labor productivity over the following four-year period without affecting profitability. However, this analysis captures a relatively short-term perspective, as investments and firm adjustments require time. When combined with the observed negative individual-level wage effects, one might anticipate an increase in firm profits if firms exploit their monopsony power to extract some benefits of the LMT policy change (e.g., see, Amior and Manning (2020)). However, we find no evidence to support this.

The policy instrument under investigation—a regulatory labor immigration policy for non-specialist—is widely used across many countries to balance the costs and benefits of immigration. Research on this policy, therefore, holds direct implications for its refinement and practical application. Given LMT's extensive adoption for relatively low-skilled labor migration, our findings are particularly relevant to policymakers worldwide.

Our findings on fiscal impacts, derived from tax payments and transfer receipts, suggest that unless incumbent workers manage to improve their labor market outcomes following the relaxation of immigration policies, the burden on public finances could grow notably.

Our work contributes to several strands of the literature. First, this paper is among the earliest to examine LMT for less-skilled workers using a robust identification strategy. The study most closely related to ours is by Signorelli (2024), which analyzes a similar policy change affecting a set of 30 high-skilled occupations in France. Signorelli (2024) finds a negative 1% effect on incumbent migrant salaries—half the impact for natives—with no adverse effect on employment. We extend this research by utilizing changes in exemptions from LMT, otherwise widely applicable in most occupations in Finland. Notably, approximately 78% of workers in Finland are employed in occupations subject to LMT, i.e., primarily non-specialist roles, and nearly one-third have worked in exempted occupations at some point.

Second, our study offers a clear identification of the effects of relatively less-skilled immigration across the earnings distribution by using a policy quasi-experiment. To our knowledge, only Clemens and Lewis (2022) have utilized a similarly rigorous research framework. As they highlight, most prior studies rely on the shift-share approach, which has been subject to criticism (Clemens and Lewis, 2022; Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). For example, wages may adjust to the negative wage effects of earlier immigration shocks, introducing a positive bias in estimates (Jaeger et al., 2018).

Third, we provide comprehensive evidence on the impact of an increased supply of foreign workers on both native and non-native workers. Our analysis examines the majority of the wage distribution, as well as employment, tax contributions, transfer receipts, and mobility, using population register data. Additionally, we advance the discussion on earnings effects by decomposing the impact into changes in working hours and hourly wages—an analysis, to our knowledge, not previously undertaken in this context.

Fourth, by incorporating sectoral collective wage agreements—which set the minimum raise for various occupations—into our empirical analysis, we aim to understand the wage-setting dynamics under changing local immigration regulations. We find that in the bottom-quartile of wage distribution, the wage increases of stayers at the occupation-region level are affected by LMT exemptions, effectively neutralizing above-minimum raise increases in wages (wage drifts). The existing literature has focused on regimes where wage floors are negotiated and finds that within-occupation wages do adjust to changes in the business cycle through lower real wage floors and lower wage premia (Adamopoulou and Villanueva, 2022; Card and Cardoso, 2022). This is the first study to integrate data on collective wage agreements with labor migration policy changes.

Fifth, beyond individual and occupation-region-level impacts, our analysis seeks to contribute to the literature by addressing the effects of low-skilled immigration on a broad set of firm-level outcomes. Clemens and Lewis (2022) find that the substitutability between foreign and native workers is quite low, indicating that the inflow of foreign workers does not reduce native employment. Our study builds on this by investigating additional dimensions, including sales, investments, profits, and labor productivity.

Findings from the existing literature vary significantly depending on the context. Some non-causal U.S. studies (e.g., Ottaviano and Peri 2006; Card 2001) have suggested that immigrants have a minimal overall impact on native wages, while others report relatively large negative effects on native wages (Borjas, 2003). Starting with Card (1990), a body of literature has examined the impact of immigration on wages and employment using natural quasi-experiments that generate exogenous variation in migrant and refugee inflows or outflows. In the context of labor migration, Clemens et al. (2018) demonstrate that the exclusion of approximately half a million seasonally employed Mexican farm workers in the 1960s had minimal impact on the domestic farm labor market. In contrast, East et al. (2023) demonstrate that an immigration enforcement policy that removed employed undocumented immigrants from the regional labor market negatively affected native' wages and employment.

In the European context, several studies investigate the causal effects of labor immigration on native wages using policy changes, e.g., related to the free labor movement within the EU/EEA countries. Kuosmanen and Meriläinen (2022) study the effects of posted workers in Finland on native wages in similar occupations and find that the Eastern enlargement of the EU decreased native wages by 9% in vulnerable occupations. Bratsberg and Raaum (2012) identify a negative impact on wages in Norway's construction sector, attributable to immigration, by examining variations in occupational licensing requirements. Dustmann et al. (2017) analyzes a 1991 policy change that facilitated commuting between Germany and Czechia, finding that a 1% increase in the employment share of Czech workers led to a 0.13% decrease in native wages and a 0.9% reduction in native employment levels. Our findings on natives' wages are consistent with these findings.[2]

The structure of this paper is as follows. Section 2 introduces the institutional framework and outlines the quasi-experimental setting employed in this study. Section 3 provides an overview of the data. Section 4 discusses our methods. Section 5 presents our main results, which are estimated at the occupation-region, individual, and firm levels. Section 6 presents the results utilizing data on collective wage agreements. Section 7 discusses the fiscal implications of the results, and Section 8 presents the robustness checks. Policy implications are discussed in Section 9, and Section 10 provides concluding remarks.

2 Institutional Setting

Countries select economic immigrants under immigration policies that can be described as supply-driven, employer-based, or a mix of both. Employer-based policies rely on firms to make hiring decisions. Supply-driven policies, or also so-called point-based systems, select skilled workers into the country based on a set of criteria that can be altered depending on the labor market's needs. Most immigration systems are a mix of both. Points-based systems include some forms of labor market testing, and demand-driven systems have alternative paths for labor immigrants (Papademetriou and Hooper, 2019).

LMT is used in many European countries, including Italy, Switzerland, Poland, Ireland, Slovenia, Croatia, and France. LMT in EU countries usually requires advertising the job opening on the EURES portal from two weeks to a month. A similar policy is effectively used also in the US. However, in the U.S., the policy is built into certain visa processes. Employers seeking to hire foreign workers for permanent or temporary positions must often obtain a Labor Certification from the Department of Labor that includes LMT to ensure that U.S. workers are given priority for available jobs.

In this section, we discuss the form and use of LMT in Finland and the source of our identying variation.

[2] Edo (2020) examines the effects of the influx of repatriates to France following Algeria's independence in 1962, noting that although native wages initially declined, they returned to their original levels within 15 years. In Denmark, Malchow-Møller et al. (2012) employ an instrumental variable (IV) approach and find that an increase in the share of immigrants from less developed countries in the workplace reduces wages for native co-workers. Conversely, Foged and Peri (2016a) demonstrate, using exogenous variation from Denmark's refugee dispersal policy, that unskilled immigration positively impacts unskilled natives' wages, employment, and occupational mobility. For relatively high-skilled immigration, Beerli et al. (2021) examine the impact of lifting restrictions on European cross-border workers in Switzerland's neighboring countries, reporting a 5% increase in the wages of high-skilled native workers.

2.1 Work permit rules in Finland

The European Union has free movement of people, and thus, individuals who are citizens of another EU country can freely move to Finland to work without restrictions. Henceforth, the foreign workers considered in this paper are those who come from outside the EU/EEA area. We refer to these workers as non-EU workers and foreign workers interchangeably.

Non-EU workers require a work permit before starting to work in Finland. After a worker has secured a job, a two-step procedure follows.[3] The Finnish Immigration Service (Migri) makes the final decision on the permit, but before that, non-specialist jobs require labor market testing by the local public employment offices. Specialist occupations are exempted from the labor market testing procedure, as long as their monthly salary exceeds a certain amount (around €3,000 per month) and if they fulfill other conditions for the specialist work permit, which should be the case in a vast majority of hires. Thus, we exclude specialist professions from our analyses.

In the first step, the non-EU worker applies for a work permit on the Migri website, and the employer fills out a form attached to the application. The local public employment offices then determine whether there are suitable labor market candidates available in the EU labor market for the position. The process aims to ensure that the residence permit for work does not prevent an unemployed person already in the labor market from being employed. The employment offices also check if the job has some health or qualification requirements, and only qualified workers can be given a work permit. The employment offices also verify that the employers meet all the basic requirements for employing an individual. Additionally, the non-EU worker must have their living expenses covered by their employment during the length of their residence permit.

In the second step, after the partial decision by the public employment office, Migri makes the final decision on the work permit. The residence permit for work is occupation-specific or sometimes employer-specific. The first residence permit is usually temporary.

2.2 Regional variation in labor market testing requirements

Our research design exploits the regional and temporal variation in labor market testing requirements for occupations. The changes in labor market testing rules are determined by ELY centers, which are regional offices of the Finnish central government. These offices are responsible for many policies related to local business, the environment, and immigration. There are 15 ELY districts in Finland, and regional guidelines regarding

[3]Legislation on residence permits is in the Aliens Act of 2004.

labor immigration are supposed to be updated every 6 months for each of these districts. ELY centers can add occupations to shortage lists to bypass the labor market testing, which essentially makes it easier for firms to hire non-EU workers.

Based on email correspondence with the authorities, the selection of occupations to be exempted was previously largely based on the Occupational Barometer. The Occupational Barometer was a summary compiled by experts at the Employment Offices. It indicated whether there was a shortage or surplus of job seekers in the region. Additionally, the number of unemployed job seekers in the region over the medium term and the number of open vacancies were considered. The third criterion was the duration of the available job positions. A high proportion of short-term employment relationships could also have prevented the exemption of a particular occupation in a region. According to the authorities, the Occupational Barometer was discontinued in the fall of 2022. One potential challenge for our identification is the influence of firm lobbying, if firms try to influence which occupations are added to the shortage list. Based on our correspondence with the ELY centers, some companies and other stakeholders have contacted the authorities. However, according to the centers, lobbying should have little effect on the decisions.

In Figure 1, we plot cumulatively the share of occupations exempted from the labor market testing requirement in different regions during different years. For example, in 2012, there were only few exemptions. Once an occupation is treated, it stays treated throughout the whole period in our analyses.[4] In the final year of our sample, there is one region (Lapland) with nearly 40% of occupations considered to be treated. In the Online Appendix, we show the same variation for a larger number of years (see Figure B1) and only for non-specialist professions (see Figure B2). In year 2021, which is not part of our sample, one region–North Karelia–even exempted all occupations from LMT even though the unemployment rate in North Karelia has traditionally been among the highest in Finland, underlining the fact that the exemption decisions are not necessarily always targeted at the tightest labor markets even though that is the official goal of having these local exemptions.

[4]Occupations can be removed from the shortage lists by the ELY centers. 28% of the exemptions are reverted during the sample period. Our empirical strategy does not allow for the treatment to turn on and off, so we only consider the first instance when the occupation is put on a shortage list in the region.

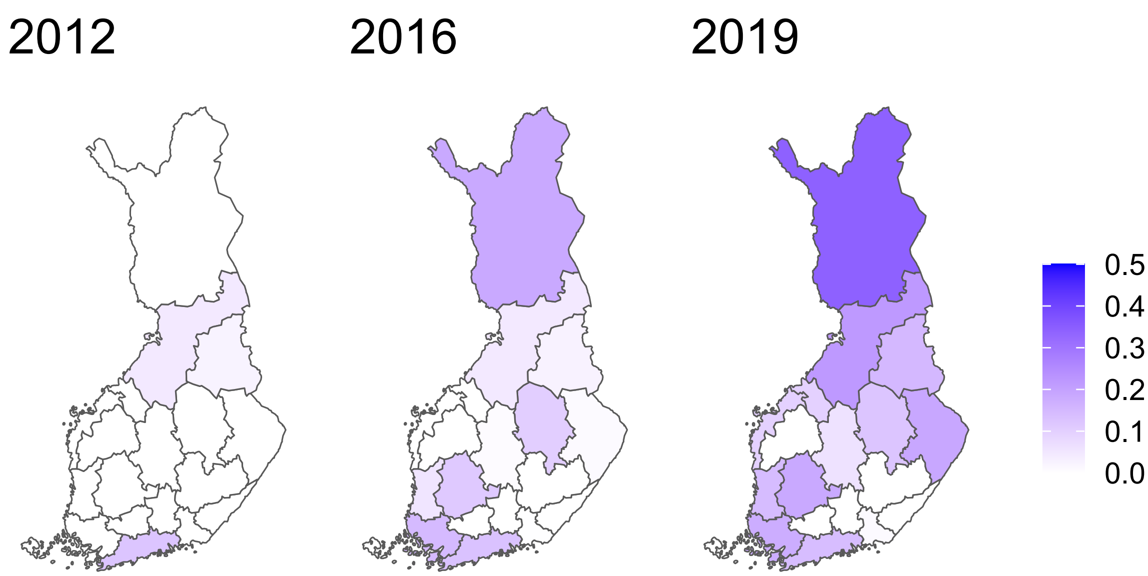


Figure 1: Expansion of treatment (cumulative share)

Notes. The figure shows cumulatively the share of occupations in each region that have been exempted from the labor market testing requirement according to our data. Once a occupation-region has been treated once, we consider it to be treated in all years after that year. Thus, this figure is cumulative, and ignores possible removals of exemptions. This figure includes all occupations, including specialist occupations. The figure produced by the authors is in R. Source of map data: National Land Survey of Finland (Maanmittauslaitos). Figure B1 shows these changes for a larger number of years, and Figure B2 shows the share of non-specialist professions that are treated.

3 Data

3.1 Administrative data on workers and firms

We use individual-level administrative datasets from Statistics Finland and the Finnish Ministry of Employment and the Economy (TEM) that include all individuals living in Finland. The datasets include wide-ranging information about incomes, employment, careers, job search, and vacancies.[5] Using our data, we can identify workers' occupations. This is crucial as we want to estimate the effects on the occupation-region level.

For additional wage estimates, we complement the data with the Finnish Structure of Earnings Survey. The annual survey data covers 55% to 75% of employees in the private sector. The data provide information on the wages of salaried employees as well as the

[5]The used data modules include FOLK basic, FOLK income, FOLK employment, TEM job seekers, TEM job search and TEM vacancies datasets The use of these datasets is restricted, but researchers can apply to use them through Statistics Finland. See https://www.tilastokeskus.fi/tup/mikroaineistot/index_en.html for guidance on how to apply for data access.

hourly paid employees and their weekly and monthly working hours, part-time status, and hourly and monthly earnings. The data allows us to decompose the earnings data into the basic hourly and monthly rates and all the additional payments the workers may have received.

We also utilize data on firms. These modules contain information on the establishment level and firm level. The variables included in these datasets are, for example, the number of employees, sector, profits, taxes paid, turnover, and profits. The datasets include also many other relevant firm and establishment-level outcomes and characteristics.

3.2 Data on local restrictions

We use hand-collected data on immigration restrictions. These data consist of all available records of regional-level exemptions to the labor market testing requirement. The information was collected by emailing all of the regional ELY centers that are responsible for drafting the documents. We received responses from all ELY centers. These data were relatively extensive in recent years, but we supplemented the data with information received from Finnish Public Employment Service offices.

The documents used to collect the data were in many different formats. Online Appendix Q shows examples of documents containing data on regional exemptions. As shown in the examples, sometimes the documents contained specific occupation codes that are exempted while sometimes they only listed occupational fields or vague occupation titles. In the latter case, we manually tried to find which occupation codes would correspond to the listed occupations or fields.

We restrict our sample to the years 2012-2019 because we do not have them for all regions in the earlier years. For the most recent years, we should have nearly perfect coverage of all rule changes. The Finnish Aliens Act is from 2004 and exemptions could have been placed any time after. We use a 4-digit occupation classification to match our hand-collected data to the administrative data.

3.3 Data on collective bargaining agreements

We also utilize data on collective salary agreements negotiated by unions. Finland does not have a minimum wage but instead unions negotiate universally binding collective agreements that set floors to wages in different sectors during the agreement period. Wages can, however, increase more than these negotiated increases through 'wage drift' which means the realized percent increases in wages after the collectively agreed raises have been subtracted. We aim to assess whether the wage drift is affected. The wage drift is calculated only for stayers who do not change firm or occupation compared to the previous year. We calculate the wage drift in the by substracting from the %-change in hourly wage the general and local components of the collective bargaining raise.

We use data that contain basic and local raises negotiated for different contracts. These data span years 2012-2012 and have been collected from public records. We explain in the Online Appendix N how we link these data on collective salary agreement to administrative datasets.

4 Empirical Strategy

Our analyses are conducted at three distinct levels: occupation-region, individual, and firm. While interconnected, these analyses address distinct, though related, questions. Different empirical strategies are employed at each level of analysis to capture causal estimates.

The occupation-region level is the most appropriate for studying labor market outcomes, as LMT exemptions (the treatment) are determined at this level. This level also allows us to test the exogeneity of our quasi-experimental variation in LMT exemptions by examining whether the difference-in-differences parallel trends assumption holds in the pre-treatment period. Analyses at this level isolate the effects of LMT exemptions on the occupation-region labor market. Assuming that these effects are primarily driven by an exogenous increase in local labor supply, these analyses provide insights into the labor demand function across different segments of the labor market. (see Section 4.1)

Second, we perform individual-level analyses to examine whether the labor market outcomes of workers are affected by the LMT exemption 'shock.' Ex-ante, it is unclear whether individuals will fare better or worse than in their initial occupation-region, as they may change occupations, relocate, or receive promotions when their occupation is exempted from LMT. These dynamics are particularly relevant when new immigrants begin working in their initial occupations. (see Section 4.2)

Third, we conclude the analysis by examining the impact of the treatment on firms. In this section, we move furthest from a clean identification strategy, as there is no single clear way to define treated firms. Given that firms make hiring and investment decisions in the labor market and are responsible for implementing the LMT procedure, it is interesting to examine whether any systematic changes in dynamics emerge among treated firms. (see Section 4.3)

4.1 Occupation-region level

Our main approach is to study the effects of LMT rule changes on the occupation-region level as treatment is assigned at that level. To estimate the difference-in-difference estimates, we use the Callaway and Sant'Anna (2021) method, which is based on estimating group-time average treatment effects, i.e., treatment effects are calculated separately for each group that is treated. Group refers here to the year when a unit (in our case, occupation-region ID) received treatment for the first time. We have 9 groups in the data, as we have 9 years (2012-2020) when treatment begins for some units. The group-time average treatment effect is defined as

$$ATT(g,t) = \mathbb{E}[Y_t - Y_{g-1} | G_g = 1] - \mathbb{E}[Y_t - Y_{g-1} | D_t = 0] \tag{1}$$

where Y_t is the outcome at time t, Y_{g-1} is the outcome during the year preceding the treatment, G_g gets value 1 for units that belong to group g. In our setting, only the never-treated group ($D_t = 0$) is used as a control group. We do not use any controls in the occupation-region level estimation as we want to have a clean and simple DiD setup. Because we use no controls, and thus, have no need for a doubly-robust (drdid) estimator, we opt to use the outcome regression option in the csdid package to estimate the group-time ATTs. For detailed estimation formulas for these different estimation options, see Callaway and Sant'Anna (2021).

Following Callaway and Sant'Anna (2021)[6], once we have calculated the group-time average treatment effects $ATT(g,t)$ expressed in equation (1) non-parametrically using the csdid package, these $ATT(g,t)$ estimates can be aggregated into a dynamic event study plot using the following formula:

$$\theta_D(e) = \sum_{g=2012}^{2020} \mathbf{1}\{g + e \leq 2020\} P(G = g | G + e \leq 2020) ATT(g, g+e). \tag{2}$$

In the above equation, $e = t - g$ is the event time, i.e., years from the start of treatment. When calculating the estimate $\theta_D(e)$ for a specific event time year e, only those $ATT(g,t)$ estimates are included where event time is e. This means that for each group g the estimate $ATT(g, g+e)$ is used in the aggregation formula for that event time year.

Because we also want to estimate pooled estimates for the whole post period, we can

[6] Specifically, Table 1 in Callaway and Sant'Anna (2021) show different weighting alternatives including the event study type of aggregation we use.

do the aggregation then by first aggregating the $ATT(g,t)$ estimates to treatment effects by group:

$$\theta_s(g) = \frac{1}{2020 - g + 1} \sum_{t=2012}^{2020} \mathbf{1}\{g \leq t\} ATT(g,t). \tag{3}$$

After that it is possible to use the estimated $\theta_s(g)$ for each group to calculate the weighted ATT to get one pooled treatment effect estimate for the whole post period:

$$\theta_s^0 := \sum_{g=2012}^{2020} \theta_s(g) P(G = g). \tag{4}$$

In our main estimations, we use a specification that includes only never-treated units as controls and uses a varying base period, which is the default option in the package we use in estimation. However, we test robustness to changing these choices.

As we use difference-in-differences, the identifying assumptions are the same as usual in this research setting. Importantly, the validity of our results requires the parallel trends assumption to hold. This means that in the absence of treatment, the trends of treated and control occupation-region units should be parallel. This should hold for all groups treated at different times. The validity of DiD design also needs the stable unit treament value assumption (SUTVA) to hold, i.e., treatment should not have an impact on the control group. We discuss potential threats to these assumptions in the subsection 4.4 where we focus on the validity of our identification strategy.

4.2 Individual level

In addition to our main analyses using occupation-region-level data, we estimate individual-level effects by examining workers employed in the occupation-region unit prior to the exception's introduction. To create the individual-level treatment group, we first identify workers who have worked in the occupation-region unit one year before the particular occupation-region unit has first been added to the list of exempted units. Similarly to the occupation-region level analyses, we use only the first treatment a particular occupation-region unit receives. Once we have identified the individuals who have worked in occupations in year -1, we need to restrict the raw sample in such a way that the event year for an individual is the first time that individual has been working in period -1 in any exempted occupation-region unit.

Forming a control group for the individual-level treatment group is challenging because period -1 is only defined for treated individuals, all of whom are employed during that period. If all never-treated individuals were used as controls, there would likely be a significant jump in employment in period -1 due to the lack of this employment restriction in the control group. Consequently, the employment trajectories of treated and never-treated individuals would differ significantly. To address this issue, we construct a control group by randomizing a placebo treatment year for control units and requiring these units to be employed in the year prior to that. Similar baseline restrictions in matching (such as requiring specific types of employment histories) have been used previously, e.g., in the literature examining the effects of job loss (see, e.g., Schmieder et al. (2023)).

We use a three-step procedure to form the control group. First, all never-treated occupation-region units are assigned a randomized placebo treatment year. Second, we identify all workers employed in these never-treated occupations one year prior to the placebo treatment year. This mirrors the treatment group definition, except that no treatment occurs in the control group. Third, matching is employed to construct a control group that is equal in size and comparable to the treated individuals. For computational efficiency, propensity score matching is used as the algorithm for the individual-level analysis. Matching is performed on a limited set of variables (age, gender, income) using values from period -1. Covariates after matching are presented in Table O18 in Online Appendix O. As can be seen from the table, the treatment and control groups are not balanced after matching, although the levels are relatively similar in treatment and control groups. However, balance of background variables is not necessary in the difference-in-differences design. Instead, the goal of using matching here is merely to ensure that parallel trends assumption would hold in the sample.

We estimate the individual-level effects by two-way fixed effects regressions of the following form:

$$Y_{it} = \gamma_i + \lambda_t + \sum_{\substack{k=-5 \\ k\neq -1}}^{5} \delta_k \cdot D_i \cdot \mathbb{1}\{K_{it} = k\} + \sum_{\substack{k=-5 \\ k\neq -1}}^{5} \theta_k \cdot \mathbb{1}\{K_{it} = k\} + \epsilon_{it} \tag{5}$$

In the above equation, coefficients δ_k are the periodic ATTs. In the regression, D_i is the treatment indicator, and γ_i and λ_t are individual and year fixed effects. We also control for event time K_{it} as it is, in our case, observed for both treated and control units (for control units, it is time to the placebo event year).

4.3 Firm level

To study firm responses, we use panel data from years 2013-2019 as this is the period for which we observe all relevant firm level outcomes in the data. These analyses come with a challenge of defining which firms were treated. Especially for large firms that have establishments in many regions, it is likely that most of those firms would have been affected in some way. The challenge with a DiD setup then is that there are likely no good controls for such firms. In the analysis, we need to rely on some comparison of less exposed vs. more exposed firms, excluding firms for which we cannot find good controls. Thus, the firm level analysis should perhaps be viewed as less definitive than our main estimations conducted at the occupation-region level.

We combine matching and difference-in-differences (two-way fixed effects) when estimating firm responses. We use coarsened exact matching (CEM) (Iacus et al., 2012) as the matching algorithm. Matching is conducted separately for each treated group (i.e., different "first event" years). We estimate CEM weights for each treated and control unit. As CEM matches values almost exactly, it cannot be used with too many matching variables. In our firm-level analyses, we only match on the number of employees in pre-periods -3, -2, and -1 and the number of non-EU immigrant workers in period -1. The rationale for using matching is to find a control group such that it would be plausible to assume parallel trends would hold. Thus, it is probably not the case that matching on these variables would create observationally similar groups but only that we would believe the parallel trends assumption would hold, conditional on the covariates used in matching.

Balance table is available in Online Appendix J Table J4. The background characteristics of firms are not balanced after matching, but similarly as discussed in relation to the individual level results, balance of background variables is not required for the DiD strategy to be valid. Instead, the goal in conducting the matching is to make it plausible that the parallel trends assumption holds.

Subsequently, we estimate difference-in-differences regressions of the following type:

$$Y_{it} = \gamma_i + \lambda_t + \beta D_i + \gamma post_t + \delta(D_i * post_t) + \epsilon_{it} \tag{6}$$

where γ_i and λ_t are unit (firm or establishment) and year fixed effects, respectively, and D_i is the treatment indicator that gets value 1 for treated firms when an establishment or a firm is treated. Because the difference-in-differences strategy relies on the parallel trends assumption, we also estimate event study regressions to assess pre-trends. The event study figures are also useful to assess treatment dynamics in the post-period. This regression takes the following form:

$$Y_{it} = \gamma_i + \lambda_t + \sum_{\substack{k=-3 \\ k\neq -1}}^{3} \theta_k \cdot D_i \cdot \mathbb{1}\{K_{it} = k\} + \epsilon_{it} \tag{7}$$

Similarly, as previously, γ_i and λ_t are firm and year fixed effects, respectively, and D_i is the treatment indicator. We only look at the first time a firm becomes treated. To clarify, the year when a previously untreated firm, for example, employs a worker in some occupation, and that occupation becomes exempted during a year, that year becomes the "event year" for that firm.

4.4 Identification

Throughout the paper we extensively assess the validity of our identification strategy. In our main analyses, we compare exempted occupation-regions to non-exempted occupation-regions. The occupations are chosen on the list of exemptions by the regional offices of the central government (ELY Centers). The stated aim of the policy is to target "occupations with increased difficulty in hiring". The concern in our setting is that the selection procedure generates a treatment and control group with non-parallel trends. Thus, we need to carefully assess the validity of our setting. We focus on the occupation-region specification (Section 4.1), since the selection takes place at that level.

As a first test of our identification strategy, we compare covariates in the treated occupation-regions to the never-treated occupation-regions (our control group in the main analyses) in the pre-treatment year in Table 1. In Panel A, we show variables, whose changes could plausibly indicate labor market tightness. None of these variables are significant at the 5 percent level, although incomes did rise around 1.1% more in the treatment group, which is significant at the 10 percent level.

In Panel B, we show other covariates. The treated occupation-regions differ from the never-treated ones in most aspects. Absolute differences (such as number of non-EU workers, open vacancies), however, are perhaps due to population differences in the size of the occupations in terms of workers, as the treated occupations are larger in population and number of workers. It also seems that before treatment, the treatment group occupations have lower salaries than the control group occupations. Level differences in these covariates are not a threat to our DiD type identification strategy.

As a second test, we estimate our staggered differences-in-differences specification discussed in Section 4.1 using the ratio of the number of vacancies to the number of unemployed individuals (V/U ratio), a measure of labor market tightness, as the outcome variable. Figure 2 shows that there is no pre-trend in labor market tightness, suggesting that the exemptions have not been successfully targeted to tightening labor markets according to this measure. The data on unemployed job seekers and open vacancies comes from the Ministry of Employment and the Economy and thus is from a different source than, e.g., our data on earnings and employment which are from Statistics Finland.

Figure 2 also shows a negative treatment effect on occupation-region level labor market tightness. Further analyses in the Online Appendix A.2 show that this negative effect comes from increased number of unemployed job seekers in the occupation. Note that this only means that the absolute number of unemployed job seekers searching jobs in the occupation-region unit increases, it does not necessarily mean that the unemployment rate would have increased, because there may also be more employed workers after treatment. Moreover, perhaps some of the increase in the count of job seekers in that occupation would be due to immigrants applying for jobs, or caseworkers labeling individuals to the exempted occupation. Thus, we would not read too much into the observed effect. Instead, the main reason to include the Figure is that it suggests no pre-trend in labor market tightness, strengthening the argument that changes in the V/U ratio does not seem to predict exemption decisions.

Third, if the exemptions are well-targeted, we expect a positive earnings pre-trend in the DiD analyses for our main outcomes. We diligently study pre-trends throughout our analyses for any suspicious anomalies that would threaten our identification strategy at the occupation-region, individual or firm level.

Lastly, a crucial assumption for a differences-in-differences design is the Stable Unit Treatment Value Assumption (SUTVA). SUTVA could be potentially violated in case the treatment affects the control labor markets either through spillover effects, general equilibrium effects or selected movement of individuals from one group to the other. We discuss these challenges and perform a range of robustness checks in Section 8, and argue that a core strength of our identification strategy is that each treated occupation-region represent a small portion of the total labor market and is thus practically impossible to have a significant effect on the never-treated occupation-regions.

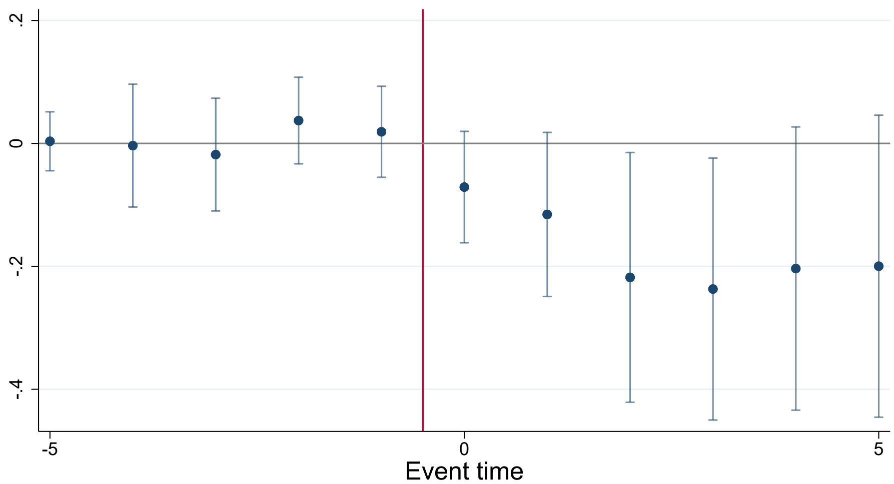


Figure 2: Labor market tightness (V/U): occupation-region level event study

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the vacancies-to-unemployment ratio (V/U). The sample used in this analysis is different from our main analyses because it does not include a large part of occupation-region units as those units with 0 unemployed job seekers are excluded. This is because V/U is not defined for those. As we use a balanced panel, occupation-region units that have 0 unemployed job seekers in any of the years are excluded from the sample. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by panel ID (occupation-region). Data points in the period preceding the treatment show the yearly change relative to the previous year. After the treatment, the effect is calculated relative to period preciding the start of treatment (year -1). We estimate effects for all pre and post years but only estimates in window [-5,5] are plotted in the figure.

Table 1: Pre-treatment Differences Between Treatment and Control Groups

	Mean, control	Mean, treat	Difference, treatment-control
Panel A: Measures of labor market tightness			
Vacancy-to-unemployment ratio (V/U)	0.203	0.256	0.046
	(1.909)	(0.866)	(0.084)
Change in income (%)	3.032	4.072	1.062*
	(13.878)	(12.124)	(0.594)
Change in vacancy length (days)	1.780	5.280	3.385
	(61.563)	(44.753)	(3.751)
Panel B: Other covariates			
Number of non-EU workers	2.704	13.365	10.526***
	(18.668)	(90.330)	(0.875)
Number of workers	236.412	689.443	454.114***
	(798.185)	(1,678.769)	(33.686)
Share of non-EU workers (%)	0.010	0.020	0.010***
	(0.045)	(0.086)	(0.002)
Average salary	35,945.219	31,535.613	-4,597.885***
	(16,184.853)	(9,734.679)	(666.356)
Median salary	35,520.348	31,813.752	-3,884.597***
	(15,685.622)	(9,655.717)	(645.978)
Sd, salary	13,410.104	11,505.502	-2,010.553***
	(7,697.778)	(4,047.067)	(322.584)
Number of unemployed	31.763	78.719	47.069***
	(101.090)	(155.535)	(4.212)
Number of open vacancies	3.519	15.741	12.098***
	(23.306)	(48.249)	(0.982)
Unemployment months	0.371	0.352	-0.018
	(0.599)	(0.516)	(0.025)
Unemployed (%)	0.087	0.088	0.001
	(0.116)	(0.105)	(0.005)
Region-level wage sum	4.303e+09	5.322e+09	9.941e+08***
	(6.451e+09)	(6.716e+09)	(2.665e+08)
Region-level population	181159.078	226440.141	45,693.441***
	(226379.063)	(247916.938)	(9,361.537)
Region-level unemployment months	1.017	1.019	0.002
	(0.274)	(0.159)	(0.011)

Notes. This table shows the difference between treated observations in the year preceding the LMT exemption, and control (never treated) units for each year (2011–2019). Each difference is computed using regressing the background characteristic on treatment status. Each row represents a separate regression. Year indicators are included in the estimation. The treatment group includes observations from -1 for each treated cohort, and the control group column includes observations for never-treated units in each -1 year. See Online Appendix E for descriptive statistics separately for each treated cohort. The vacancy/unemployment ratio is not observed for all units as a significant fraction of the occupation-region units have U = 0, i.e., zero job seekers who are considered to belong to that specific 4-digit occupation in the specific region.

5 Results

5.1 Occupation-region level

5.1.1 Effects on the stock and inflow of non-EU workers

Before analyzing subsequent outcomes, we assess to what extent regional exemptions from labor market testing have any effects on the inflows of non-EU workers to the occupation-region. A large part of any subsequent labor market effects of the policy change are likely to follow from this effect. However, we are interested in the total effect of the policy and do not in general assume that the only channel is through the number of immigrants.

Figure 3 presents the estimation results. Panel A of this figure presents the effects of removing labor market testing requirements on the inflow of non-EU workers to the occupation-region, while panel B presents the effect on the stock. These new non-EU workers may be either new immigrants or individuals from non-EU countries who do not have work authorization for full-time work (see Section 5.1.2 for more details).

Results in Figure 3 show that removing labor market testing requirements increased the inflow and stock of non-EU workers employed in treated occupation-region units. The effect on the inflow of non-EU workers is around +5 in year 3-4 and even +10 in year 5. The stock effect is around 25 individuals per occupation-region in year 5. These effects increase over time during the five year observation window. Firms may take some time to respond to the new rules and the work permit process even without the labor market testing could take up to 6 months.[7] Also, exemption decisions are made during the year zero, inlcuding late in the year, and we aggregate decisions to a yearly level.

The pre-trends in both panels of Figure 3 indicate that the number of non-EU workers slightly increased in the treatment group already in the year before the treatment began. This increase is, however, small in size compared to the effects observed in the post-period. We test robustness by using different methods and including the not-yet-treated unit in the Online Appendix D. The results are robust to including not-yeat treated units and to using other event study methods.

[7]The median processing time in 2020 was 70 days (Migri, 2021).

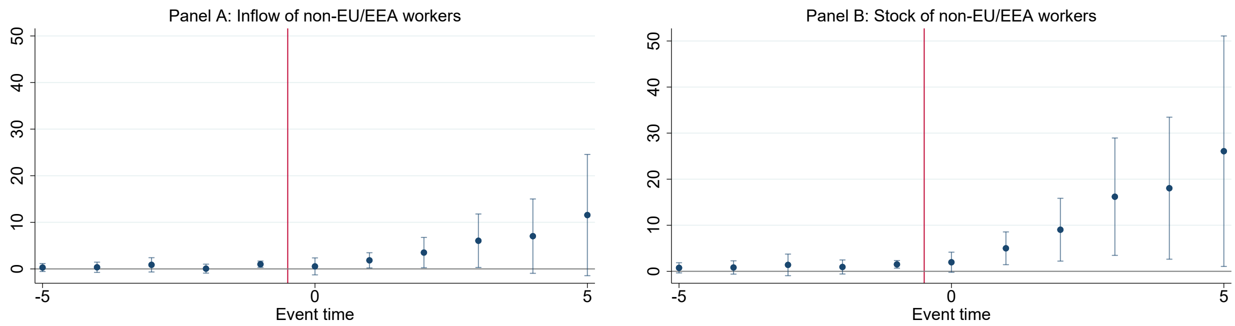


Figure 3: Effect on the inflow and stock of non-EU workers

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the inflow or inflow of non-EU workers. non-EU workers are defined as those workers who have migrated to Finland from outside the EU/EEA during some year between 2006 and 2019 (i.e., relatively recent migrants to Finland), and who are not citizens of any EU country or born in EU countries. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by panel ID (occupation-region). Data points in the period preceding the treatment show the yearly change relative to the previous year. After the treatment, the effect is calculated relative to period preciding the start of treatment (year -1). We estimate effects for all pre and post years but only estimates in window [-5,5] are plotted in the figure.

5.1.2 Decomposing the effect on the inflow of non-EU workers

We decompose the increase in non-EU workers in the treatment group into new immigrants and immigrants already residing in Finland to understand the composition of the inflow. The first group accounts for less than 20% of the first-stage effect in year 5 and it consists of workers who were not in Finland in the previous year, i.e., new immigrants (Panel A of Figure 4). The rest are immigrants who were in Finland the year the occupation was exempted (Panel B of Figure 4). It includes workers who change occupations[8], non-married partners of foreign workers, international students (only part-time work allowed with a student visa), and asylum seekers (proxy), shown in Panels A-D of Online Appendix Figure A1.[9] These groups are not mutually exclusive.

Panel A of Online Appendix Figure A1 indicates that the policy shift increases the inflow of asylum seekers by 1.5 in year 5. Asylum seekers are proxied by using the top 4

[8]Before 2019, changing occupation in most cases required the worker to go through a new LMT procedure. According to HE 273/2018, in 2017, 3,138 applications for extended permits were under LMT. Starting June 1, 2019, the LMT procedure was removed from individuals applying for an extended permit. The change aimed to ease occupational mobility and increase labor supply (especially in cleaning, manufacturing, construction, and agriculture). It would also make the process for extended permits faster. The new law still requires the worker to have worked for at least a year in the occupation to prevent misuse of the permit system.

[9]Asylum seekers who have not received a decision on their application, or those who have been denied asylum, have the option to "change the track" and apply for a work-based residence permit. The number of asylum seekers applying for work-based residence permits has been around 1,100 during 2015-2018, little less than half of which have been granted (Keskisuomalainen 2019, see `https://www.ksml.fi/paikalliset/2398591`)

countries where most asylum seekers come to Finland (Afghanistan, Syria, Iran, Iraq). The effect is statistically significant and represents around 17% of the whole effect on the inflow of *non-recent* migrants to treated units. If we expanded the set of countries included when proxying asylum seekers to include the top 9 countries, the effect would be an increase of 2 individuals in year 5.

Panel B of Online Appendix Figure A1 shows that international students are potentially a very large group of individuals who are affected by LMT, as the inflow of non-EU citizens who are also enrolled in education, increases in treated units. In Finland, international students can work for up to 30 hours a week, but there could be reasons – such as wanting to stay in Finland long-term, wanting to work full-time, or not wanting to finish studies — why these individuals may still want a work permit. Getting a work permit would possibly be challenging under the LMT requirement but significantly easier without it.

Panels C and D of Online Appendix Figure A1 show that non-EU spouses of non-EU workers and occupation changers are relevant channels. The latter (occupation changers) should be mainly relevant before the law change in 2019 which removed LMT from some occupation changers (in some cases the requirement may still apply). In addition, some international students could still fit in this category even after 2019 if they worked in part-time jobs and regional exemptions from LMT then made it possible for them to switch to full-time jobs in a different field.

Approximately half of the overall effect on the inflow of non-EU immigrants comes from individuals who move from other occupations and another half from people who did not work in any occupation in Finland during the previous year (see Online Appendix A.11, Figures A28 and A29).

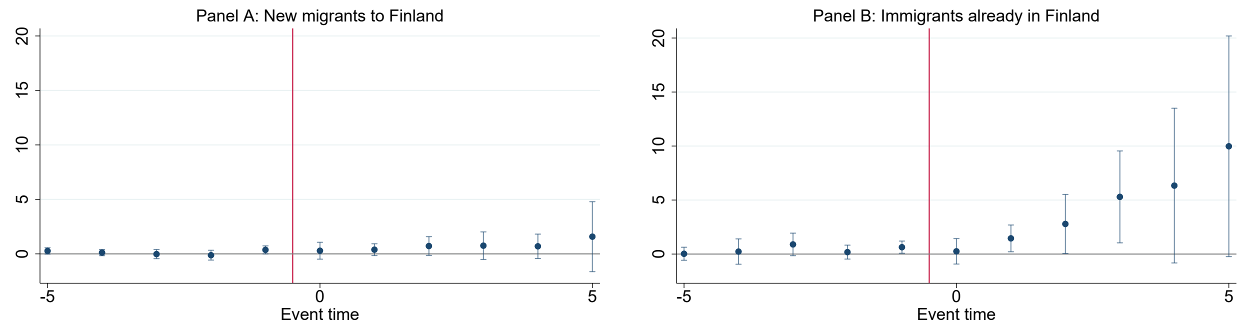


Figure 4: Decomposition of the effect on the inflow of foreign workers

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the inflow of non-EU workers to the occupation region. In Panel A, only new immigrants, meaning those who move to Finland during the same year when they start working in the occupation, are included. Panel B includes all non-EU workers. We aim to decompose the effect in Panel B to different subgroups in Online Appendix Figure A1. We estimate effects for all pre and post years but only estimates in window [-5,5] are plotted in the figure.

5.1.3 Effect on average earnings

Panel A of Figure 5 shows our main estimates on the effects of lifting labor market testing requirements on the annual earnings of native workers in treated occupation-regions. We observe a decrease in annual earnings for native workers, which shows in years 4 and 5, of more than €500.

We then separate the main effect by occupation mean salary. We define mean salary as the national mean for all workers in an occupation in 2012–2019. We include 10 percent of occupations in each regression and move the observation window up by one percentage point between each regression. No control variables are used in estimation, similarly to main estimations. In such fashion, we estimate the earnings effect for the whole post period (different for each treated unit) in 91 separate regressions. The results are shown in Figure 6. The earnings effect is clearly delineated into two groups. There is a negative effect in the bottom quarter of occupations, and barely anything noticeable in the top three quarters besides a potentially spurious drop at around the 90th percentile.

We will analyse the bottom quarter separately in our occupation-region level results. Panel B of Figure 5 shows that for the bottom quartile, earnings are negatively impacted by a point estimate of close to €2,000 by year five. In the top three quartiles we observe no significant effect. However, there is some evidence of pre-trends in Panel B, indicating that the control group (never treated occupations) may not be as good of a control group for the lowest quartile of occupations as it is for the whole treatment group.

In Figure 7 we go back to our results on the stock of non-EU workers and separate them also by occupation salary percentile. There is no discontinuity in the effect around the 25th percentile, ruling out the simple explanation that above the 25th percentile there would be no effect on labor supply and thus no effect on earnings. There is, however, a steep drop in the effect around the 40th percentile, implying that perhaps the negative effect observed at high salary occupations in Figure 6 is the result of mere randomness.

Table 2 shows the occupation-region estimates for all individuals, natives, EU immigrants and non-EU immigrants. ATT estimates shown in the table are calculated for the whole post-period (Panel A) and for year +5 (Panel B). Panel A shows that the treatment effects calculated for the whole post period are not significant in any group for the whole sample and top 3 earnings quartiles of occupations. In the bottom quartile we find that for natives there is an earnings effect -€1,188 (-7.1%) annually. This reflects also in a similar estimate for all individuals, since they are mostly natives. In panel B, we show the estimates for year 5 effect, which we consider the medium-term effect. We find a significant effect for natives of -€647 (-2.2%) annually. The estimate for the bottom quartile is -€1,790 (-10.7%) for natives, and -€2,065 (-11.4%) for EU immigrants. We find no effect for the whole post period or in the medium-term at occupation-region level for the top 3 quartiles. The results show that the ATT estimate for the lowest salary quartile of occupations is negative, sizable, and statistically significant. The table also shows that there is no effect in any earnings quartile for non-EU workers at the occupation-region level from the policy.

The treated occupations-regions may be selected to target tight labor markets in particular. It is thus particularly necessary to study whether our assumption of parallel trends holds in the pre-treatment period. With a well-targeted policy, one would expect to see an increasing trend in earnings compared to a comparison group. In such a case, the true earnings effect of the policy would be higher than what we estimate. We observe no noticeable pre-trends in Panel A of Figure 5, as pre-treatment coefficients are close to zero and not statistically significant. It seems that the policy does not succeed in targeting occupations in regions with a comparatively increasing earnings trend. In Panel B, there are no significant pre-treatment estimates. However, the pre-treatment estimates are mostly positive for the top 3 quartiles and negative for the bottom quartile implying more need for caution when interpreting these results. It is possible that using all never-treated occupation-regions as a control group also for quartile-level analyses, induces some bias. We test in Online Appendix K how the results change if we limit the control group to include only occupations in the lowest quartile. This robustness check yields the same finding that the effects are more pronounced in the lowest quartile of occupations, but the magnitude of the estimate for the lowest quartile is much smaller (around -€700 in year +5). The specification does not exhibit significant pre-trends. However, because many occupations in the lowest quartile have been exempted at some point, the never-exempted group of lowest quartile occupations may also be somewhat selected, and thus, perhaps not the best control group either.

In the Online Appendix A Figure A15, we also estimate effects separately for each 1-digit occupation class, except for specialists, who are generally exempted from labor market testing. These results show that the group that is driving the negative earnings effects is service workers (group 5 in the ISCO 2010 occupation classification). We then pool the occupations into two groups, services (group 5 and service occupations in group 9) and the rest. In Online Appendix I, we show the number of immigrants for service workers (Figure I3) and the rest (Figure I4), similar to Figure 7. Qualitatively the pattern is similar in the two subgroups: LMT exceptions induce more immigration in occupations below the 40th salary percentile. Quantitatively the numbers are an order of magnitude smaller in non-services. The earnings effect for the service sector is negative below the 25th salary percentile occupations (Figure I5). It seems that services drive the 25th percentile change. In non-service sectors, the negative earnings effect becomes more pronounced only for occupations below around the 15th percentile (Figure I6).

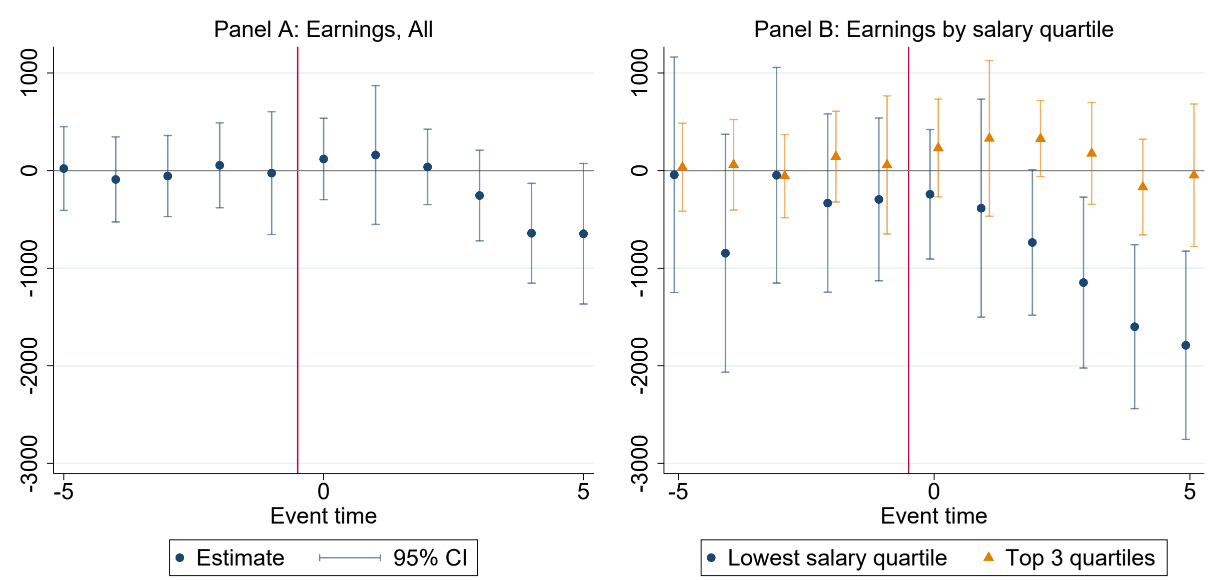


Figure 5: Effects on total annual labor earnings of natives

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the annual earnings of natives. The control group includes only never-treated units, and the control group is the same (all never-treated occupations) in both figures. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID. We estimate effects for all pre and post years, but only estimates in window [-5,5] are plotted in the figure.

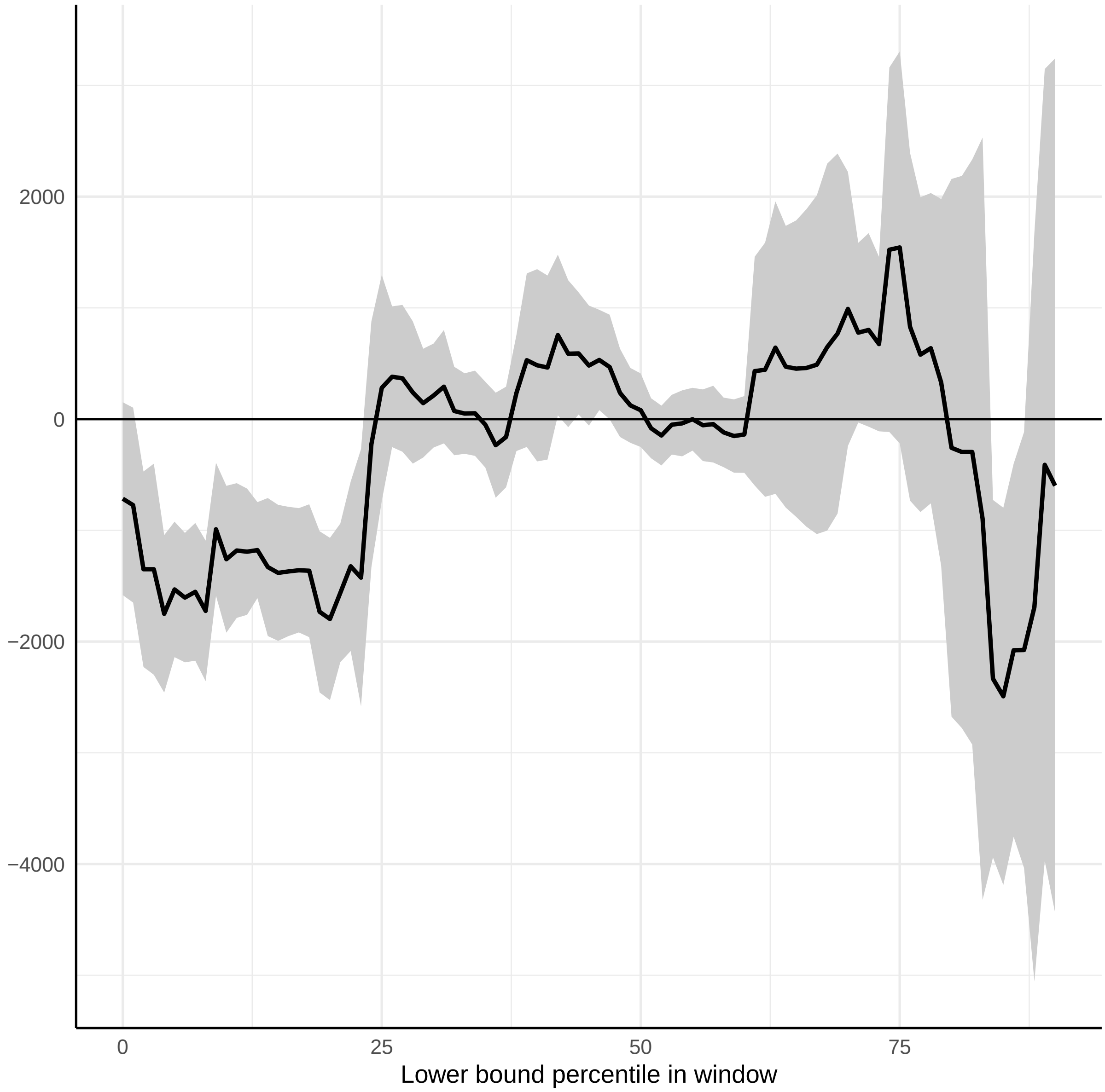


Figure 6: Effect on native earnings for occupations (excluding specialists) in 10 point percentile windows

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is annual earnings separately for different professions in 10 percentile intervals in the earnings distribution. The control group is always the same (all never-treated occupations). The 25th percentile is €24,069 in annual earnings. The 50th percentile is €31,143. The 75th percentile is €39,568. The x-axis shows the lower bound of the estimation window, e.g., the estimate at percentile 1 on the x-axis includes percentiles 1 to 10.

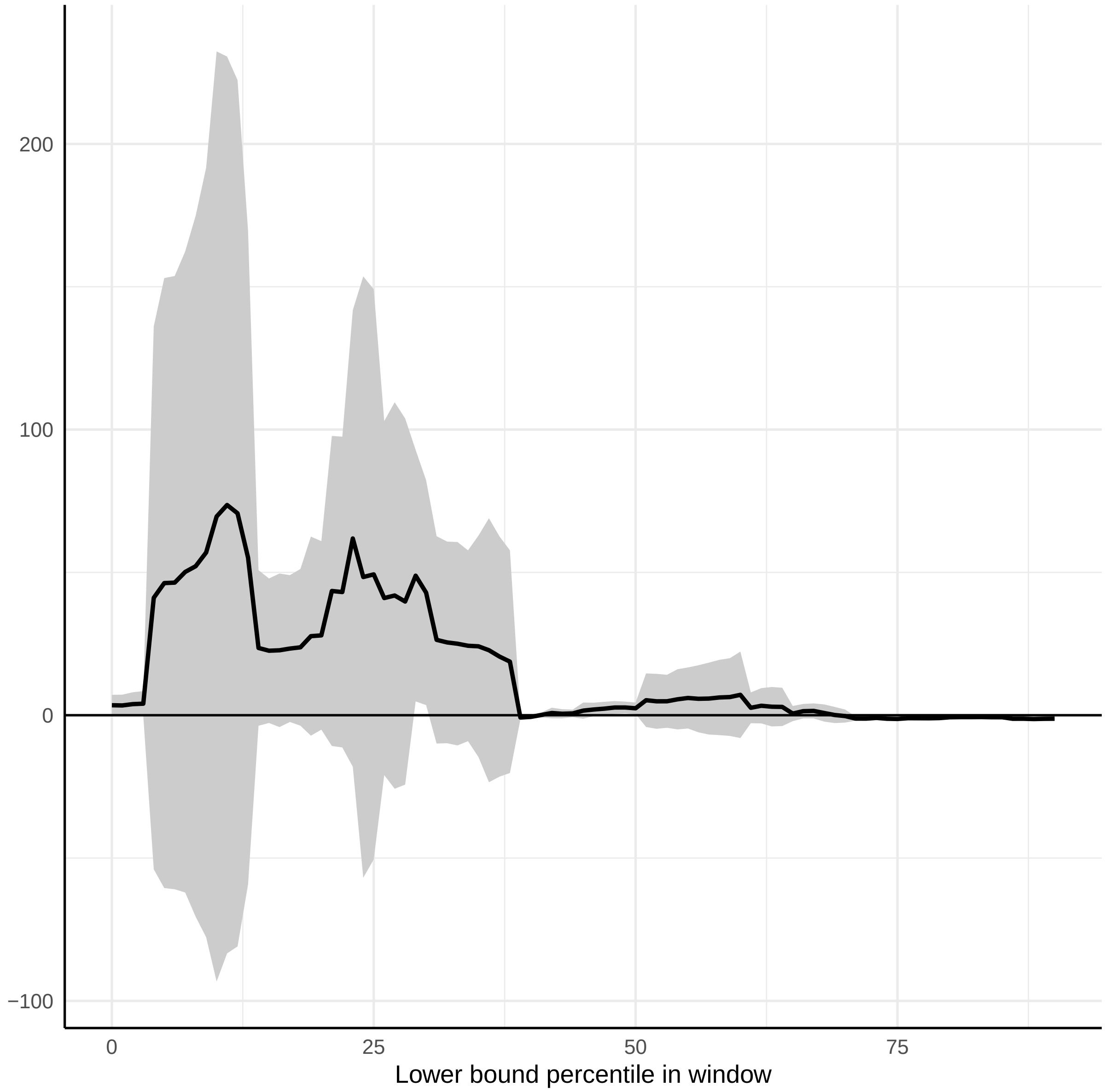


Figure 7: Effect on the stock of non-EU workers for occupations (excluding specialists) in 10 point percentile windows

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the stock of non-EU workers separately for different professions in 10% intervals in the earnings distribution. The control group is always the same (all never-treated occupations). The 25th percentile is €24,069 in annual earnings. The 50th percentile is €31,143. The 75th percentile is €39,568. The x-axis shows the lower bound of the estimation window, e.g., the estimate at percentile 1 on the x-axis includes percentiles 1 to 10.

Table 2: Effect on average annual earnings, occupation-region level

	(1) Earnings	(2) Native earnings	(3) Earnings, EU immigrants	(4) Earnings, non-EU immigrants
Panel A: Simple Callaway & Sant'Anna ATT estimates (whole post period)				
All occupations				
Treatment effect	-126.26	-207.08	-31.18	329.30
	(129.49)	(131.37)	(506.30)	(627.27)
Outcome mean (treated)	29451.02	29567.27	26524.25	20989.55
Occupations in the bottom quartile of the occupational salary distribution				
Treatment effect	-1021.6***	-1187.84***	-832.46*	467.42
	(238.24)	(244.46)	(491.55)	(556.31)
Outcome mean (treated)	16763.22	16769.34	18083.41	14847.22
Occupations in the top 3 quartiles of the occupational salary distribution				
Treatment effect	230.60	183.82	405.13	188.22
	(150.16)	(135.14)	(684.76)	(1233.64)
Outcome mean (treated)	33521.29	33672.87	29757.81	23837.67
Panel B: Medium-term (year +5) Callaway & Sant'Anna dynamic estimates				
All occupations				
Treatment effect	-550.46**	-646.81***	-1187.31	-77.06
	(233.46)	(229.33)	(768.11)	(861.89)
Outcome mean (treated)	29451.02	29567.27	26524.25	20989.55
Occupations in the bottom quartile of the occupational salary distribution				
Treatment effect	-1571.67***	-1789.89***	-2064.75**	153.82
	(387.08)	(393.59)	(879.19)	(775.26)
Outcome mean (treated)	16763.22	16769.34	18083.41	14847.22
Occupations in the top 3 quartiles of the occupational salary distribution				
Treatment effect	-15.36	-47.68	-459.70	-333.59
	(249.94)	(266.57)	(1022.67)	(1596.46)
Outcome mean (treated)	33521.29	33672.87	29757.81	23837.67

Notes. The table shows occupation-region level Callaways & San't Anna estimates where the outcome variables are the mean earnings in the occupation-region unit. Standard errors clustered by occupation-region in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome means is the mean for treatment group in year -2.

The results shown in this subsection merely tell how the *average earnings* of the workers in the occupation-region has been affected by the policy. The negative effect observed for natives working in the lowest quartile of occupations, could, therefore, come either from the existing workers' salaries being affected, new workers' salaries being affected, or from changing composition.

5.1.4 Mechanisms and heterogeneity

The results presented in the figures above are not informative on why the average earnings of natives are negatively affected in treated occupation-regions. It could merely reflect changing worker flows to and from the occupation-region cell. In order to understand the negative earnings effect better we assess potential mechanisms, such as working hours and worker composition.

We first break the earnings effect to its components, hourly wage and working hours, during a specific month when the survey is collected. When doing this, we clean the hourly wage from overtime hours, since overtime hours are better paid than other hours, and thus, overtime affects also the hourly wage and we are interested in the base hourly wage. We do the decomposition to hourly wage and working hours using detailed earnings data available for one month of the year for most workers, but not all (see Section 3). In Figure 8, we plot the event study estimates for monthly salary (Panel A), hourly wage (Panel B), and working hours (Panel C). These are estimated separately for the group that was most clearly affected, i.e., the lowest salary quartile of occupations and the three highest salary quartiles. Panel A shows a monthly salary estimate of a bit under -€200 for the bottom quartile in years 4 and 5, which is roughly in line with our main result from the annual earnings data. Based on Panel C, it seems that there is negative effect on the working hours of the lowest salary quartile workers, likely explaining to a large part of the negative earnings effect for the lowest quartile of occupations at the occupation-region level.

A negative effect on the occupation-region average salaries could follow partly or fully from a change in worker composition after the policy change is introduced. If workers with the highest productivity leave for other occupations or regions, earnings could fall by change of composition, and vice versa. We thus study worker inflows and outflows. In these analyses, the outcome variable is the annual *share* of workers that exits the occupation-region and moves to various other states (e.g., unemployment, working in another occupation). We do not find any significant effects on either the inflow or outflow of workers, although the point estimate on the share of workers who move to unemployment is relatively high in the last two post-period years for the lowest salary quartile professions (but the CIs are very wide). These results are shown in Online Appendix A (Figures A10-A12). These results give little indication that worker composition change would explain much of the earnings effect. We also assess the effects on the salaries of new workers in Online Appendix A (Figure A5) and find no effects.

Dustmann et al. (2017) found negative wage effects for young workers from a wave of immigration, which affected the whole local labor market. We exploit our occupation-

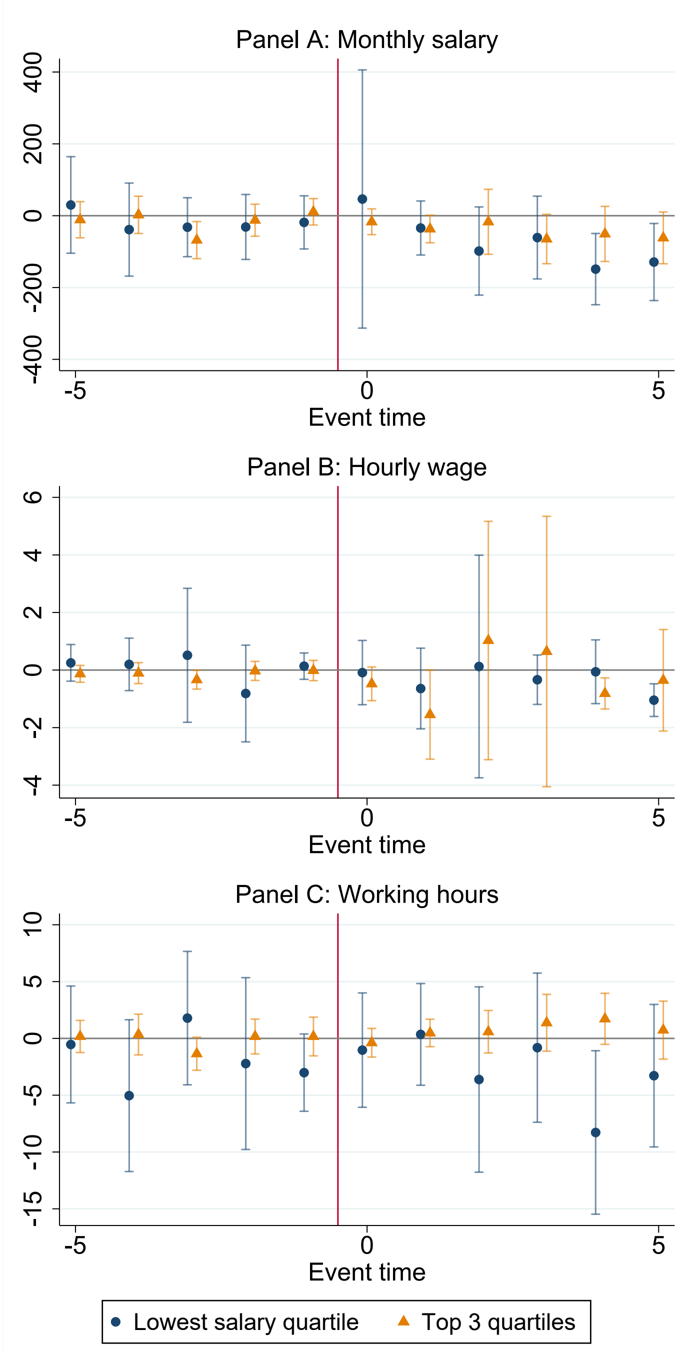


Figure 8: Effects on salary components, one-month information (Earnings Structure Survey)

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variables are different salary components. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID. The figure is based on a different dataset (Structure of Earnings Survey) than our other earnings results.

region level variation in immigration to study the effect on earnings and outflow to unemployment separately for young ($\leq$ 30 years old) and old ($>$ 50 years old) native workers. Our results, shown in Figure 9, indicate that the negative earnings effect we observe for the bottom quartile is especially pronounced for older workers (Panel B), while the earnings of young workers (Panel A) are not affected. This does not mean that there would be no effects for individuals between ages 30 and 50, but merely that the effect is larger for older workers, and that there is no earnings effect for workers younger than 30 years old.

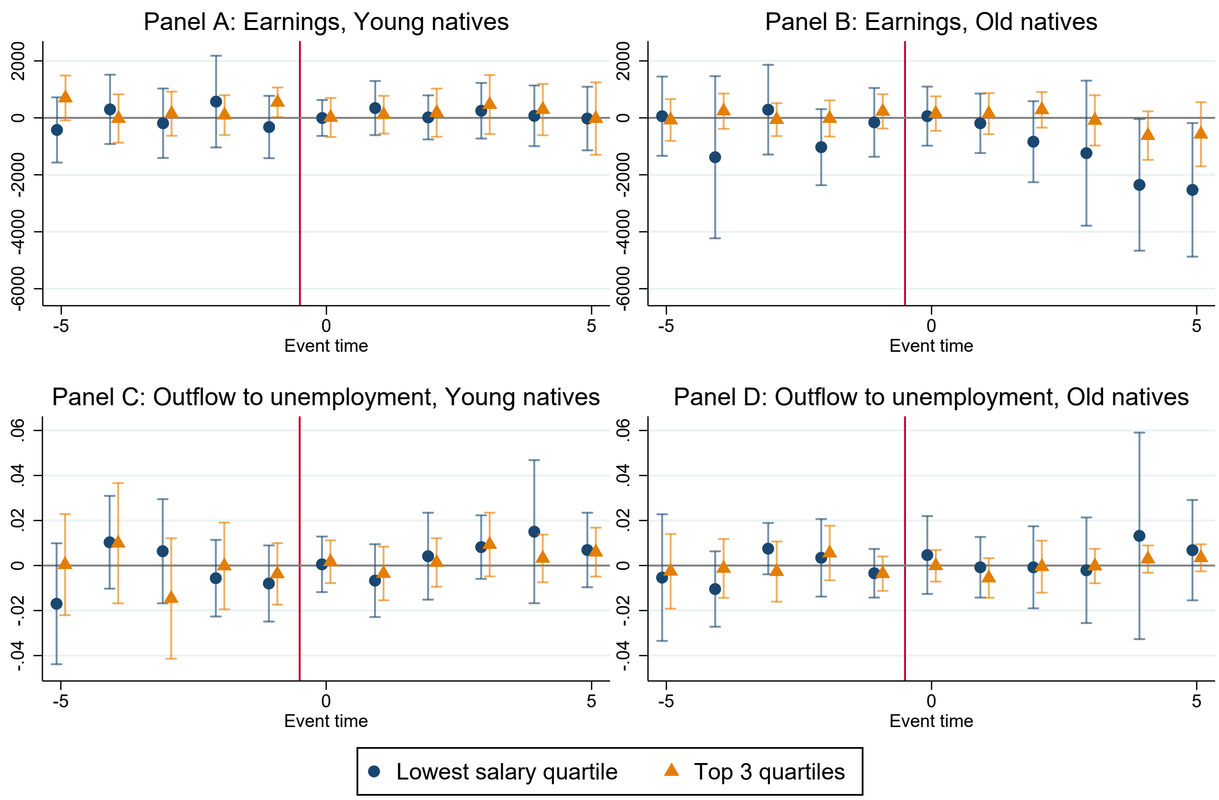


Figure 9: Effect on the annual earnings and inflow to unemployment of young ($\leq$ 30) and old ($>$ 50) native workers

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variables are earnings of young workers, earnings of old workers, and the share of young and old workers who outflow to unemployment. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

We show in Online Appendix C that there is heterogeneity between groups (i.e., units treated at different times). We show the by-group estimates for the main outcomes in Figures C2 and C3. The heterogeneity across cohorts is likely due to different types of occupations being treated at different times, as we also show that the wage effect depends on the occupation, with low-paying occupations being the most affected.

In Online Appendix A.9, we show there is heterogeneity between urban areas (70% of Finnish municipalities) and countryside (30% of Finnish municipalities). The effect on earnings seems to come from urban municipalities (cities or non-city urban municipalities) only, while no earnings effects are observed in rural municipalities.

5.2 Individual level

5.2.1 Earnings and employment

In Section 5.1, we focused on the effects of exemptions from labor market testing at the occupation-region labor market level. Now, we turn our attention to the individual level. The earnings effect on the treated individuals can be seen as a compound effect on their current job plus any adjustments they make. There are multiple possible ways for individuals to adapt to the change in circumstances. In fact, individuals might even benefit if a wave of immigration within the occupation creates opportunities for upward mobility.

In the individual-level analyses presented in this section, an individual is considered treated when they work in the occupation-region unit one year before the exemption. They remain treated even if they change their occupation or region or if they stop working altogether. For computational reasons, we choose to use standard two-way fixed effects (TWFE) models in these individual-level analyses. The high number of individual-level observations makes the estimation of the C&S model infeasible. Additionally, we prefer the TWFE approach because it allows us to cluster standard errors at the occupation-region level, which is not possible using the C&S method due to the lack of nesting of individuals within clusters.

Figure 10 presents the TWFE event study estimates showing the impact of a policy change on the annual earnings of individuals for all workers. Earnings effects become significantly negative three years after the policy change, starting a declining trend in earnings up to five years after the policy change. The pre-treatment period shows a statistically insignificant but positive trend, suggesting that any existing trends prior to the policy change would have been in the opposite direction of the post-treatment estimates. We do not consider the pre-treatment trends at the individual level to be a valid test for our research setting since the selection was made at the occupation-region level, and the individual-level analyses' pre-trends follow the individuals' earnings paths. However, there is a possibility of an attenuating effect due to a positive pre-trend in the individual-level estimates.

Table 3 collects both pooled earnings effects for the whole post-period and medium-term (year +5) effects for all workers, workers in the lowest quartile of occupations (in terms of the average salary in the occupation), and workers in the lowest quartile of occupations. Effects are also estimated separately for native workers, EU immigrants, and non-EU immigrants. The pooled estimates for the whole post period, in turn, are -€314 (-1.1%) for all workers, -€335 (-1.2%) for native workers, and -€1,167 (-6.4%) for non-EU immigrants (Panel A). Similarly to the occupation-region estimates, the effect is more pronounced for the bottom quartile at -€699 (-3.4%) for all workers and -€715 (-3.5%) for native workers. The first significant effect for the top 3 quartiles we observe is for non-EU immigrants at -€1,734 (-8.7%). Panel B shows a negative earnings effect of -€1,067 (-3.8%) for all workers, -€1,121 (-4.0%) for natives, and -€1,784 (-9.7%) for non-EU immigrants five years after the policy change. These earnings effects at the individual level are more pronounced for workers in bottom quartile occupations, including a significant estimate of -€2,932 (-14.5%) for EU immigrants. For the top 3 salary quartile occupations, the year 5 effect is significant for all workers (-€1,034, -3.4%), natives (-€1,088, -3.6%), and non-EU immigrants (-€2,561, -12.8%).

The absolute magnitude of the overall negative effect for all workers is larger at the individual level than the occupation-region level. However, this is not the case for the workers in the lowest salary quartile, although due to issues with pre-trends in the occupation-region level estimates for the lowest salary quartile, it is possible that the occupation-region level estimate for the lowest quartile is larger than the true causal effect. There are also several possible reasons why the individual effect is different from the occupation-region effect. First, the two specifications weight the observations differently. In the previous occupation-region analysis, a unit with few workers has the same weight as a unit with many workers, whereas, in the individual-level analysis, the weight of an occupation-region is proportional to its size. Around 40% of treated units in the occupation-region analyses and around 66% of treated workers in the individual level analyses are in the bottom salary quarter of occupations. This point is relevant for the all occupation estimates. Second, it is possible that new entrants – which affect the occupation-region but not the individual level estimates – have higher earnings than the incumbent workers. This would moderate the occupation-region level estimate compared

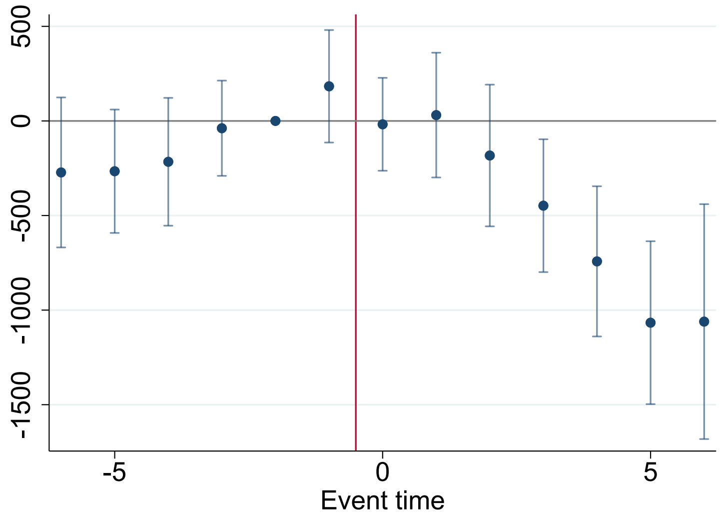


Figure 10: Earnings effect at the individual level

Notes. The figure shows the individual-level TWFE estimates where the outcome variable is the annual earnings of all workers. Confidence intervals are 95% confidence intervals. Standard errors are clustered by occupation-region. Year -2 is used as a reference period.

to what we observe at the individual level. Third, possible outflows outside of the labor force or to unemployment would not decrease occupation-region level wages but would impact individual-level estimates.

Table 4 depicts the effect on year-end employment for similar groups. Interestingly, these estimates for employment, driven by natives in the three highest salary quartiles, indicate that LMT rule exemptions increase employment by 1.2 percentage points (1.3%) when looking at pooled results and close to 2 percentage points (2.1%) five years after the rule change. These results, combined with our later firm-level analysis in Section 5.3 showing the effect on firm employees' growth, suggest that there are benefits in terms of average employment for the three highest salary quartiles.

Table 3: Earnings effects, individual level

	(1) Earnings	(2) Native earnings	(3) Earnings, EU immigrants	(4) Earnings, non-EU immigrants
Panel A: Pooled TWFE estimates				
Workers in all occupations (original matched groups)				
Treatment effect	-314.0**	-334.9**	348.4	-1,166.9**
	(139.5)	(139.0)	(500.5)	(568.1)
N	6,403,800	5,975,170	59,260	82,880
Outcome mean (treated)	27,770.23	28,013.52	25,646.46	18,320.94
Workers in bottom quartile occupations				
Treatment effect	-699.2***	-714.7***	-1,499.9*	-623.5
	(234.8)	(222.6)	(772.2)	(696.1)
N	1,643,650	1,468,410	25,870	45,630
Outcome mean (treated)	20,310.11	20,415.04	20,190.48	15,848.51
Workers in top 3 quartiles of occupations				
Treatment effect	-224.1	-249.4*	940.5*	-1,734.2***
	(145.5)	(145.9)	(518.3)	(610.3)
N	4,760,150	4,506,760	33,390	37,250
Outcome mean (treated)	30,168.92	30,403.89	27,594.77	19,975.39
Panel B: Medium-term (year 5) TWFE estimates				
Workers in all occupations (original matched groups)				
Treatment effect	-1,066.6***	-1,120.9***	-15.07	-1,783.6**
	(219.7)	(221.4)	(675.6)	(697.4)
N	6,403,800	5,975,170	59,260	82,880
Outcome mean (treated)	27,770.23	28,013.52	25,646.46	18,320.94
Workers in bottom quartile occupations				
Treatment effect	-1,216.5***	-1,244.5***	-2,932.4***	-2,001.5**
	(367.5)	(358.6)	(1,049.9)	(939.4)
N	1,643,650	1,468,410	25,870	45,630
Outcome mean (treated)	20,310.11	20,415.04	20,190.48	15,848.51
Workers in top 3 quartiles of occupations				
Treatment effect	-1,034.1***	-1,087.7***	945.6	-2,560.6**
	(264.5)	(265.7)	(902.8)	(1,160.5)
N	4,760,150	4,506,760	33,390	37,250
Outcome mean (treated)	30,168.92	30,403.89	27,594.77	19,975.39

Notes. The table shows TWFE estimates where the outcome variables are the earnings of different types of workers. Standard errors clustered by occupation-region in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome means is the mean for treatment group in year -2.

Table 4: Employment effects, individual level

	(1) Employment	(2) Employment, native	(3) Employment, EU immigrants	(4) Employment, non-EU immigrants
Panel A: Pooled TWFE estimates				
Workers in all occupations (original matched groups)				
Treatment effect	0.0102**	0.0117**	-0.00382	0.000299
	(0.00516)	(0.00471)	(0.00998)	(0.0235)
N	6,403,800	5,975,170	59,260	82,880
Outcome mean (treated)	0.92	0.92	0.88	0.81
Workers in bottom quartile occupations				
Treatment effect	0.00899	0.0121	0.0113	-0.0217
	(0.0121)	(0.0101)	(0.0244)	(0.0370)
N	1,643,650	1,643,650	106,809	45,630
Outcome mean (treated)	0.90	0.91	0.92	0.81
Workers in top 3 quartiles of occupations				
Treatment effect	0.0118**	0.0123***	0.00676	0.0172
	(0.00459)	(0.00461)	(0.0154)	(0.0187)
N	4,760,150	4,506,760	33,390	37,250
Outcome mean (treated)	0.92	0.92	0.87	0.81
Panel B: Medium term (year +5) TWFE estimates				
Workers in all occupations (original matched groups)				
Treatment effect	0.0147***	0.0159***	0.000973	-0.00644
	(0.00522)	(0.00451)	(0.0206)	(0.0302)
N	6,403,800	5,975,170	59,260	82,880
Outcome mean (treated)	0.92	0.92	0.88	0.81
Workers in bottom quartile occupations				
Treatment effect	0.00338	0.00714	-0.0716*	-0.0698
	(0.0141)	(0.0123)	(0.0420)	(0.0490)
N	1,643,650	1,643,650	106,809	45,630
Outcome mean (treated)	0.90	0.91	0.92	0.81
Workers in top 3 quartiles of occupations				
Treatment effect	0.0196***	0.0193***	0.0350	0.0340
	(0.00523)	(0.00505)	(0.0304)	(0.0308)
N	4,760,150	4,506,760	33,390	37,250
Outcome mean (treated)	0.92	0.92	0.87	0.81

Notes. The table shows TWFE estimates where the outcome variables are the probability of employment (at the end of the year) of different types of workers. Standard errors clustered by occupation-region in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome means is the mean for treatment group in year -2.

5.2.2 Mechanisms and heterogeneity

In this subsection, we aim to shed light on the individual-level mechanisms behind the observed earnings effects, as well as on the heterogeneity of the individual level estimates. The main heterogeneity analyses we conducted at the occupation-region level differentiated between old and young individuals, similarly as in Dustmann et al. (2017). In this subsection, we interact treatment with individual characteristics to further study how LMT exemptions affected different sub-populations. We focus on age, sex and education level. We divide age into those under 30 (20%), 30 to 50 (53%), and above 50 years (27%) and education into those with no secondary education (11%), secondary education (55%) and tertiary education (33%). Tertiary education as regards LMT in most cases would likely mean vocational tertiary education, since specialist fields are excluded from LMT. Moreover, we analyze a diverse set of outcomes to gain a more comprehensive understanding of the possible mechanisms at play. The share of females in our individual level sample is 55 %.

We begin the mechanism analysis by examining the effect on working hours and hourly wage. These analyses help assess whether the negative earnings effect stems more from decreased working hours or from a negative impact on the hourly wage. To estimate these analyses, we use the Finnish Earnings Structure Survey data, and thus, have a somewhat different sample compared to our main analyses. The earnings structure information is collected only for one month during the year and covers around 70 % of individuals who work in the private sector. Table 5 shows the estimates for the Earnings Structure Survey variables, separately for all workers, workers in the lowest salary quartile, and workers in the top 3 salary quartiles. Panel A shows pooled TWFE estimates and Panel B shows heterogeneity analyses. The results in Panel A indicate that the negative impact on earnings stems from both decreased working hours (-1.3 hours/month) and a negative impact on the hourly wage (-€0.38). When only individuals in the bottom salary quartile are included, the decrease in working hours becomes much more pronounced (-3.9 hours/month) and the effect on hourly wage disappears. For top 3 quartiles, in turn, the effect on working hours is much smaller (-0.5 hours) but the effect on the hourly wage is larger (-0.5). These results suggest that the negative earnings impact is due to different reasons for the lowest quartile compared to the top 3 quartiles. It is also likely that the occupations in the lowest salary quartile are the ones with more part-time workers (compared to higher quartiles of occupations) and thus it is consistent that the negative effect on working hours is more driven by the lowest quartile.

Panels B.B2 and B.B3 of Table 5 repeat the analysis step-by-step by gender and education. Females and those without secondary degree are, on average, less likely to

Table 5: Mechanisms and heterogeneity at the individual level, Earnings Structure Survey variables

	All Workers			Bottom Quartile			Top 3 Quartiles		
	(1) Working hours	(2) Overtime	(3) Hourly wage	(4) Working hours	(5) Overtime	(6) Hourly wage	(7) Working hours	(8) Overtime	(9) Hourly wage
Panel A: Pooled individual level estimates (standard TWFE)									
Treatment effect	-1.272***	-0.118*	-0.385***	-3.888***	-0.126	-0.0324	-0.517**	-0.121	-0.503***
	(0.235)	(0.0619)	(0.142)	(0.728)	(0.0844)	(0.460)	(0.247)	(0.0750)	(0.0939)
Panel B: Heterogeneity analyses									
B1. Heterogeneity by age									
Treatment effect	-0.348	-0.101	-0.359***	-2.351***	-0.0824	-0.187	0.246	-0.107	-0.423***
	(0.236)	(0.0674)	(0.124)	(0.694)	(0.0805)	(0.379)	(0.239)	(0.0805)	(0.113)
Treat × Over 50	-3.036***	-0.0484*	-0.201	-4.071***	-0.0896**	0.425	-2.623***	-0.0448	-0.442***
	(0.220)	(0.0290)	(0.178)	(0.496)	(0.0445)	(0.533)	(0.219)	(0.0332)	(0.110)
Treat × Under 30	2.747***	0.0136	0.615**	4.132***	-0.0377	-0.00365	2.238***	0.0276	0.811***
	(0.335)	(0.0533)	(0.241)	(0.919)	(0.0512)	(0.277)	(0.332)	(0.0647)	(0.291)
B2. Heterogeneity by gender									
Treatment effect	-1.377***	-0.136**	-0.420**	-3.811***	-0.105	0.0565	-0.626**	-0.155**	-0.638***
	(0.243)	(0.0631)	(0.168)	(0.724)	(0.0794)	(0.465)	(0.264)	(0.0769)	(0.124)
Treat × Man	0.631	0.0284	0.338	-0.362	-0.332***	-0.791*	0.521*	0.0942	0.648*
	(0.292)	(0.0808)	(0.295)	(0.765)	(0.102)	(0.428)	(0.314)	(0.0910)	(0.331)
B3. Heterogeneity by education level									
Treatment effect	-2.890***	-0.219***	-0.191	-4.089***	-0.178*	0.495	-2.480***	-0.349***	-0.660***
	(0.353)	(0.0805)	(0.394)	(0.657)	(0.108)	(0.817)	(0.427)	(0.108)	(0.185)
Treat × Secondary	1.710***	0.200***	-0.328	1.382**	0.0684	-0.574	1.866***	0.357***	-0.0910
	(0.338)	(0.0687)	(0.328)	(0.550)	(0.0714)	(0.700)	(0.384)	(0.0896)	(0.230)
Treat × Bachelor's	1.771***	-0.0209	-0.0325	-1.109	0.0608	-0.412	2.086***	0.117	0.401
	(0.397)	(0.107)	(0.379)	(1.187)	(0.0874)	(0.482)	(0.444)	(0.120)	(0.249)
Outcome mean	159.50	1.81	15.87	155.56	0.97	13.32	160.80	2.09	16.71
N	3,450,234	3,450,234	3,435,744	820,885	820,885	816,043	2,629,349	2,629,349	2,619,701

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in year -2.

benefit from the rule change. The results for females are understandable because they are more likely to work in service sector jobs than men. Ex ante, we also expected that the lowest educational category, those without a degree, would bear the observed cost. This is also what we observe when we look at the results presented in Panel B3.

Table M9 shows the results regarding other mechanisms. As shown in the previous section, Panel A of Table M9 further highlights with different measures, that on average, we observe a decline in annual earnings but simultaneously a decline in unemployment (measured in months or as long-term unemployment risk) and a higher probability of full employment (where the individual is employed for 12 months). These observations can coexist, as the previous table depicts the impact on working hours (a decline of 0.8

percent), overtime hours (a decline of 6.5 percent), and hourly wage (a decline of 2.4 percent).

To summarize, and as hinted at by the earlier occupation-region level analysis, we show that the negative earnings effect at the individual level is not explained by transitions to unemployment or worse occupations (column 9 of Table M9), but rather that individuals working in treated occupations earn less on average due to a decline in their capacity to earn more through extra hours. We also observe evidence of a negative effect on hourly wages, indicating that the salaries in treated occupations grow more modestly after the LMT exemption which was somewhat unclear in occupation-region level analysis. The reason for differing results for the top 3 quartiles may be that regions and occupation are obviously weighted very differently in the individual level analysis compared to the occupation-region level analysis where each occupation-region unit has the same weight. The observed negative effect in hourly wages (which was observed especially for workers in the top 3 salary quartiles) suggests that LMT exemptions might have had an impact on wage bargaining at the local level. The results in the Panel A of Table M9 also suggest that treated individuals are less likely to change firm or to move elsewhere. These observations, together with the positive employment effects, suggest that LMT policy may help firms to expand and this would not come at the cost of native employment.

In Panel B of Table M9, we turn our focus on possible heterogeneity. Results in Panel B.B1 show how above 50-year-olds fare worse compared to those aged 30 to 50 years. The older workers are hit harder than the middle-aged workers by most measures. Oldest group is also less likely to transit to other occupations, more likely to becaome pensioners and works less hours. The magnitude of the earnings effects by age look quite stark, and thus, it raises questions if different age groups are on different trends. Because of this, we estimate additional models in the Online Appendix M.3 where we include linear time trends for different groups, and separately for all workers, and workers in the lowest and top 3 quartiles. We show these results in Online Appendix Tables M12-M17. In those results, we still observe that old individuals drive the observed earnings effect, but the size of the effect becomes somewhat less pronounced. In addition, also some of the other estimates change somewhat, but it is not clear whether the models which include a large number of linear trends would be preferable compared to a standard specification, if we believe the parallel trends assumption would hold.

For the youngest group (those under 30), the results in column 7 indicate that the risk of leaving the labor force increases after the rule change. We observed similar indications for the lowest salary quartile in our occupation-region level analysis, as shown in Panel C of Figure A12 in the Online Appendix.

5.3 Firm level

This section presents firm level results on various outcome variables. Firm results are only estimated for period 2013–2019 as this is the period for which we have available data for each firm level outcome of interest. We aim to understand how firms react when less-educated immigration becomes less restricted in a sector of the economy in which they employ individuals. We use the matched specification here because the firms that hire more in treated occupation tend to be quite different from those that hire less in those occupations. As described at the end of the previous section, matching is conducted separately for each treated group (i.e., different "first event" years).

It is not clear which firms should be classified as treated because in principle any firm could respond to the change, for example, by setting up an establishment in a region where a particular occupation is exempted from labor market testing. Importantly, it is also very plausible that exempting one occupation may not affect a firm much, but instead what is important is how many exemptions there are in a region. However, there is no obvious way to study these impacts causally. Instead, we focus on a narrower approach, i.e., studying how firm outcomes are affected when a firm first faces an exemption. Thus, this analysis does not necessarily fully reflect how firms' outcomes are affected by LMT rules.

Figure 11 shows the effect on the absolute number of non-EU employees in the firm, indicating that the number of non-EU workers increases by 0.04 employees in years 3 and 4 after treatment. We do not observe significant pre-trends in Figure 11 which gives no reason to doubt the plausibility of the parallel trends assumption. Pooled estimates in Table J3 suggest somewhat lower impacts, which is due to the effect being zero in year 0 and 1 where the number of observations are larger. The effect is only visible in later years in the event study figure, which is consistent with our occupation-region level first-stage results, which also showed the effect on the stock of non-EU workers was not instant after the removal of labor market testing.

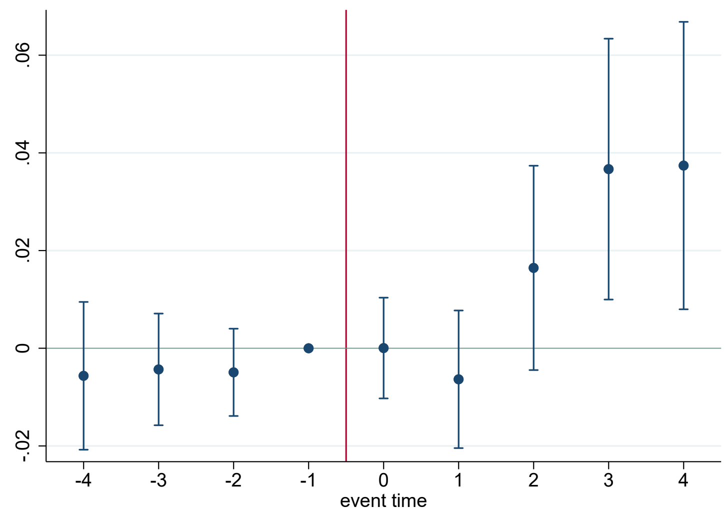


Figure 11: Effect on the number of non-EU workers employed by the firm

Notes. The figure shows the firm-level TWFE estimates where the outcome variable is the number of non-EU workers employed at the firm at the end of the year. Confidence intervals are 95% confidence intervals. Standard errors are clustered by firm. Year -1 is used as a reference period.

We also study other outcomes besides the stock of non-EU employees. In addition to basic variables such as firm size and turnover, we also investigate effects on profit share, investments, and labor productivity (value added per worker). Table J3 shows pooled difference-in-differences estimates on various outcomes. The key observation from Table J3 is that the treated firms seem to expand in terms of the full-time equivalent number of workers. Most of the increase would also seem to come from an increase in the number of native workers. It is, however, questionable whether the estimates regarding firm expansion can be interpreted as causal, as Online Appendix Figure J2 shows some evidence of pre-trends, especially for the number of native workers.

Regarding other firm-level outcomes, results in Table J3 indicate there may be a negative effect on investments (-€31,000) and a negative effect on labor productivity (value added per worker). We do not observe pre-trends for these outcomes in Online Appendix Figure J2, but one should still be cautious when interpreting these estimates. These firm-level estimates do not necessarily capture the whole effect of removing LMT on firms, as the sample is limited due to it being impossible to find controls for the larger firms and due to us being able to study only the first time a firm faces an occupation on the shortage list. It is possible that from the point of view of the firms, removal of LMT matters more when many occupations have been exempted instead of just one occupation being exempted.

Table 6: Pooled firm-level DiD estimates with coarsened exact matching

	Size and personnel, number of				Investments, €1,000				Other			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Non-EU	Workers FT equiv.	Native workers	EU	All	Buildings	Machines	IT	Labor share	Turnover, €1,000	Profit ratio	Labor productivity
Panel A: All matched firms												
Treatment effect	0.00133	0.275***	0.282***	0.00722	-31.64*	-28.64*	-2.599	-0.399	0.0250	-20.05	-0.214	-1475.3*
	(0.00634)	(0.0565)	(0.0587)	(0.00571)	(16.90)	(15.97)	(3.729)	(0.281)	(0.324)	(42.87)	(0.230)	(800.5)
Panel B: Heterogeneity analyses												
B1. Heterogeneity by firm size (baseline: firm size < 10)												
Treat × Post	-0.0148**	-0.121***	-0.0251	-0.000549	-34.75**	-28.42*	-5.908	-0.418	0.00121	-143.4***	-0.295	-1180.6
	(0.00601)	(0.0431)	(0.0468)	(0.00516)	(17.34)	(16.33)	(3.735)	(0.278)	(0.331)	(39.82)	(0.315)	(899.2)
Treat × Post × (≥ 10 employees)	0.0652***	1.598***	1.239***	0.0314**	12.58	-0.875	13.37**	0.0787	0.0973	498.3***	0.324	-1206.3
	(0.0159)	(0.178)	(0.178)	(0.0158)	(10.22)	(7.432)	(6.243)	(0.156)	(0.0850)	(74.62)	(0.346)	(1083.7)
B2. Heterogeneity by industry in terms of ranking (1.-20.) based on the number of foreign workers (baseline: ranks 15.-20.)												
Treat × Post	-0.0310***	0.0236	-0.00462	0.00201	-2.059	-39.21	38.14	-0.990*	0.149	51.48	0.0385	3515.1
	(0.00867)	(0.235)	(0.242)	(0.0143)	(44.05)	(26.37)	(36.62)	(0.548)	(0.389)	(93.58)	(0.0240)	(3468.7)
Treat × Post × 1-5	0.0791***	0.357	0.233	0.0153	-32.83	11.32	-44.69	0.533	-0.0757	-151.2*	0.000589	-8943.5**
	(0.0196)	(0.286)	(0.276)	(0.0172)	(39.67)	(18.68)	(36.56)	(0.489)	(0.121)	(90.22)	(0.0451)	(3532.4)
Treat × Post × 6-10	0.0243***	0.236	0.307	0.00328	-28.74	11.99	-41.37	0.648	-0.126	-46.38	0.0199	-5652.5
	(0.00928)	(0.241)	(0.249)	(0.0151)	(40.01)	(18.79)	(36.66)	(0.491)	(0.126)	(90.66)	(0.0402)	(3464.6)
Treat × Post × 11-15	0.0164	0.223	0.307	0.00199	-30.96	8.324	-39.87	0.578	-0.164	-65.90	-0.927	-1430.3
	(0.0115)	(0.268)	(0.267)	(0.0178)	(40.12)	(19.52)	(36.60)	(0.483)	(0.124)	(94.15)	(0.868)	(3703.7)
N	126077	126077	126077	126077	126077	126077	126077	126077	123758	126077	124609	124103
Outcome mean	0.3090909	25.78507	25.67418	0.3046061	191.3294	47.27018	135.6696	8.389582	0.7170176	7596.564	0.052119	68133.14
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes. The table shows difference-in-differences estimates. Standard errors clustered by firm in parentheses. Coarsened exact matching procedure does not find controls for larger (number of workers $\geq$ 50) firms and thus drops most of them. This is because most of the larger firms are treated at some point due to having establishments in many places, and because it is enough to employ 1 worker in a treated occupation in order to be treated. Significance levels: (*) 0.1 (**) 0.05 (***) 0.01.

6 Sectoral Collective Bargaining and Wage Drift

Finland has a collective bargaining system in which raises (proportional increases) to current wages are usually negotiated at the sector level, with some exceptions.[10] The Finnish sectoral bargaining system thus differs from many other European countries, in which the agreements are made on the sector wage floors (Adamopoulou and Villanueva, 2022; Bhuller et al., 2022; Card and Cardoso, 2022). In this section, we study whether there is a systematic difference in wage drift—the difference between the negotiated and the actual change in wages—by LMT exemption status. We aim to expand our understanding of the drivers of wage drift and whether immigration, which increases the occupation-region labor supply, could play a role. To our understanding, this is the first attempt to study the effect of immigration on sectoral bargaining dynamics.

We analyze whether LMT exemptions impacted the raises negotiated by unions at the occupation-region level and, more importantly, the wage drift. Our focus is on the latter since the wage drift can vary at the occupation-region level while the negotiated part is determined at the occupation level or higher (many occupations belong to the same contract). The collective bargaining raise works as a minimum raise for all, and the non-negative wage drift allows for individual variation.

In the differences-in-differences analysis, our outcome variables are the two components of the change in hourly wage, the collectively bargained raise and the wage drift, measured during a specific month of the year. The treatment is the occupation-region level LMT exemption. We focus our analysis on stayers—the workers who remain with the same firm and in the same occupation as in the previous year. We are not able to match all workers to an agreement, and thus, the sample of workers used in these analyses is somewhat different compared to the main sample used in the other analyses presented in this paper. Around 60% of workers in the Earnings Structure Survey are matched to one contract. The remaining workers are matched to several contracts or no contract at all. In our main sample, we include only workers who can be matched to one contract to minimize measurement error. We also show estimates for a secondary sample, which includes individuals for whom we find multiple contracts. In this case, we match the workers to the last contract matched to them. Contracts are allocated roughly in an alphabetic order, and thus, the order is almost as good as random. Moreover, the Earnings Structure Survey (which has information about the hourly wage) contains only around 70% of all workers in Finland, and these are private sector workers. In addition, as we

[10] Bhuller et al. (2022) categorize Finland in 2018 as having "some sectoral" bargaining. Our data is based on sectoral agreements; thus, our results focus on sectoral bargaining.

focus on stayers, only a subset of those individuals (65%) are included when calculating occupation-region level means in these analyses. At the individual level, our main sample would contain approximately $0.6 * 0.7 * 0.65 = 27\%$ of the total number of individuals. In our analyses, we aggregate these data to the occupation-region level, and the number of occupation-region level observations does not drop as much as the number of individual observations used in calculating the averages.

Table 7 shows our main estimates related to collective bargaining. We focus on year 4 effects, since the wage drift effects seem to take time to materialize. The main finding is that in the lowest salary quartile of occupations, there is a negative effect from LMT exemptions on wage drift, with a year 4 point estimate of -1.8% in our main sample (Panel A, not statistically significant) and -2.7% when we also include individuals, who match with multiple agreements (Panel B, statistically significant). Since the pre-treatment wage is 2.2% on average, being in an LMT exempted occupation-region seems to neutralize a large portion of wage drift. Variation in wage drift is a channel through which local labor markets can adapt to variation in labor market tightness, even in the presence of national sectoral collective bargaining agreements. The pace of adjustment is constrained by mean wage drift, which is 2.2% and 1.5% in the bottom and top three quartiles, respectively. In a setting where the wage floors are collectively bargained, Adamopoulou and Villanueva (2022) find that wages do adjust downward to a change in cycle and that downward rigidity is confined to wages close to the wage floor. Card and Cardoso (2022) additionally find that a change of cycle also affects real wage floors and reallocates workers to lower wage floors. Since we focus on stayers, we do not observe reallocation of workers. Also, the "shock" we study is not a change in cycle but a local shock through LMT exemption.

For the top three quartiles, we estimate a positive effect of 1.1% to 1.3%. It seems that those stayers for whom we observe the collective bargaining agreement actually benefit from the LMT exemptions. Wage drift dynamics under LMT exemptions seem to differ strongly at different ends of the wage distribution.

The estimated effects on collective bargaining agreements are statistically insignificant for the bottom quartile and small and positive for the top three quartiles.

Table 7: Effect on collective bargaining at the occupation-region level, unbalanced panel

	Wage drift, stayers		Collective bargaining raise, stayers	
	Bottom quartile	Top 3 quartiles	Bottom quartile	Top 3 quartiles
Panel A: Workers matched to single collective agreement				
Pooled estimate				
Treatment effect	-0.007031	0.002407	0.000253	0.000194
	(0.006604)	(0.001956)	(0.000399)	(0.000182)
Estimate for year 4+				
Treatment effect	-0.017928*	0.010800**	0.000317	0.000354*
	(0.010111)	(0.004213)	(0.000610)	(0.000205)
Outcome mean	0.022	0.015	0.012	0.012
N	4,012	19,948	4,012	19,948
Panel B: Workers matched to many agreements				
Pooled estimate				
Treatment effect	-0.009612	0.003726**	0.00003	0.00012
	(0.006551)	(0.001983)	(0.00036)	(0.00017)
Estimate for year 4+				
Treatment effect	-0.026679**	0.013044***	0.00035	0.00045**
	(0.010452)	(0.004218)	(0.00061)	(0.00021)
Outcome mean	0.022	0.015	0.012	0.012
N	4,306	20,869	4,306	20,869

Notes. Only stayers (workers who work in the same firm and occupation as the year before) are included when calculating occupation-region level outcomes for this analysis. Significance levels: * 0.1 ** 0.05 *** 0.01. See Online Appendix Figure N5 for event study estimates. In Panel B, all workers are included and matched to the last contract allocated to that person. The collective bargaining contracts are organized in alphabetic order. In Panel A, only workers associated with a single collective bargaining contract are included.

7 Taxes and Transfers

To shed light on the fiscal impact of removing labor market testing requirements, we estimate the causal effects of the exemptions on transfers received, taxes paid, and net transfers (transfers-taxes) both at the occupation-region level and at the individual level. The analysis is conducted for three different groups of workers: natives, non-EU workers, and EU/EEA workers. Transfers received include all transfers, including pensions, sickness and family benefits, rehabilitation benefits, child benefits, income support, housing allowance, study benefits, and unemployment benefits. Taxes paid include all taxes and tax-like payments paid for the central and local governments. Church tax is not included here as it is not considered a mandatory tax.

We subsequently provide a calculation of the total net transfers in a scenario in which all occupation-regions were to be exempted from LMT, assuming that our estimates would remain constant even with such a large expansion of exemptions. This is a relatively strong assumption as some of the effect we estimate on the inflow of non-EU workers is due to occupation switching as discussed earlier. However, if the policy was expanded to cover all occupations and regions, it is likely firms would expand the hiring of foreign employees directly from abroad in case there would not be enough occupation switchers willing to work for them.

Table 8 shows that natives receive more net transfers in the medium term, both at the occupation-region level (+€665.21 annually) and at the individual level (+€754.9 annually). This is driven by a negative effect on tax revenues from natives, consistent with negative earnings impacts observed in in this paper. The estimates shown in the table are pooled difference-in-difference estimates. They include all available years and thus are based on longer pre and post-periods than the estimates shown in our event study figures. Table P19 shows these estimates separately for the bottom and top three salary quartiles.

Taking the point estimates on the effect on net transfers at the individual level (+€754.9), the total amount of decreased revenues would amount to €1.79 billion during a year as there are 2.6 million employed workers in Finland of which 91% (2.37 million) are natives. This would likely be an upper bound for the potential negative effect of exempting all occupations in all regions due to the fact that the inflow of immigrants per occupation-region would likely be smaller if all occupation-regions were exempted.

As the non-EU workers come to a large extent from inside Finland, there is also a potential positive impact resulting from immigrants moving from non-employment to employment, or from part-time to full-time employment due to the LMT exemptions. Panel A of Figure 12 shows descriptively how the net transfers of immigrants already in Finland and those of new immigrants evolve around the year they start in the occupation. The figure shows that net transfers decrease by approximately €2,000 after the individuals start in the occupation for both new immigrants and those who already resided in Finland. In fact, the amount of net transfers is negative for new immigrants, i.e., they pay more taxes than they receive in transfers. This is at least partly due to them having less children as shown in the Panel B of Figure 12. Thus, if our estimates regarding the inflow of non-EU workers after LMT exemptions generalized to nationwide removal of LMT, we could expect that the increased inflow of immigrants would decrease net transfers by approximately $2,000 * 6,000 * 26 = 300$ million euros as there are around

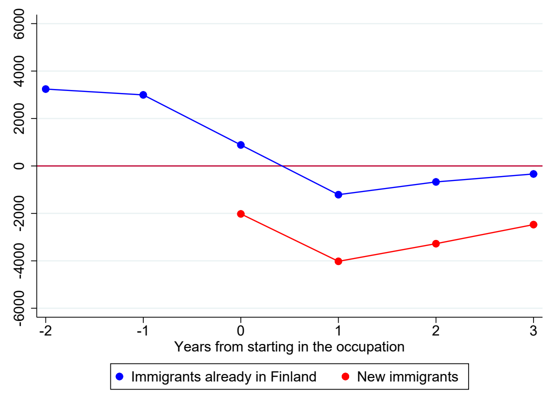
6,000 occupation-region units and the increase in the number of non-EU workers per occupation-region unit is 26 individuals in the medium term. Again, the assumption is that the increase would remain constant even with a nation-wide total abolishment of LMT.

This simple exercise, undeniably, has some significant limitations. First, it does not take into account indirect taxation, such as the value-added tax. Second, it excludes indirect fiscal effects that arise from general equilibrium effects that Colas and Sachs (2024) estimate to amount to a positive effect of 750 dollars per immigrant in the US. The indirect fiscal effect would outweigh the costs for low-skilled immigrants with a high school degree and reduce the fiscal burden for immigrants with no secondary degree in the US.

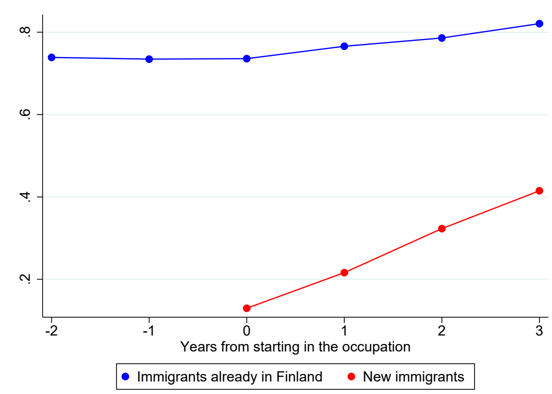
Table 8: Taxes, transfers, and the number of workers, pooled occupation-region level estimates

	Number of Workers			Taxes			Transfers			Net transfers		
	Native	Non-EU	EU	Native	Non-EU	EU	Native	Non-EU	EU	Native	Non-EU	EU
	Occupation-region level estimates											
Panel A: Pooled estimate												
Treatment effect	-15.56	15.39**	8.66*	-78.56	51.18	234.02	-13.45	-34.21	75.85	65.11	-85.39	-158.17
	(20.65)	(6.89)	(5.05)	(220.63)	(244.22)	(215.70)	(37.46)	(144.62)	(149.39)	(227.01)	(265.12)	(261.39)
Panel B: Medium term (year +5) estimate												
Treatment effect	13.31***	26.08**	12.15	-646.83***	12.22	-24.98	28.38	88.97	146.85	665.21***	76.75	171.84
	(4.14)	(11.70)	(7.09)	(139.09)	(338.95)	(338.82)	(75.80)	(201.56)	(280.16)	(161.18)	(410.50)	(465.43)
	Individual level estimates											
Panel C: Pooled estimate												
Treatment effect	—	—	—	-389.4***	-372.2**	143.8	-130.7**	198.8	30.43	258.7***	571.0*	-113.4
				(72.82)	(180.8)	(195.9)	(57.58)	(178.9)	(125.5)	(98.67)	(302.6)	(291.9)
Panel D: Medium term (year +5) estimate												
Treatment effect	—	—	—	-972.8***	-460.9*	-165.2	-117.5**	31.55	-416.0*	754.9***	840.3**	119.0
				(139.4)	(239.3)	(445.5)	(52.77)	(214.7)	(234.7)	(165.7)	(383.0)	(548.2)

Notes. The table shows occupation-region level Callaway & Sant'Anna ATT estimates and individual level TWFE estimates with standard errors in parentheses. Significance levels: (*) 0.1, (**) 0.05, (***) 0.01.



(a) Net transfers (transfers-taxes)



(b) Average number of children

Figure 12: Average net transfers and number of children when non-EU immigrants start in a new occupation

8 Robustness and Validity

In this section, we discuss the validity of our results, and results from robustness checks we have conducted. We conduct several tests for robustness to assess concerns related to our research setup and method. We also discuss heterogeneous treatment effects by treated cohort and by other dimensions.

Spillovers

In principle, the LMT exemptions introduced in specific occupations could have spillover effects to occupations that are included in our control group. However, there are hundreds of occupations and 15 regions in total, while the yearly changes in treatment are small. For example, if 10 more foreign workers changed their occupation to work in a specific, exempted occupation-region unit, they would come from around 5,000 different never-treated occupation-region units, and thus, on average a unit in the control group would only lose 0.002 individuals. Thus, we argue that there is no reason to suspect that this would invalidate our research setting. However, we conduct a validity check, described below, regarding this issue.

SUTVA check regarding the occupation switching of immigrants

As we observe that the inflow of immigrant workers is partly due to immigrant workers switching occupations, i.e., them moving from control occupation-regions to treated occupation-regions, it is possible that some of the positive effect on the inflow of foreign workers would be due to a decrease in the number of foreign workers in control occupation-region units. As a validity check, we assess this by limiting the control group to occupation-region units that have no immigrant workers. Occupation-regions in this alternative control group are not contaminated by immigrants' occupation switching as there are no immigrants working in these occupation-region units. Our results are qualitatively the same if all of the potentially contaminated occupation-region units are removed from the control group. The magnitude of the estimates is also very similar. These results for our main occupation-region level outcomes are presented in Online Appendix A Figures A31 and A32. Estimates where instead the control group is limited to those occupations where the immigrant share is less than 1 percent are shown in Online Appendix A Figures A33 and A34.

Choice of method and specification at the occupation-region level analyses

Our occupation-region level results are robust to using other event study methods instead of the Callaway and Sant'Anna (2021) method used in the analyses shown in the main text. Results with other estimators are shown in Online Appendix G. Regarding options chosen when estimating csDiD estimates, our occupation-region level results are, in practice, identical if we use the not-yet-treated option in Callaway and Sant'Anna (2021) method instead of using only never-treated units in the control group. The main estimates on the inflow of non-EU workers and earnings, where not-yet-treated units are included, are presented in Appendix D.

Dropping seasonal worker occupations from the occupation-region level analyses

The occupation-region level results are robust to dropping seasonal worker occupations defined either as i) those occupation-regions where an average worker has less than 6 employment months per year or ii) those occupation-regions where no worker has 12 employment months. As Lapland has lots of seasonal workers, we also show robustness to dropping the region of Lapland altogether. All of these results are shown in Online Appendix A.10.

Placebo treatment

We use a placebo treatment timing test for some of our main outcome variables in the occupation-region level analyses. An alternative treatment timing does not show significant effects. See Online Appendix F for these results.

Matching procedure in the firm-level analyses

The use of coarsened exact matching means that we cannot include too many matching variables. This is because the method aims to find controls with almost exactly the same values for each matching variable, and thus, only a few of the most important variables are often included when CEM is used. Otherwise, the method would not find suitable controls. In our main analysis, we only match on the number full-time workers and the number of non-EU workers, but we test robustness to matching exactly on the 1-digit industry classification. In these robustness analyses, presented in Online Appendix J, the main results of firm analyses stay qualitatively similar. As can be seen from Table J5, adding this one additional matching variable drops the number of observations considerably. If we were to add more variables (turnover, number of establishments, profits, investment, and taxes, as demonstrated in Table J6), the number of observations would drop so much that such a specification is not sensible to use.

9 Discussion: Implications for Policy

Our results show a sharp division in the occupation-region level effects of the removal of labor immigration restrictions. In the bottom salary quartile, there is a medium-term 10.7% drop in earnings from the pre-treatment mean relative to the control group. However, the earnings effect of the treated individuals is smaller (-6.1%). In the top three quartiles, the impact on average earnings at the occupation-region level is negligible, yet at the individual level, we observe a decrease of 3.6%. We do not find a positive earnings effect in any earnings bracket, unlike, e.g., Foged and Peri (2016a) in Denmark, Beerli et al. (2021) in Switzerland and East et al. (2023) in the U.S.. Our findings are more in line with the findings from European studies, e.g., Dustmann et al. (2017), Bratsberg and Raaum (2012), and Kuosmanen and Meriläinen (2022). Small effects for the highest earnings quartile are expected since the LMT exemptions have a insignificant effect on the number of non-EU workers compared to that in the bottom quartile of occupations. In the second quartile, and to some extent in the third quartile, however, we do observe some increases in the number of immigrants albeit they are smaller than in the the bottom quartile.

In terms of welfare-maximizing policy design, the top three quartiles pose a puzzle. The effects of lifting immigration restriction could be seen as mostly beneficial, since we observe no occupation-region earnings penalty for natives, or costly, since the natives' individual earnings trajectories in the medium-run fall behind unaffected peers by 3.6%. For the bottom quartile, these results paint a more concerning picture of work-based immigration, which is probably the core reason for the existence of the LMT policy. Earnings in the treated occupation-regions and of pre-existing workers fall steeply when the restriction is lifted. The effect is large enough to potentially make the total effect on net transfers negative for the public sector. This cost comes in addition to the welfare cost of lower earnings for the incumbents and the potential social cost of increased inequality. The trade-off is that the immigrants themselves are likely to benefit markedly, and the possible support for the lifting of LMT for the bottom quartile depends on the relative social weight given to the new entrants relative to incumbents.

The estimated effects of LMT could potentially depend on the ability of policy makers to target the tightest labor markets. We find little evidence of effective targeting of exemptions, as we observe no significant pre-trends in earnings or the V/U-ratio in the targeted occupation-regions versus controls. This lack of targeting also allows us to identify the effects of the policy, which would have been impossible if the exemptions had been targeted to tightening labor markets. If the goal of the policy is to alleviate labor shortages, we recommend policy makers to rethink the targeting of the LMT policy to more accurately target occupation-region units with tightening labor markets, as originally intended. If the ability to target tightening labor markets in the bottom quartile cannot be improved, explicit rules for minimum earnings as a condition for work permits could be considered as a less bureaucratic tool than and equally blunt tool as LMT.

Our research setting exploits exogenous variation in the number of workers in a specific occupation in a region, which also allows us to draw more general conclusions about the nature of labor markets at different points of the income distribution. For the following discussion, we assume that all the observed occupation-region level earnings effects arise from the relative change in the number of workers, that is, we assume that labor market testing exemptions do not affect the occupation-region level earnings directly or through other channels. In our setting, this seems to be a relatively plausible assumption. If the change in wage rate is seen as moving along the labor demand curve as a response to an exogenous shift in the labor supply curve, our causal estimates could be used to calculate an implied *elasticity of labor demand*, meaning the proportional change in labor demand relative to a proportional change in the wage rate, at different points of the income distribution. The elasticity of labor demand is inversely related to the slope of the labor demand curve, meaning that more elastic demand would mean a flatter labor demand curve and, thus, smaller earnings effects.

Our results demonstrate that the shock of removing labor market testing requirements led to a decrease of 10.7 % in the earnings of bottom quartile employees by the fifth year after treatment while increasing their employment by approximately 8% in years 3-5 (based on the statistically insignificant point estimates in event study shown in Appendix A, Figure A6). The log specification used in Figure A6 drops units with no employment in some years. These estimates would imply a medium-run labor demand elasticity of $8\%/-10.7\% = -0.75$. Since we do not observe any earnings effects for high-earnings occupations, it would suggest an infinite labor demand elasticity (i.e., a flat labor demand curve) for these groups. For all occupations combined, we estimate a wage effect of around -€500 (2 %) and an effect of 7% on the number of employees (again using a log specification), suggesting an elasticity of $7\%/-2\% = -3.5$. This elasticity estimate is in the same region as Borjas (2003), who estimate a labor demand elasticity of -2.5 for all occupations using variation in the number of immigrants (see Rothstein (2010)). These calculations do not take into account the possibility of shifts in the labor demand curve (i.e., general equilibrium effects). Thus, these calculations merely indicate that our findings would be consistent with labor demand elasticities of those sizes, assuming no general equilibrium effects and all wage effects coming from the exogenous change in the number of workers.

The estimated labor demand elasticities of -0.64 for the bottom quartile and infinite for the rest are meaningful for optimal transfer policies. Rothstein (2010) discusses the relative merits of earned income tax credit (EITC) vs. Negative Income Tax (NIT) type policies. The first type increases low-income labor supply, while the latter type discourages low-income work. Rothstein (2010) shows that NIT can be an effective way to improve the well-being of low-income individuals, assuming an inelastic labor demand. Our results give a more nuanced view of labor demand elasticity at the bottom of the income distribution compared to the rest of the labor market.

The literature on the wage effects of immigration often also estimates the *wage elasticity of immigration* in order to put the observed wage effects into context, that is, to assess how large the percentage change in wages is relative to the percentage change in the immigrant stock. When assessing this elasticity in our case one needs to take into account that the stock of non-EU immigrants in most occupation-region units is very small prior to the rule change, and thus, the effects of the LMT removal on the stock of immigrants are large relative to pre-treatment means. For example, the outcome mean for the treatment group one year before the event is 16 immigrants, and the pooled effect (+20 immigrants) observed in year 5 is thus a 125 % increase in the stock of immigrants, even though the increase would not be very large relative to the whole worker population. If we use this pre-treatment mean, it would imply a wage elasticity of immigration of around $-2\%/125\% = -0.02$. However, a log-specification–which drops all occupation-regions with zero immigrants (of which there are many) in any of the years and is, thus, more of an intensive margin estimate–suggests a smaller relative increase (20%) in the stock of immigrants (see Online Appendix D, Figure D1, Panel B). This would imply a wage elasticity of immigration of $-2\%/20\% = -0.1$. Our calculations, thus, imply an elasticity in the range from -0.02 to -0.1. This range is roughly in line with the picture given by estimates in the previous literature such as Bratsberg and Raaum (2012) (-0.06), Borjas (2013) (-0.13), and Edo and Rapoport (2019) (between -0.02 and -0.1).

Finally, when generalizing our results, one has to be cautious. First, Finland is a small country with a relatively homogeneous population and a small number of immigrants. Our results may not generalize to countries that are very different from Finland. Another limitation of our paper is that the evaluated policy changes, i.e., regional changes in labor market testing rules, are particular, and thus, the effects could very well be different in other contexts. Moreover, we are using recent data and evaluate the short-term to medium-term effects on wages and employment. The long-run effects of these policies are left for future research. Finally, additional avenues for future research could include the general equilibrium effects of lifting immigration restrictions.

10 Conclusion

This article finds that the removal of labor market testing– a policy whose purpose is to strike a balance between safeguarding native workers in relatively low-skilled occupations and addressing firms' labor demands from the flow of immigrant labor–has an adverse effect on earnings, especially for workers who are older and work in low-paying, service-oriented occupations. The adverse wage effect is partly driven by reduced working hours, although there is also some evidence of a negative impact on hourly wages. In addition, for incumbent native workers in the upper tail of the occupational salary distribution, we observed a positive employment effect.

Our findings on earnings are consistent with earlier European research on low-skilled labor immigration, while studies in the U.S. context have shown more mixed results. We find no evidence that movements into higher-paid occupations play a significant role. Previous research has suggested that such upward career mobility could explain why modest wage effects are observed in the U.S. for less educated native workers (e.g., Foged and Peri (2016b)). However, as we do not observe evidence of upward mobility along career ladders, relaxed immigration policies may impose a fiscal burden on the public sector.

Importantly, this paper's findings highlight trade-offs that warrant consideration when implementing more liberal policies for less-skilled labor immigration. As countries develop new legal pathways for low-skilled labor migration, caution and informed discourse are critical. This paper demonstrates that relaxed labor immigration policies can promote firm growth and may have modest effects on wages and employment in the upper segments of the salary distribution, while potentially imposing larger adverse effects on workers in the lowest segments of the salary distribution if they don't have pathways to upskill.

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Regulating Labor Immigration: The Effects of Lifting Labor Market Testing

Supplementary Appendix for Online Publication

Contents

A Online Appendix: Additional analyses at the occupation-region level

A.1 Decomposition of the inflow effect

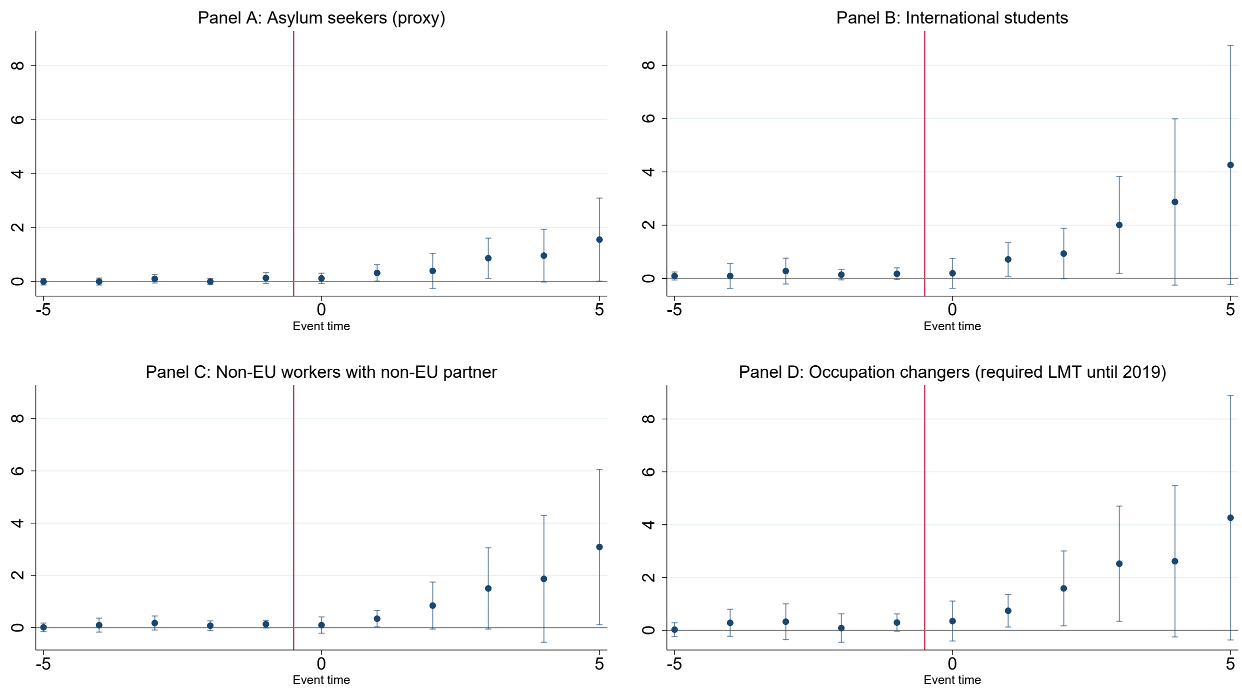


Figure A1: Decomposition of the effect on the inflow of foreign workers

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the inflow of non-EU workers belonging to different groups. Groups are not mutually exclusive. We decompose the effect in Panel B of Figure 4 to different subgroups. We estimate effects for all pre and post years but only estimates in window [-5,5] are plotted in the figure.

A.2 Occupation-region level V/U, V & U by quartile

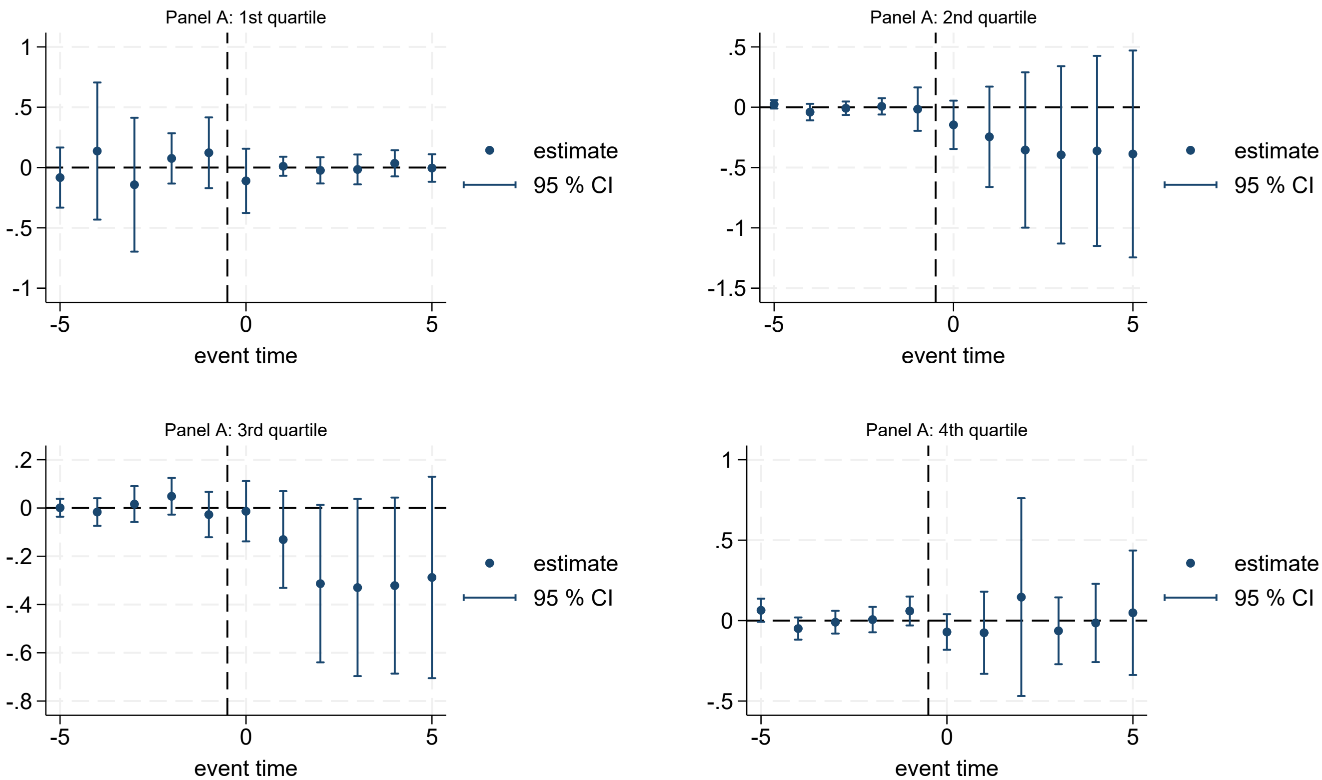


Figure A2: Occupation-region level V/U by quartile

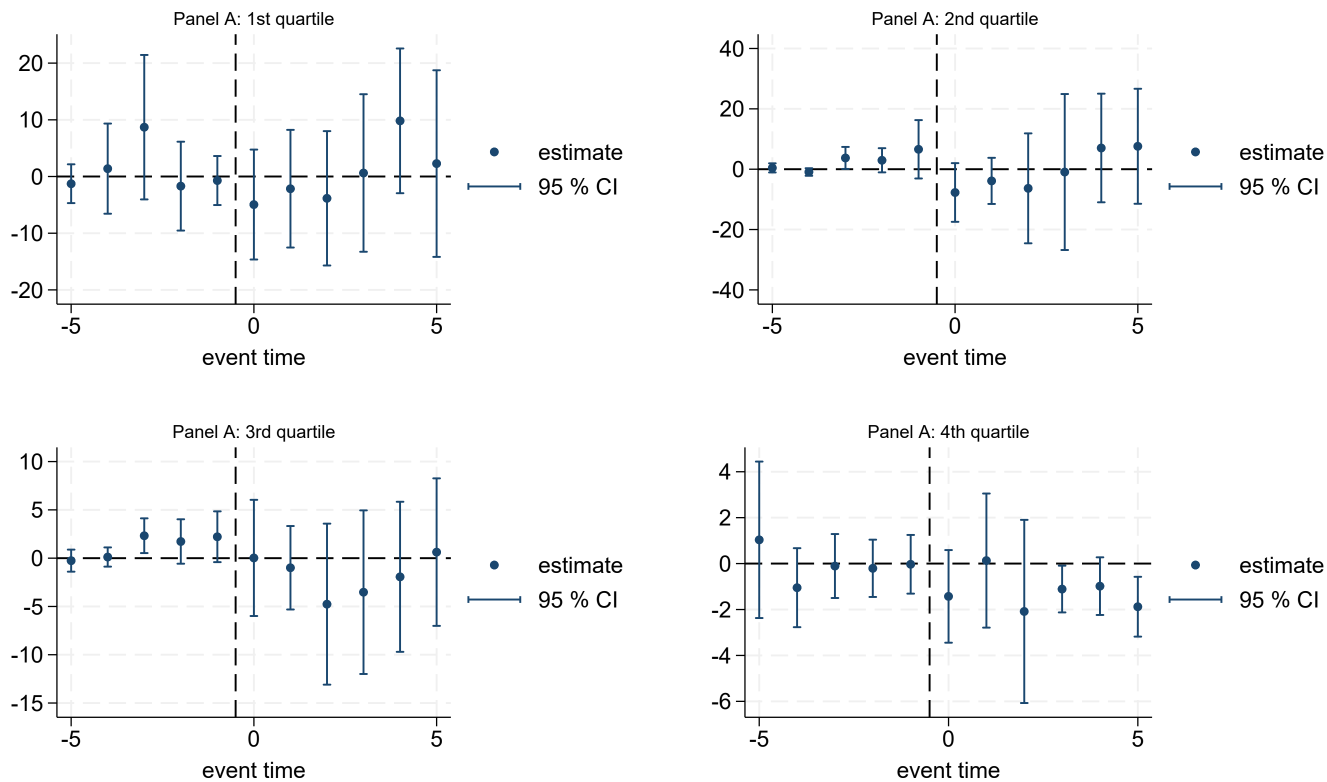


Figure A3: Occupation-region level V by quartile

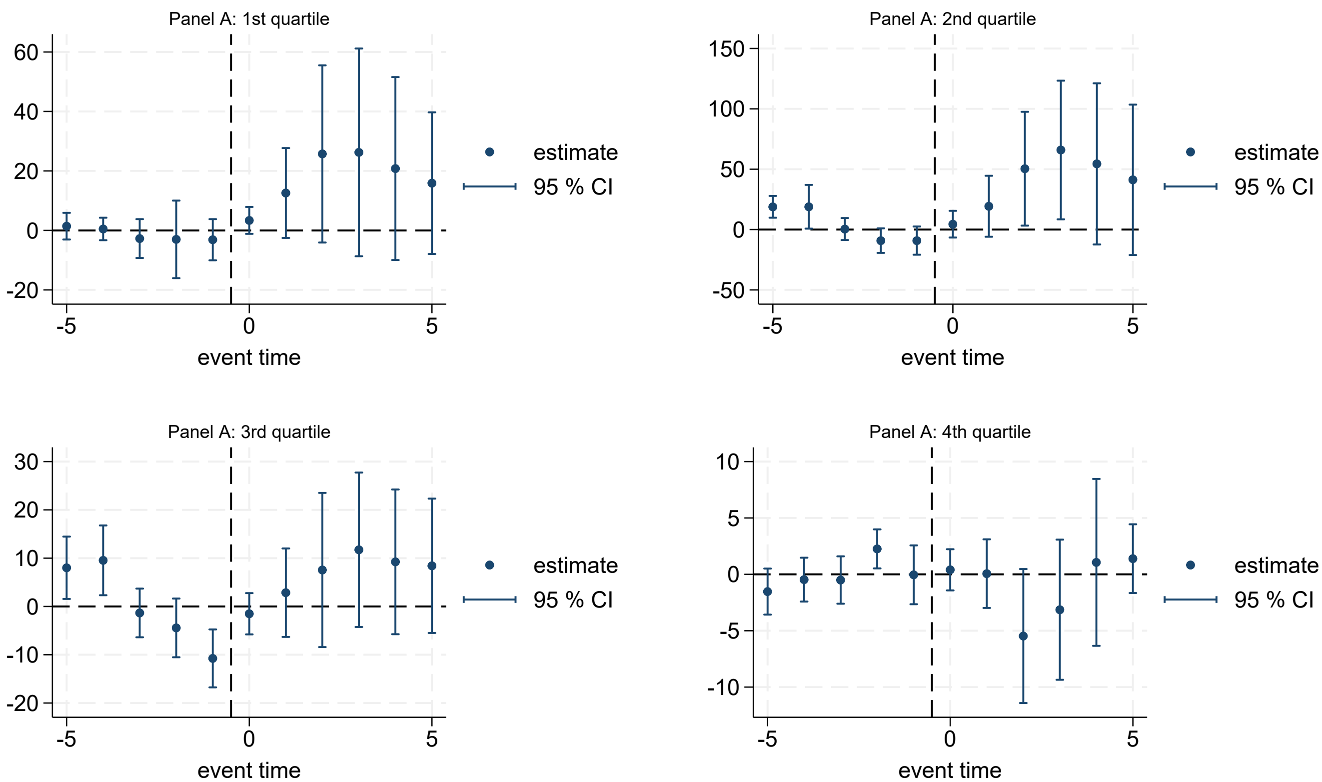


Figure A4: Occupation-region level U by quartile

A.3 Effect on the earnings of new workers

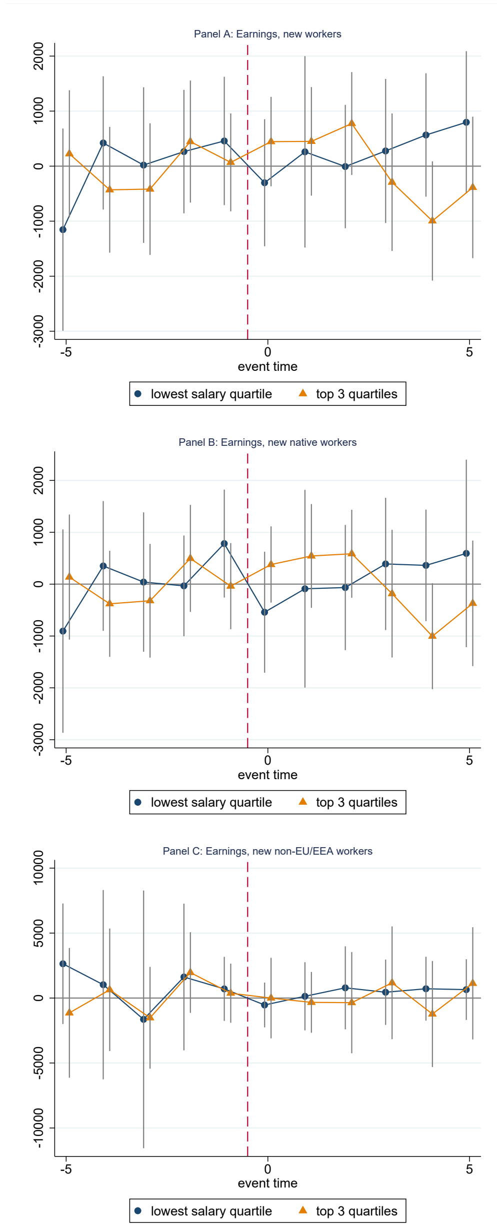


Figure A5: Effects on annual earnings, new workers

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is annual earnings. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

A.4 Effect on log(number of employed workers)

As the objective of lifting labor market testing requirements is to ease labor market shortages, it is interesting to test whether the policy has any effect on this. Although we do not presently estimate the effects of labor shortages, we can estimate the effects on the total number of workers employed in the treated occupation-region. Figure A6 presents results where the outcome variable is the logarithm of all workers in an occupation-region. It can be seen from the figure that lifting labor market testing requirements leads to a 5% increase in the overall stock of employed workers during the first 5 post-treatment years.

Figure A6: Log(number of workers)

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the number of all workers. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

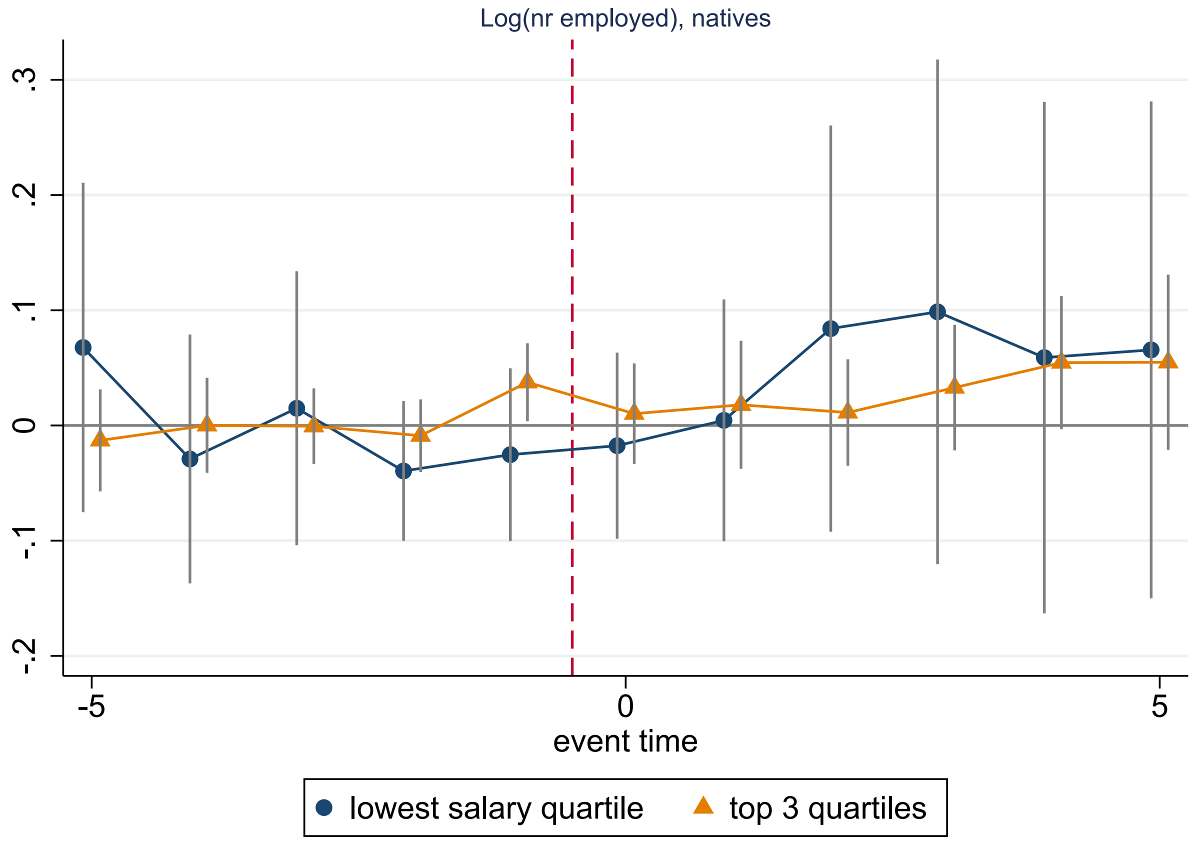


Figure A7: Log(number of native workers)

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the number of all workers. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

A.5 Working hours and overtime

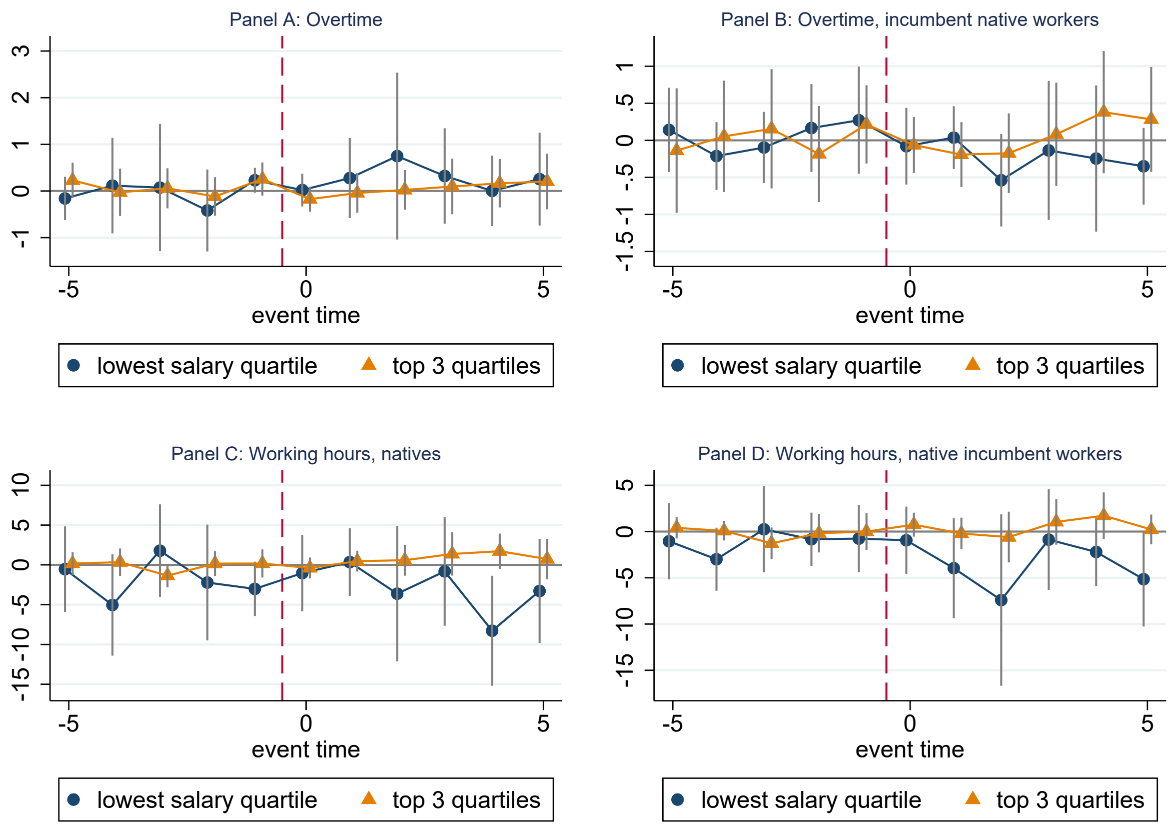


Figure A8: Effects on overtime and total working hours

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is either overtime working hours (upper row) or total working hours (lower row) for native workers. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

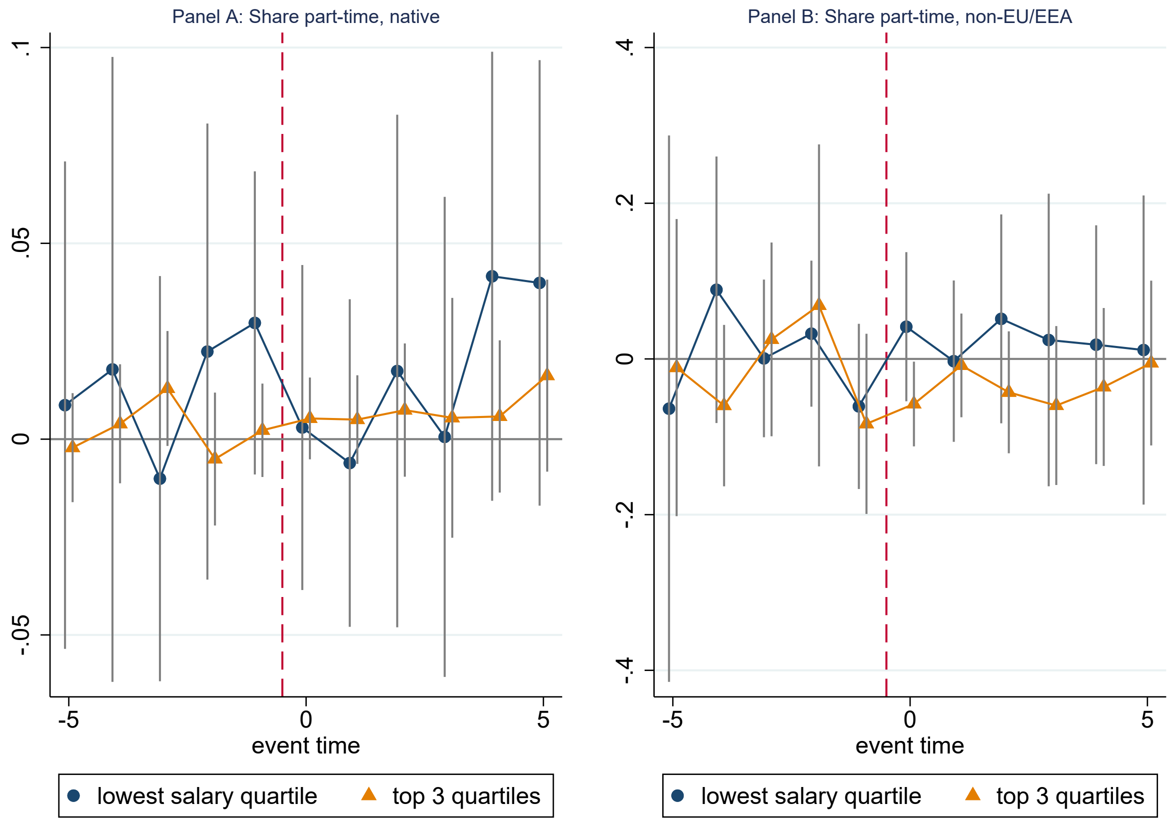


Figure A9: Effects on the share of part-time workers for natives and non-EU workers

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is either the number or share of part-time workers in the occupation-region. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

A.6 Outflows

Figure A10 shows the effects of lifting labor market testing requirements on the outflow of workers from treated professions. Figure A11 shows that most of the increase in the outflow to other professions comes from native workers moving to professions with higher average salaries than in their previous profession. Figure A12 shows outflows to education, unemployment, outside of the labor force, and pension.

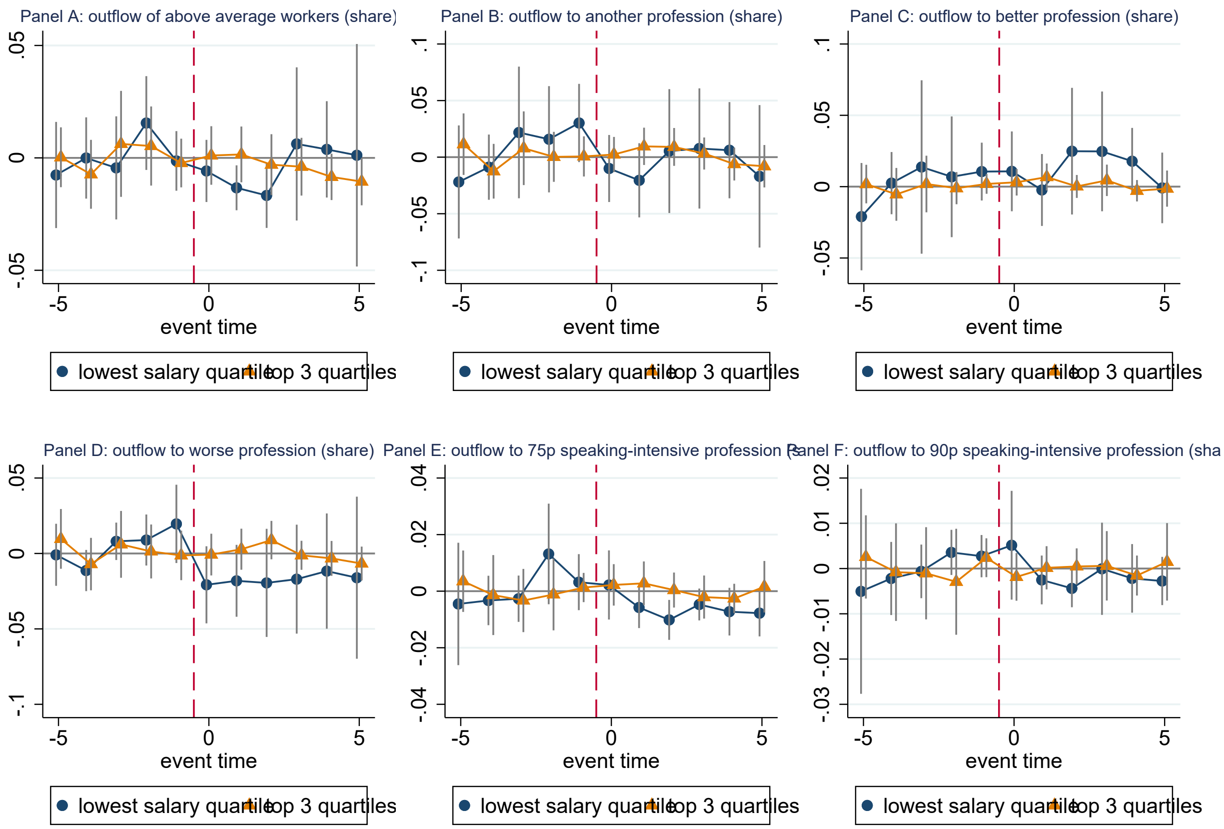


Figure A10: Effects on outflow to other professions

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the number of workers who worked in a occupation-region during the previous year (t-1) but changed profession in year t. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

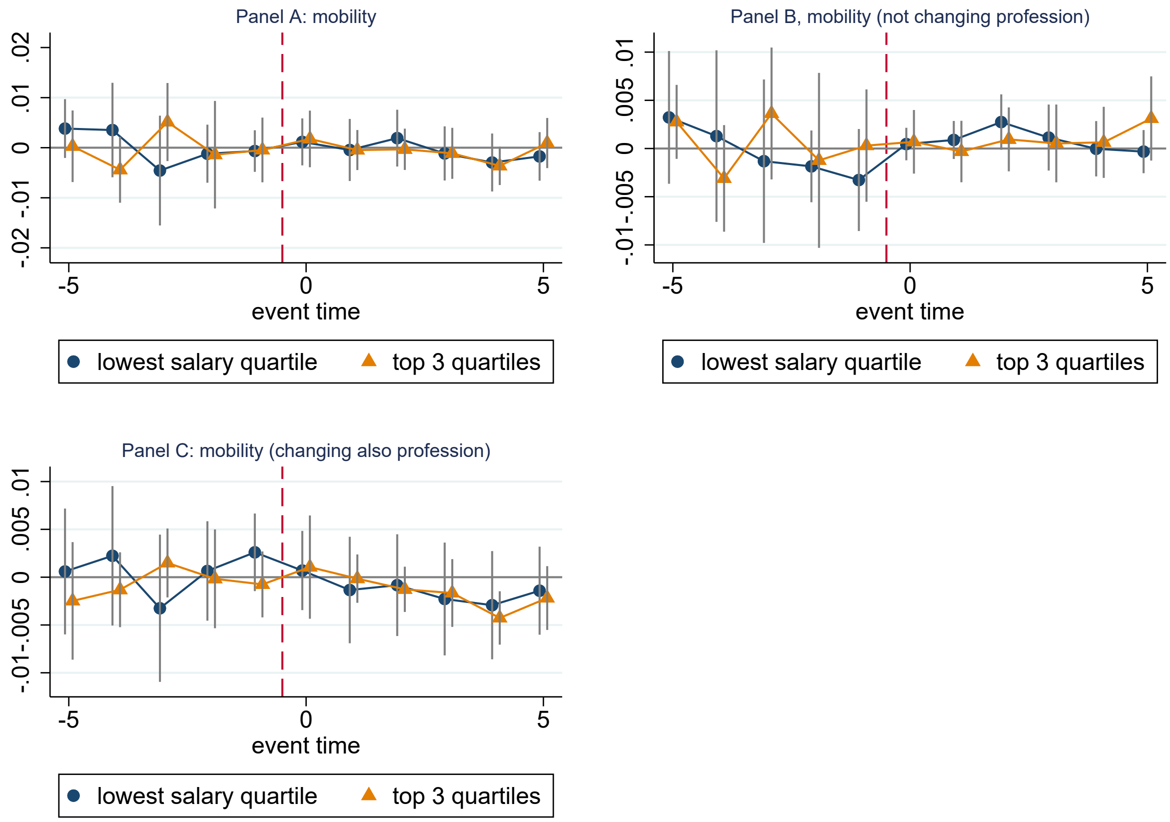


Figure A11: Mobility

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the number of workers who worked in a occupation-region during the previous year (t-1) but change profession in year t. The control group includes only never-treated units. Varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

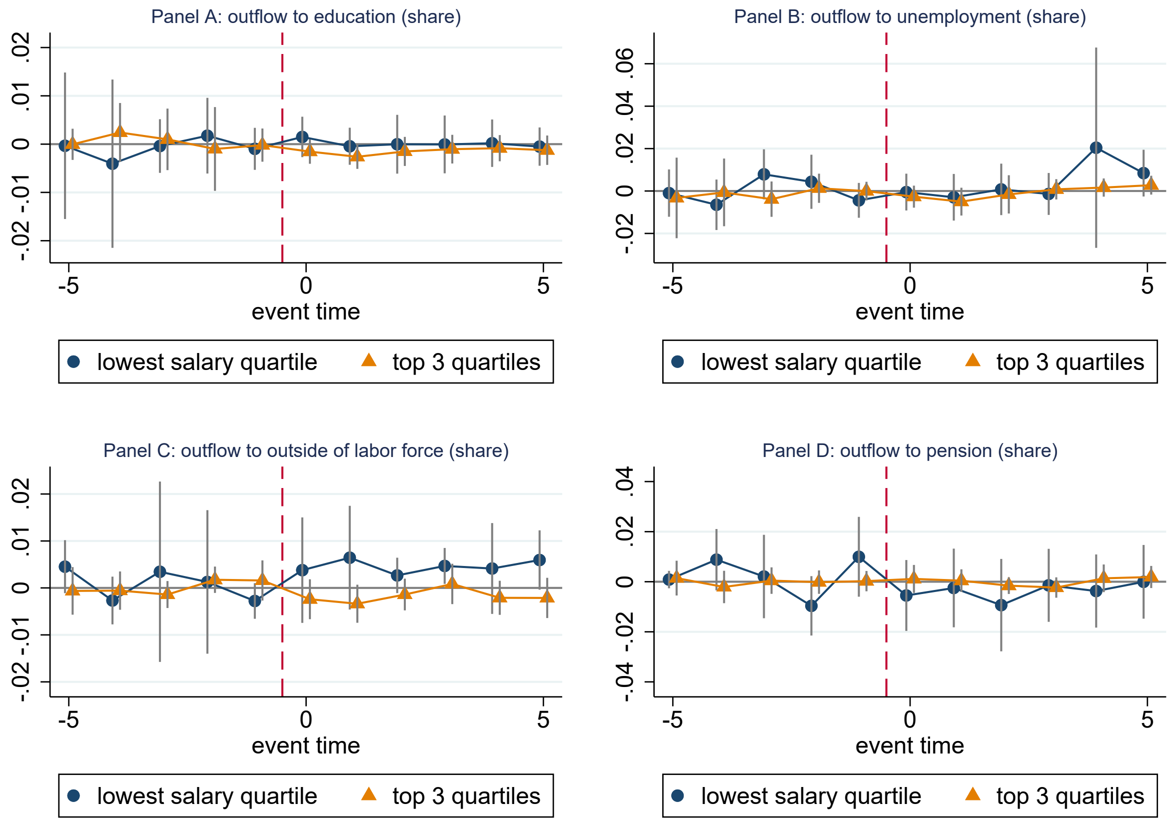


Figure A12: Effects on other outflows

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is the number of workers who worked in a occupation-region during the previous year (t-1) but change profession in year t. The control group includes only never-treated units. Varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

A.7 Heterogeneity by public/private sector and gender

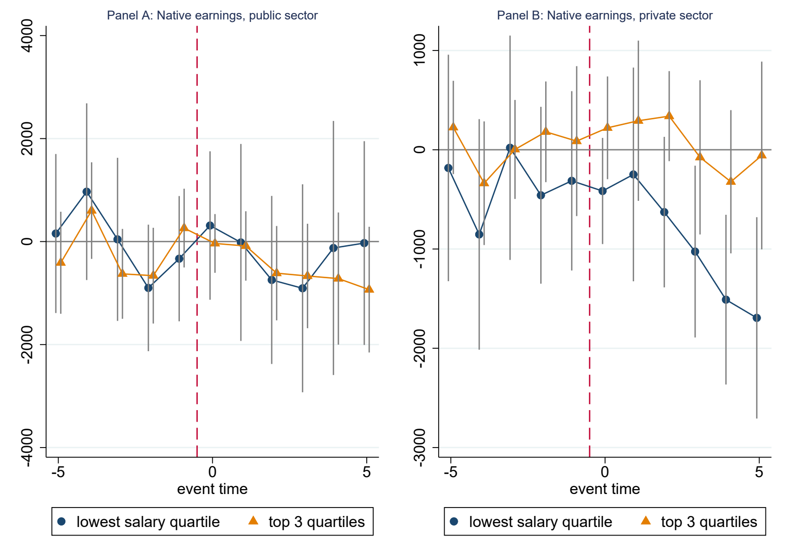


Figure A13: Heterogeneity of the earnings effect, public vs. private sector jobs

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variables is annual native earnings in public or private sector jobs.

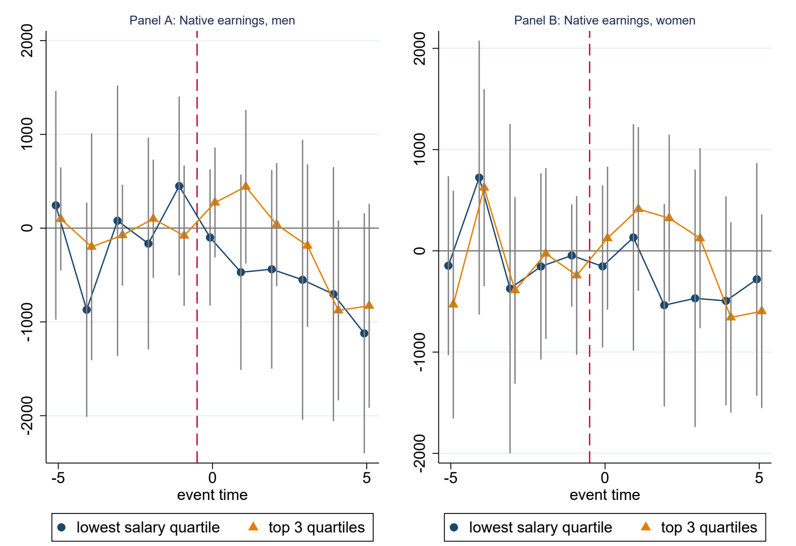


Figure A14: Heterogeneity of the earnings effect by gender

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is annual native earnings by gender.

A.8 Effect on annual earnings: heterogeneity by profession group (1-digit level)

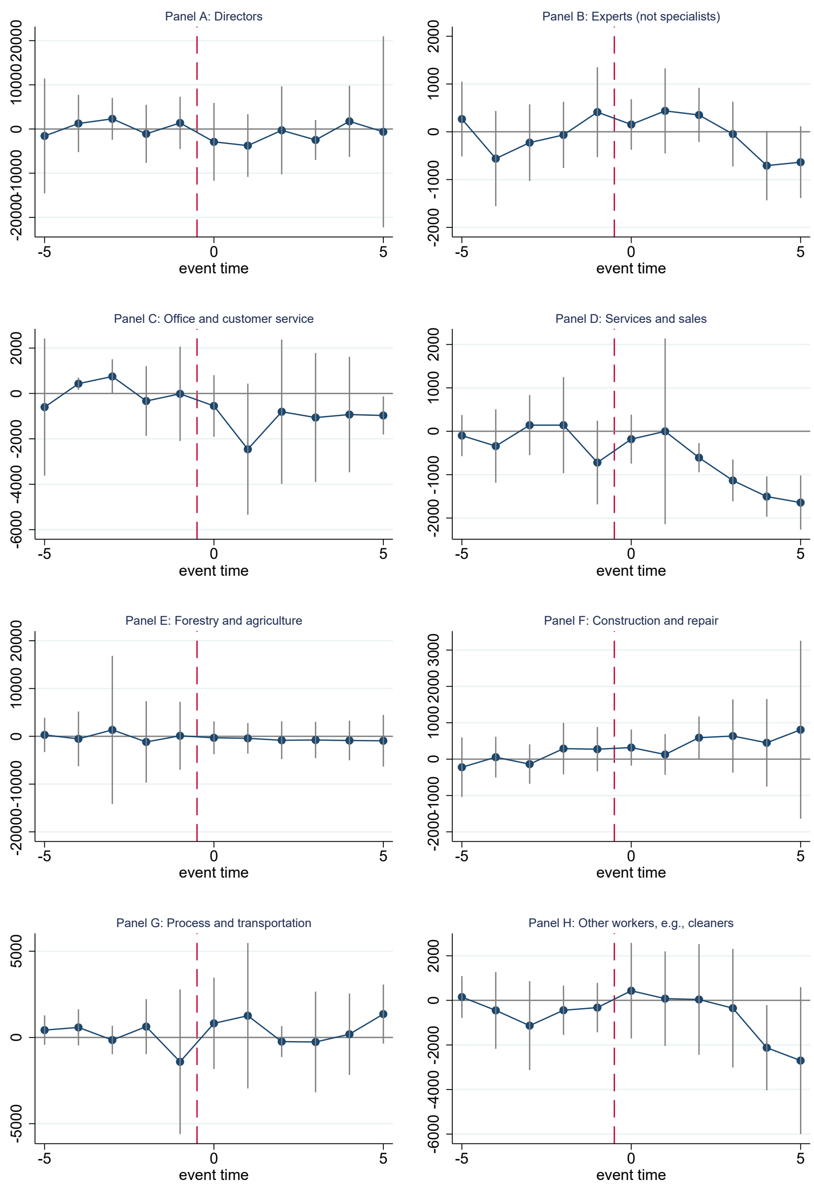


Figure A15: Effect on native earnings, heterogeneity by profession group

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is earnings separately for different profession groups. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

A.9 Heterogeneity between cities and countryside

Stock of foreign workers

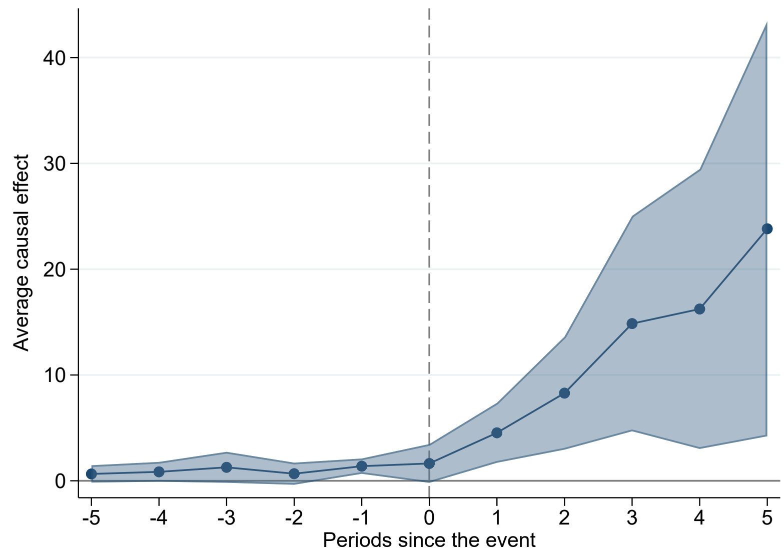


Figure A16: Stock of foreign workers in cities

Notes. The figure shows the estimate of the effect on the stock of foreign workers in cities.

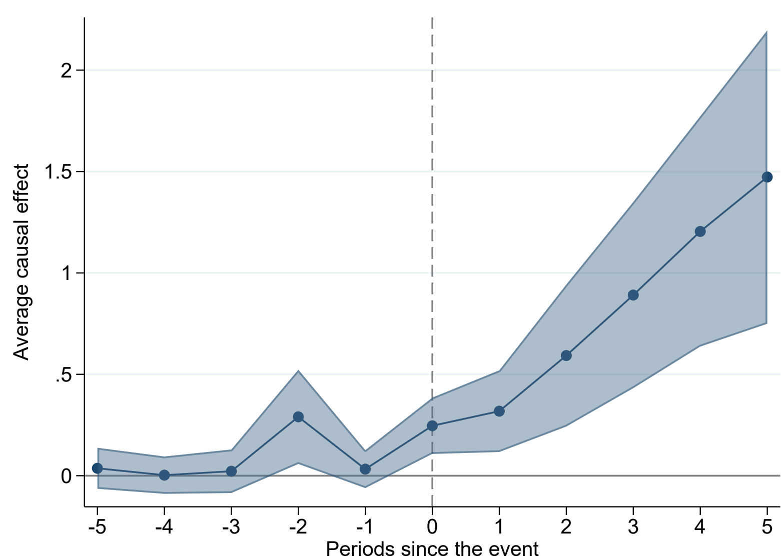


Figure A17: Stock of foreign workers in non-city urban areas

Notes. The figure shows the estimate of the effect on the stock of foreign workers in non-city urban areas.

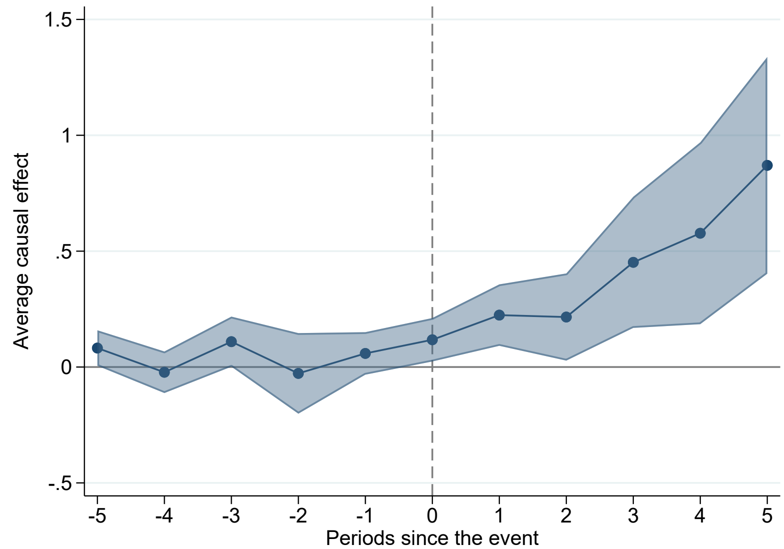


Figure A18: Stock of foreign workers in rural areas

Notes. The figure shows the estimate of the effect on the stock of foreign workers in rural municipalities (30% of Finnish municipalities).

Earnings effect

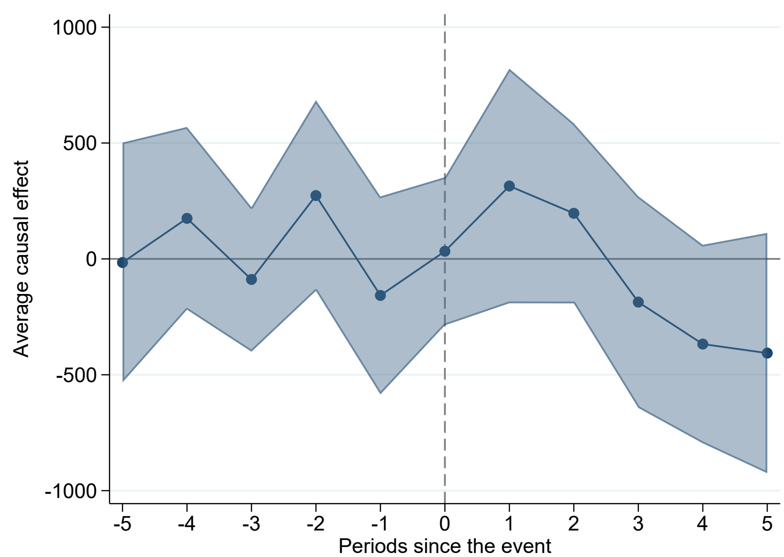


Figure A19: Earnings effect in cities

Notes. The figure shows the estimate of the earnings effect in cities.

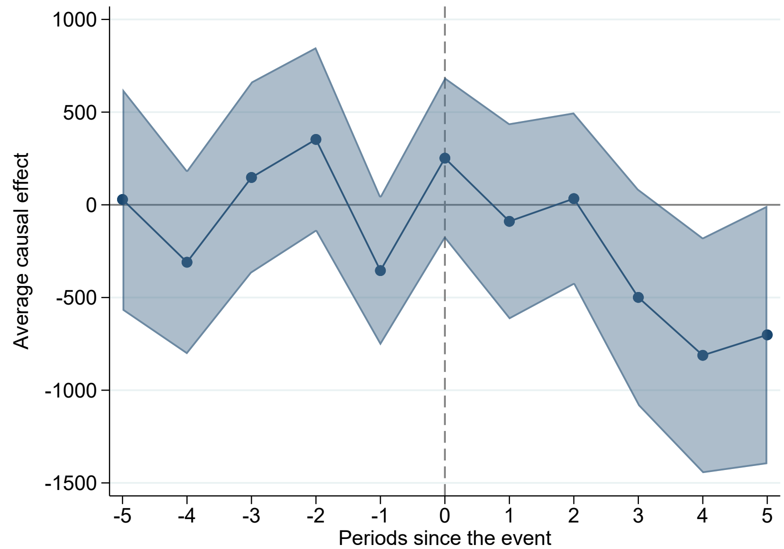


Figure A20: Earnings effect in non-city urban areas

Notes. The figure shows the estimate of the earnings effect in non-city urban areas.

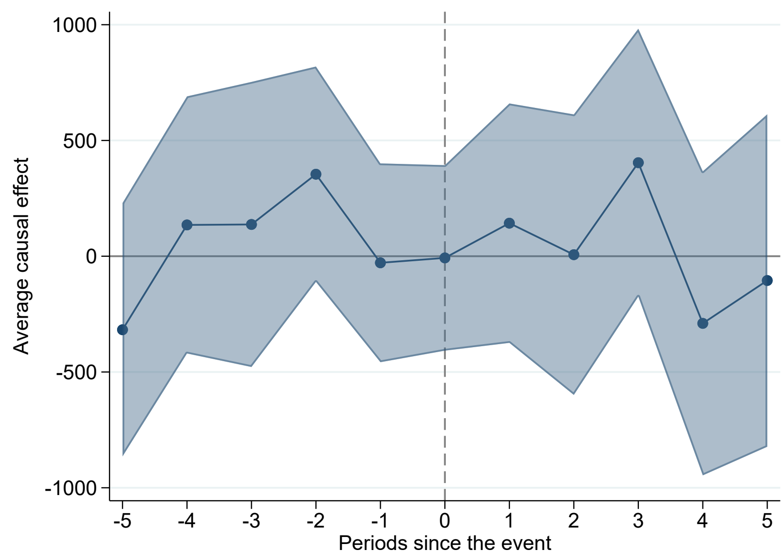


Figure A21: Earnings effect in rural areas

Notes. The figure shows the estimate of the earnings effect in rural municipalities (30% of Finnish municipalities).

A.10 Robustness to dropping Lapland and seasonal worker occupations

Stock of foreign workers

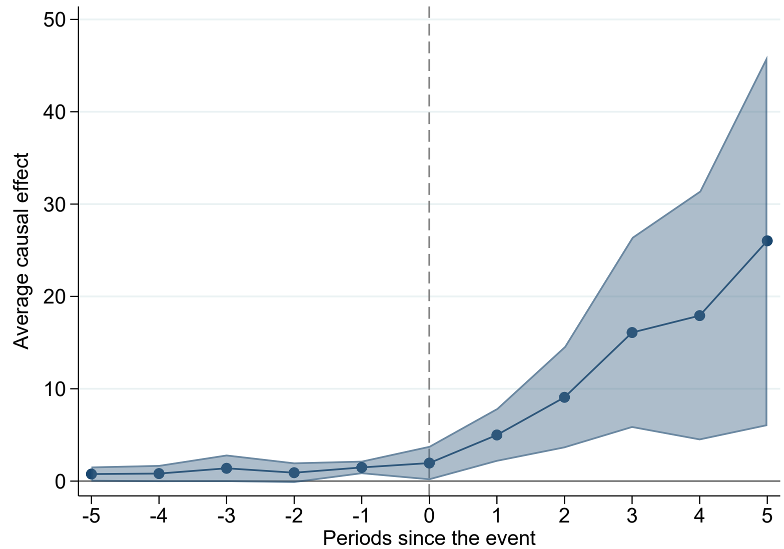


Figure A22: Stock of foreign workers, seasonal workers dropped (definition 1)

Notes. The figure shows the estimate of the effect on the stock of foreign workers when seasonal workers are dropped.

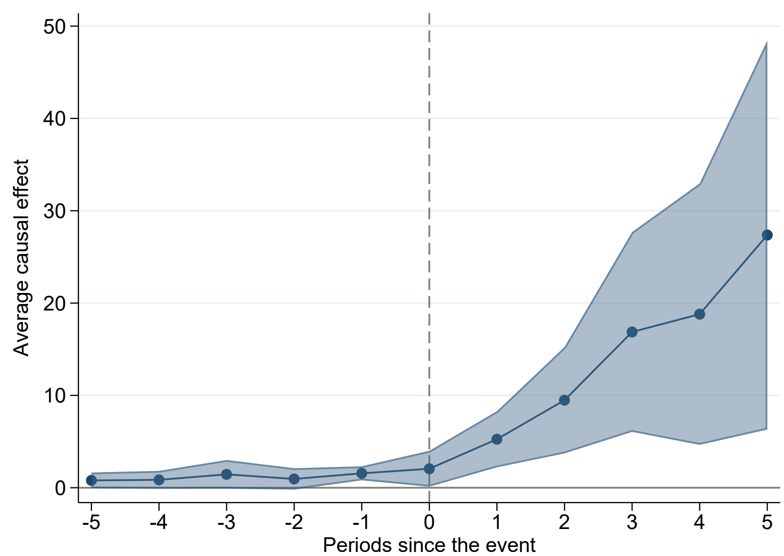


Figure A23: Stock of foreign workers, seasonal workers dropped (definition 2)

Notes. The figure shows the estimate of the effect on the stock of foreign workers when seasonal workers are dropped.

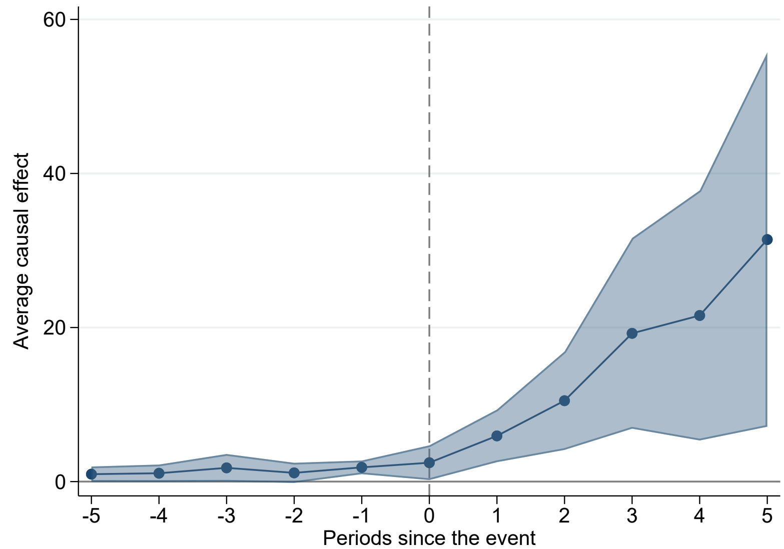


Figure A24: Stock of foreign workers, without Lapland

Notes. The figure shows the estimate of the effect on the stock of foreign workers when the region of Lapland is dropped.

Earnings effect

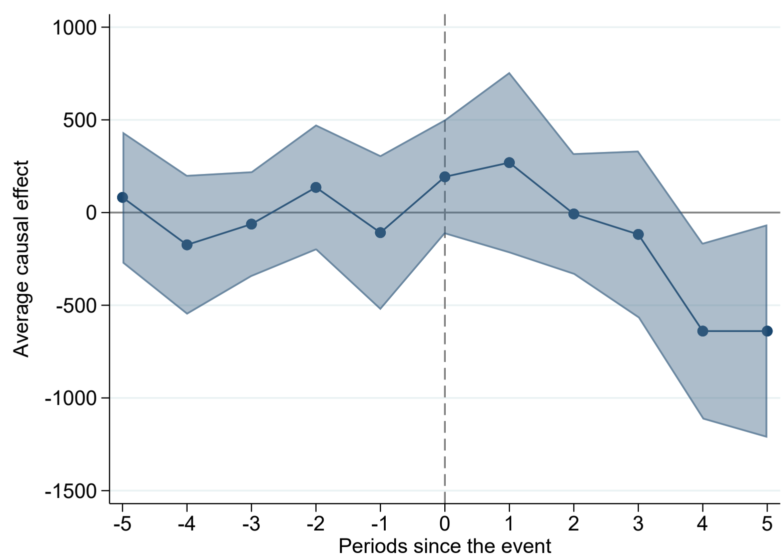


Figure A25: Earnings effect, seasonal workers dropped (definition 1)

Notes. The figure shows the estimate of the earnings effect when seasonal workers are dropped.

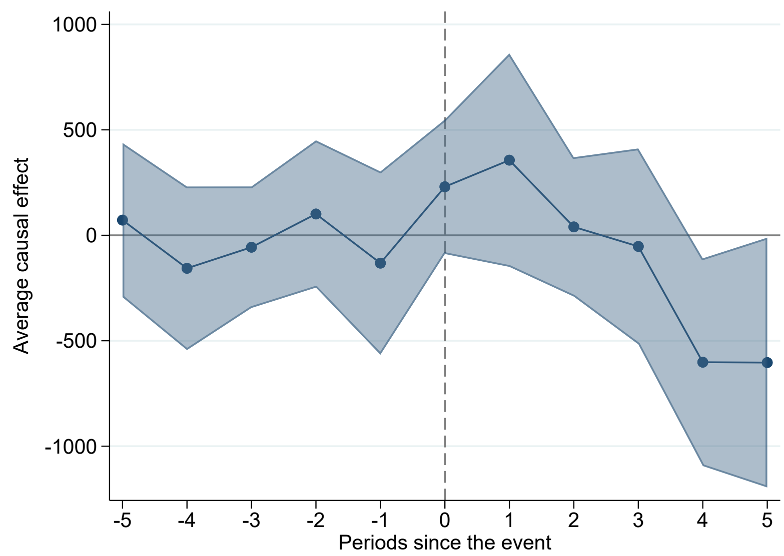


Figure A26: Earnings effect, seasonal workers dropped (definition 2)

Notes. The figure shows the estimate of the earnings effect when seasonal workers are dropped.

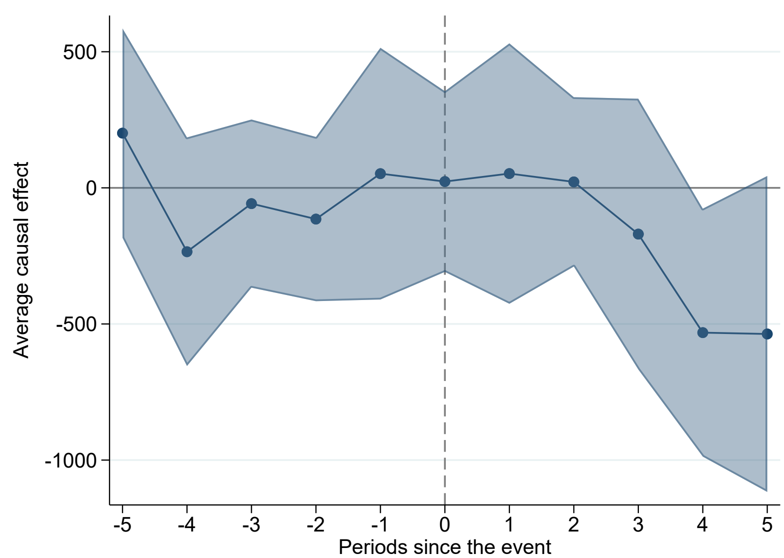


Figure A27: Earnings effect, without Lapland

Notes. The figure shows the estimate of the earnings effect when the region of Lapland is dropped.

A.11 Decomposing the inflow effect into components based on previous employment: those coming from other occupations and those entering from non-employment

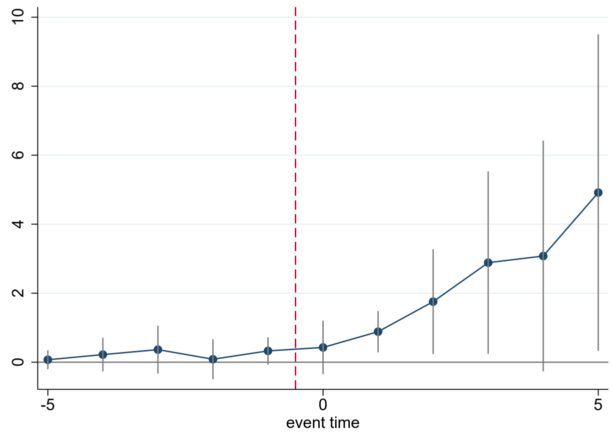


Figure A28: Inflow of non-EU workers who already worked in some occupation during the previous year

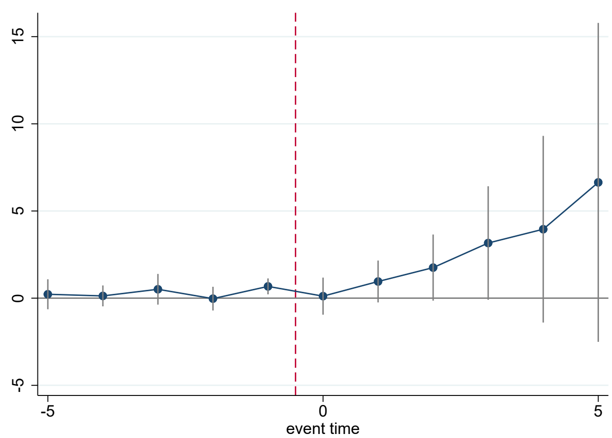


Figure A29: Inflow of non-EU workers who did not work in any occupation during the previous year

A.12 Heterogeneity by establishment size (effects on occupation-region level averages in specific types of establishments)

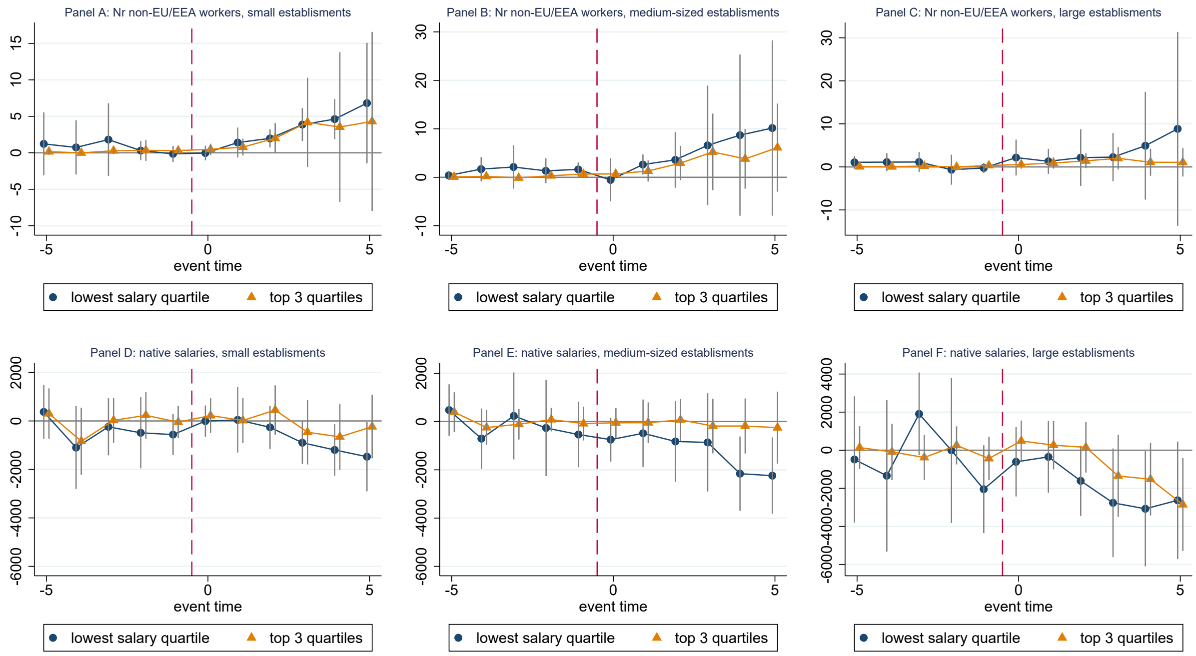


Figure A30: Effect on the earnings of native workers by the size of firm establishment

Notes. The figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variables are the stock of foreign workers to small, medium-sized, and large establishments and the native earnings by the establishment size. The control group includes only never-treated units. A varying base period (the default option) is used. Confidence intervals are 95% confidence intervals. Standard errors are clustered by ID.

A.13 SUTVA check regarding occupation switching of immigrants

Limiting the control group to only include occupations without any immigrant workers

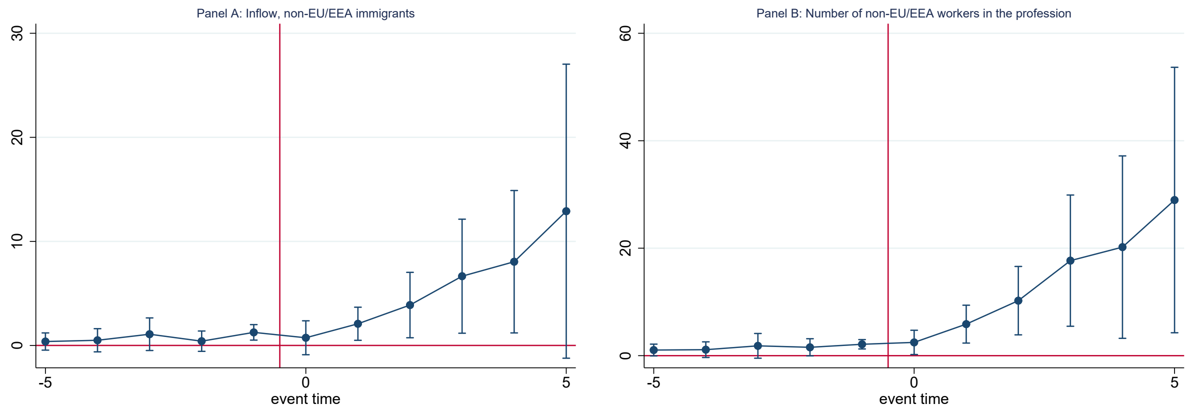


Figure A31: Effect on the inflow and stock of non-EU workers, including only occupations without immigrants in the control group

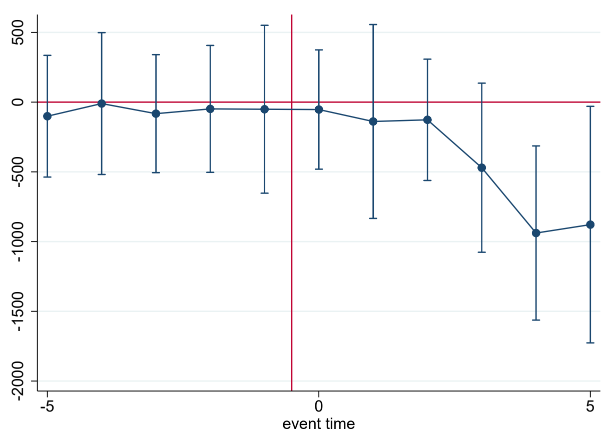


Figure A32: Effect on the annual earnings of natives, including only occupations without immigrants in the control group

Limiting the control group to only include occupations where the share of immigrant is less than 1 %

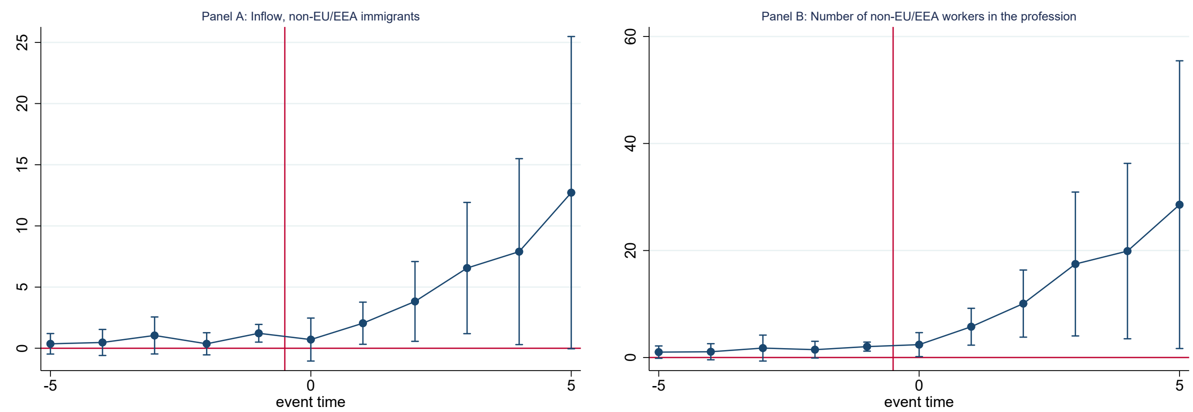


Figure A33: Effect on the inflow and stock of non-EU workers, including only occupations with immigrant share less than 1 % in the control group

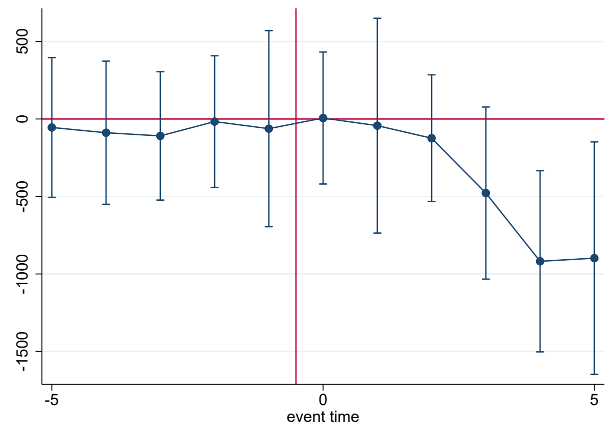


Figure A34: Effect on the annual earnings of natives, including only occupations with immigrant share less than 1 % in the control group

B Online Appendix: Expansion of treatment, 2012-2021

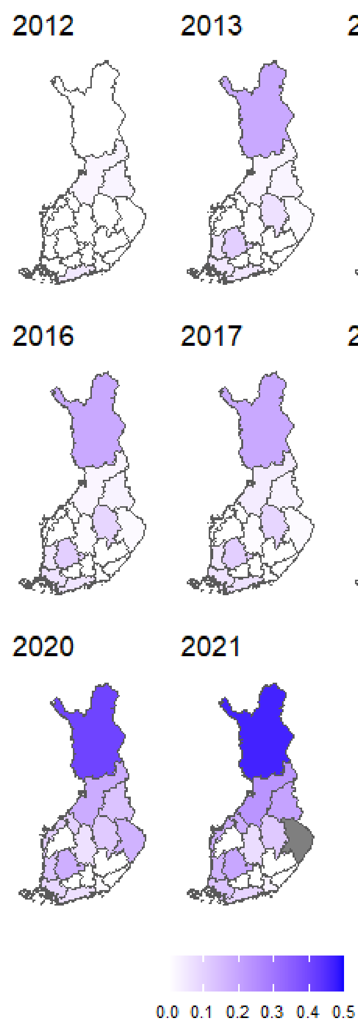


Figure B1: Staggered treatment: share occupations that are treated, 2012-2021

Notes. The figure shows the share of occupations in each region that have been exempted from the labor market testing requirement. In 2021, the region of Pohjois-Karjala (colored with gray in 2021 figure) abolished labor market testing for all professions. Figure produced by the authors in R. Source of map data: National Land Survey of Finland (Maanmittauslaitos).

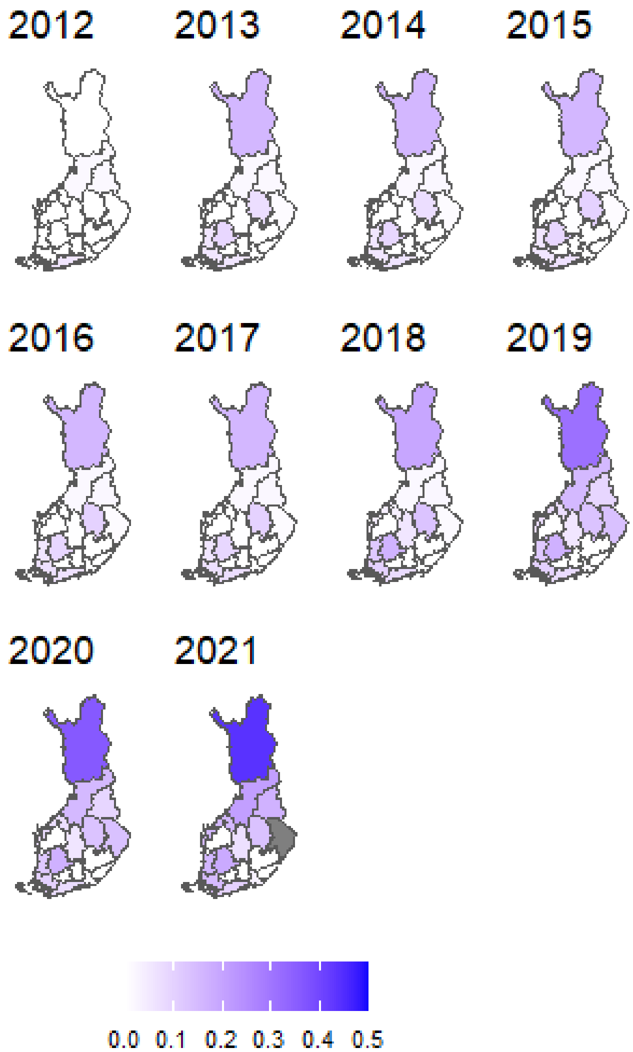


Figure B2: Staggered treatment: share of non-specialist occupations that are treated, 2012-2021

Notes. The figure shows the share of non-specialist occupations in each region that have been exempted from the labor market testing requirement. In 2021, the region of Pohjois-Karjala (colored with gray in the 2021 figure) abolished labor market testing for all professions. Figure produced by the authors in R. Source of map data: National Land Survey of Finland (Maanmittauslaitos).

C Online Appendix: Main estimates by group/treatment cohort

When Callaway and Sant'Anna (2021) method is used, treatment effects are calculated separately for each group, where one group consists of units that are treated at the same time. While we present the aggregated event study estimates in the main text, here we show treatment effects separately for each group.

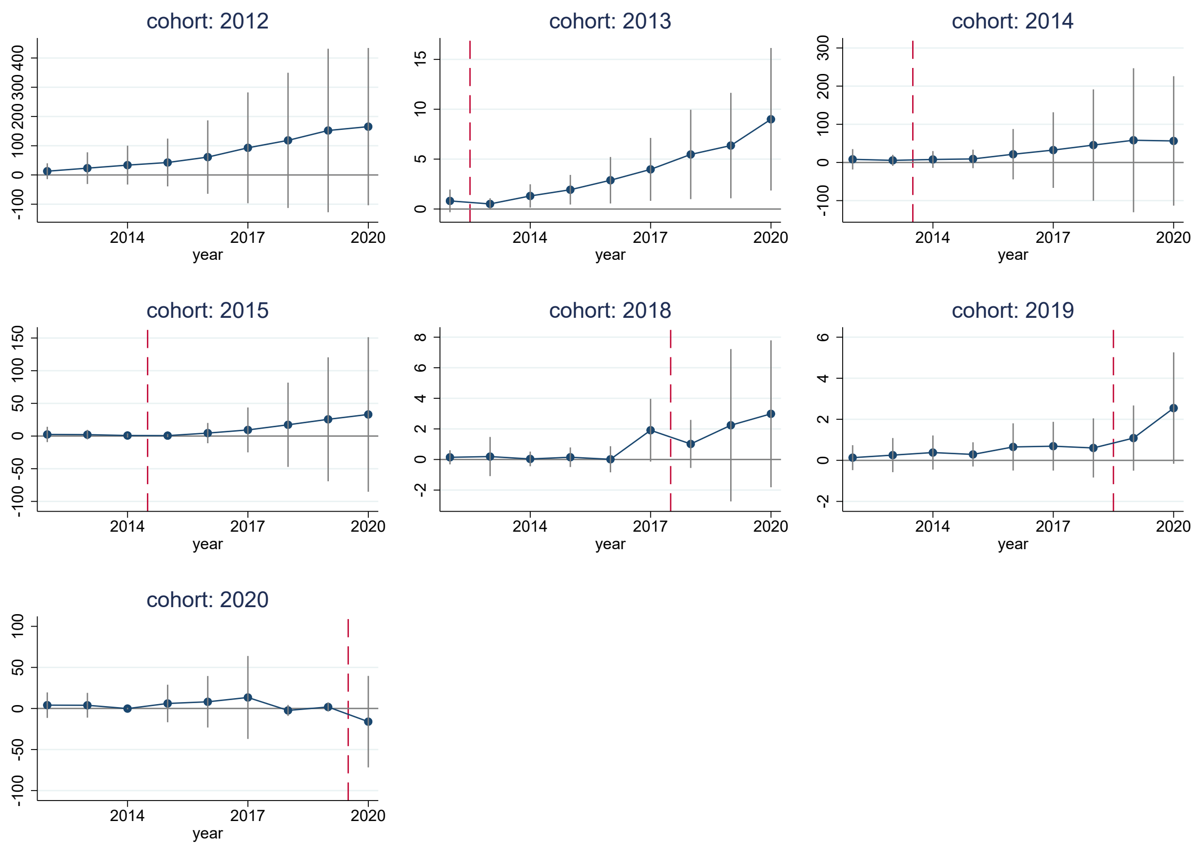


Figure C1: Number of non-EU workers

Notes. The figure shows event study plots for each treated cohort.

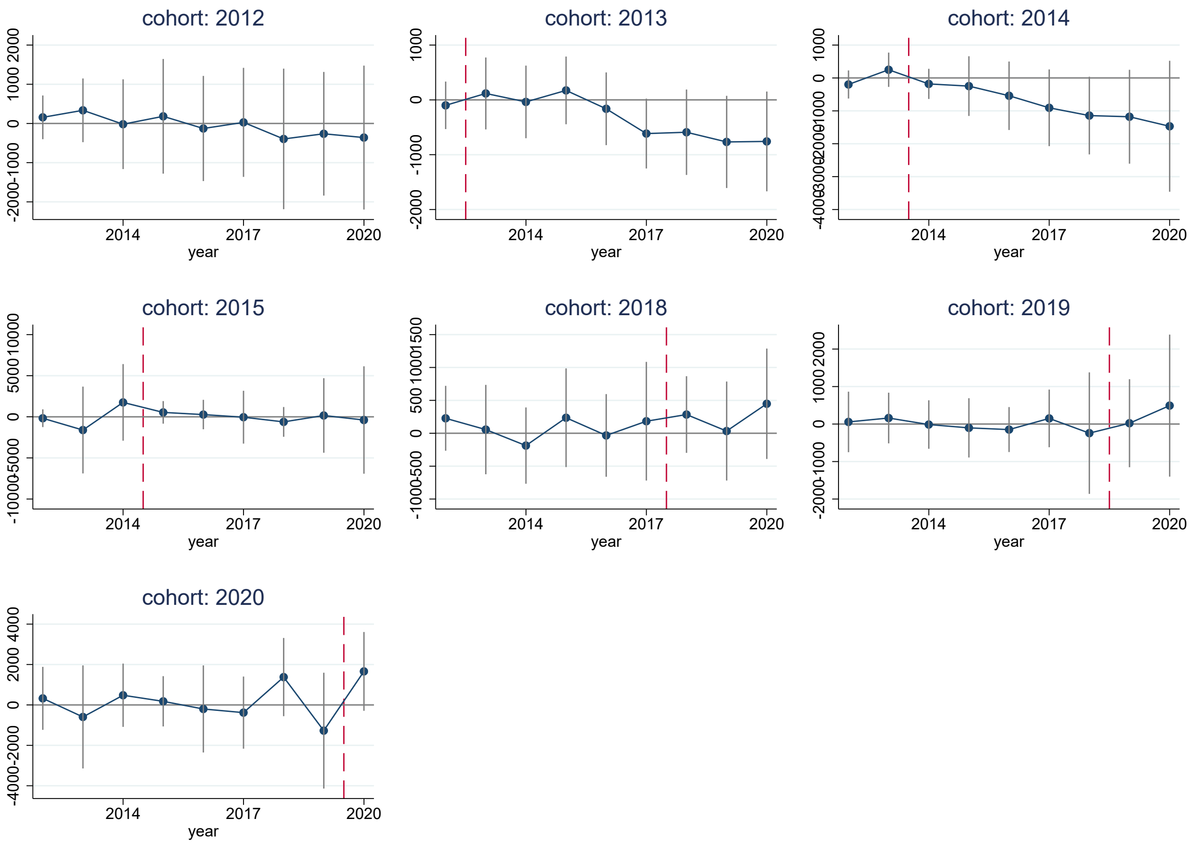


Figure C2: Annual earnings

Notes. The figure shows event study plots for each treated cohort.

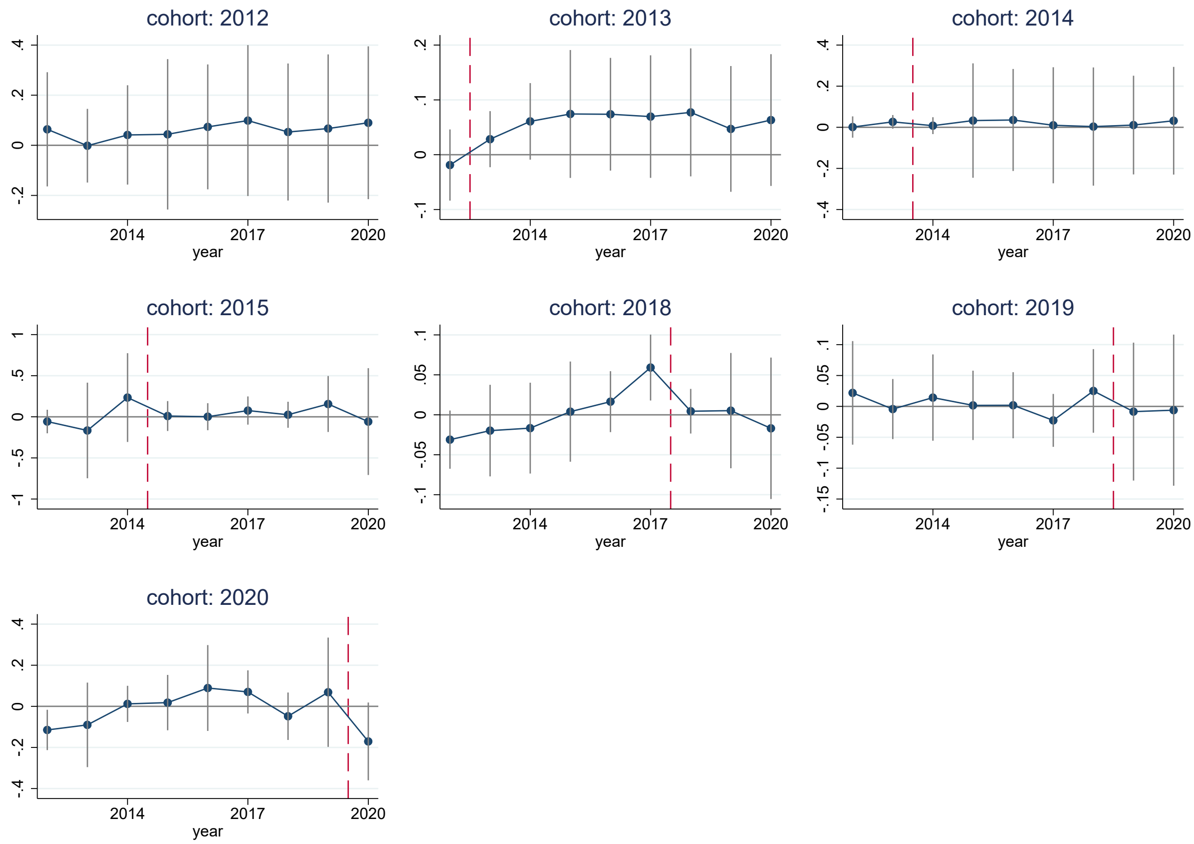


Figure C3: Number of workers

Notes. The figure shows event study plots for each treated cohort.

D Online Appendix: Including not-yet-treated units in the control group, main outcomes

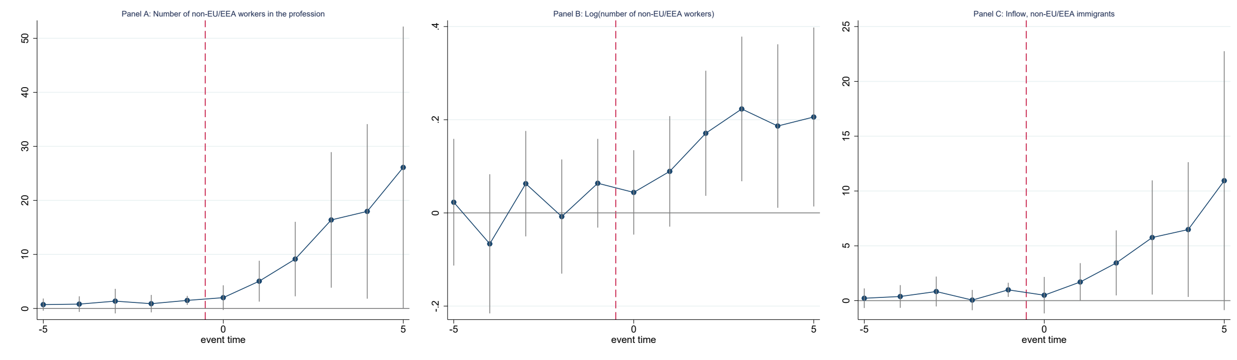


Figure D1: Stock and inflow of foreign workers, not yet treated

Notes. Stock and inflow of foreign workers, including not-yet-treated units in the control group.

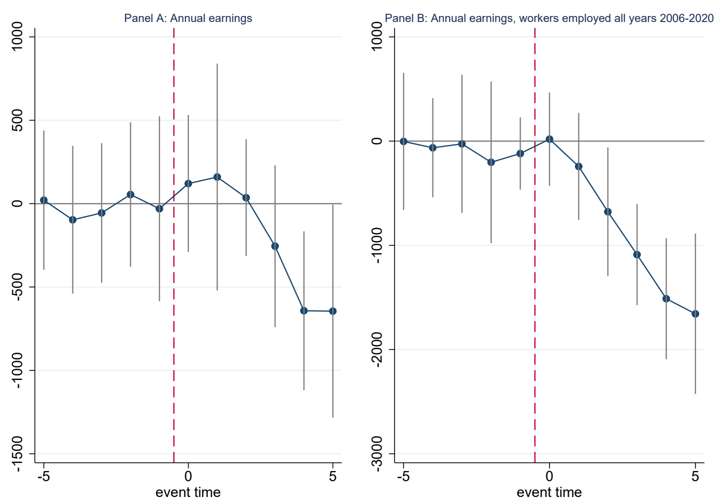


Figure D2: Earnings

Notes. Effects on native earnings, including not-yet-treated units in the control group.

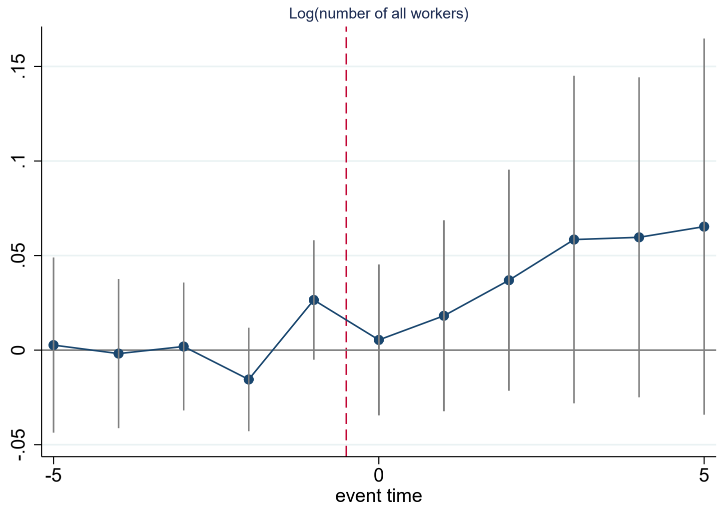


Figure D3: Log(nr all workers)

Notes. Effects on log(number of all workers), including not-yet-treated units in the control group.

E Online Appendix: Descriptive figures and tables by cohort

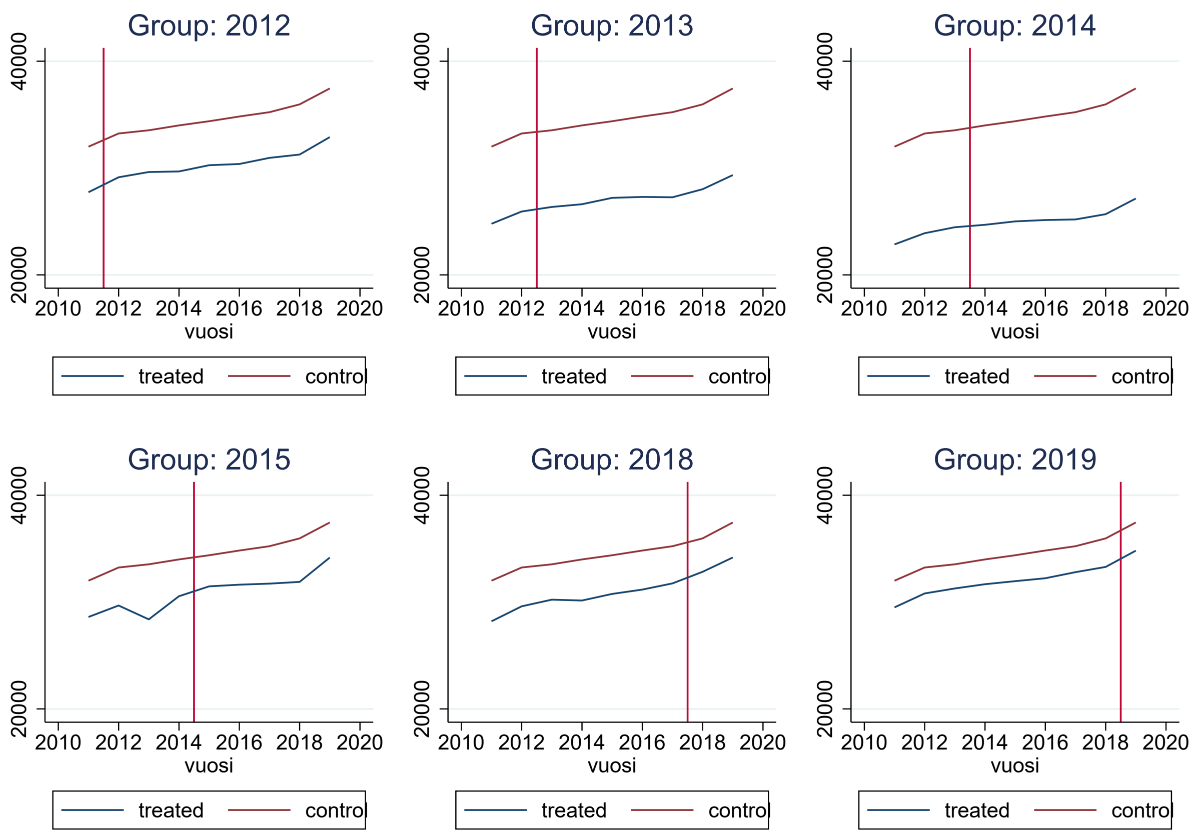


Figure E1: Salaries in treated vs. control professions

Notes. The figure shows descriptive trends in salaries.

Table E1: Descriptive statistics by treatment cohort

Variable	Cohort 2012 Mean Control	Mean Treat.	Difference	S.E.	Cohort 2013 Mean Control	Mean Treat.	Difference	S.E.
nr foreign workers	1.68	78.94	77.27***	(5.41)	2.01	4.39	2.38**	(1.10)
nr workers	262.37	2,499.29	2,236.92***	(166.06)	260.49	744.92	484.43***	(71.85)
share foreign	0.60%	2.60%	2.00%***	(0.50%)	0.70%	1.40%	0.70%**	(0.30%)
mean earnings	31,231.14	27,065.62	-4,165.52	(2,666.46)	32,588.77	25,201.54	-7,387.23***	(1,298.33)
median earnings	31,271.10	28,060.00	-3,211.10	(2,639.84)	32,647.67	25,940.49	-6,707.18***	(1,287.66)
sd, earnings	13,404.43	12,422.33	-982.10	(1,216.01)	13,757.68	11,508.18	-2,249.51***	(578.60)
nr unemployed	26.43	146.23	119.80***	(13.88)	28.86	57.42	28.57***	(6.87)
open vacancies	2.62	66.20	63.58***	(3.61)	2.60	8.63	6.04***	(1.62)
length, vacancies open	34.66	558.09	523.43***	(39.26)	40.31	135.07	94.77***	(17.67)
tightness (V/U)	0.16	1.16	1.00***	(0.20)	0.16	0.18	0.02	(0.09)
change in income, %	4.47	2.51	-1.96	(2.22)	4.31	5.53	1.22	(0.92)
unemp. months prev.	0.28	0.09	-0.18*	(0.11)	0.37	0.32	-0.05	(0.05)
unemp. prev.	6.70%	2.80%	-3.90%*	(2.10%)	8.50%	7.80%	-0.80%	(0.90%)
region-level wage sum, millions	3,976.00	27,340.00	23,364.00***	(979.30)	4,129.00	4,042.00	-87.79	(471.80)
region-level population	183,167.34	1,044,000.00	860,373.69***	(36,991.32)	182,720.14	193,965.27	11,245.13	(17,232.35)
region-level unemp. months	0.87	0.64	-0.23***	(0.04)	0.91	1.00	0.09***	(0.02)
N	5,141	35	5,176		5,096	165	5,261	

Variable	Cohort 2014 Mean Control	Mean Treat.	Difference	S.E.	Cohort 2015 Mean Control	Mean Treat.	Difference	S.E.
nr foreign workers	2.29	35.18	32.89***	(3.59)	2.55	11.21	8.66*	(4.71)
nr workers	256.47	1,321.75	1,065.28***	(172.52)	248.78	1,256.57	1,007.79***	(233.41)
share foreign	0.80%	1.70%	0.90%	(0.70%)	0.90%	0.30%	-0.60%	(1.20%)
mean earnings	32,875.36	23,305.39	-9,569.97***	(3,139.94)	33,197.39	27,006.28	-6,191.11	(4,509.38)
median earnings	32,920.40	24,316.07	-8,604.33***	(3,098.04)	33,252.05	28,325.00	-4,927.05	(4,486.66)
sd, earnings	13,998.58	11,496.06	-2,502.52*	(1,441.33)	14,308.78	10,874.41	-3,434.37	(2,165.22)
nr unemployed	33.44	106.43	72.99***	(19.75)	36.87	157.71	120.84***	(31.23)
open vacancies	2.41	14.14	11.73***	(3.99)	2.59	9.86	7.27	(5.38)
length, vacancies open	42.90	342.37	299.47***	(52.58)	49.10	628.64	579.55***	(70.78)
tightness (V/U)	0.11	0.13	0.02	(0.18)	0.15	0.04	-0.11	(0.29)
change in income, %	2.27	1.99	-0.28	(2.48)	2.21	6.04	3.83	(4.77)
unemp. months prev.	0.40	0.17	-0.23**	(0.11)	0.45	0.47	0.02	(0.19)
unemp. prev.	9.70%	4.70%	-5.00%**	(2.30%)	10.50%	12.20%	1.70%	(3.80%)
region-level wage sum, millions	4,166.00	3,831.00	-334.70	(1,163.00)	4,192.00	4,888.00	695.90	(1,659.00)
region-level population	182,246.45	161,638.83	-20,607.63	(42,197.93)	181,732.59	214,849.78	33,117.20	(60,029.57)
region-level unemp. months	1.02	0.93	-0.10**	(0.05)	1.13	1.11	-0.02	(0.07)
N	5,085	28	5,113		5,096	14	5,110	

Variable	Cohort 2018 Mean Control	Mean Treat.	Difference	S.E.	Cohort 2019 Mean Control	Mean Treat.	Difference	S.E.
nr foreign workers	3.97	7.74	3.77	(2.33)	4.80	9.73	4.93**	(2.13)
nr workers	248.89	554.64	305.74***	(79.43)	253.42	410.12	156.70**	(61.47)
share foreign	1.20%	2.50%	1.30%***	(0.40%)	1.40%	2.10%	0.70%*	(0.30%)
mean earnings	34,286.71	31,564.06	-2,722.65*	(1,642.51)	35,023.20	32,642.42	-2,380.78*	(1,252.12)
median earnings	34,168.78	32,133.04	-2,035.73	(1,615.83)	34,892.38	33,257.39	-1,634.99	(1,237.55)
sd, earnings	14,984.21	13,270.42	-1,713.79**	(730.99)	15,310.30	13,402.19	-1,908.11***	(578.40)
nr unemployed	30.48	90.74	60.26***	(9.47)	26.45	51.34	24.89***	(6.25)
open vacancies	4.25	20.17	15.92***	(2.08)	4.94	10.47	5.53**	(2.17)
length, vacancies open	103.27	1,157.06	1,053.79***	(61.91)	138.47	350.33	211.86***	(43.59)
tightness (V/U)	0.24	0.22	-0.02	(0.19)	0.26	0.23	-0.02	(0.11)
change in income, %	2.69%	2.62%	-0.08%	(1.29%)	3.98%	4.11%	0.13%	(0.93%)
unemp. months prev.	0.34	0.41	0.07	(0.06)	0.30	0.35	0.05	(0.04)
unemp. prev.	7.80%	10.20%	2.50%**	(1.00%)	7.50%	8.70%	1.20%	(0.80%)
region-level wage sum	4.396e+09	4.827e+09	431.4e+06	(622.9e+06)	4.596e+09	2.858e+09	-1.738e+09***	(489.8e+06)
region-level population	180,247.41	207,452.34	27,204.95	(21,561.71)	179,770.03	127,021.42	-52,748.61***	(16,282.30)
region-level unemp. months	1.07	1.10	0.03	(0.02)	0.91	1.05	0.14***	(0.02)
N	5,098	115	5,213		5,068	205	5,273	

Notes. The table shows the baseline (year -1) characteristics of treated cohorts 2012, 2013, 2014, 2015, 2018, and 2019 compared to those of the never-treated units. The years 2017 and 2020 are shown in the other table. The year 2016 only had 4 treated units and is hence omitted.

Table E2: Descriptives for treated cohort 2016

	Mean, control	Mean, treated	Diff (S.E.)
nr foreign workers	2.785	7.500	4.715 (5.782)
nr employed	240.346	424.700	184.354 (258.485)
share foreign	0.010	0.116	0.106*** (0.014)
mean earnings	33,600.996	31,905.885	-1,695.112 (5,356.013)
median earnings	33,592.016	32,500.000	-1,092.015 (5,288.349)
sd, earnings	14,640.673	11,397.578	-3,243.095 (2,610.900)
nr unemployed	38.099	131.500	93.401** (38.426)
nr open vacancies	3.345	5.900	2.555 (7.765)
length, vacancien open	59.387	114.778	55.390 (102.332)
tightness	0.221	0.054	-0.167 (0.814
change in income, %	1.657	3.637	1.981 (6.443)
unemp. months. prev.	0.468	0.669	0.202 (0.237)
unemp. prev.	0.103	0.155	0.051 (0.043)
region-level wage sum	4.237e+09	4.923e+09	6.857e+08 (2.001e+09)
region-level population	181338.078	222145.797	40,807.727 (71,544.227)
region-level unemp. monhts	1.204	1.229	0.025 (0.089)
Observations	5,103	10	5,113

Table E3: Descriptives for treated cohort 2020

	Mean, control	Mean, treated	Diff (S.E.)
nr foreign workers	5.307	53.053	47.746*** (8.253)
employed	245.075	456.053	210.978 (197.518)
share foreign	0.017	0.059	0.041*** (0.015)
mean earnings	36,494.430	32,676.299	-3,818.130 (4,158.063)
median earnings	36,221.645	33,186.844	-3,034.802 (4,087.463)
sd, earnings	15,255.629	12,433.454	-2,822.174 (1,961.084)
nr unemployed	26.328	82.947	56.620*** (19.839)
nr open vacancies	5.722	15.211	9.488 (6.952)
length, vacancien open	230.645	551.000	320.355 (204.701)
tightness	0.347	0.169	-0.177 (0.681)
change in income, %	3.343	0.552	-2.790 (2.703)
unemp. months. prev.	0.334	0.655	0.321** (0.138)
unemp. prev.s	0.082	0.179	0.098*** (0.027)
region-level wage sum	4.729e+09	4.184e+09	-5.448e+08 (1.672e+09)
region-level population	178190.500	159378.047	-18812.443 (53,673.156)
region-level unemp. monhts	0.852	0.995	0.143*** (0.045)
Observations	5,085	19	5,104

F Online Appendix: Placebo analysis at the occupation-region level

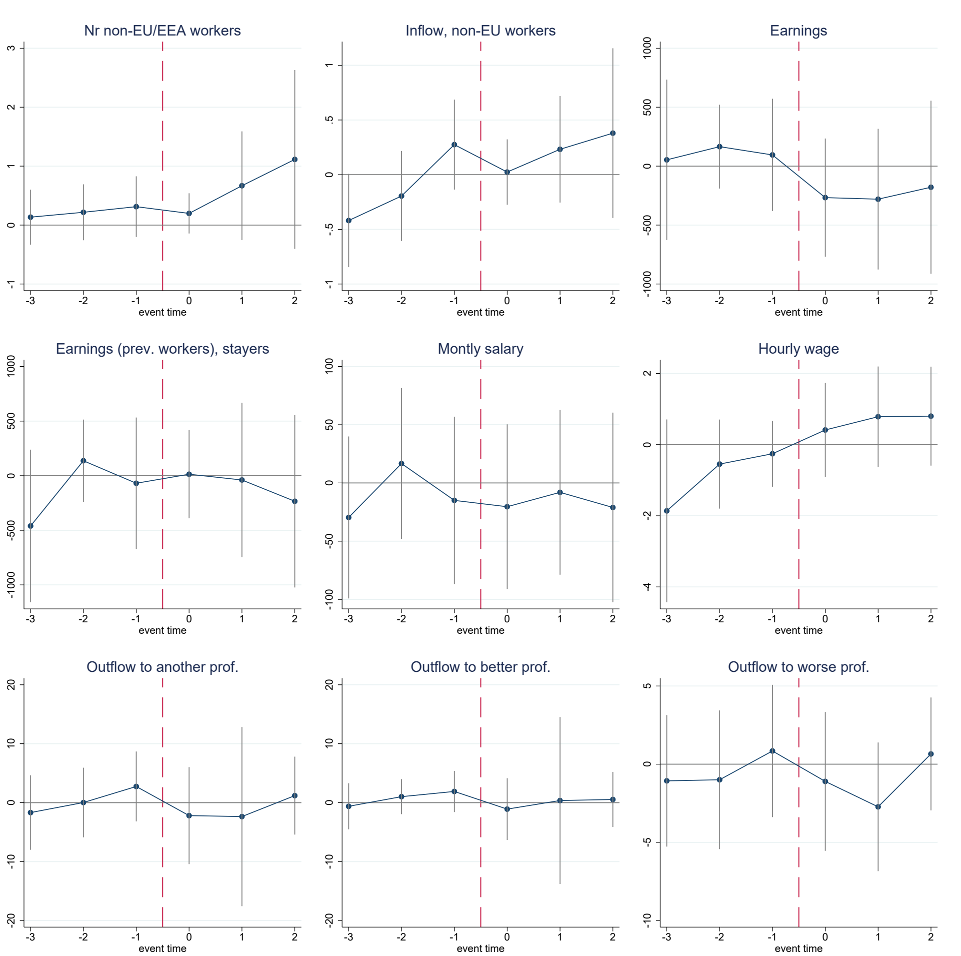


Figure F1: Placebo

Notes. The figure shows placebo estimates for different outcome variables.

G Online Appendix: Main occupation-region level results using other event study estimators

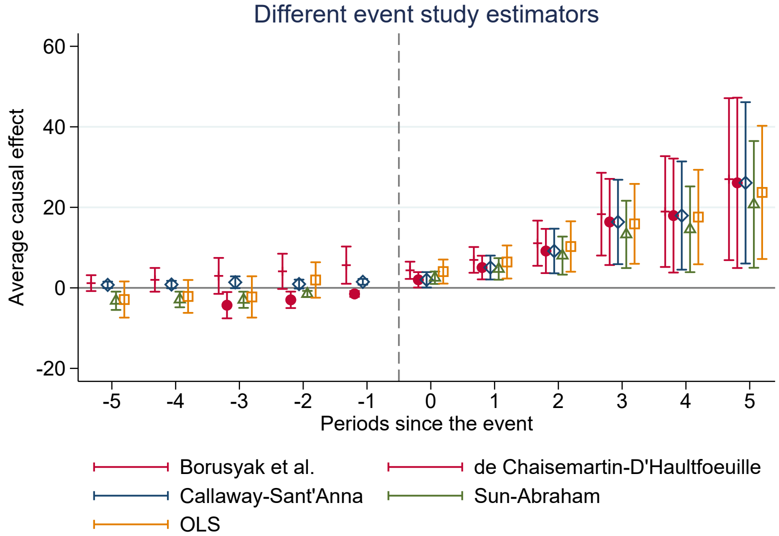


Figure G1: Effect on the number of non-EU workers

Notes. Effect on the number of non-EU workers The figure shows 5 different event study estimates.

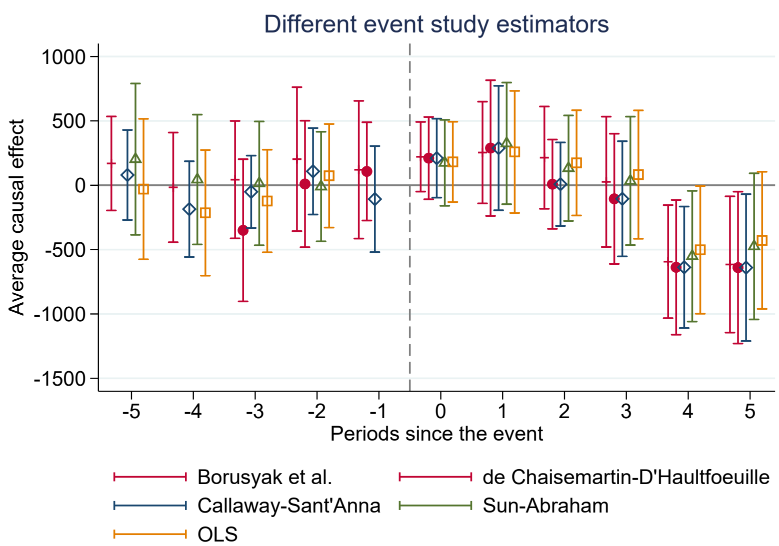


Figure G2: Effect on annual earnings of native workers

Notes. Effect on native annual earnings. The figure shows 5 different event study estimates.

H Online Appendix: Main Callaway & Sant'Anna results using universal base period

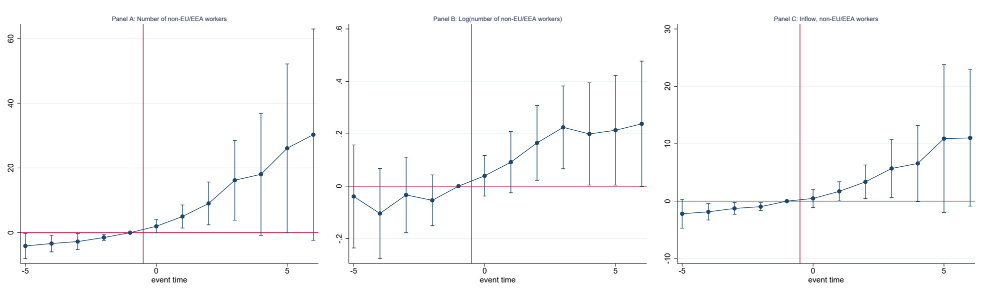


Figure H1: Effect on the stock and inflow of non-EU workers

Notes. The figure show Callaway & Sant'Anna estimates with a universal base period

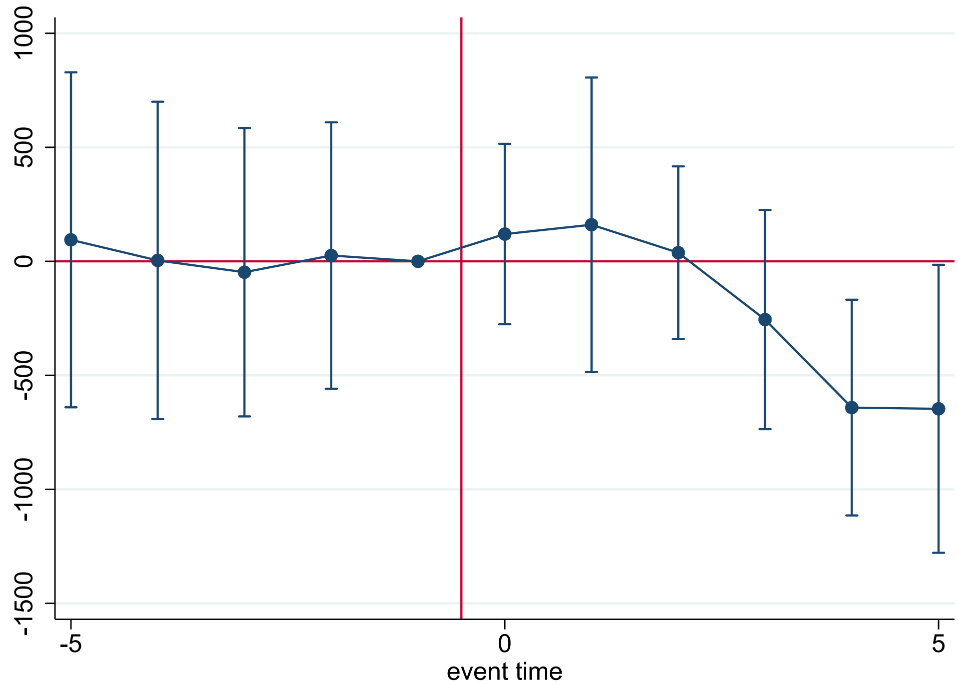


Figure H2: Effect on annual earnings

Notes. The figure show Callaway & Sant'Anna estimates with a universal base period

I Online Appendix: Earnings effects by occupation type

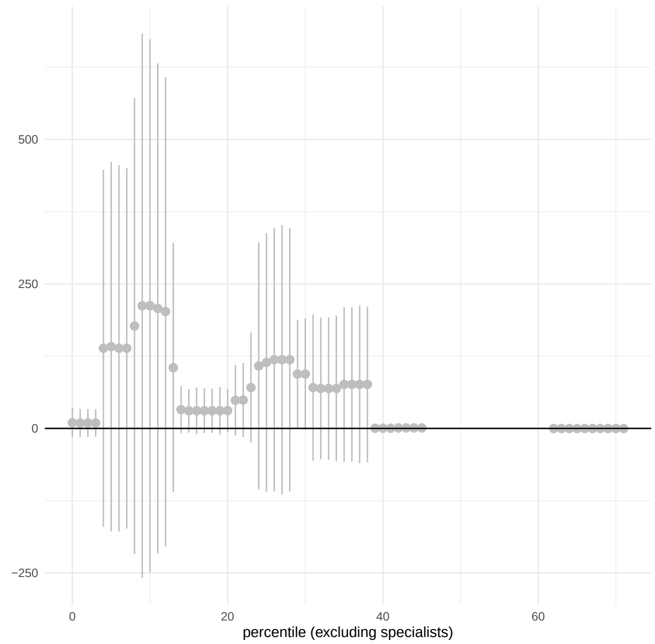


Figure I3: Service workers (groups 5 and service occupations in group 9), nr immigrants

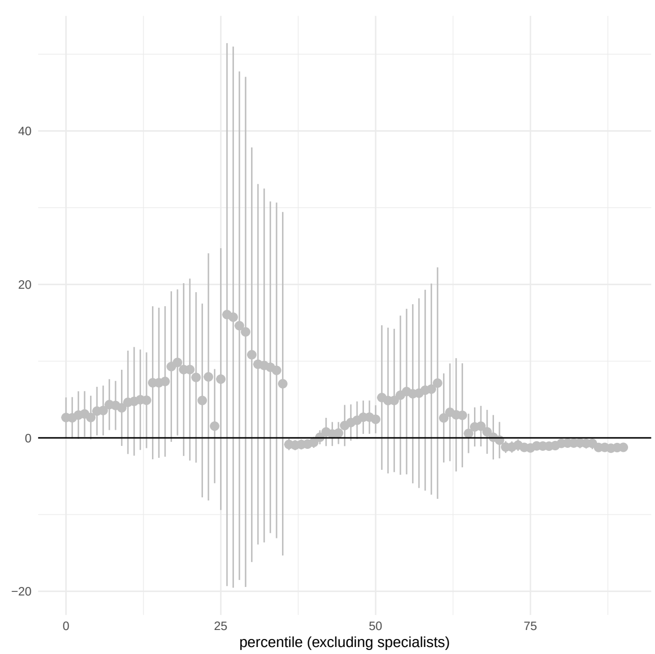


Figure I4: Non-service workers, nr immigrants

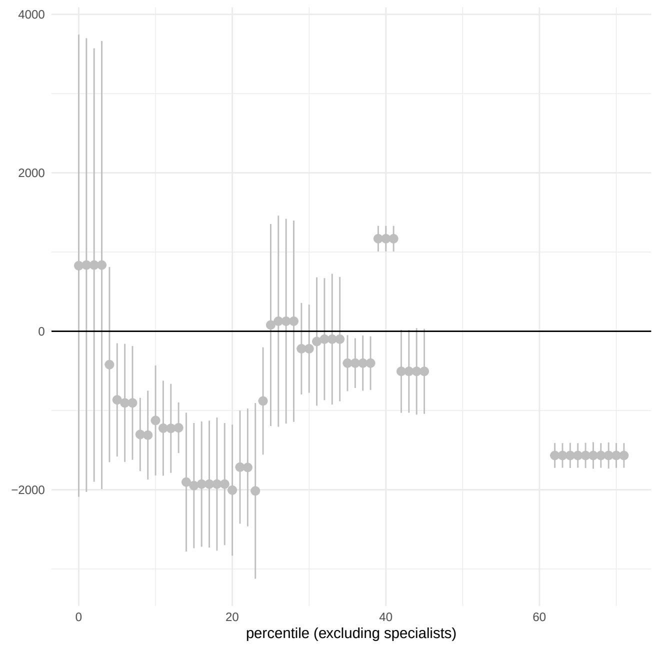


Figure I5: Service workers (groups 5 and selected occupations in group 9), earnings

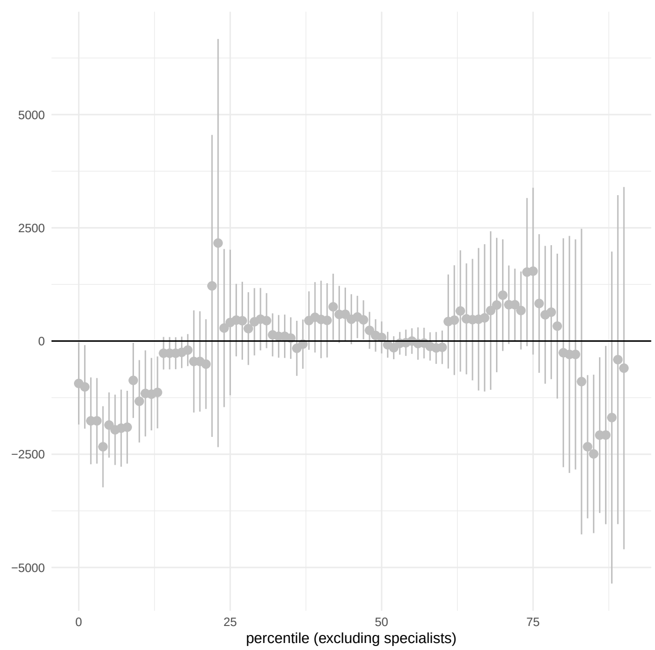


Figure I6: Non-service workers, earnings

J Online Appendix: Additional firm-level tables and figures

Table J2: Pooled firm-level DiD estimates with coarsened exact matching (>= 10 % treatment intensity)

	Size and personnel, number of				Investments, €1,000				Other			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Non-EU	Workers FT equiv.	Native workers	EU	All	Buildings	Machines	IT	Labor share	Turnover, €1,000	Profit ratio	Labor productivity
Panel A: All matched firms												
Treat × Post	0.00246	0.130**	0.197***	0.00639	-33.33*	-29.05*	-3.869	-0.408	-0.0717	-70.66	-0.256	-1525.3*
	(0.00666)	(0.0556)	(0.0581)	(0.00566)	(17.77)	(16.81)	(3.753)	(0.290)	(0.331)	(43.11)	(0.268)	(860.4)
Panel B: Heterogeneity analyses												
B1. Heterogeneity by firm size (baseline: firm size < 10)												
Treat × Post	-0.0134**	-0.0787*	0.0181	0.00136	-33.69*	-27.23	-6.026	-0.435	-0.0781	-120.7***	-0.312	-1284.4
	(0.00604)	(0.0419)	(0.0463)	(0.00513)	(17.85)	(16.80)	(3.857)	(0.288)	(0.335)	(39.34)	(0.327)	(920.5)
Treat × Post × (≥ 10 employees)	0.0964***	1.327***	1.121***	0.0306	18.49**	5.239	13.09**	0.161	0.0386	373.3***	0.334	-2356.1**
	(0.0237)	(0.246)	(0.244)	(0.0209)	(8.062)	(3.439)	(6.541)	(0.120)	(0.0516)	(89.38)	(0.357)	(1176.8)
B2. Heterogeneity by industry (baseline 15-20 ranks)												
Treat × Post	-0.0265***	0.111	0.0404	0.0103	21.97	-43.05*	65.70*	-0.676**	0.0929	-14.50	0.0314	715.3
	(0.00980)	(0.221)	(0.222)	(0.0127)	(44.03)	(25.17)	(39.32)	(0.300)	(0.406)	(60.06)	(0.0239)	(2583.5)
Treat × Post × 1-5	0.0716***	0.182	0.181	0.0108	-59.64	13.34	-73.18*	0.198	-0.0653	-100.8*	0.00906	-6102.2**
	(0.0210)	(0.236)	(0.238)	(0.0163)	(39.12)	(15.71)	(39.26)	(0.169)	(0.136)	(53.82)	(0.0532)	(2694.8)
Treat × Post × 6-10	0.0204**	-0.0308	0.136	-0.0132	-57.14	13.24	-70.70*	0.321**	-0.189	-43.29	0.0266	-2722.1
	(0.0104)	(0.226)	(0.228)	(0.0132)	(39.55)	(15.84)	(39.38)	(0.161)	(0.138)	(53.89)	(0.0463)	(2580.6)
Treat × Post × 11-15	0.0169	-0.00542	0.184	0.00120	-53.18	16.65	-70.07*	0.245*	-0.203	-51.52	-1.018	970.8
	(0.0128)	(0.243)	(0.242)	(0.0170)	(39.11)	(15.84)	(39.19)	(0.144)	(0.138)	(51.14)	(0.955)	(2920.2)
N	119308	119308	119308	119308	119308	119308	119308	119308	117125	119308	117892	117464
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes. The table shows difference-in-differences estimates. Standard errors clustered by firm in parentheses. Coarsened exact matching procedure does not find controls for larger (number of workers ≥ 50) firms and thus drops most of them. This is because most of the larger firms are treated at some point due to having establishments in many places, and because it is enough to employ 1 worker in a treated occupation in order to be treated. Significance levels: (*) 0.1 (**) 0.05 (***) 0.01.

Table J3: Pooled firm-level DiD estimates with coarsened exact matching (>= 50 % treatment intensity)

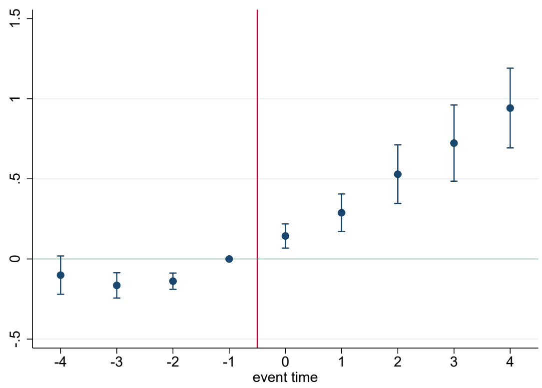
	Size and personnel, number of				Investments, €1,000				Other			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Non-EU	Workers FT equiv.	Native workers	EU	All	Buildings	Machines	IT	Labor share	Turnover, €1,000	Profit ratio	Labor productivity
Panel A: All matched firms												
Treat × Post	0.00340	0.0559	0.176**	0.0106	-35.03	-34.13	-0.355	-0.548	-0.0659	-131.3***	0.00750	-3020.0***
	(0.0103)	(0.0651)	(0.0709)	(0.00927)	(21.86)	(20.99)	(3.996)	(0.366)	(0.426)	(48.69)	(0.0306)	(951.0)
Panel B: Heterogeneity analyses												
B1. Heterogeneity by firm size (baseline: firm size < 10)												
Treat × Post	-0.0180**	-0.0947*	0.0559	0.00724	-35.18	-31.42	-3.210	-0.551	-0.0760	-157.6***	0.00808	-2827.9***
	(0.00845)	(0.0562)	(0.0619)	(0.00947)	(21.70)	(20.83)	(3.941)	(0.365)	(0.432)	(45.99)	(0.0308)	(985.6)
Treat × Post × (≥ 10 employees)	0.124***	0.945***	0.740***	0.0196	20.00***	3.366	16.62**	0.0178	0.0592	235.2***	-0.00297	-2148.3*
	(0.0409)	(0.229)	(0.256)	(0.0284)	(7.534)	(2.901)	(6.893)	(0.0378)	(0.115)	(51.90)	(0.00597)	(1235.8)
B2. Heterogeneity by industry (baseline 15-20 ranks)												
Treat × Post	-0.0318**	0.129	0.0270	0.0289	-43.32	-40.30	-2.464	-0.559*	0.00114	-65.00	0.0647***	2099.7
	(0.0127)	(0.390)	(0.381)	(0.0246)	(28.43)	(25.66)	(13.99)	(0.333)	(0.479)	(70.64)	(0.0245)	(2446.1)
Treat × Post × 1-5	0.0736**	0.190	0.337	0.0252	12.73	5.441	7.401	-0.115	0.0848	-83.37	-0.0810***	-11813.0***
	(0.0310)	(0.409)	(0.420)	(0.0326)	(14.86)	(8.891)	(14.64)	(0.237)	(0.0853)	(66.73)	(0.0230)	(3524.5)
Treat × Post × 6-10	0.0305**	-0.0554	0.124	-0.0325	8.832	5.282	3.508	0.0424	-0.184	-84.92	-0.0487***	-5225.9**
	(0.0146)	(0.395)	(0.388)	(0.0253)	(13.71)	(8.492)	(13.77)	(0.0934)	(0.218)	(55.29)	(0.0116)	(2364.8)
Treat × Post × 11-15	0.0280	-0.204	0.114	-0.0212	6.545	7.932	-1.414	0.0268	-0.00253	-43.84	-0.0619*	-2719.6
	(0.0209)	(0.398)	(0.390)	(0.0307)	(14.39)	(9.532)	(13.73)	(0.108)	(0.0998)	(58.09)	(0.0324)	(2421.5)
N	89187	89187	89187	89187	89187	89187	89187	89187	87337	89187	88062	87528
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

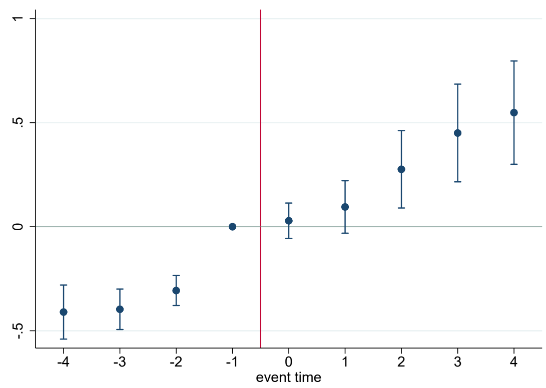
Notes. The table shows difference-in-differences estimates. Standard errors clustered by firm in parentheses. Coarsened exact matching procedure does not find controls for larger (number of workers ≥ 50) firms and thus drops most of them. This is because most of the larger firms are treated at some point due to having establishments in many places, and because it is enough to employ 1 worker in a treated occupation in order to be treated. Significance levels: (*) 0.1 (**) 0.05 (***) 0.01.

Table J1: Industry Classification by Groups

Group	Letter	Meaning
Group 1: Ranks 1-5	S	Other service activities
	J	Information and communication
	N	Administrative and support service activities
	A	Agriculture, forestry and fishing
	I	Accommodation and food service activities
Group 2: Ranks 6-10	C	Manufacturing
	G	Wholesale and retail trade; repair of motor vehicles and motorcycles
	M	Professional, scientific and technical activities
	H	Transportation and storage
	P	Education
Group 3: Ranks 11-15	L	Real estate activities
	R	Arts, entertainment and recreation
	K	Financial and insurance activities
	Q	Human health and social work activities
	F	Construction
Group 4: Ranks 16-20	X	Industry unknown
	O	Public administration and defence; compulsory social security
	D	Electricity, gas, steam and air conditioning supply
	B	Mining and quarrying
	E	Water supply; sewerage, waste management and remediation activities

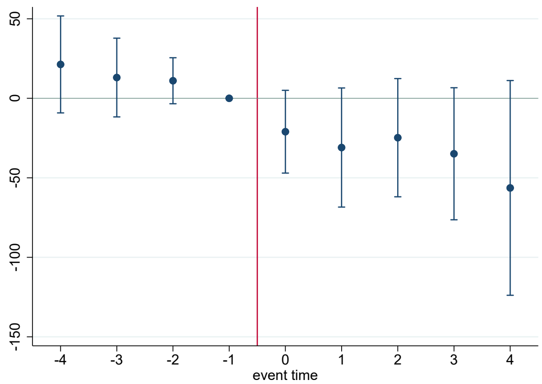
Figure J1: Distribution of treatment intensity in the treatment group (firm analysis)

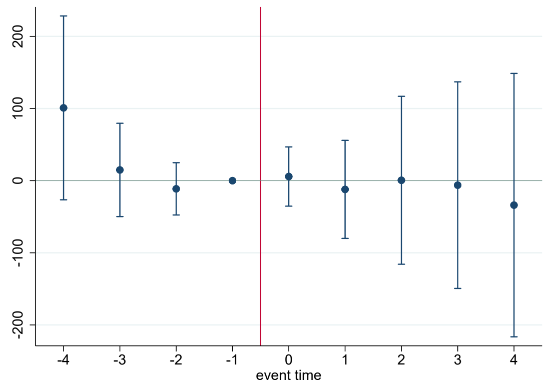




Panel A: Full time equivalent number of employees

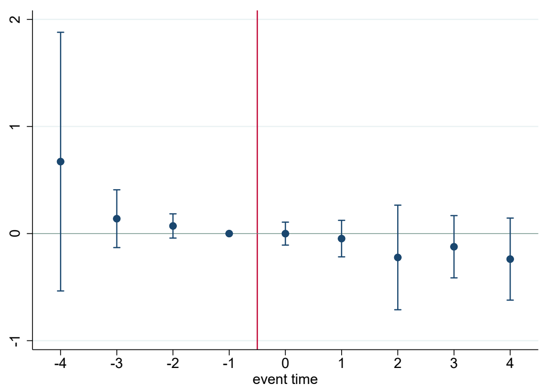
Panel B: Number of native workers (not ft. equiv.)

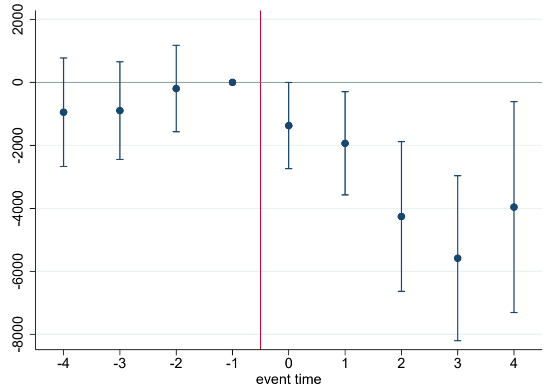




Panel C: Investments

Panel D: Turnover





Panel E: Profit ratio

Panel F: Labor productivity

Figure J2: Firm-level event studies

	control	treat	difference (T-C)
nr non-EU workers	0.253	0.307	0.053
	(1.730)	(2.304)	(0.041)
nr workers	21.170	19.753	-1.417
	(120.394)	(46.342)	(1.849)
sales	6.913e+06	5.084e+06	-1.828e+06**
	(4.661e+07)	(2.366e+07)	(749218.938)
taxes paid	113189.156	72,741.586	-40447.570
	(2.086e+06)	(1.260e+06)	(34,942.141)
investment	237482.188	162491.313	-74990.875
	(3.883e+06)	(1.040e+06)	(57,627.891)
share foreign workers	0.011	0.014	0.003**
	(0.058)	(0.059)	(0.001)
nr establishments	1.504	1.163	-0.341***
	(4.649)	(0.933)	(0.068)
profits	1.086e+06	333205.031	-7.530e+05*
	(2.838e+07)	(4.119e+06)	(411095.313)
value added per worker	79,921.000	70,290.914	-9,654.312***
	(224985.203)	(94,572.328)	(3,524.090)
Observations	4,866	4,866	9,732

Table J4: Balance table for firms after matching

Table J5: Pooled firm-level DiD estimates with coarsened exact matching (MATCH 2)

	Size and personnel				Investments, €1,000							
	(1) size of firm	(2) nr native workers	(3) nr non-EU	(4) nr EU	(5) all	(6) buildings	(7) machines	(8) IT	(9) labor share	(10) turnover, €1,000	(11) profit ratio	(12) labor productivity
All matched firms												
Treatment effect	0.197***	0.176***	0.00774	0.00835	-12.32**	-8.618**	-3.519	-0.183	-0.402	44.25	-0.266	-1113.5
	(0.0614)	(0.0642)	(0.00623)	(0.00569)	(5.509)	(3.432)	(2.978)	(0.140)	(0.393)	(32.42)	(0.320)	(816.6)
N	87332	87332	87332	87332	87332	87332	87332	86242	87332	87332	86569	86488
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes. The table shows difference-in-differences estimates. Standard errors clustered by firm in parentheses. Coarsened exact matching procedure does not find controls for larger (number of workers >= 50) firms and thus drops most of them. This is because most of the larger firms are treated at some point due to having establishments in many places, and because it is enough to employ 1 worker in a treated occupation in order to be treated. Significance levels: (*) 0.1 (**) 0.05 (***) 0.01

Table J6: Pooled firm-level DiD estimates with coarsened exact matching (MATCH 3)

	Size and personnel				Investments, €1,000							
	(1) size of firm	(2) nr native workers	(3) nr non-EU	(4) nr EU	(5) all	(6) buildings	(7) machines	(8) IT	(9) labor share	(10) turnover, €1,000	(11) profit ratio	(12) labor productivity
All matched firms												
Treatment effect	0.0265	0.0379	0.0209**	0.00254	3.984***	0.456	3.532***	-0.00443	0.111	-16.35*	0.00976	-978.8
	(0.0783)	(0.0792)	(0.0101)	(0.00683)	(1.147)	(0.433)	(1.064)	(0.0226)	(0.509)	(9.331)	(0.00730)	(1275.2)
N	18550	18550	18550	18550	18550	18550	18550	18550	18550	18550	18550	18550

Notes. The table shows difference-in-differences estimates. Standard errors clustered by firm in parentheses. Coarsened exact matching procedure does not find controls for larger (number of workers >= 50) firms and thus drops most of them. This is because most of the larger firms are treated at some point due to having establishments in many places, and because it is enough to employ 1 worker in a treated occupation in order to be treated. Significance levels: (*) 0.1 (**) 0.05 (***) 0.01

K Online Appendix: Changing also the control group when estimating earnings effects by quartile and percentile

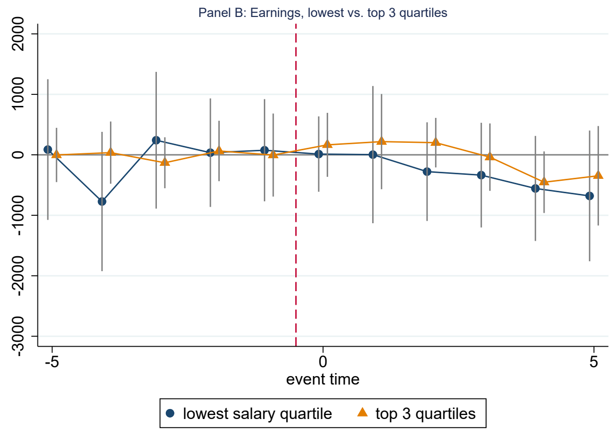


Figure K3: Earnings by quartile, changing also the control group

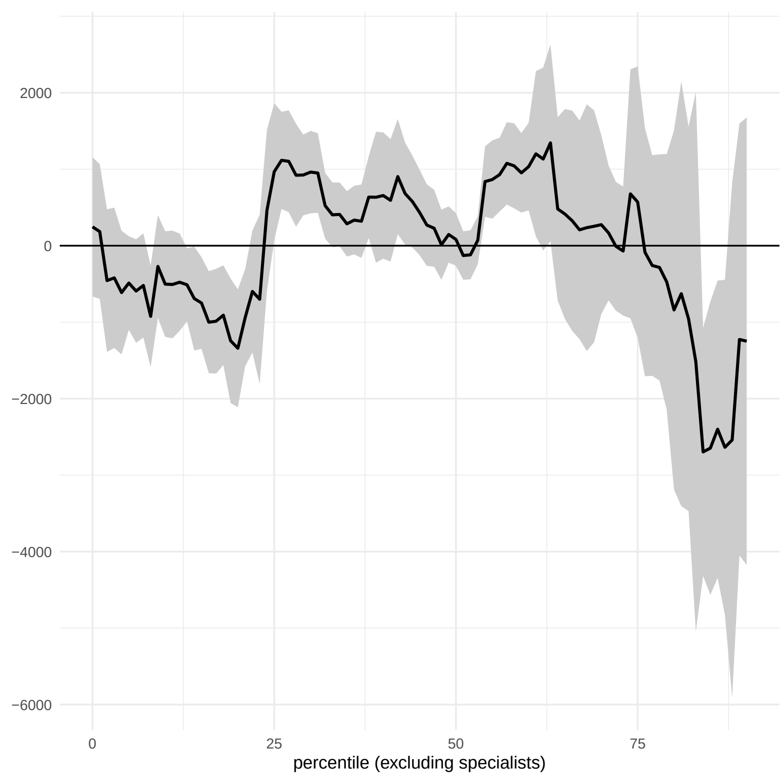


Figure K4: Percentile figure showing the pooled estimate (whole post period) for annual earnings, changing also the control group

L Online Appendix: Most common occupations in different income quartiles for all workers and non-EU workers

L.1 Most common occupations (all workers)

Occupation code	Occupation	Count
Lowest Quartile		
Occupation code	**Occupation**	**Count**
5223	Shop Sales Assistants	100652
9112	Cleaners and Helpers in Offices, Hotels and Other Establishments	62531
5311	Child Care Workers	39517
5322	Home-based Personal Care Workers	38891
6121	Field Crop and Vegetable Growers	18818
Second Lowest Quartile		
Occupation code	**Occupation**	**Count**
5321	Nursing Associate Professionals	77570
3412	Social Work Associate Professionals	39730
9333	Freight Handlers	36360
7111	House Builders	33358
5120	Cooks	32646
Second Highest Quartile		
Occupation code	**Occupation**	**Count**
3221	Nursing Professionals	72158
8332	Heavy Truck and Lorry Drivers	41585
7231	Motor Vehicle Mechanics and Repairers	22226
3313	Accounting Associate Professionals	19970
7233	Agricultural and Industrial Machinery Mechanics and Repairers	18357
Highest Quartile		
Occupation code	**Occupation**	**Count**
3322	Commercial Sales Representatives	38084
3115	Mechanical Engineering Technicians	14333
3119	Physical and Engineering Science Technicians (not elsewhere classified)	12141
3334	Real Estate Agents and Property Managers	10192
3112	Civil Engineering Technicians	9477

Table L7: Most common occupations in different salary quartiles

L.2 Most common occupations for non-EU workers

Occupation code	Occupation	Count
Lowest Quartile		
Occupation code	**Occupation**	**Count**
9112	Cleaners and Helpers in Offices, Hotels and Other Establishments	8647
5322	Home-based Personal Care Workers	1545
5223	Shop Sales Assistants	1524
9412	Hand Packers	1158
8322	Car, Taxi, and Van Drivers	1101
Second Lowest Quartile		
Occupation code	**Occupation**	**Count**
5120	Cooks	3599
5321	Nursing Associate Professionals	2835
7111	House Builders	1555
9333	Freight Handlers	1416
8160	Food and Related Products Machine Operators	702
Second Highest Quartile		
Occupation code	**Occupation**	**Count**
8331	Bus and Tram Drivers	947
3221	Nursing Professionals	870
7231	Motor Vehicle Mechanics and Repairers	647
8332	Heavy Truck and Lorry Drivers	524
3313	Accounting Associate Professionals	428
Highest Quartile		
Occupation code	**Occupation**	**Count**
3322	Commercial Sales Representatives	461
3119	Physical and Engineering Science Technicians (not elsewhere classified)	405
3115	Mechanical Engineering Technicians	272
3114	Electronics Engineering Technicians	147
3511	Information and Communications Technology Operations Technicians	124

Table L8: Most common occupations for non-EU workers in different salary quartiles

M Online Appendix: More individual level heterogeneity analyses

M.1 Heterogeneity estimates for whole sample

Table M9: Mechanisms and heterogeneity, individual level (all workers)

	(1) Earnings	(2) Native earnings	(3) Unemployment months	(4) Long-term unemployment	(5) Full-year employed	(6) Outflow to education	(7) Outflow out of labor force	(8) Outflow to better occupation	(9) Outflow to worse occupation	(10) Outflow to another firm	(11) Outflow to another occupation	(12) Main activity: education	(13) Main activity: unemployed	(14) Main activity: out of labor force	(15) Main activity: pensioner	(16) Enrolled	(17) Has degree	(18) Completed degree	(19) Moving
Panel A: Pooled individual level estimates (standard TWFE specification)																			
Treatment effect	-314.0**	-334.9**	-0.0995***	-0.00386***	0.0198***	-0.000700	0.000319	-0.00590	0.00280	-0.00665**	-0.00310	-0.00105	-0.00935***	-0.0000848	-0.000133	-0.00173	0.00390*	-0.00497***	-0.000940*
	(139.5)	(139.0)	(0.0231)	(0.00102)	(0.00447)	(0.000660)	(0.000645)	(0.00420)	(0.00286)	(0.00326)	(0.00523)	(0.00190)	(0.00313)	(0.00103)	(0.00197)	(0.00333)	(0.00236)	(0.00124)	(0.000550)
Panel B: Heterogeneity analyses																			
B1. Heterogeneity by age																			
Treatment effect	148.2	154.2	-0.116***	-0.00530***	0.0228***	-0.000518	-0.000204	-0.00319	0.00344	-0.00606*	0.000255	0.00247	-0.0111***	-0.000458	-0.0118***	0.00188	0.00280	0.000802	-0.00138**
	(153.0)	(153.2)	(0.0249)	(0.00116)	(0.00460)	(0.000639)	(0.000568)	(0.00443)	(0.00290)	(0.00329)	(0.00545)	(0.00189)	(0.00310)	(0.00114)	(0.00219)	(0.00363)	(0.00240)	(0.00139)	(0.000543)
Treat × Over 50	-2632.8***	-2696.6***	0.0764***	0.00421***	-0.0499***	-0.00238***	-0.000171	-0.00512**	-0.00401***	-0.00321	-0.00913***	-0.00298**	0.00767***	0.000306	0.0350***	0.0242***	-0.00584***	-0.000380	0.000873***
	(195.5)	(200.7)	(0.0158)	(0.000816)	(0.00782)	(0.000842)	(0.000396)	(0.00229)	(0.00122)	(0.00208)	(0.00281)	(0.00146)	(0.00148)	(0.000911)	(0.00730)	(0.00302)	(0.000781)	(0.000646)	(0.000326)
Treat × Under 30	3145.0***	3235.3***	-0.0710***	0.0000773	0.103***	0.00468	0.00431***	-0.00738*	0.00533**	0.00379	-0.00205	-0.0184***	-0.00633***	0.00203***	-0.00166**	-0.0866***	0.0226***	-0.0418***	0.00118
	(174.8)	(183.9)	(0.0134)	(0.000597)	(0.00570)	(0.00341)	(0.00159)	(0.00377)	(0.00214)	(0.00348)	(0.00456)	(0.00567)	(0.00204)	(0.000685)	(0.000723)	(0.00581)	(0.00150)	(0.00191)	(0.00139)
B2. Heterogeneity by gender																			
Treatment effect	-655.7***	-671.3***	-0.0907***	-0.00470***	0.0200***	-0.000226	0.000830	-0.0117***	0.000190	-0.0105***	-0.0115**	-0.00138	-0.0101***	-0.000377	-0.000341	-0.00688**	0.00400	-0.00655***	-0.00112**
	(152.8)	(152.4)	(0.0242)	(0.00103)	(0.00517)	(0.000803)	(0.000768)	(0.00414)	(0.00288)	(0.00325)	(0.00486)	(0.00203)	(0.00349)	(0.00133)	(0.00233)	(0.00349)	(0.00249)	(0.00138)	(0.000538)
Treat × Man	1097.5***	1106.1***	-0.0267	0.00274***	-0.000826	-0.00154*	-0.00162**	0.0181***	0.00815***	0.0123***	0.0263***	0.000987	0.00289	0.00101	0.000520	0.0170***	-0.000272	0.00502***	0.000588
	(120.1)	(123.9)	(0.0233)	(0.00106)	(0.00358)	(0.000877)	(0.000660)	(0.00407)	(0.00238)	(0.00325)	(0.00546)	(0.00124)	(0.00369)	(0.00124)	(0.00143)	(0.00276)	(0.00105)	(0.00107)	(0.000621)
B3. Heterogeneity by education level																			
Treatment effect	-778.0***	-1112.7***	0.00825	0.00122	-0.00120	0.00147	0.00448**	-0.00602	0.00552	0.00608	-0.000498	-0.00326	0.00936*	0.00558**	0.00842***	0.0292***	-0.000134	0.0256***	0.00151
	(227.7)	(198.0)	(0.0482)	(0.00189)	(0.00931)	(0.00292)	(0.00214)	(0.00497)	(0.00480)	(0.00422)	(0.00684)	(0.00440)	(0.00510)	(0.00252)	(0.00322)	(0.00349)	(0.000510)	(0.00140)	(0.000924)
Treat × Secondary	282.0	591.1***	-0.106**	-0.00413***	0.0218***	-0.00101	-0.00442**	-0.00133	0.00195	-0.00998***	0.000613	0.00199	-0.0161***	-0.00518**	-0.00687***	-0.0316***	0.000328	-0.0311***	-0.00149**
	(175.7)	(142.8)	(0.0415)	(0.00152)	(0.00813)	(0.00251)	(0.00200)	(0.00295)	(0.00278)	(0.00282)	(0.00430)	(0.00428)	(0.00475)	(0.00211)	(0.00218)	(0.00401)	(0.000609)	(0.00168)	(0.000693)
Treat × Bachelor's	999.2***	1373.6***	-0.149**	-0.00864***	0.0268**	-0.00523	-0.00504***	0.00300	-0.0124***	-0.0224***	-0.00939	0.00381	-0.0301***	-0.00830***	-0.0146***	-0.0420***	-0.00000128	-0.0403***	-0.00509***
	(232.4)	(205.1)	(0.0614)	(0.00240)	(0.0119)	(0.00469)	(0.00188)	(0.00502)	(0.00426)	(0.00459)	(0.00655)	(0.00646)	(0.00746)	(0.00234)	(0.00324)	(0.00511)	(0.000579)	(0.00279)	(0.000999)
Outcome mean	27770.23	28013.52	0.56	0.01	0.81	0.01	0.003	0.05	0.05	0.12	0.10	0.02	0.05	0.01	0.002	0.13	0.89	0.04	0.01
N	6403800	5975170	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year. See Online Appendix Tables M10 and M11 for the same analyses separately for the bottom and top-3 quartiles.

M.2 Bottom quartile and top 3 quartiles

Table M10: Mechanisms and heterogeneity, individual level (lowest quartile)

	(1) earnings	(2) native earnings	(3) unemployment months	(4) long-term unemployment	(5) full-year employed	(6) outflow to education	(7) outflow out of labor force	(8) outflow to better occupation	(9) outflow to worse occupation	(10) outflow to another firm	(11) outflow to another occupation	(12) main activity: education	(13) main activity: unemployed	(14) main activity: out of labor force	(15) main activity: pensioner	(16) enrolled	(17) has degree	(18) completed degree	(19) moving
Panel A: Pooled individual level estimates (standard TWFE specification)																			
1.D	-699.2***	-714.7***	-0.0530	-0.00288	0.0134	-0.000992	-0.0135	0.00804	-0.00808	-0.00542	-0.00358	-0.00792	-0.00400*	0.00500	0.000870	0.00599	-0.000858	-0.000858	-0.000816
	(234.8)	(222.6)	(0.0542)	(0.00183)	(0.00884)	(0.00161)	(0.0105)	(0.00526)	(0.00616)	(0.0112)	(0.00249)	(0.00684)	(0.00218)	(0.00463)	(0.00520)	(0.00588)	(0.00340)	(0.00340)	(0.00128)
Panel B: Heterogeneity analyses																			
B1. Heterogeneity by age																			
1.D	-61.83	10.48	-0.0786	-0.00437**	0.0252***	-0.00289*	-0.00124	-0.00961	0.00645	-0.00930	-0.00316	-0.00202	-0.00970	-0.00390	-0.00924***	-0.00902	0.00866	0.00183	-0.00139
	(240.4)	(226.7)	(0.0567)	(0.00180)	(0.00919)	(0.00162)	(0.00152)	(0.0111)	(0.00559)	(0.00634)	(0.0115)	(0.00247)	(0.00712)	(0.00282)	(0.00288)	(0.00692)	(0.00637)	(0.00425)	(0.00123)
1.D#1.yli50	-2273.7***	-2419.7***	0.0684***	0.00461***	-0.0566***	-0.00335**	-0.00126	-0.0164***	0.00619***	-0.00245	-0.0102**	-0.00464*	0.00470*	-0.000808	0.0393***	0.0450***	-0.0126***	-0.0000535	0.000357
	(220.0)	(242.1)	(0.0223)	(0.00137)	(0.0109)	(0.00137)	(0.000766)	(0.00509)	(0.00171)	(0.00436)	(0.00518)	(0.00273)	(0.00267)	(0.00263)	(0.00957)	(0.00756)	(0.00242)	(0.00199)	(0.000658)
1.D#1.alle30	2128.6***	2164.7***	-0.0105	-0.00137	0.0754***	0.0147**	0.00512*	0.0172**	-0.00531	0.0128	0.0119	-0.00186	-0.00104	0.000281	-0.00369**	-0.0727***	0.0216***	-0.0212***	0.00124
	(366.3)	(384.0)	(0.0318)	(0.00147)	(0.0108)	(0.00683)	(0.00291)	(0.00729)	(0.00342)	(0.00800)	(0.00954)	(0.00832)	(0.00407)	(0.00119)	(0.00153)	(0.00709)	(0.00268)	(0.00481)	(0.00255)
B2. Heterogeneity by gender																			
1.D	-738.2***	-741.2***	-0.0592	-0.00295	0.0143	-0.00199	-0.000826	-0.0174	0.00750	-0.00865	-0.00993	-0.00359	-0.00813	-0.00397*	0.00483	0.00227	0.00586	-0.00194	-0.000628
	(242.0)	(230.8)	(0.0548)	(0.00184)	(0.00897)	(0.00151)	(0.00165)	(0.0106)	(0.00556)	(0.00637)	(0.0112)	(0.00237)	(0.00707)	(0.00234)	(0.00496)	(0.00544)	(0.00560)	(0.00355)	(0.00129)
1.D#1.mies	258.3	232.5	0.0662***	0.00101	-0.00711**	0.00000258	-0.000889	0.0230***	0.00230	0.00377	0.0253***	-0.000697	0.00507*	0.000898	-0.000740	-0.00904*	0.00205	0.00657***	-0.00102
	(232.7)	(239.5)	(0.0230)	(0.00113)	(0.00358)	(0.00123)	(0.000843)	(0.00755)	(0.00455)	(0.00748)	(0.00769)	(0.00234)	(0.00276)	(0.00144)	(0.00278)	(0.00541)	(0.00396)	(0.00219)	(0.000870)
B3. Heterogeneity by education level																			
1.D	-1187.1***	-1770.2***	0.132	0.00236	-0.00588	-0.000529	0.00479	-0.0250**	0.0188***	0.00630	-0.00615	-0.0123	0.0129	0.00496	0.0162**	0.0220***	-0.000517	0.0364***	0.0000596
	(359.9)	(272.0)	(0.0990)	(0.00286)	(0.0196)	(0.00700)	(0.00416)	(0.00990)	(0.00489)	(0.00724)	(0.0107)	(0.00989)	(0.0126)	(0.00493)	(0.00726)	(0.00688)	(0.000819)	(0.00410)	(0.00169)
1.D#2.koulutus	617.2**	1218.5***	-0.210***	-0.00597***	0.0234	-0.00202	-0.00670*	0.0107	-0.00852**	-0.0151***	0.00214	0.00727	-0.0239***	-0.0102**	-0.0122***	-0.0308***	0.000712	-0.0352***	-0.000825
	(252.7)	(171.3)	(0.0640)	(0.00183)	(0.0152)	(0.00629)	(0.00357)	(0.00677)	(0.00421)	(0.00565)	(0.00613)	(0.0106)	(0.00822)	(0.00429)	(0.00445)	(0.00765)	(0.000954)	(0.00370)	(0.00100)
1.D#3.koulutus	1399.7***	2327.2***	-0.210***	-0.00714***	0.0289	-0.00517	-0.00607**	0.0282***	-0.0271***	-0.0195***	0.00107	0.0124	-0.0233***	-0.00989***	-0.0181***	-0.0536***	0.000855	-0.0508***	-0.00160
	(322.6)	(245.6)	(0.0505)	(0.00138)	(0.0211)	(0.0138)	(0.00307)	(0.00834)	(0.00459)	(0.00623)	(0.00921)	(0.0203)	(0.00569)	(0.00300)	(0.00534)	(0.0115)	(0.000969)	(0.0103)	(0.00152)
Outcome mean	20310.11	20415.04	0.65	0.01	0.77	0.01	0.004	0.03	0.06	0.11	0.09	0.03	0.05	0.02	0.004	0.15	0.87	0.05	0.01
N	1643650	1468410	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year.

Table M11: Mechanisms and heterogeneity, individual level (top 3 quartiles)

	(1) earnings	(2) native earnings	(3) unemployment months	(4) long-term unemployment	(5) full-year employed	(6) outflow to education	(7) outflow out of labor force	(8) outflow to better occupation	(9) outflow to worse occupation
Panel A: Pooled individual level estimates (standard TWFE specification)									
1.D	-224.1	-249.4*	-0.118***	-0.00443***	0.0220***	0.000759	-0.00408	0.00178	-0.00555
	(145.5)	(145.9)	(0.0279)	(0.00115)	(0.00487)	(0.000539)	(0.00421)	(0.00312)	(0.00399)
Panel B: Heterogeneity analyses									
B1. Heterogeneity by age									
1.D	151.7	138.6	-0.128***	-0.00578***	0.0222***	0.000325	0.000206	-0.000712	0.00200
	(162.9)	(163.7)	(0.0298)	(0.00130)	(0.00479)	(0.000523)	(0.000462)	(0.00445)	(0.00318)
1.D#1.yli50	-2708.3***	-2736.7***	0.0763***	0.00402***	-0.0482***	-0.00205***	0.0000667	-0.00324	-0.00520***
	(218.9)	(221.0)	(0.0172)	(0.000773)	(0.00805)	(0.000707)	(0.000332)	(0.00253)	(0.00146)
1.D#1.alle30	3538.0***	3611.9***	-0.0962***	0.000463	0.113***	0.000895	0.00391***	-0.0178***	0.0110***
	(208.4)	(209.5)	(0.0145)	(0.000605)	(0.00609)	(0.00199)	(0.00122)	(0.00441)	(0.00207)
B2. Heterogeneity by gender									
1.D	-575.6***	-595.1***	-0.101***	-0.00538***	0.0214***	0.0000447	0.00119*	-0.0136***	0.00127
	(164.0)	(164.7)	(0.0301)	(0.00123)	(0.00578)	(0.000721)	(0.000661)	(0.00402)	(0.00309)
1.D#1.mies	983.6***	991.5***	-0.0466	0.00269**	0.00166	-0.000876	-0.00120**	0.0265***	0.00132
	(122.7)	(124.9)	(0.0309)	(0.00134)	(0.00402)	(0.000837)	(0.000563)	(0.00432)	(0.00312)
B3. Heterogeneity by education level									
1.D	-553.2**	-793.1***	-0.0598	-0.000176	-0.000184	0.000903	0.00358**	-0.00568	0.00946*
	(251.5)	(203.0)	(0.0432)	(0.00187)	(0.00741)	(0.00145)	(0.00148)	(0.00599)	(0.00488)
1.D#2.koulutus	110.3	318.6**	-0.0482	-0.00283*	0.0220***	0.000351	-0.00288**	-0.00287	0.00124
	(192.7)	(140.7)	(0.0357)	(0.00161)	(0.00587)	(0.00118)	(0.00135)	(0.00371)	(0.00339)
1.D#3.koulutus	643.9**	869.7***	-0.0911	-0.00756***	0.0262***	-0.00333	-0.00350**	0.00842	-0.0235***
	(268.8)	(209.0)	(0.0581)	(0.00261)	(0.00976)	(0.00243)	(0.00137)	(0.00583)	(0.00448)
Outcome mean	30168.92	30403.89	0.53	0.008	0.82	0.007	0.003	0.06	0.04
N	4760150	4506760	4760150	4760150	4760150	4760150	4760150	4760150	4760150

	(10) outflow to another firm	(11) outflow to another occupation	(12) main activity: education	(13) main activity: unemployed	(14) main activity: out of labor force	(15) main activity: pensioner	(16) enrolled	(17) has degree	(18) completed degree	(19) moving
Panel A: Pooled individual level estimates (standard TWFE specification)										
1.D	-0.00230	-0.000380	-0.0103***	0.000743	-0.00186	-0.00241	0.00276**	-0.00628***	-0.00628***	-0.000969
	(0.00630)	(0.00200)	(0.00367)	(0.000729)	(0.00197)	(0.00408)	(0.00124)	(0.00124)	(0.00124)	(0.000621)
Panel B: Heterogeneity analyses										
B1. Heterogeneity by age										
1.D	-0.00430	0.00129	0.00389*	-0.0118***	0.000330	-0.0125***	0.00414	0.00139	0.000611	-0.00136**
	(0.00398)	(0.00660)	(0.00218)	(0.00366)	(0.000746)	(0.00242)	(0.00428)	(0.00126)	(0.00127)	(0.000616)
1.D#1.yli50	-0.00384	-0.00844**	-0.00245*	0.00852***	0.000374	0.0332***	0.0194***	-0.00514***	-0.000475	0.00104***
	(0.00236)	(0.00344)	(0.00144)	(0.00158)	(0.000767)	(0.00706)	(0.00299)	(0.000714)	(0.000620)	(0.000379)
1.D#1.alle30	0.000722	-0.00682	-0.0249***	-0.00858***	0.00222**	-0.00110	-0.0904***	0.0219***	-0.0494***	0.00114
	(0.00414)	(0.00572)	(0.00586)	(0.00210)	(0.000904)	(0.000678)	(0.00612)	(0.00193)	(0.00219)	(0.00152)
B2. Heterogeneity by gender										
1.D	-0.0119***	-0.0123**	-0.000883	-0.0114***	0.000946	-0.00248	-0.00935**	0.00180	-0.00830***	-0.00148**
	(0.00396)	(0.00597)	(0.00229)	(0.00421)	(0.00110)	(0.00231)	(0.00445)	(0.00146)	(0.00136)	(0.000591)
1.D#1.mies	0.0178***	0.0278***	0.00138	0.00309	-0.000544	0.00171	0.0196***	0.00273***	0.00564***	0.00143**
	(0.00379)	(0.00651)	(0.00163)	(0.00482)	(0.00122)	(0.00130)	(0.00314)	(0.00105)	(0.00117)	(0.000721)
B3. Heterogeneity by education level										
1.D	0.00788	0.00377	-0.000531	0.00609	0.00350***	0.00409	0.0319***	0.000599*	0.0242***	0.00205*
	(0.00498)	(0.00875)	(0.00208)	(0.00413)	(0.00120)	(0.00299)	(0.00271)	(0.000314)	(0.00133)	(0.00107)
1.D#2.koulutus	-0.00853***	-0.00162	-0.000320	-0.0119***	-0.00191*	-0.00371*	-0.0316***	-0.000502	-0.0310***	-0.00173**
	(0.00314)	(0.00556)	(0.00106)	(0.00409)	(0.00103)	(0.00208)	(0.00279)	(0.000357)	(0.00224)	(0.000782)
1.D#3.koulutus	-0.0251***	-0.0150*	0.00304	-0.0281***	-0.00473***	-0.0107***	-0.0419***	-0.000844**	-0.0409***	-0.00589***
	(0.00529)	(0.00826)	(0.00335)	(0.00728)	(0.00148)	(0.00297)	(0.00410)	(0.000347)	(0.00232)	(0.00117)
Outcome mean	0.12	0.11	0.02	0.05	0.008	0.001	0.13	0.90	0.04	0.01
N	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year.

M.3 Including general and group-specific linear time trends

Table M12: Mechanisms and heterogeneity, individual level (all workers)

	(1) earnings	(2) native earnings	(3) unemployment months	(4) long-term unemployment	(5) full-year employed	(6) outflow to education	(7) outflow out of labor force	(8) outflow to better occupation	(9) outflow to worse occupation	(10) outflow to another firm	(11) outflow to another occupation	(12) main activity: education	(13) main activity: unemployed	(14) main activity: out of labor force	(15) main activity: pensioner	(16) enrolled	(17) has degree	(18) completed degree	(19) moving
Heterogeneity analyses, linear time trends (general and group-specific trends) included as controls																			
B1. Heterogeneity by age																			
1.D	339.1**	327.0**	-0.0846***	-0.00249**	0.0133**	-0.00539***	-0.00116	-0.00724	-0.00167	-0.0137***	-0.00891	-0.00553**	-0.0104**	-0.00216	-0.00535***	-0.00274	-0.00459***	0.00251*	-0.00268**
	(150.8)	(146.9)	(0.0239)	(0.00117)	(0.00617)	(0.00200)	(0.00116)	(0.00663)	(0.00426)	(0.00475)	(0.00938)	(0.00224)	(0.00431)	(0.00156)	(0.00208)	(0.00342)	(0.00152)	(0.00140)	(0.00118)
1.D#1.yli50	-1738.4***	-1766.9***	0.0986***	0.00337***	-0.0385***	-0.00449***	-0.000720	-0.00914***	-0.00232	-0.00777***	-0.0115***	-0.00328***	0.00997***	0.000943*	0.0213**	0.0185***	-0.000225	0.000579	-0.000948*
	(291.9)	(290.4)	(0.0190)	(0.000678)	(0.00982)	(0.00168)	(0.00101)	(0.00259)	(0.00157)	(0.00289)	(0.00340)	(0.00113)	(0.00249)	(0.000553)	(0.00857)	(0.00580)	(0.000921)	(0.00109)	(0.000501)
1.D#1.alle30	352.0	349.5	0.00292	0.000366	0.0484***	0.0202**	0.00708***	-0.0154***	0.00874***	0.00315	-0.00666	0.0124	0.00270	0.00772***	0.00567***	-0.0359***	0.0140***	-0.0311***	0.00493***
	(227.1)	(234.4)	(0.00991)	(0.000549)	(0.0100)	(0.00938)	(0.00252)	(0.00531)	(0.00311)	(0.00477)	(0.00555)	(0.00847)	(0.00226)	(0.00164)	(0.00154)	(0.00716)	(0.00288)	(0.00285)	(0.00183)
B2. Heterogeneity by gender																			
1.D	-125.7	-145.7	-0.0563**	-0.00282***	0.0167**	-0.00133	0.00160	-0.0217***	-0.00330	-0.0218***	-0.0251***	-0.00459*	-0.0150***	0.000810	0.000508	-0.0193***	-0.00144	-0.00845***	-0.00203
	(190.7)	(186.6)	(0.0229)	(0.000933)	(0.00743)	(0.00220)	(0.00183)	(0.00717)	(0.00450)	(0.00515)	(0.00956)	(0.00235)	(0.00451)	(0.00148)	(0.00364)	(0.00398)	(0.00127)	(0.00161)	(0.00152)
1.D#1.mies	506.2***	485.1***	-0.0153	0.00264**	0.00177	-0.00301	-0.00373**	0.0224***	0.00731**	0.0172***	0.0297***	0.0000412	0.0184***	-0.00350**	-0.000314	0.0266***	0.00102	0.00964***	0.0000540
	(151.4)	(151.4)	(0.0330)	(0.00130)	(0.00473)	(0.00205)	(0.00168)	(0.00565)	(0.00357)	(0.00522)	(0.00740)	(0.00181)	(0.00548)	(0.00172)	(0.00265)	(0.00377)	(0.00109)	(0.00153)	(0.00112)
B3. Heterogeneity by education level																			
1.D	423.1*	393.4	-0.0458	-0.00107	0.0168**	-0.000761	0.00804	-0.0160**	0.00286	-0.00233	-0.0131	-0.00456	0.00934	0.00815***	0.00137	-0.00838*	0.00180***	0.00667***	-0.00119
	(229.1)	(240.8)	(0.0353)	(0.00187)	(0.00654)	(0.00293)	(0.00566)	(0.00776)	(0.00716)	(0.00525)	(0.0118)	(0.00360)	(0.00830)	(0.00149)	(0.00420)	(0.00491)	(0.000380)	(0.00147)	(0.00189)
1.D#2.koulutus	-344.8**	-335.4**	-0.0217	-0.000169	0.00125	0.000373	-0.00819	-0.00431	0.00343	-0.0121***	-0.000885	0.00303	-0.0145**	-0.00871***	-0.00200	0.000874	-0.00202***	-0.0153***	-0.000306
	(151.9)	(142.7)	(0.0315)	(0.00126)	(0.00488)	(0.00230)	(0.00526)	(0.00410)	(0.00424)	(0.00353)	(0.00670)	(0.00386)	(0.00730)	(0.00165)	(0.00192)	(0.00366)	(0.000430)	(0.00253)	(0.00124)
1.D#3.koulutus	-243.0	-258.3	-0.0215	-0.00224	0.0000925	-0.00661*	-0.0102*	0.0181**	-0.0176***	-0.0193***	0.000530	-0.00493	-0.0294***	-0.0117***	-0.00106	-0.00199	-0.00232***	-0.0102***	-0.00241
	(194.2)	(212.4)	(0.0380)	(0.00138)	(0.00563)	(0.00360)	(0.00571)	(0.00737)	(0.00642)	(0.00545)	(0.00978)	(0.00501)	(0.00959)	(0.00174)	(0.00300)	(0.00588)	(0.000406)	(0.00266)	(0.00197)
Outcome mean	27770.23	28013.52	0.56	0.01	0.81	0.01	0.003	0.05	0.05	0.12	0.10	0.02	0.05	0.01	0.002	0.13	0.89	0.04	0.01
N	6403800	5975170	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year.

Table M13: Mechanisms and heterogeneity, individual level (lowest salary quartile)

	(1) earnings	(2) native earnings	(3) unemployment months	(4) long-term unemployment	(5) full-year employed	(6) outflow to education	(7) outflow out of labor force	(8) outflow to better occupation	(9) outflow to worse occupation	(10) outflow to another firm	(11) outflow to another occupation	(12) main activity: education	(13) main activity: unemployed	(14) main activity: out of labor force	(15) main activity: pensioner	(16) enrolled	(17) has degree	(18) completed degree	(19) moving
Heterogeneity analyses, linear time trends (general and group-specific trends) included as controls																			
B1. Heterogeneity by age																			
1.D	310.8	316.4	-0.0746**	-0.00404*	0.0209*	-0.0102**	-0.00234	-0.0157	0.00515	-0.0154*	-0.0106	-0.0117**	-0.0125	-0.00645*	-0.00657*	-0.0178**	-0.0166***	0.00117	-0.00674**
	(285.8)	(263.3)	(0.0361)	(0.00213)	(0.0124)	(0.00481)	(0.00346)	(0.0193)	(0.00687)	(0.00888)	(0.0200)	(0.00587)	(0.00804)	(0.00358)	(0.00378)	(0.00719)	(0.00372)	(0.00362)	(0.00302)
1.D#1.yli50	-887.5***	-936.7***	0.0800**	0.00151	-0.0295**	-0.00908**	-0.00350	-0.0301***	0.0142***	-0.00747	-0.0159*	-0.00911***	0.00471	0.000107	0.0211*	0.0164	0.00446**	-0.00148	-0.00125
	(330.1)	(337.7)	(0.0323)	(0.00122)	(0.0130)	(0.00409)	(0.00248)	(0.00916)	(0.00286)	(0.00582)	(0.00926)	(0.00316)	(0.00452)	(0.00166)	(0.0121)	(0.0138)	(0.00222)	(0.00252)	(0.00103)
1.D#1.alle30	-429.4	-486.6	0.0387*	-0.00165	0.0242	0.0416**	0.00931**	0.0301***	-0.0104**	0.0216*	0.0197	0.0411**	0.00533*	0.00867***	0.00560*	-0.00347	0.0252***	-0.0122*	0.00954**
	(642.0)	(655.7)	(0.0216)	(0.00158)	(0.0191)	(0.0183)	(0.00446)	(0.0102)	(0.00452)	(0.0125)	(0.0125)	(0.0163)	(0.00314)	(0.00329)	(0.00300)	(0.0119)	(0.00655)	(0.00696)	(0.00422)
B2. Heterogeneity by gender																			
1.D	211.2	205.2	-0.0328	-0.00373*	0.0222	-0.00442	-0.000793	-0.0279	0.00808	-0.0150	-0.0198	-0.00737	-0.0101	-0.00338	-0.00152	-0.0197**	-0.00862***	-0.00479	-0.00531
	(336.3)	(319.9)	(0.0384)	(0.00209)	(0.0145)	(0.00543)	(0.00371)	(0.0179)	(0.00696)	(0.00933)	(0.0186)	(0.00507)	(0.00844)	(0.00288)	(0.00607)	(0.00857)	(0.00268)	(0.00397)	(0.00342)
1.D#1.mies	-530.6	-693.1**	-0.0461	-0.00132	-0.00440	0.000787	-0.00252	0.0445***	-0.00649	0.00994	0.0380***	0.00391	0.00194	-0.00566**	0.00411	0.0104	-0.00114	0.0103***	0.000594
	(339.1)	(305.4)	(0.0371)	(0.00165)	(0.00614)	(0.00213)	(0.00163)	(0.0141)	(0.00683)	(0.0109)	(0.0144)	(0.00242)	(0.00505)	(0.00221)	(0.00365)	(0.00642)	(0.00393)	(0.00332)	(0.00163)
B3. Heterogeneity by education level																			
1.D	437.3	395.4	0.0166	-0.00362	0.0232	-0.00426	0.00938	-0.0398**	0.0186***	-0.00167	-0.0212	-0.0102**	0.00486	0.00661**	0.000926	-0.0253***	0.00136**	0.00748*	-0.00674*
	(330.8)	(327.7)	(0.0690)	(0.00329)	(0.0148)	(0.00831)	(0.0109)	(0.0198)	(0.00578)	(0.00883)	(0.0203)	(0.00457)	(0.0168)	(0.00324)	(0.00742)	(0.00942)	(0.000569)	(0.00449)	(0.00408)
1.D#2.koulutus	-198.9	-133.2	-0.0721	-0.0000521	0.00199	0.0000387	-0.0127	0.0149	-0.00808	-0.0162***	0.00682	0.00271	-0.0186	-0.0130***	-0.00268	0.00442	-0.00178***	-0.0178***	0.00174
	(164.6)	(194.7)	(0.0586)	(0.00221)	(0.00780)	(0.00532)	(0.00931)	(0.0118)	(0.00567)	(0.00551)	(0.0109)	(0.00469)	(0.0119)	(0.00385)	(0.00294)	(0.00705)	(0.000550)	(0.00526)	(0.00173)
1.D#3.koulutus	-739.3**	-935.9**	-0.0452	-0.00213	-0.0153	-0.00263	-0.0139	0.0813***	-0.0404***	0.00119	0.0409**	0.00299	-0.0119	-0.0164**	0.00227	0.00282	-0.00178***	0.0348***	0.00290
	(329.8)	(403.6)	(0.0427)	(0.00201)	(0.0108)	(0.00400)	(0.00975)	(0.0135)	(0.00659)	(0.00748)	(0.0166)	(0.00699)	(0.0101)	(0.00676)	(0.00503)	(0.0134)	(0.000563)	(0.0110)	(0.00186)
Outcome mean	20310.11	20415.04	0.65	0.01	0.77	0.01	0.004	0.03	0.06	0.11	0.09	0.03	0.05	0.02	0.004	0.15	0.87	0.05	0.01
N	1643650	1468410	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year.

Table M14: Mechanisms and heterogeneity, individual level (top 3 salary quartiles)

	(1) earnings	(2) native earnings	(3) unemployment months	(4) long-term unemployment	(5) full-year employed	(6) outflow to education	(7) outflow out of labor force	(8) outflow to better occupation	(9) outflow to worse occupation	(10) outflow to another firm	(11) outflow to another occupation	(12) main activity: education	(13) main activity: unemployed	(14) main activity: out of labor force	(15) main activity: pensioner	(16) enrolled	(17) has degree	(18) completed degree	(19) moving
Heterogeneity analyses, linear time trends (general and group-specific trends) included as controls																			
B1. Heterogeneity by age																			
1.D	310.0**	292.7*	-0.0873***	-0.00213	0.0107**	-0.00386*	-0.000652	-0.00441	-0.00440	-0.0126**	-0.00881	-0.00369**	-0.00990*	-0.000830	-0.00461**	0.000665	-0.000223	0.00287*	-0.00131
	(149.6)	(149.5)	(0.0281)	(0.00135)	(0.00541)	(0.00199)	(0.000778)	(0.00689)	(0.00595)	(0.00579)	(0.0114)	(0.00188)	(0.00508)	(0.000949)	(0.00212)	(0.00346)	(0.000577)	(0.00156)	(0.00116)
1.D#1.yli50	-2025.7***	-2031.7***	0.103***	0.00412***	-0.0420***	-0.00299***	0.0000164	-0.00522*	-0.00482**	-0.00792**	-0.0100***	-0.00139*	0.0114***	0.00109	0.0212***	0.0203***	-0.00165***	0.00119	-0.000895
	(313.5)	(313.0)	(0.0187)	(0.000727)	(0.00947)	(0.00106)	(0.000667)	(0.00269)	(0.00191)	(0.00344)	(0.00383)	(0.000796)	(0.00236)	(0.000718)	(0.00772)	(0.00531)	(0.000511)	(0.00120)	(0.000564)
1.D#1.alle30	627.9***	623.3***	-0.00503	0.00114**	0.0569***	0.0120**	0.00601***	-0.0342***	0.0178***	-0.00333	-0.0164**	0.00152	0.00218	0.00721***	0.00560***	-0.0505***	0.0114***	-0.0376***	0.00316*
	(196.7)	(198.7)	(0.0143)	(0.000577)	(0.00878)	(0.00583)	(0.00182)	(0.00529)	(0.00272)	(0.00468)	(0.00640)	(0.00573)	(0.00272)	(0.00145)	(0.00147)	(0.00662)	(0.00217)	(0.00353)	(0.00181)
B2. Heterogeneity by gender																			
1.D	-166.0	-189.2	-0.0654**	-0.00234***	0.0149**	-0.00137	0.00185	-0.0290***	0.000220	-0.0262***	-0.0288**	-0.00476***	-0.0187***	0.00228*	0.00101	-0.0206***	0.00117	-0.0108***	-0.00121
	(183.6)	(183.3)	(0.0316)	(0.000787)	(0.00702)	(0.00155)	(0.00140)	(0.00770)	(0.00715)	(0.00670)	(0.0130)	(0.00178)	(0.00628)	(0.00137)	(0.00307)	(0.00457)	(0.00117)	(0.00186)	(0.00161)
1.D#1.mies	492.0***	494.4***	-0.00134	0.00272*	0.00226	-0.00195	-0.00285**	0.0356***	-0.00409	0.0242***	0.0315***	0.000737	0.0256***	-0.00342**	-0.000293	0.0278***	0.00219	0.0118***	0.000555
	(143.1)	(143.1)	(0.0504)	(0.00161)	(0.00533)	(0.00171)	(0.00133)	(0.00596)	(0.00600)	(0.00654)	(0.01000)	(0.00205)	(0.00848)	(0.00171)	(0.00247)	(0.00448)	(0.00139)	(0.00179)	(0.00136)
B3. Heterogeneity by education level																			
1.D	342.5	265.5	-0.0627	-0.00000638	0.0157**	-0.00219	0.00560*	-0.0205**	0.0107	-0.000239	-0.00985	-0.00773	0.0121**	0.00723***	0.00352	-0.0113*	0.00204***	0.00580***	0.000748
	(222.3)	(226.3)	(0.0402)	(0.00221)	(0.00693)	(0.00159)	(0.00311)	(0.00909)	(0.00738)	(0.00682)	(0.0136)	(0.00475)	(0.00619)	(0.00167)	(0.00282)	(0.00642)	(0.000603)	(0.00134)	(0.00172)
1.D#2.koulutus	-317.4**	-267.5*	-0.0125	-0.000737	-0.000898	0.00226	-0.00486	-0.00525	-0.000185	-0.0120***	-0.00544	0.00779*	-0.0148***	-0.00615***	-0.00363*	0.00563	-0.00214***	-0.0135***	-0.00131
	(156.7)	(155.4)	(0.0294)	(0.00121)	(0.00582)	(0.00204)	(0.00299)	(0.00479)	(0.00493)	(0.00465)	(0.00688)	(0.00461)	(0.00569)	(0.00128)	(0.00198)	(0.00394)	(0.000676)	(0.00317)	(0.00116)
1.D#3.koulutus	-218.6	-157.8	-0.00557	-0.00237	0.000678	-0.00336	-0.00649**	0.0284***	-0.0352***	-0.0251***	-0.00687	0.000423	-0.0324***	-0.00848***	-0.00311	0.00602	-0.00252***	-0.0164***	-0.00329
	(217.8)	(225.8)	(0.0390)	(0.00165)	(0.00660)	(0.00292)	(0.00320)	(0.00786)	(0.00644)	(0.00671)	(0.0109)	(0.00586)	(0.00841)	(0.00134)	(0.00250)	(0.00689)	(0.000638)	(0.00256)	(0.00206)
Outcome mean	30168.92	30403.89	0.53	0.008	0.82	0.007	0.003	0.06	0.04	0.12	0.11	0.02	0.05	0.008	0.001	0.13	0.90	0.04	0.01
N	4760150	4506760	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year.

M.4 Including only group-specific linear time trends

Table M15: Mechanisms and heterogeneity, individual level (all workers)

	(1) earnings	(2) native earnings	(3) unemployment months	(4) long-term unemployment	(5) full-year employed	(6) outflow to education	(7) outflow out of labor force	(8) outflow to better occupation	(9) outflow to worse occupation	(10) outflow to another firm	(11) outflow to another occupation	(12) main activity: education	(13) main activity: unemployed	(14) main activity: out of labor force	(15) main activity: pensioner	(16) enrolled	(17) has degree	(18) completed degree	(19) moving
Heterogeneity analyses, linear time trends (group-specific trends) included as controls																			
B1. Heterogeneity by age																			
1.D	839.5***	873.5***	-0.138***	-0.00561***	0.0375***	-0.00405**	-0.000826	-0.000728	0.00300	-0.00518	0.00227	-0.00474***	-0.0135***	-0.00177	-0.0141***	-0.0107***	0.00558**	-0.00210	-0.00221***
	(167.8)	(171.0)	(0.0246)	(0.00115)	(0.00513)	(0.00159)	(0.000525)	(0.00429)	(0.00277)	(0.00334)	(0.00547)	(0.00181)	(0.00312)	(0.00111)	(0.00256)	(0.00359)	(0.00227)	(0.00143)	(0.000557)
1.D#1.yli50	-2028.6***	-2087.4***	0.129***	0.00518***	-0.0525***	-0.00527***	-0.000914	-0.0129***	-0.00503***	-0.0127***	-0.0179***	-0.00374**	0.0117***	0.000718	0.0264***	0.0231***	-0.00612***	0.00326***	-0.00122**
	(354.8)	(341.2)	(0.0248)	(0.000835)	(0.0120)	(0.00157)	(0.000779)	(0.00271)	(0.00182)	(0.00356)	(0.00355)	(0.00178)	(0.00353)	(0.00147)	(0.00882)	(0.00647)	(0.00185)	(0.00113)	(0.000541)
1.D#1.alle30	192.3	173.0	0.0198*	0.00136**	0.0407***	0.0198**	0.00698***	-0.0175***	0.00725**	0.000451	-0.0102	0.0121	0.00367*	0.00759***	0.00846***	-0.0333***	0.0108***	-0.0296***	0.00478**
	(221.6)	(234.1)	(0.0102)	(0.000641)	(0.00977)	(0.00949)	(0.00267)	(0.00552)	(0.00335)	(0.00480)	(0.00628)	(0.00814)	(0.00217)	(0.00136)	(0.00188)	(0.00732)	(0.00232)	(0.00284)	(0.00203)
B2. Heterogeneity by gender																			
1.D	-564.4***	-573.6***	-0.0974***	-0.00488***	0.0197***	0.000271	0.00128	-0.0116***	0.000794	-0.0105***	-0.0108**	-0.000779	-0.0135***	0.000638	-0.000223	-0.00795**	0.00428	-0.00752***	-0.000888
	(152.7)	(151.3)	(0.0235)	(0.00101)	(0.00520)	(0.000950)	(0.000920)	(0.00440)	(0.00297)	(0.00348)	(0.00514)	(0.00235)	(0.00339)	(0.00152)	(0.00232)	(0.00376)	(0.00273)	(0.00146)	(0.000582)
1.D#1.mies	698.6***	672.6***	0.00270	0.00355***	0.000472	-0.00371**	-0.00359***	0.0179***	0.00551	0.0123**	0.0235***	-0.00163	0.0178***	-0.00342	0.00000656	0.0216***	-0.00149	0.00923***	-0.000445
	(160.5)	(152.0)	(0.0320)	(0.00137)	(0.00536)	(0.00181)	(0.00136)	(0.00551)	(0.00358)	(0.00513)	(0.00731)	(0.00230)	(0.00553)	(0.00216)	(0.00229)	(0.00383)	(0.00174)	(0.00152)	(0.000999)
B3. Heterogeneity by education level																			
1.D	423.1*	393.4	-0.0458	-0.00107	0.0168**	-0.000761	0.00804	-0.0160**	0.00286	-0.00233	-0.0131	-0.00456	0.00934	0.00815***	0.00137	-0.00838*	0.00180***	0.00667***	-0.00119
	(229.1)	(240.8)	(0.0353)	(0.00187)	(0.00654)	(0.00293)	(0.00566)	(0.00776)	(0.00716)	(0.00525)	(0.0118)	(0.00360)	(0.00830)	(0.00149)	(0.00420)	(0.00491)	(0.000380)	(0.00147)	(0.00189)
1.D#2.koulutus	-344.8**	-335.4**	-0.0217	-0.000169	0.00125	0.000373	-0.00819	-0.00431	0.00343	-0.0121***	-0.000885	0.00303	-0.0145**	-0.00871***	-0.00200	0.000874	-0.00202***	-0.0153***	-0.000306
	(151.9)	(142.7)	(0.0315)	(0.00126)	(0.00488)	(0.00230)	(0.00526)	(0.00410)	(0.00424)	(0.00353)	(0.00670)	(0.00386)	(0.00730)	(0.00165)	(0.00192)	(0.00366)	(0.000430)	(0.00253)	(0.00124)
1.D#3.koulutus	-243.0	-258.3	-0.0215	-0.00224	0.0000925	-0.00661*	-0.0102*	0.0181**	-0.0176***	-0.0193***	0.000530	-0.00493	-0.0294***	-0.0117***	-0.00106	-0.00199	-0.00232***	-0.0102***	-0.00241
	(194.2)	(212.4)	(0.0380)	(0.00138)	(0.00563)	(0.00360)	(0.00571)	(0.00737)	(0.00642)	(0.00545)	(0.00978)	(0.00501)	(0.00959)	(0.00174)	(0.00300)	(0.00588)	(0.000406)	(0.00266)	(0.00197)
Outcome mean	27770.23	28013.52	0.56	0.01	0.81	0.01	0.003	0.05	0.05	0.12	0.10	0.02	0.05	0.01	0.002	0.13	0.89	0.04	0.01
N	6403800	5975170	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800	6403800

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year.

Table M16: Mechanisms and heterogeneity, individual level (lowest salary quartile)

	(1) earnings	(2) native earnings	(3) unemployment months	(4) long-term unemployment	(5) full-year employed	(6) outflow to education	(7) outflow out of labor force	(8) outflow to better occupation	(9) outflow to worse occupation	(10) outflow to another firm	(11) outflow to another occupation	(12) main activity: education	(13) main activity: unemployed	(14) main activity: out of labor force	(15) main activity: pensioner	(16) enrolled	(17) has degree	(18) completed degree	(19) moving
Heterogeneity analyses, linear time trends (group-specific trends) included as controls																			
B1. Heterogeneity by age																			
1.D	534.0**	635.9***	-0.0922	-0.00427**	0.0384***	-0.00897**	-0.00216	-0.0120	0.00775	-0.0108*	-0.00425	-0.0121***	-0.0111	-0.00583**	-0.0116***	-0.0255***	0.0101*	-0.000417	-0.00293**
	(257.2)	(239.7)	(0.0612)	(0.00194)	(0.00990)	(0.00355)	(0.00139)	(0.0110)	(0.00554)	(0.00621)	(0.0115)	(0.00389)	(0.00756)	(0.00264)	(0.00339)	(0.00782)	(0.00580)	(0.00427)	(0.00116)
1.D#1.yli50	-1020.8**	-1133.0**	0.0905*	0.00164	-0.0400**	-0.00983***	-0.00361**	-0.0324***	0.0126***	-0.0102	-0.0197***	-0.00889***	0.00384	-0.000261	0.0241*	0.0211	-0.0115***	-0.000534	-0.00353***
	(500.4)	(467.7)	(0.0509)	(0.00217)	(0.0187)	(0.00278)	(0.00149)	(0.00580)	(0.00311)	(0.00648)	(0.00636)	(0.00299)	(0.00672)	(0.00298)	(0.0130)	(0.0162)	(0.00431)	(0.00298)	(0.00125)
1.D#1.alle30	-512.7	-609.2	0.0452	-0.00156	0.0177	0.0411**	0.00924*	0.0287**	-0.0114**	0.0199	0.0173	0.0412**	0.00478	0.00844***	0.00746**	-0.000586	0.0153***	-0.0116	0.00812*
	(637.6)	(637.9)	(0.0347)	(0.00199)	(0.0202)	(0.0196)	(0.00520)	(0.0128)	(0.00457)	(0.0131)	(0.0151)	(0.0166)	(0.00432)	(0.00210)	(0.00291)	(0.0132)	(0.00578)	(0.00754)	(0.00480)
B2. Heterogeneity by gender																			
1.D	-681.6***	-667.7***	-0.0454	-0.00259	0.0136	-0.00196	-0.000608	-0.0198*	0.00865	-0.00915	-0.0111	-0.00401	-0.00761	-0.00311	0.00450	0.000797	0.00705	-0.00229	-0.000599
	(242.0)	(226.1)	(0.0602)	(0.00201)	(0.00919)	(0.00151)	(0.00168)	(0.0109)	(0.00564)	(0.00660)	(0.0115)	(0.00261)	(0.00732)	(0.00246)	(0.00501)	(0.00556)	(0.00600)	(0.00368)	(0.00129)
1.D#1.mies	-182.1	-363.3	-0.0412	-0.00176	-0.00103	-0.000174	-0.00259	0.0413***	-0.00671	0.00765	0.0346**	0.00260	0.000977	-0.00576**	0.00176	0.00242	-0.00725*	0.00928***	-0.00124
	(379.1)	(321.3)	(0.0475)	(0.00220)	(0.00873)	(0.00307)	(0.00159)	(0.0156)	(0.00666)	(0.0115)	(0.0164)	(0.00362)	(0.00586)	(0.00283)	(0.00423)	(0.00729)	(0.00404)	(0.00340)	(0.00203)
B3. Heterogeneity by education level																			
1.D	437.3	395.4	0.0166	-0.00362	0.0232	-0.00426	0.00938	-0.0398**	0.0186***	-0.00167	-0.0212	-0.0102**	0.00486	0.00661**	0.000926	-0.0253***	0.00136**	0.00748*	-0.00674*
	(330.8)	(327.7)	(0.0690)	(0.00329)	(0.0148)	(0.00831)	(0.0109)	(0.0198)	(0.00578)	(0.00883)	(0.0203)	(0.00457)	(0.0168)	(0.00324)	(0.00742)	(0.00942)	(0.000569)	(0.00449)	(0.00408)
1.D#2.koulutus	-198.9	-133.2	-0.0721	-0.0000521	0.00199	0.0000387	-0.0127	0.0149	-0.00808	-0.0162***	0.00682	0.00271	-0.0186	-0.0130***	-0.00268	0.00442	-0.00178***	-0.0178***	0.00174
	(164.6)	(194.7)	(0.0586)	(0.00221)	(0.00780)	(0.00532)	(0.00931)	(0.0118)	(0.00567)	(0.00551)	(0.0109)	(0.00469)	(0.0119)	(0.00385)	(0.00294)	(0.00705)	(0.000550)	(0.00526)	(0.00173)
1.D#3.koulutus	-739.3**	-935.9**	-0.0452	-0.00213	-0.0153	-0.00263	-0.0139	0.0813***	-0.0404***	0.00119	0.0409**	0.00299	-0.0119	-0.0164**	0.00227	0.00282	-0.00178***	0.0348***	0.00290
	(329.8)	(403.6)	(0.0427)	(0.00201)	(0.0108)	(0.00400)	(0.00975)	(0.0135)	(0.00659)	(0.00748)	(0.0166)	(0.00699)	(0.0101)	(0.00676)	(0.00503)	(0.0134)	(0.000563)	(0.0110)	(0.00186)
Outcome mean	20310.11	20415.04	0.65	0.01	0.77	0.01	0.004	0.03	0.06	0.11	0.09	0.03	0.05	0.02	0.004	0.15	0.87	0.05	0.01
N	1643650	1468410	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650	1643650

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year.

Table M17: Mechanisms and heterogeneity, individual level (top 3 salary quartiles)

	(1) earnings	(2) native earnings	(3) unemployment months	(4) long-term unemployment	(5) full-year employed	(6) outflow to education	(7) outflow out of labor force	(8) outflow to better occupation	(9) outflow to worse occupation	(10) outflow to another firm	(11) outflow to another occupation	(12) main activity: education	(13) main activity: unemployed	(14) main activity: out of labor force	(15) main activity: pensioner	(16) enrolled	(17) has degree	(18) completed degree	(19) moving
Heterogeneity analyses, linear time trends (group-specific trends) included as controls																			
B1. Heterogeneity by age																			
1.D	877.7***	888.7***	-0.155***	-0.00625***	0.0374***	-0.00216**	-0.000260	0.00379	0.000774	-0.00257	0.00456	-0.00225	-0.0147***	-0.000855	-0.0148***	-0.00578	0.00416***	-0.00258*	-0.00187***
	(178.9)	(182.6)	(0.0299)	(0.00133)	(0.00510)	(0.000900)	(0.000406)	(0.00438)	(0.00317)	(0.00405)	(0.00659)	(0.00191)	(0.00381)	(0.000725)	(0.00278)	(0.00413)	(0.00131)	(0.00134)	(0.000624)
1.D#1.yli50	-2350.4***	-2374.8***	0.142***	0.00648***	-0.0573***	-0.00396***	-0.000208	-0.00992***	-0.00778***	-0.0137***	-0.0177***	-0.00221	0.0142***	0.00110	0.0271***	0.0240***	-0.00416***	0.00430***	-0.000574
	(348.7)	(344.8)	(0.0218)	(0.00103)	(0.0109)	(0.00137)	(0.000698)	(0.00301)	(0.00215)	(0.00407)	(0.00412)	(0.00196)	(0.00309)	(0.00126)	(0.00774)	(0.00630)	(0.000963)	(0.00119)	(0.000552)
1.D#1.alle30	459.0**	443.2**	0.0151	0.00237***	0.0489***	0.0114**	0.00589***	-0.0367***	0.0162***	-0.00633	-0.0204***	0.00109	0.00361	0.00721***	0.00864***	-0.0486***	0.0101***	-0.0360***	0.00333*
	(199.7)	(206.2)	(0.0154)	(0.000821)	(0.00840)	(0.00561)	(0.00185)	(0.00569)	(0.00326)	(0.00482)	(0.00745)	(0.00519)	(0.00265)	(0.00141)	(0.00189)	(0.00638)	(0.00206)	(0.00349)	(0.00197)
B2. Heterogeneity by gender																			
1.D	-485.7***	-504.8***	-0.119***	-0.00582***	0.0222***	0.000567	0.00159**	-0.0142***	0.00305	-0.0119***	-0.0111*	-0.000143	-0.0171***	0.00162	-0.00237	-0.0102**	0.00205	-0.00979***	-0.00126*
	(159.6)	(160.9)	(0.0324)	(0.00124)	(0.00577)	(0.000944)	(0.000809)	(0.00429)	(0.00341)	(0.00440)	(0.00655)	(0.00275)	(0.00470)	(0.00128)	(0.00218)	(0.00488)	(0.00162)	(0.00145)	(0.000657)
1.D#1.mies	642.1***	642.4***	0.0241	0.00435**	-0.00117	-0.00286	-0.00273**	0.0287***	-0.00541	0.0175***	0.0232**	-0.00143	0.0249***	-0.00311	0.00129	0.0229***	0.00178	0.0113***	0.000579
	(130.6)	(128.9)	(0.0491)	(0.00193)	(0.00540)	(0.00187)	(0.00113)	(0.00562)	(0.00520)	(0.00627)	(0.00908)	(0.00298)	(0.00814)	(0.00189)	(0.00180)	(0.00465)	(0.00162)	(0.00165)	(0.00114)
B3. Heterogeneity by education level																			
1.D	342.5	265.5	-0.0627	-0.00000638	0.0157**	-0.00219	0.00560*	-0.0205**	0.0107	-0.000239	-0.00985	-0.00773	0.0121**	0.00723***	0.00352	-0.0113*	0.00204***	0.00580***	0.000748
	(222.3)	(226.3)	(0.0402)	(0.00221)	(0.00693)	(0.00159)	(0.00311)	(0.00909)	(0.00738)	(0.00682)	(0.0136)	(0.00475)	(0.00619)	(0.00167)	(0.00282)	(0.00642)	(0.000603)	(0.00134)	(0.00172)
1.D#2.koulutus	-317.4**	-267.5*	-0.0125	-0.000737	-0.000898	0.00226	-0.00486	-0.00525	-0.000185	-0.0120***	-0.00544	0.00779*	-0.0148***	-0.00615***	-0.00363*	0.00563	-0.00214***	-0.0135***	-0.00131
	(156.7)	(155.4)	(0.0294)	(0.00121)	(0.00582)	(0.00204)	(0.00299)	(0.00479)	(0.00493)	(0.00465)	(0.00688)	(0.00461)	(0.00569)	(0.00128)	(0.00198)	(0.00394)	(0.000676)	(0.00317)	(0.00116)
1.D#3.koulutus	-218.6	-157.8	-0.00557	-0.00237	0.000678	-0.00336	-0.00649**	0.0284***	-0.0352***	-0.0251***	-0.00687	0.000423	-0.0324***	-0.00848***	-0.00311	0.00602	-0.00252***	-0.0164***	-0.00329
	(217.8)	(225.8)	(0.0390)	(0.00165)	(0.00660)	(0.00292)	(0.00320)	(0.00786)	(0.00644)	(0.00671)	(0.0109)	(0.00586)	(0.00841)	(0.00134)	(0.00250)	(0.00689)	(0.000638)	(0.00256)	(0.00206)
Outcome mean	30168.92	30403.89	0.53	0.008	0.82	0.007	0.003	0.06	0.04	0.12	0.11	0.02	0.05	0.008	0.001	0.13	0.90	0.04	0.01
N	4760150	4506760	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150	4760150

Notes. The table shows TWFE estimates. Standard errors are clustered by occupation-region. Significance levels: * p <0.10, ** p<0.05, *** p<0.01. Outcome means are calculated for the treatment group in period -2. Meaning of different education variables: *outflow to education* indicates whether the main activity of the individual changes to participation in education. When main activity is determined by Statistics Finland, being employed overrules being in education. *Main activity: education* indicates whether the main activity of an individual is participation in education. *Enrolled* indicates whether an individual is enrolled in an educational institution. *Has degree* indicates whether an indivual has a degree of any kind, and *completed degree* indicates whether a degree was completed during a year.

N Online Appendix: Collective agreement data and analyses

N.1 Linking collective agreement data to administrative data

In order to link the collective bargaining data to administrative datasets, we first assign each contract that we have information on a specific contract number. Each of these contracts are then matched to workers in the administrative data using the linking code described below:

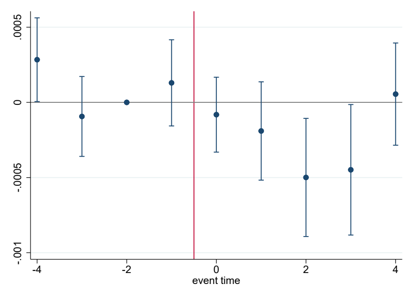
1. Linking code

- Assigns each individual one contract number. The same contact usually has raises for different years, but sometimes we need to take into account the year when matching contracts, if the same criteria apply to many contracts that exist in different years.
  - Specifies which contract number (`sopimusnumero`) applies to the individual on an annual basis
  - Examples:
    - `replace sopimusnro=93 if (Yrtol=="42110" | Yrtol=="42120" | Yrtol=="42130" | Yrtol == "42210" | Yrtol == "42910" | Yrtol == "42991" | Yrtol == "42999" | Yrtol == "43110" | Yrtol == "43120" | Yrtol == "43991") & (sopala == "4200")`
    - `replace sopimusnro=105 if (Yrtol=="10510" | Yrtol=="10520" | Yrtol==" " | Yrtol==" " | Yrtol==" " | Yrtol==" " | Yrtol=="" | Yrtol=="" | Yrtol=="" | Yrtol=="") & (Isco4 == "2141" | Isco4 == "3115" | Isco4 == "3116" | Isco4 == "3139" | Isco4 == "7513" | Isco4 == "7515" | Isco4 == "3323" | Isco4 == "3324")`
    - `replace sopimusnro=101 if (Yrtol=="55101" | Yrtol=="55109" | Yrtol=="55201" | Yrtol=="55209" | Yrtol=="55300" | Yrtol=="55901" | Yrtol=="55902" | Yrtol=="55903" | Yrtol=="55909" | Yrtol=="56101" | Yrtol=="56102" | Yrtol=="56210" | Yrtol=="56290" | Yrtol=="56301" | Yrtol=="56302") & (Isco5 == "51202" | Isco4 == "3434") & vuosi >= 2010`
- The correct contract is identified using the key which utilized the following variables: `sopala` (contract field), `Yrtol` (industry), `Isco1, Isco2, Isco3, Isco4, Isco5` (occupation codes at different levels), `vuosi` (year), and indicator variables for different types of workers ("työntekijä", "toimihenkilö", "ylempi toimihenkilö").
  - The key is based on data previously collected by Annaliina Kotilainen, and augmented in 2024 by Veera Nippala, Jeremias Nieminen & Sanni Kiviholma

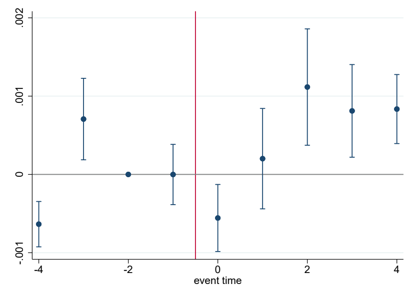
Once we have matched individuals to contracts, we merge the data on raises that have been granted in these different contracts. The data on raises originally consisted of all raises on separate lines, but we transform it into a format that can be merged into the register data which containts the contract identifier (as decribed above). The final salary increase data (which is then linked to the administrative data) is expressed in the following format (i.e., one row per contract number x year):

Year	Contract number	Global	Local
2014	1	3.0%	2.5%
2015	1	2.9%	1.7%
2016	2	3.2%	2.7%
2015	2	2.6%	1.9%
...	...	...	...

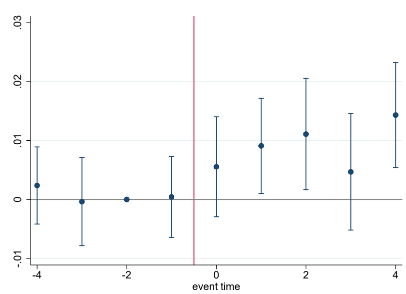
N.2 Event studies for collective bargaining estimates



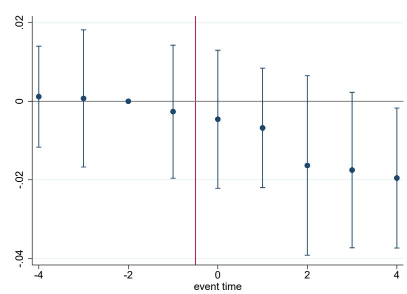
(a) Collective bargaining salary increase, top-3 q



(b) Collective bargaining salary increase, bottom q



(c) Wage drift, top-3 q



(d) Wage drift, bottom q

Figure N5: Effect on collective bargaining, occupation-region level estimates

O Online Appendix: Individual level balance table after matching

	mean, C	mean, T	difference, T-C
male	0.479	0.412	-0.068***
	(0.500)	(0.492)	(0.001)
employment months, year -1	11.321	11.226	-0.095***
	(1.986)	(2.090)	(0.005)
earnings, year -1	29,248.936	28,192.441	-1,056.494***
	(16,323.492)	(13,803.243)	(33.924)
age, year -1	41.006	40.832	-0.174***
	(12.861)	(12.630)	(0.029)
Observations	396,071	396,071	792,142

Table O18: Individual level balance table after matching

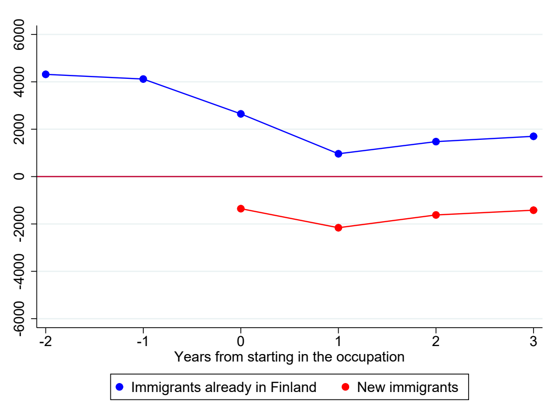
Notes. Table shows covariate balance between the individual level treatment and control groups after matching.

P Online Appendix: Net tranfers separately for top 3 quartiles and the bottom quartile

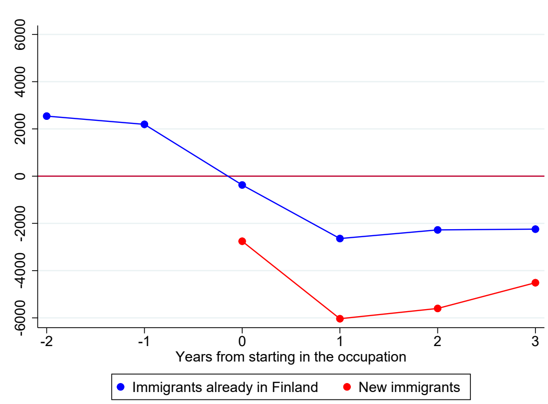
Table P19: Taxes, transfers, and the number of workers, pooled occupation-region level estimates

	Number of Workers			Taxes			Transfers			Net transfers		
	Native	Non-EU	EU	Native	Non-EU	EU	Native	Non-EU	EU	Native	Non-EU	EU
	Occupation-region level estimates											
Panel A: Lowest quartile, pooled estimate												
Treatment effect	-51.61	28.04	14.55	1.24	16.88	-70.46	372.73***	-30.50	50.96	371.49	-47.39	121.42
	(65.64)	(23.20)	(17.01)	(770.34)	(181.16)	(294.66)	(77.23)	(196.89)	(208.14)	(728.83)	(196.89)	(339.56)
Outcome mean	682.6	36.65	6.2	5728.8	2722.7	3696.6	2712.3	2277.7	2829.6	-3016.5	-445.0	-867.0
Panel B: Top 3 quartiles, pooled estimate												
Treatment effect	-0.32	10.23**	6.17***	-112.31*	77.64	404.89	-176.75***	-37.06	89.81	-64.44	-114.70	-315.08
	(16.29)	(4.13)	(2.11)	(67.11)	(432.09)	(257.73)	(35.52)	(203.18)	(188.85)	(83.45)	(458.71)	(368.44)
Outcome mean	564.7	9.1	3.5	12603.5	6768.0	8984.0	2049.5	2606.0	1924.9	-10553.9	-4162.0	-7059.1
Panel C: Lowest quartile, year +5 estimate												
Treatment effect	-80.16	41.29	18.60	-1190.54***	79.06	-401.69	500.20***	-104.97	214.81	1690.73***	-184.03	616.50
	(89.95)	(31.80)	(20.32)	(273.33)	(205.40)	(378.12)	(160.60)	(228.58)	(511.81)	(303.45)	(281.95)	630.87
Outcome mean	682.6	36.65	6.2	5728.8	2722.7	3696.6	2712.3	2277.7	2829.6	-3016.5	-445.0	-867.0
Panel D: Top 3 quartiles, year +5 estimate												
Treatment effect	20.69	21.77*	8.55***	-327.5	-65.03	302.90	-235.19***	313.07	87.71	92.34	378.09	-215.19
	(34.59)	(12.32)	(2.62)	(143.71)	(703.81)	(480.20)	(69.26)	(305.87)	(358.83)	(167.66)	(753.84)	(568.50)
Outcome mean	564.7	9.1	3.5	12603.5	6768.0	8984.0	2049.5	2606.0	1924.9	-10553.9	-4162.0	-7059.1
	Individual level estimates											
Panel E: Lowest quartile, pooled estimate												
Treatment effect	—	—	—	-195.5**	-238.9*	-412.4**	-99.12	322.6	479.3*	96.42	561.5	891.7**
				(84.51)	(136.8)	(176.4)	(104.1)	(317.8)	(278.9)	(132.4)	(388.5)	(371.5)
Outcome mean				5517.4	3268.5	4125.4	3466.3	4004.1	2635.0	-2051.643	735.678	-1490.445
Panel F: Top 3 quartiles, pooled estimate												
Treatment effect	—	—	—	-460.0***	-522.7**	358.6*	-143.4**	147.4	-153.6	316.6***	670.1*	-512.2*
				(78.41)	(208.0)	(205.3)	(64.31)	(202.2)	(140.6)	(107.6)	(351.6)	(282.3)
Outcome mean				8485.9	4862.1	7143.4	2634.4	3450.8	2283.0	-5851.465	-1411.275	-4860.451
Panel G: Lowest quartile, year +5 estimate												
Treatment effect	—	—	—	-284.8**	-462.6**	-768.6***	-219.2	864.8*	-129.3	65.60	1327.4**	639.2
				(138.6)	(204.6)	(258.5)	(185.8)	(476.1)	(461.9)	(221.5)	(559.7)	(541.8)
Outcome mean				5517.4	3268.5	4125.4	3466.3	4004.1	2635.0	-2051.643	735.678	-1490.445
Panel H: Top 3 quartiles, year +5 estimate												
Treatment effect	—	—	—	-1181.4***	-765.7*	94.81	-225.2**	103.8	-76.65	956.2***	869.5	-171.5
				(170.9)	(432.3)	(631.3)	(89.00)	(341.7)	(314.9)	(205.5)	(581.9)	(760.4)
Outcome mean				8485.9	4862.1	7143.4	2634.4	3450.8	2283.0	-5851.465	-1411.275	-4860.451

Notes. The table shows Callaway & Sant'Anna ATT estimates with standard errors in parentheses. Significance levels: (*) 0.1, (**) 0.05, (***) 0.01.

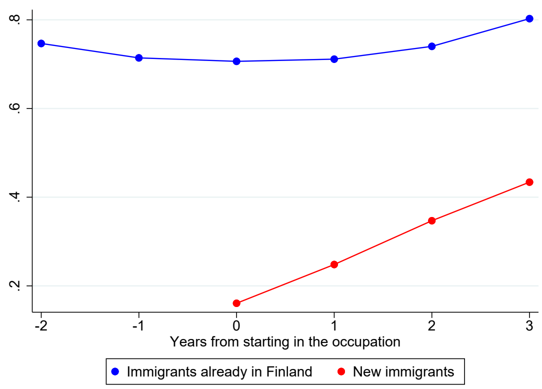


(a) Net transfers (transfers-taxes), lowest quartile

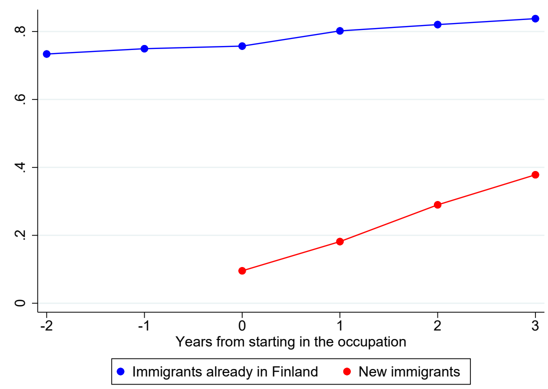


(b) Net transfers (transfers-taxes), top 3 quartiles

Figure P6: Average net transfers when non-EU immigrants start in a new occupation

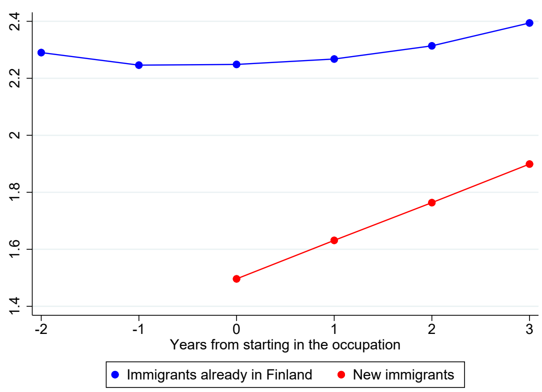


(a) Number of children , lowest quartile

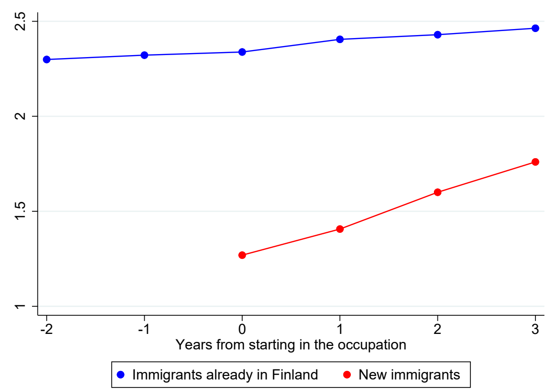


(b) Number of children, top 3 quartiles

Figure P7: Number of children when non-EU immigrants start in a new occupation



(a) Size of family, lowest quartile

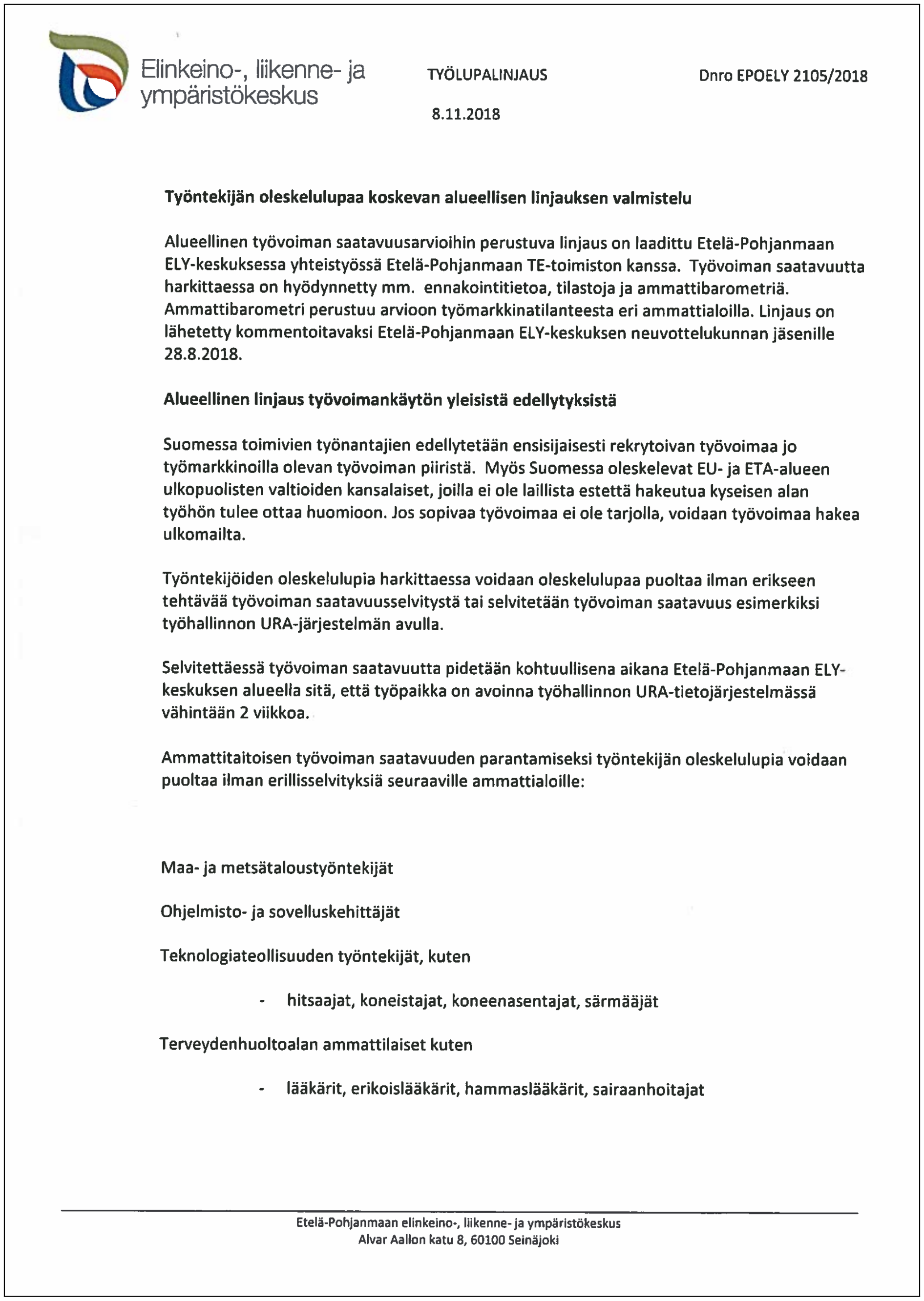


(b) Size of family, top 3 quartiles

Figure P8: Size of family when non-EU immigrants start in a new occupation

Q Online Appendix: Examples of pdf files used when collecting exemption data

Q.1 Example 1: No occupational codes



Elinkeino-, liikenne- ja ympäristökeskus

TYÖLUPALINJAUS

8.11.2018

Dnro EPOELY 2105/2018

Työntekijän oleskelulupaa koskevan alueellisen linjauksen valmistelu

Alueellinen työvoiman saatavuusarvioihin perustuva linjaus on laadittu Etelä-Pohjanmaan ELY-keskuksessa yhteistyössä Etelä-Pohjanmaan TE-toimiston kanssa. Työvoiman saatavuutta harkittaessa on hyödynnetty mm. ennakointitietoa, tilastoja ja ammattibarometriä. Ammattibarometri perustuu arvioon työmarkkinatilanteesta eri ammattialoilla. Linjaus on lähetetty kommentoitavaksi Etelä-Pohjanmaan ELY-keskuksen neuvottelukunnan jäsenille 28.8.2018.

Alueellinen linjaus työvoimankäytön yleisistä edellytyksistä

Suomessa toimivien työnantajien edellytetään ensisijaisesti rekrytoivan työvoimaa jo työmarkkinoilla olevan työvoiman piiristä. Myös Suomessa oleskelevat EU- ja ETA-alueen ulkopuolisten valtioiden kansalaiset, joilla ei ole laillista estettä hakeutua kyseisen alan työhön tulee ottaa huomioon. Jos sopivaa työvoimaa ei ole tarjolla, voidaan työvoimaa hakea ulkomailta.

Työntekijöiden oleskelulupia harkittaessa voidaan oleskelulupaa puoltaa ilman erikseen tehtävää työvoiman saatavuusselvitystä tai selvitetään työvoiman saatavuus esimerkiksi työhallinnon URA-järjestelmän avulla.

Selvitettäessä työvoiman saatavuutta pidetään kohtuullisena aikana Etelä-Pohjanmaan ELY-keskuksen alueella sitä, että työpaikka on avoinna työhallinnon URA-tietojärjestelmässä vähintään 2 viikkoa.

Ammattitaitoisen työvoiman saatavuuden parantamiseksi työntekijän oleskelulupia voidaan puoltaa ilman erillisselvityksiä seuraaville ammattialoille:

Maa- ja metsätaloustyöntekijät

Ohjelmisto- ja sovelluskehittäjät

Teknologiateollisuuden työntekijät, kuten

- hitsaajat, koneistajat, koneenasentajat, särmääjät

Terveydenhuoltoalan ammattilaiset kuten

- lääkärit, erikoislääkärit, hammaslääkärit, sairaanhoitajat

Etelä-Pohjanmaan elinkeino-, liikenne- ja ympäristökeskus
Alvar Aallon katu 8, 60100 Seinäjoki

Figure Q9: Example 1 (No occupational codes)

Q.2 Example 2: Exemptions determined at 1-digit level

2(2)

Ammattiala	Ammattiluokka	Ammattinimike ja poikkeukset
1 Johtajat	kaikki ammattiluokat	kaikki ammattinimikkeet
2 Erityisasiantuntijat	kaikki ammattiluokat	kaikki ammattinimikkeet
3 Asiantuntijat	kaikki ammattiluokat	kaikki ammattinimikkeet
4 Toimisto- ja asiakaspalvelutyöntekijät	kaikki ammattiluokat	kaikki ammattinimikkeet
5 Palvelu- ja myyntityöntekijät	kaikki ammattiluokat	kaikki ammattinimikkeet
6 Maanviljelijät-, metsätyöntekijät ym.	kaikki ammattiluokat	kaikki ammattinimikkeet
7 Rakennus-, korjaus- ja valmistustyöntekijät	kaikki ammattiluokat	kaikki ammattinimikkeet, paitsi 7114 Betonirakentajat ja raudoittajat
8 Prosessi- ja kuljetustyöntekijät	kaikki ammattiluokat	kaikki ammattinimikkeet
9 Muut työntekijät	kaikki ammattiluokat	kaikki ammattinimikkeet
X Tuntematon	kaikki ammattiluokat	kaikki ammattinimikkeet

4) **Lisätietoja työntekijän oleskeluluvasta ja hakemusprosessista**

Maahanmuuttoviraston verkkosivut:
www.migri.fi

TE-toimiston työlupapalvelut, valtakunnallinen verkkosivu:
https://www.te-palvelut.fi/te/fi/tyonantajalle/loyda_tyontekija/tyolupapalvelut/index.html

Ammattialojen ISCO-luokitus
https://www.tilastokeskus.fi/fi/luokitukset/ammatti/

5) Alueellisen linjauksen voimassaoloaika: *30.06.2022* asti.

Lisätietoja:

Kaakkois-Suomen työ- ja elinkeinotoimisto, Työlupa-asiat
PL 1010, 45101 Kouvola
Sähköposti: tyolupapalvelut: kaakkois-suomi@te-toimisto.fi
Puhelinpalvelu: arkisin klo 9-11, puh. 0295 042 006
http://toimistot.te-palvelut.fi/kaakkois-suomi/tyolupa-asiat

Pohjois-Karjalan ELY-keskus, maahanmuuttoasiantuntija [redacted]

Figure Q10: Example 2 (Exemptions determined at 1-digit level)

Q.3 Example 3: Some occupation codes and some entire industries

henkilöitä ja tarvetta on myös kokoaikaisille työntekijöille ko. toimialoilla. Myös metsätaloudessa on pulaa erityisesti metsätalouskoneiden kuljettajista. Pohjois-Karjalan ELY-keskus katsoo, että yllämainituilla toimialoilla osaavan työvoiman saatavuus on vaikeutunut. Osaavan työvoiman saatavuuden parantamiseksi **Pohjois-Karjalan alueella työntekijän oleskelulupaa voidaan puoltaa ilman erillisselvitystä työvoiman saatavuudesta seuraavilla ammattialoilla tai työtehtävissä:**

1) Sosiaali- ja terveysala kokonaan
2) Tietotekniikka/ICT-ala kokonaan
3) Maatalous- ja puutarha-ala
   - 921 Maa-, metsä- ja kalatalouden avustavat työntekijät
4) Metsätalous
   - 8341 Maa- ja metsätaloustyökoneiden kuljettajat
5) Rakennusala
   711 Rakennustyöntekijät ym.
6) Metalliteollisuus
   - 72 Konepaja- ja valimotyöntekijät sekä asentajat ja korjaajat
7) Muoviteollisuus
   - 814 Kumi-, muovi- ja paperituotteiden valmistuksen prosessityöntekijät
8) Ravintola-ala
   - 3434 keittiöpäälliköt
   - 512 ravintola- ja suurtaloustyöntekijät

4. Työntekijän oleskelulupahakemusten käsittely

Pohjois-Karjalan ELY-keskuksen alueella työntekijän oleskelulupahakemuksien käsittely on keskitetty Kaakkois-Suomen TE-toimiston työlupayksikköön Lappeenrannassa, jossa annetaan osaratkaisu. Osaratkaisua harkittaessa otetaan huomioon Ulkomaalaislain 301/2004 73§:n mukaisesti **työvoiman saatavuus, työntekijän ammattitaito ja pätevyys tehtävään, työnantajan edellytykset suoriutua työnantajavelvollisuuksista sekä muiden työsuhteen ehdoista säädettyjen edellytysten täyttyminen.**

Lopullisen päätöksen työntekijän oleskeluluvasta tekee Maahanmuuttovirasto.

5. Alueellisen linjauksen voimassaolo

Tämän päätöksen mukainen alueellinen linjaus tulee voimaan allekirjoituspäivänä ja se on voimassa toistaiseksi. Alueellisen linjauksen ajantasaisuutta ja päivitystarvetta arvioidaan vähintään kerran vuodessa.

6. **Lisätietoja**

Kaakkois-Suomen työ- ja elinkeinotoimisto, Työlupa-asiat
PL 1010, 45101 Kouvola
Sähköposti: tyolupapalvelut.kaakkois-suomi@te-toimisto.fi

Figure Q11: Example 3 (Some occupation codes and some entire industries)

Q.4 Example 4: List of specific occupation codes exempted

2(3)

Ammattiala	Ammattiluokka	Ammattinimike ja poikkeukset
3 Asiantuntijat	32 Terveydenhuollon asiantuntijat	32211 Sairaanhoitajat
5 Palvelu- ja myyntityöntekijät	53 Hoivapalvelun ja terveydenhuollon työntekijät	5321 Lähihoitajat (kaikki ammattinimikkeet)
6 Maanviljelijät, metsätyöntekijät ym.	61 Maanviljelijät ja eläintenkasvattajat ym.	61112 Peltoviljelytyönjohtajat ja -työntekijät 61132 Puutarha- ja kasvihuonetyönjohtajat ja -työntekijät 61214 Maatalouslomittajat
7 Rakennus-, korjaus- ja valmistustyöntekijät	72 Konepaja- ja valimotyöntekijät sekä asentajat ja korjaajat	7212 Hitsaajat ja kaasuleikkaajat 7223 Koneenasettajat ja koneistajat
9 Muut työntekijät	91 Siivoojat, kotiapulaiset ja muut puhdistustyöntekijät	911 Koti-, hotelli- ja toimistosiivoojat ym. (kaikki ammattinimikkeet, pois lukien 91123 Sairaala- ja laitosapulaiset ja 91124 Päiväkotiapulaiset)

4) Lisätietoja työntekijän oleskeluluvasta ja hakemusprosessista

Maahanmuuttoviraston verkkosivut:
www.migri.fi
TE-toimiston työlupapalvelut, valtakunnallinen verkkosivu:
https://www.te-palvelut.fi/te/fi/tyonantajalle/loyda_tyontekija/tyolupapalvelut/index.html
Ammattialojen ISCO-luokitus
https://www.tilastokeskus.fi/fi/luokitukset/ammatti/

5) Alueellisen linjauksen voimassaoloaika: 31.12.2021 asti

Lisätietoja

Pohjois-Savon ELY-keskus: Maahanmuuttopäällikkö [redacted]
ja ylitarkastaja [redacted] (etunimi.sukunimi@ely-keskus.fi)

Kaakkois-Suomen TE-toimisto: www.te-palvelut.fi (Paikalliset TE-palvelut)

Kaakkois-Suomen työ- ja elinkeinotoimisto
Työntekijän oleskelulupa-asiat
Villimiehenkatu 2 B
53100 Lappeenranta

Puhelimitse arkisin klo 9–11
Puh. 0295 042 006

Figure Q12: Example 4 (List of specific occupation codes exempted)

Labore eli Työn ja talouden tutkimus LABORE (ent. Palkansaajien tutkimuslaitos) on vuonna 1971 perustettu itsenäinen tutkimuslaitos, jossa keskitytään yhteiskunnallisesti merkittävään ja tieteen kansainväliset laatukriteerit täyttävään soveltavaan taloustieteelliseen tutkimukseen. Tutkimuksen painopistealueisiin kuuluvat työn taloustiede, julkistaloustiede sekä makrotaloustiede ja toimialan taloustiede. Lisäksi teemme suhdanne-ennusteita ja toimialakatsauksia sekä julkaisemme Talous & Yhteiskunta -lehteä ja podcasteja.

Vahvuuksiamme ovat tutkijoiden korkea tieteellinen osaaminen sekä tiivis yhteistyö kotimaisten ja ulkomaisten yliopistojen ja tutkimuslaitosten kanssa. Tutkijoillamme on tärkeä asiantuntijarooli eri yhteyksissä ja he osallistuvat aktiivisesti yhteiskunnalliseen keskusteluun.

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