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Regulating Labor Immigration: The Effects of Lifting Labor Market Testing*

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ABSTRACT

We study the effects of lifting labor market testing (LMT) requirements for non-EU workers in Finland utilizing regional variation in occupations exempted from labor market testing. We use individual and firm-level administrative data from 2011–2020 and hand-collected data on local changes in labor market testing rules since 2012. We estimate the effects using a staggered difference-in-differences design. We find that lifting the LMT requirement leads to an increase in the inflow of non-EU workers to treated occupation-regions. A further breakdown of this inflow shows that the effect is mainly driven by non-EU individuals already in Finland. In five years, treatment effect on the annual earnings of natives is -€647 (around 2%) at the occupation-region level and -€1,121 (around 4%) at the individual level. The observed earnings effects, especially at the occupation-region level, are driven by low-wage and service-oriented occupations. Despite the negative effects on earnings, we observe positive employment effects for some incumbent worker groups at the individual level. Conversely, at the occupation-region level, there is an increase in the number of job seekers in the exempted occupations. At the firm level, we observe an increase in the number of non-EU employees and suggestive evidence of firms expanding in general.

JEL Codes: J20, J38, J61, J68

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

TIIVISTELMÄ

Tarkastelemme ulkomaalaisen työvoiman saatavuusharkinnan poistamisen vaikutuksia Suomessa hyödyntäen alueellista vaihtelua saatavuusharkinnasta vapautetuissa ammateissa. Käytämme yksilö- ja yritystason rekisteriaineistoja vuosilta 2011–2020 sekä käsin kerättyjä ELY-keskusten työlupalinjauksia vuodesta 2012 lähtien. Arvioimme vaikutuksia ns. erot-eroissa-menetelmällä. Saatavuusharkinnan poistaminen johtaa ulkomaalaisten työntekijöiden määrän kasvuun poikkeusammateissa. Tarkempi analyysi osoittaa, että valtaosa näistä ulkomaalaisista henkilöistä asuu jo tätä ennen valmiiksi Suomessa. Havaitsemme kotimaisten työntekijöiden vuosiansioihin negatiivisen vaikutuksen, joka on suuruudeltaan 647 euroa (noin 2 %) ammatti-aluetasolla ja 1 121 euroa (noin 4 %) yksilötasolla viiden vuoden päästä saatavuusharkinnan poistosta. Negatiiviset vaikutukset ansiotuloihin syntyvät erityisesti matalapalkkaisista ja palveluammateista. Huolimatta negatiivisista ansiotulovaikutuksista, havaitsemme positiivisia työllisyysvaikutuksia osalle työntekijöistä yksilötasolla. Ammatti-aluetasolla työttömien työnhakijoiden määrä taas kasvaa. Yritystason analyysi osoittaa, että saatavuusharkinnan poiston jälkeen yritykset palkkaavat lisää sekä ulkomaalaisia että kotimaisia työntekijöitä.

Avainsanat:
Saatavuusharkinta, maahanmuutto, työvoiman tarjonta, palkat

1 Introduction

All countries have regulatory policies for labor immigration. Labor market testing (LMT) is used in most EU countries as part of employer-driven policies.[1] It requires employers to prove that no suitable local workers are available for a position before a foreign worker can be hired. LMT aims to safeguard the employment and wage levels of vulnerable workers while simultaneously meeting the needs of firms. Empirical research on the effects of regulatory policies aimed at less-skilled labor immigration is scarce. This study is one of the first attempts to estimate the causal effects of LMT.

We base our analysis on a quasi-experiment arising from the phased removal of labor market testing across various occupations and regions over time. We employ a staggered differences-in-differences framework, utilizing population-wide register data from a Nordic welfare state, enabling us to examine a wide array of outcomes with high detail at the occupation-region, individual, and firm levels. Furthermore, we explore the implications for government transfers and inequality.

The first-stage results at the occupation-region level show that abolishing LMT requirements has a positive effect on the inflow and stock of non-EU workers in the affected regions and occupations.[2] Around 80% of the increase originates from immigrants already residing in the country. These workers belong to various groups that have limited or no work authorization, and exemptions make it possible for them to get a work permit in specific occupations. The significance of these different channels is thoroughly evaluated in our paper.

We observe a negative treatment effect of -€647 (around 2%) in year 5 after treatment on the annual earnings of native workers at the occupation-region level, while the mean earnings of non-EU workers in the same occupation-region are not affected. It is important to note that these are effects on occupation-region level averages, not on individual earnings. The negative earnings effects at the occupation-region level are only visible in low-paid occupations, meaning occupations that belong to the lowest quartile in the salary distribution of occupations. The observed negative effect is driven by decreased working hours and is more pronounced among older workers.

At the individual level, we find a negative wage effect of -€1,121 (around 4%) five

[1]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).

[2]In this paper, we refer to all workers from outside of the EU/EEA area as non-EU workers.

years after the policy change. In contrast to the occupation-region level analysis, this individual-level impact extends to non-EU and EU workers, particularly in the lowest salary quartile. Similar to the occupation-region level, the negative effect is primarily due to decreased working hours and is more pronounced among older workers. Additionally, we observe a negative impact on average hourly wages, which we do not observe at occupation-region level estimates.

As for the firm-level effects, removing LMT leads to the affected firms expanding the number of employees, resulting in declining labor productivity in the following four-year period with no effect on profitability. This might indicate that affected firms are investing in new hires, which could result in higher growth and profitability later, as firms go through their natural lifecycles (e.g., Hyytinen and Maliranta 2013).

Our research makes four significant contributions to the literature. First, our study provides a clear identification of the effects of relatively less-skilled immigration across various points of the earnings distribution by leveraging a policy quasi-experiment. To our knowledge, only Clemens and Lewis (2022) have utilized a similarly well-defined research setup and, as they highlight, most previous studies have relied on the shift-share approach.

Second, this is the first paper studying LMT with a solid identification strategy.[3] Our instrument, a regulatory labor immigration policy, is noteworthy on its own. This policy is widely used in many countries and is designed to balance the costs and benefits of immigration. Consequently, research on this policy has direct implications for refining its application. Since LMT is widely adopted for relatively low-skilled labor migration, our results are of significant interest to policymakers in many countries.[4]

Third, we provide detailed evidence on how an increased supply of foreign workers affects both natives and non-natives. We examine the entire wage distribution, employment, taxes paid, transfers received, and mobility using population registers. Additionally, we expand the discussion on earnings effects by decomposing the earnings impact into working hours and hourly wages, an analysis that has not been done before.

The results from previous literature can vary by country as different policies affect workers with various skills differently. Some non-causal studies from the U.S. (e.g., Ottaviano and Peri 2006; Card 2001) have suggested that immigrants' overall impact

[3] An essay in the Ph.D. thesis by Bratu (2019) is the only previous study on the effects of LMT we are aware of. The paper studies the effects of a Swedish reform that removed all labor market testing and made Sweden's labor immigration policy fully employer-driven in 2008.

[4] Czaika and Parsons (2017) analyze migration policies targeting high-skilled workers. They find that supply-driven policies such as points-based systems are more effective in attracting foreign workers than labor market testing and shortage lists. There is a wide literature on high-skilled migration policies, for example, on VISA policies (Kerr et al. (2015) and Doran et al. (2022))

on native wages is small, while some have argued differently, finding relatively large negative wage effects for natives (Borjas, 2003). Starting from Card (1990), there is also a body of literature studying the impact of immigration on wages and employment based on natural quasi-experiments that create exogenous variation in the inflow or outflow of migrants and refugees. Related to labor migration, Clemens et al. (2018) show that the exclusion of roughly half a million seasonally-employed Mexican farm workers in the 1960s had little effect on the labor market for domestic farm workers. On the contrary, East et al. (2023) show that an immigration enforcement policy that removed employed undocumented immigrants from the regional labor market had a negative effect on natives' wages and employment.

In the European context, several papers study 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) identified a negative impact on wages in Norway's construction sector, attributable to immigration, by examining variations in occupational licensing requirements. Dustmann et al. (2017) analyzed 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.[5]

Fourth, in addition to individual and occupation-region level impacts, our analysis extends to firm-level outcomes, an area where existing research, particularly on firm responses to immigration, is less abundant compared to wage effects studies. Clemens and Lewis (2022) find that the substitutability of foreign and native workers is quite low, meaning that the inflow of foreign workers would not decrease the employment of natives. Olney (2013), highlights immigration's positive association with the number of establishments in cities and increased employment within existing firms. Our study

[5] Edo (2020) examine 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) use an IV approach and find that an increase in the share of immigrants from less developed countries in the workplace lowers wages for native co-workers. On the contrary, Foged and Peri (2016) show, using exogenous variation originating from the dispersal policy of refugees in Denmark, that the effect of unskilled immigration has a positive effect on unskilled natives' wages, employment, and occupational mobility. In the case of relatively high-skilled immigration, Beerli et al. (2021) investigate the impact of lifting all restrictions on European cross-border workers in the neighboring countries of Switzerland, finding a 5% increase in the wages of high-skilled native workers.

expands on this by examining additional dimensions such as sales, investments, profits and labor productivity. For instance, studies by Beerli et al. (2021) and Kerr et al. (2015), using Swiss and U.S. data, respectively, indicate that high-skilled immigration fosters innovation and enhances firm performance. Nonetheless, these findings do not address the impact of low-skilled immigration. Dustmann and Glitz (2015) suggest that firm market entry and exit play a crucial role in adjusting to increased immigration. Furthermore, Mitaritonna et al. (2017) demonstrate that immigration boosts total factor productivity, particularly in small and less productive firms, and is linked to higher exports, increased native wages, and accelerated capital growth. Our work contributes to this literature by exploring a broader set of firm-level outcomes.

Our findings align with the framework of labor migration theory, which suggests that immigration increases the supply of workers, reduces the capital-worker ratio, and lowers wages if capital is fixed (Edo, 2019). Immigrants' effects on natives' and incumbent immigrants' wages would be negative or positive depending on whether they are complements or substitutes (Peri and Sparber, 2009). Immigrants who share similar skills to native workers may decrease the native wages. In contrast, immigrants whose skills complement the native's skills may cause the native wages to rise as production increases. Peri and Sparber (2009) show that natives start specializing in tasks that require more local knowledge, such as language skills, as a result of low-skilled immigration. This can lead to increases in both immigrants' and natives' wages.

This paper is organized as follows. Section 2 presents the institutions and describes the quasi-experimental setting we utilize. Section 3 describes the data. Section 4 discusses our methods. Section 5 presents our main results which are estimated at occupation-region, individual, and firm level. Section 6 discusses the fiscal implications of our results. We discuss policy implications in Section 7, and Section 8 concludes.

2 Institutional Setting

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.[6] 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.

In the first step, the non-EU worker applies for a work permit on the Migri website and the employer fills out a form to be 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. They 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. Specialist professions 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.

2.2 Regional variation in labor market testing requirements

Our research design exploits the regional and temporal variation in labor market testing requirements in part of the 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 labor immigration should be updated every 6 months for each of these districts. ELY centers in the regions 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 occupation-region units to be exempted was previously largely based on the Occupational Barometer. The Occupational Barometer was a summary compiled by experts at the Employment Office (TE-office) on the situation of various job titles indicating whether there was a shortage or

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

surplus of job seekers. 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 been something that would have prevented the exemption of a particular occupation-region unit. The Occupational Barometer was discontinued in the fall of 2022 according to the authorities. Nowadays, the evaluation emphasizes another tool ("Työvoiman saatavuus ja kohtaanto -tietomalli"). Based on this model, professions already exempted are reviewed, and potential new shortage professions are elevated for examination by the immigration affairs committee. The authorities have also developed a new forecasting tool, the Labor Market Barometer, in 2023, but it is not used extensively yet. Regarding firm lobbying, our contact working in the center stated that companies and other stakeholders have had some contact with the authorities. However, according to them, lobbying should have little effect on decisions that are made.

In Figure 1, we plot cumulatively the share of occupations that have been 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. In the final year of our sample, there is one region (Lapland) with nearly 40% of occupations considered to be treated. 28 % of the exemptions are reverted during the sample period. 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).

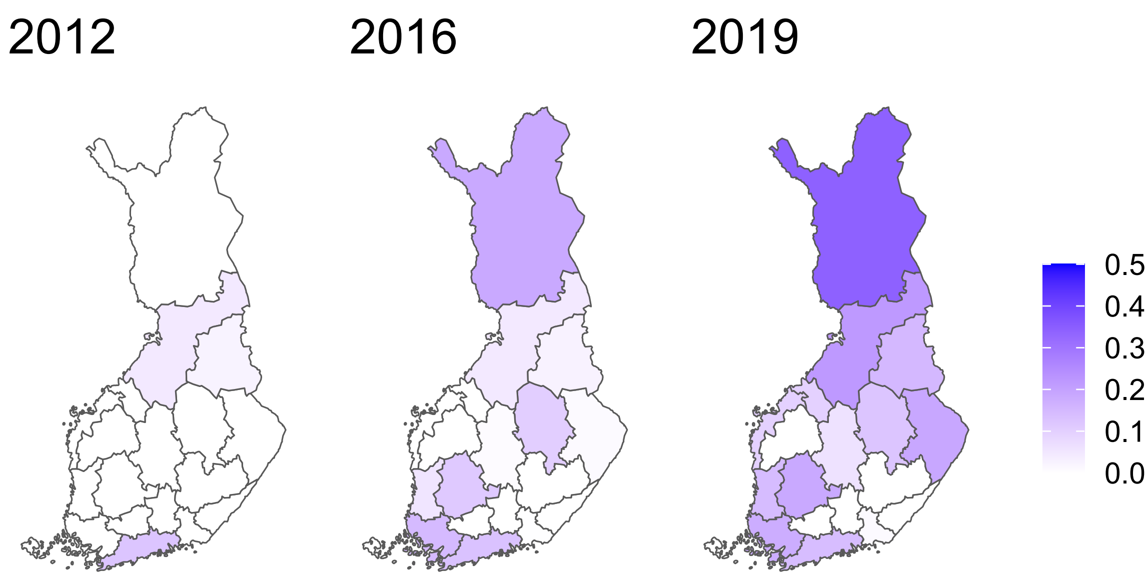


Figure 1: Staggered treatment

Notes. 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. This figure includes all occupations, including specialist occupations. Figure produced by the authors 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.[7] Using our data, we can identify workers who work in specific professions. This is crucial as we want to estimate the effects on the occupation-

[7]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.

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 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 were relatively extensive in recent years, but we supplemented these with information received from Finnish Public Employment Service offices.

We restrict our sample to the years 2012-2019 because, for some regions, we did not receive any earlier documents. 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.

4 Empirical Strategy

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 on the occupation-region level. In our main analyses, we find a year when an occupation-region has first been treated, and then classify that region as treated for all subsequent periods. This is because the Callaway and Sant'Anna (2021) method does not allow for treatments to turn on and off and because an occupation in the region generally stays on the shortage list once it has been treated. Thus, we only identify the effect of the *first* treatment each unit faces.

Because we use a staggered difference-in-differences design, we estimate the treatment effects on occupation-region level using the Callaway and Sant'Anna (2021) estimation method. The method is based on estimating group-time average treatment effects, i.e., treatment effects 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 12 groups in the data, as we have 12 years (2009-2020) when treatment begins for some units. Following Callaway and Sant'Anna (2021), if the parallel trends assumption holds, the group-time average treatment effect is

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

where Y_t is the outcome at time t, C_{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 is used as a control group. The identifying assumption is parallel trends in the absence of treatment. Once we have calculated the group-time average treatment effects $ATT(g,t)$ described in equation 1, we can aggregate these into a dynamic event study plot. 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 in the package used to estimate the Callaway and Sant'Anna (2021) estimates.

4.2 Individual level

4.2.1 Individual level treatment and control groups

At the individual level, we want to estimate effects on individuals who worked in the occupation-region unit before the exception was introduced. 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 aforementioned individual level treatment group is challenging, as the treated individuals are all working in period -1, and the period -1 is only defined for treated individuals. If we used all never-treated individuals as controls, there would likely be a strong jump in period -1 because treatment group members would all be working in period -1 while no such restriction would be in place for individuals in the control group. Thus, we need to somehow create a more convincing control group.

We use a three-step procedure to form the control group. First, we take all never-treated occupation-region units, and randomize a placebo treatment year for each unit. After this, we take all workers who worked in these never-treated occupations one year before the placebo treatment year. This is similar to how the treatment group is defined, although in the case of the the control group, no treatment takes place. We then use matching to get an equal-sized and similar control group for the treated individuals. Due to computational reasons, we use propensity score matching as the matching algorithm in the individual level analysis. We match only on a small number of variables (age, gender, income) using values from period -1.

4.2.2 Estimation

In addition to our main analyses that are estimated using occupation-region level data, we also estimate effects on individual level outcomes. At the individual level, we estimate two-way fixed effects regressions of the following form:

$$Y_{it} = \gamma_i + \lambda_t + \sum_{\substack{k=-5 \\ k\neq -1}}^{5} \delta_k \cdot D_i \cdot \mathbb{1}\{K_{it} = k\} + \sum_{\substack{k=-5 \\ k\neq -1}}^{5} \theta_k \cdot \mathbb{1}\{K_{it} = k\} + \epsilon_{it} \tag{2}$$

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 how to define 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. In the firm-level analyses, we use a matched difference-in-differences strategy. First, we match firms that employ workers in exempted occupations to firms that do not. We use coarsened exact matching as the matching algorithm. Subsequently, we estimate difference-in-differences regressions of the following type:

$$Y_{it} = \gamma_i + \lambda_t + \beta D_i + \gamma post_t + \delta(D_i * post_t) + \epsilon_{it} \tag{3}$$

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 trated 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:

$$Y_{it} = \gamma_i + \lambda_t + \sum_{\substack{k=-3 \\ k \neq -1}}^{3} \theta_k \cdot D_i \cdot \mathbb{1}\{K_{it} = k\} + \epsilon_{it} \tag{4}$$

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 a previously untreated firm, for example, employs in some occupation, and that occupation becomes exempted during a year, that year becomes the "event year" for that firm.

We combine matching and difference-in-differences when estimating firm responses. Matching is conducted separately for each treated group (i.e., different "first event" years). We estimate cem weights for each trated 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.

4.4 Predictors of rule change

In our main analyses, we compare exempted occupation-regions to non-exempted occupation-regions. The occupations are chosen to the list of exemptions by the regional offices of the central government (ELY-centers). Since the stated aim of the policy is to target occupations that are deemed to suffer from "labor shortages", we need to carefully assess the validity of our setting.

We first compare covariates in the treated occupation-regions to the never-treated occupation-regions (which our control group in 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 dif-in-dif type identification strategy.

We also show an event study for the V/U ratio in the Figure 2. This figure shows that there is no pre-trend in labor market tightness, suggesting that the exemptions have not been targeted using this measure. The figure also shows a negative effect on occupation-region level labor market tightness. Further analyses in the Online Appendix A 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 V/U ratio does not seem to predict exemption decisions.

Second, if the exemptions are well-targeted, we expect a positive earnings pre-trend in our dif-in-dif analyses. We diligently study pretrends throughout our analyses for any suspicous anomalies that would threaten our identification strategy. We also perform a range of robustness tests in Section 6.

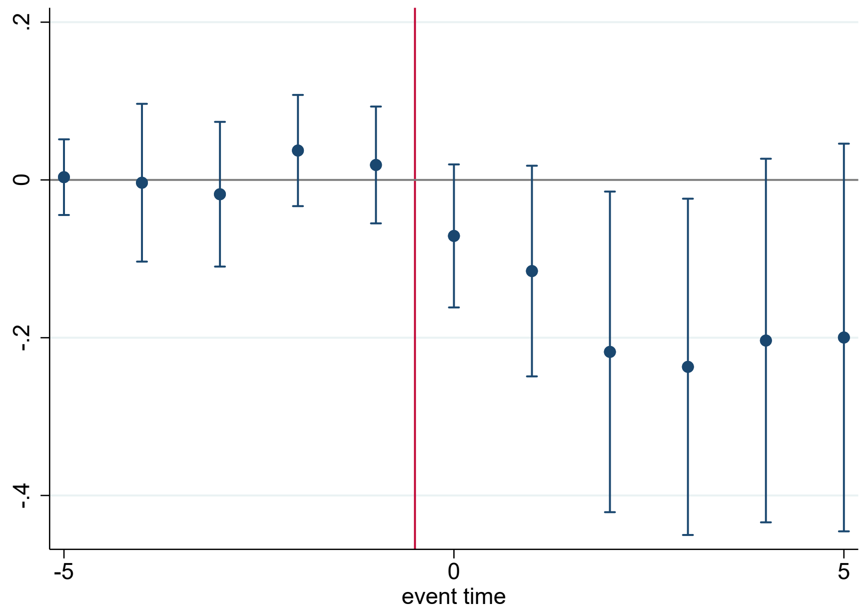


Figure 2: Labor market tightness (V/U): occupation-region level event study

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 (millions)	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. 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 First stage

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 first stage 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 first stage 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.[8] 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 first stage figures 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.

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

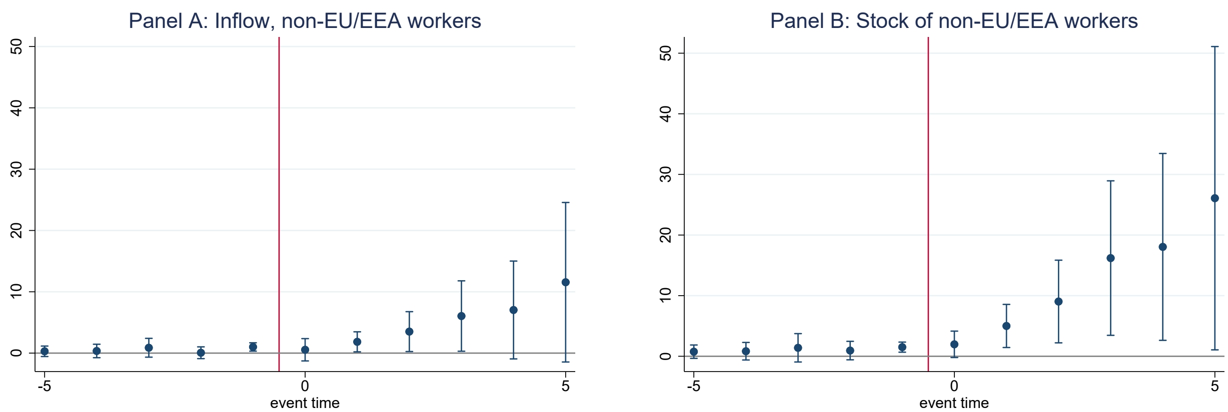


Figure 3: Effect on the inflow and stock of non-EU workers

Notes. 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 our first stage. 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[9], non-married partners of foreign workers, international students (only part-time work allowed with a student visa), and asylum seekers (proxy), shown in

[9] 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.

Panels C-F of Figure 4.[10] These groups are not mutually exclusive.

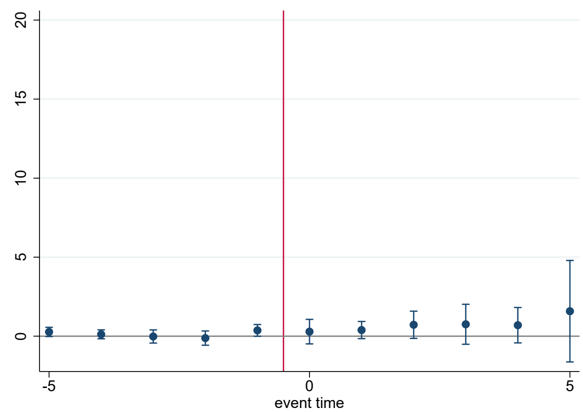
Panel C indicates that the policy shift increases the inflow of asylum seekers by around 1.5 in year 5. Asylum seekers are proxied by using the top 4 countries where most asylum seekers come to Finland (Afghanistan, Syria, Iran, Iraq). The effect is statistically significant and represents around 17% 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 D 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.

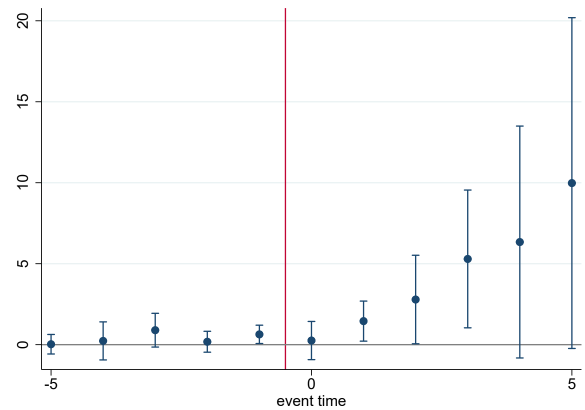
Panels E and F show that non-EU spouses of non-EU workers, and occupation changers, are relevant channels. The latter (occupation changers) should only be relevant before the law change in 2019 which removed LMT from occupation changers. However, some international students could still fit in this category 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.9, Figures A27 and A28).

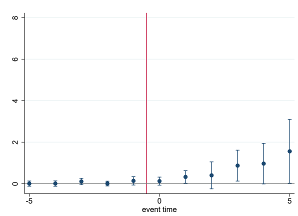
[10]Asylum seekers who have not received a decision on their application, or those who have been denied asylum, have the option to "change the track" and apply for a work-based residence permit. The number of asylum seekers applying for work-based residence permits has been around 1,100 during 2015-2018,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



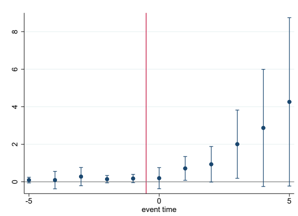
Panel A: New migrants to Finland (effect on inflow)



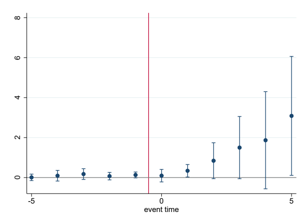
Panel B: Immigrants already in Finland (effect on inflow)



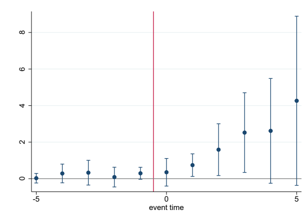
Panel C: Asylum seekers (proxy)



Panel D: International students



Panel E: Non-EU workers with non-EU partner



Panel F: Occupation changers (required LMT up until 2019)

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

Notes. 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 in Panel A and Panel B are mutually exclusive, while groups in Panels C-F are not. We aim to decompose the effect in Panel B to different subgroups in Panels C-F. We estimate effects for all pre and post years but only estimates in window [-5,5] are plotted in the Figure

5.2 Occupation-region level

5.2.1 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.

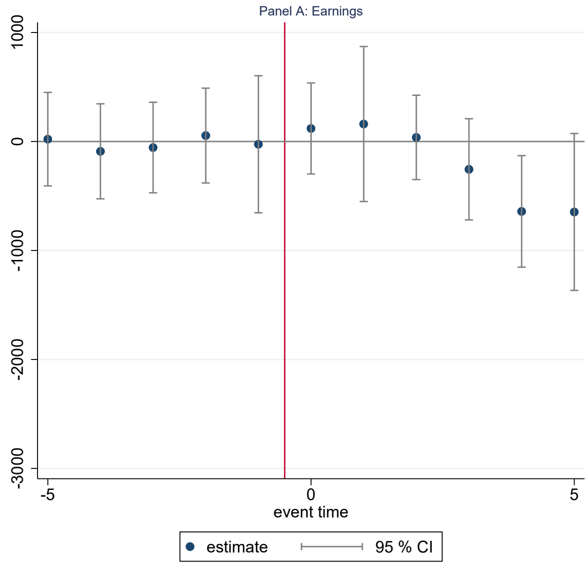
We will analyse the bottom quarter separately in our occupation-region level results. Panel B of Figure 5 shows that for the bottom quartile, earnings fall by a point estimate of close to €2,000 by year five. In the top three quartiles we observe no significant effect. However, there is some evidence 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 first stage results and separate them also by occupation salary percentile. There is no discontinuity in the first stage 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 first stage 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 are 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 A14, 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).



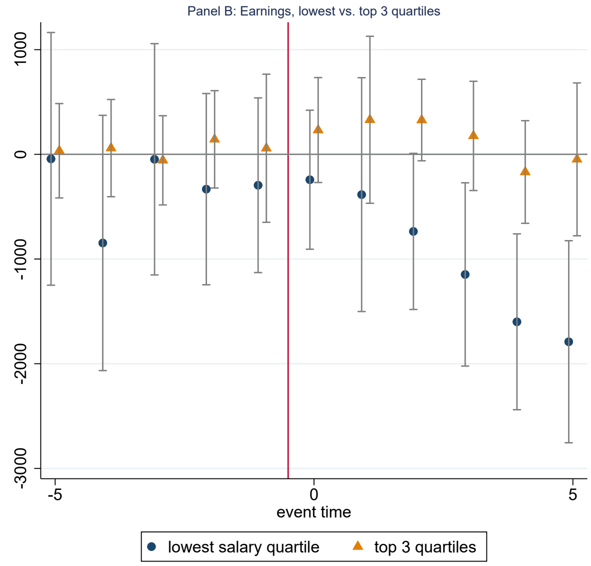


Figure 5: Effects on total annual labor earnings of natives

Notes. 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 nevertreated 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.

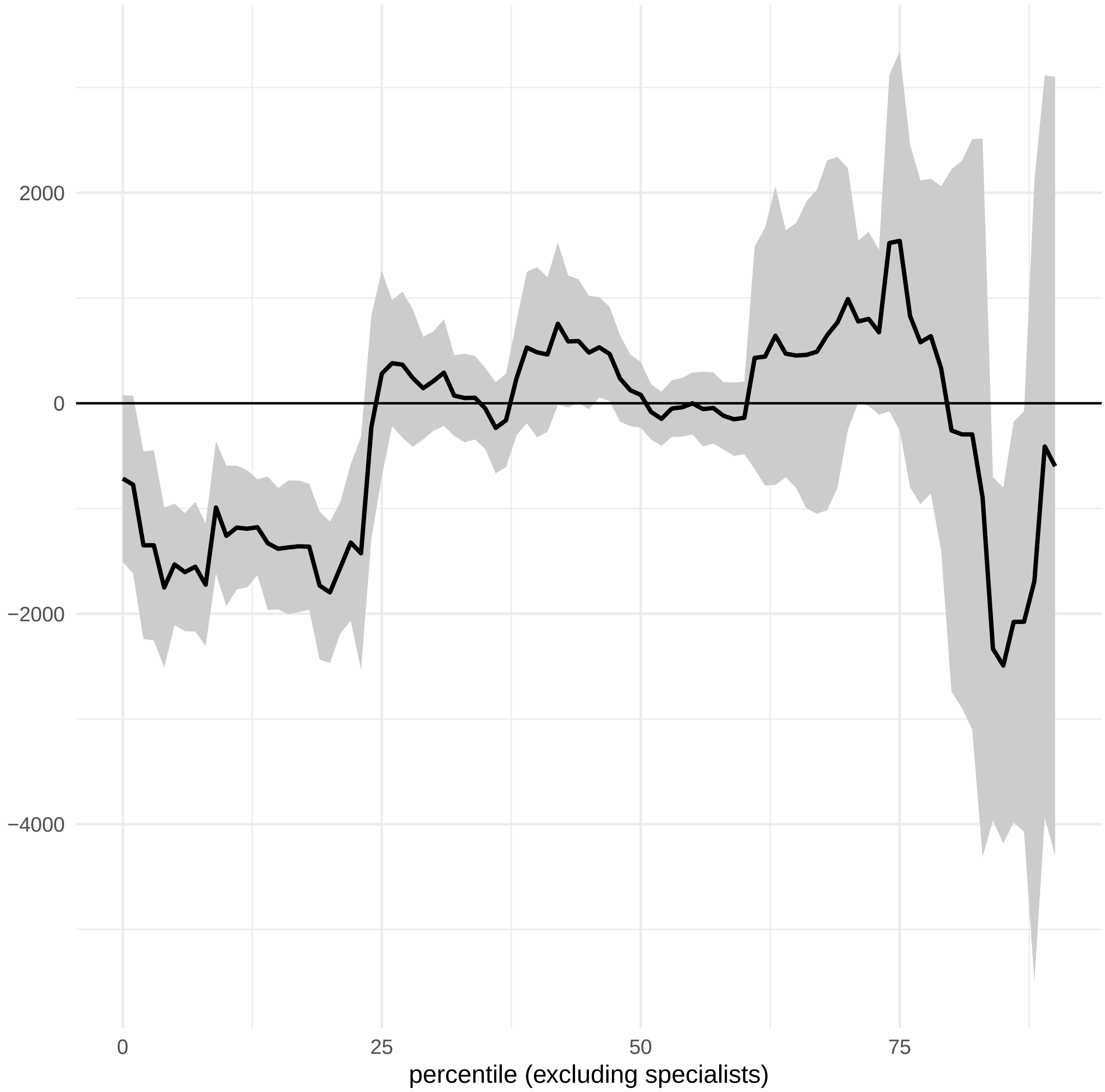


Figure 6: Effect on native earnings for professions at different points of the salary distribution (excluding specialists)

Notes. Figure shows the Callaway and Sant'Anna (2021) estimates where the outcome variable is annual earnings separately for different professions in 10% intervals in the earnings distribution. Control group is always the same (all nevertreated occupations).

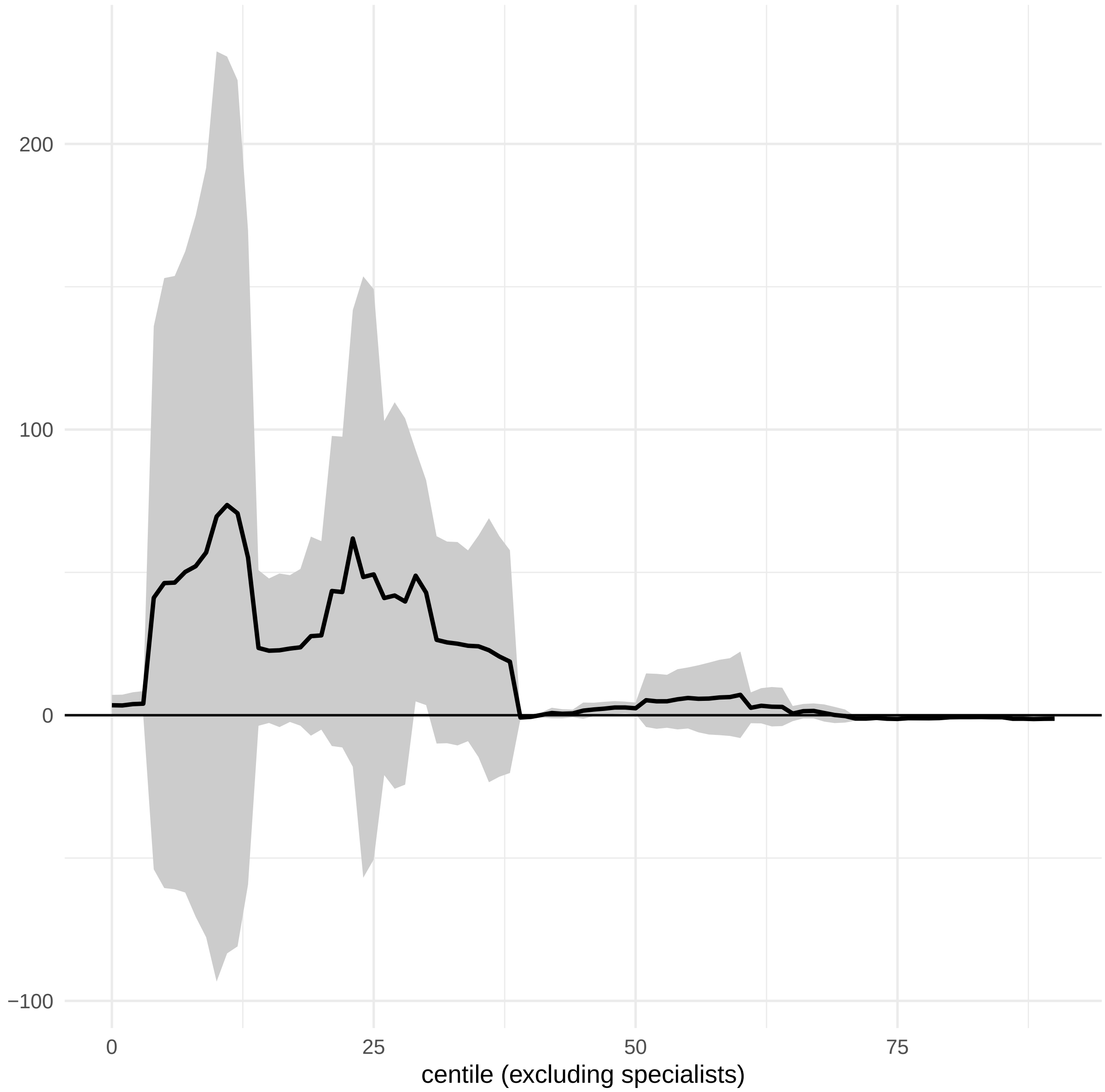


Figure 7: Effect on the stock of non-EU workers at different points of the salary distribution (excluding specialists)

Notes. 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. Control group is always the same (all nevertreated occupations).

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. 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.

5.2.2 Possible mechanisms

The results presented in the figures above are not informative on why the average salary of natives is negatively affected in treated occupation-regions. It could merely reflect changing worker flows to and from the occupation-region. 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. 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.

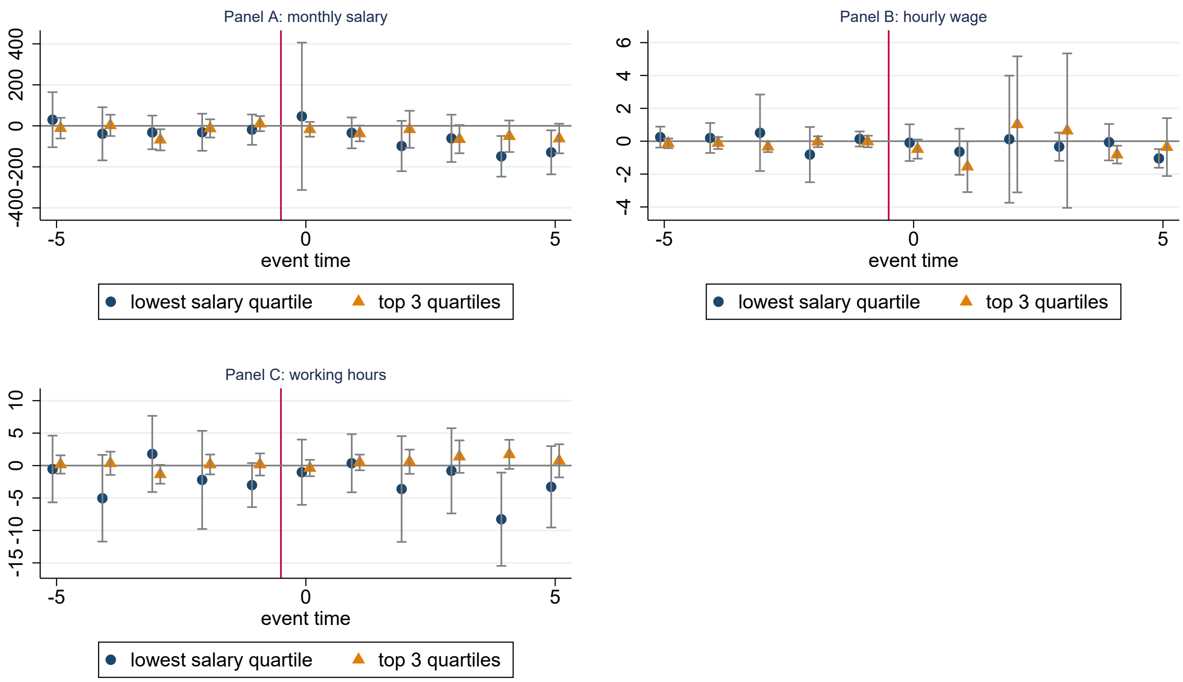


Figure 8: Effects on salary components, one-month information (Earnings Structure Survey)

Notes. 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.

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 A9-A11). 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

A4) and find no effects.

5.2.3 Heterogeneity

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-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.

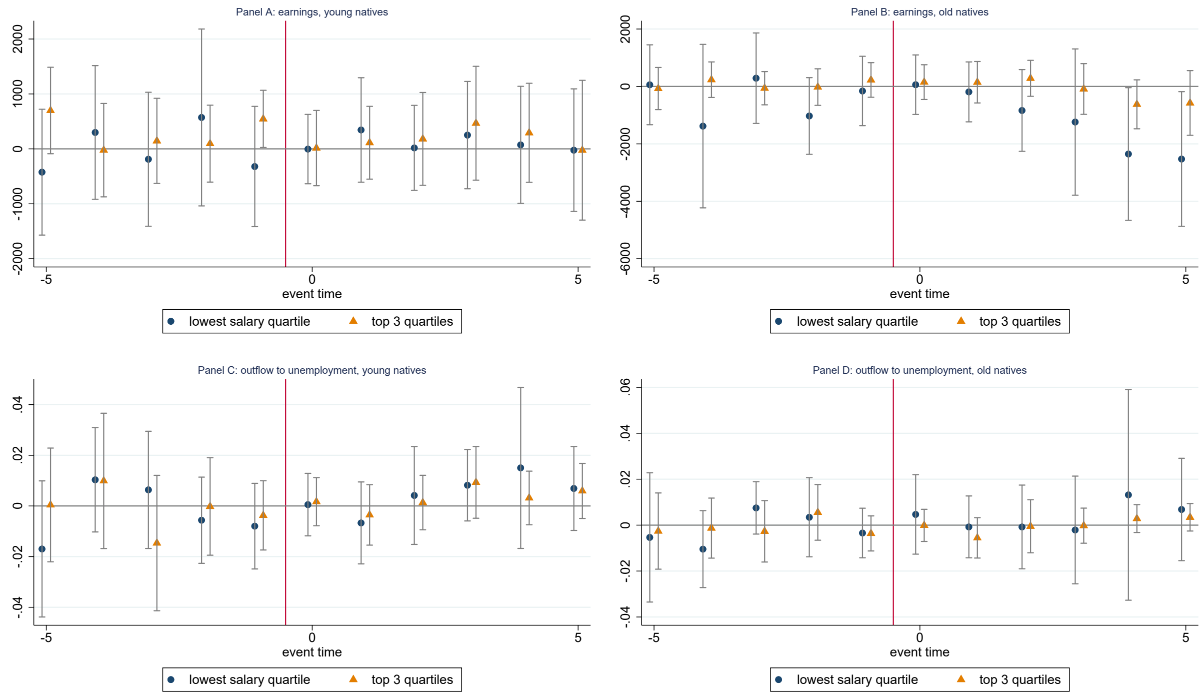


Figure 9: Effect on the annual earnings and inflow to unemployment of young (≤ 30) and old (> 50) native workers

Notes. 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.

5.3 Individual level

5.3.1 Earnings

In Section 5.2, 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 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 5.4 showing the effect on firm employees' growth, suggest that there are benefits in terms of average employment for the three highest salary quartiles.

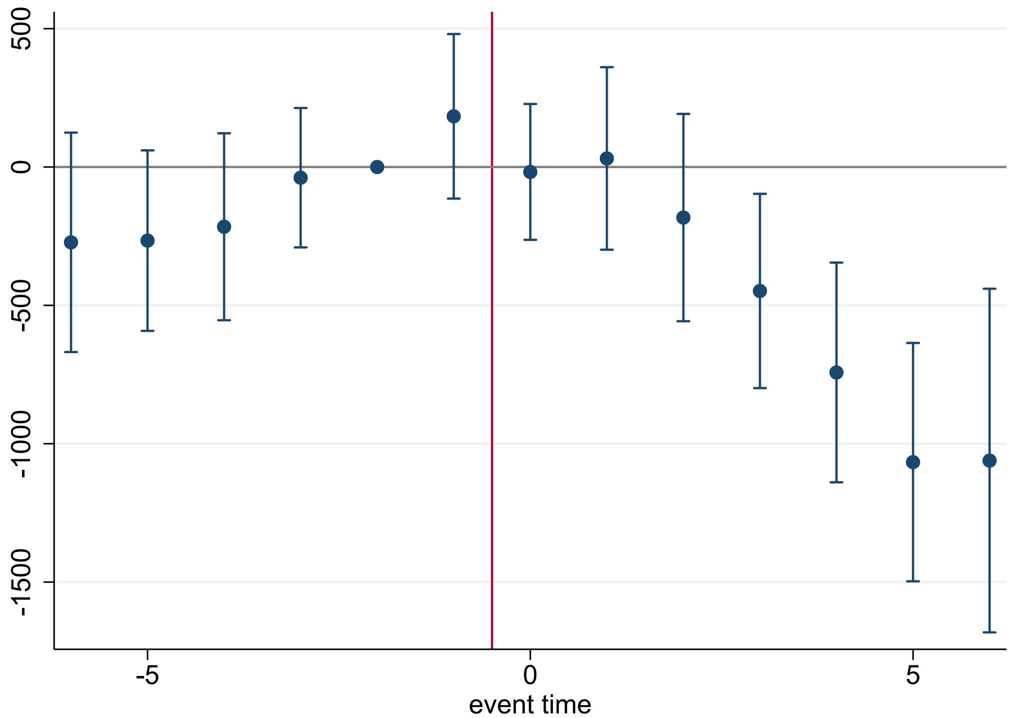


Figure 10: Earnings effect at the individual level

Notes. 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.

Table 3: Earnings effects, individual level

	(1)	(2)	(3)	(4)
	earnings	native earnings	earnings, EU immigrants	earnings, non-EU immigrants
Panel A: Pooled TWFE estimates				
Workers in all occupations (original matched groups)				
Treatment effect	-314.0**	-334.9**	348.4	-1166.9**
	(139.5)	(139.0)	(500.5)	(568.1)
N	6403800	5975170	59260	82880
Outcome mean (treated)	27770.23	28013.52	25646.46	18320.94
Workers in bottom quartile occupations				
Treatment effect	-699.2***	-714.7***	-1499.9*	-623.5
	(234.8)	(222.6)	(772.2)	(696.1)
N	1643650	1468410	25870	45630
Outcome mean (treated)	20310.11	20415.04	20190.48	15848.51
Workers in top 3 quartiles of occupations				
Treatment effect	-224.1	-249.4*	940.5*	-1734.2***
	(145.5)	(145.9)	(518.3)	(610.3)
N	4760150	4506760	33390	37250
Outcome mean (treated)	30168.92	30403.89	27594.77	19975.39
Panel B: Medium term (year 5) TWFE estimates				
Workers in all occupations (original matched groups)				
Treatment effect	-1066.6***	-1120.9***	-15.07	-1783.6**
	(219.7)	(221.4)	(675.6)	(697.4)
N	6403800	5975170	59260	82880
Outcome mean (treated)	27770.23	28013.52	25646.46	18320.94
Workers in bottom quartile occupations				
Treatment effect	-1216.5***	-1244.5***	-2932.4***	-2001.5**
	(367.5)	(358.6)	(1049.9)	(939.4)
N	1643650	1468410	25870	45630
Outcome mean (treated)	20310.11	20415.04	20190.48	15848.51
Workers in top 3 quartiles of occupations				
Treatment effect	-1034.1***	-1087.7***	945.6	-2560.6**
	(264.5)	(265.7)	(902.8)	(1160.5)
N	4760150	4506760	33390	37250
Outcome mean (treated)	30168.92	30403.89	27594.77	19975.39

Notes. 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	6403800	5975170	59260	82880
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	1643650	1643650	106809	45630
Outcome mean (treated)	0.90	0.91	0.92	0.81
Workers in top 3 quartiles of occupations				
1.D	0.0118**	0.0123***	0.00676	0.0172
	(0.00459)	(0.00461)	(0.0154)	(0.0187)
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.-0.00644
	(0.00522)	(0.00451)	(0.0206)	(0.0302)
N	6403800	5975170	59260	82880
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	1643650	1643650	106809	45630
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	4760150	4506760	33390	37250
Outcome mean (treated)	0.92	0.92	0.87	0.81

Notes. 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.

5.3.2 Mechanisms and heterogeneity

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 %.

As shown in the previous section, Panel A of Table 5 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). To further understand how these observations can coexist, columns 10, 11, and 13 depict 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), 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. However, we also observe some evidence of a negative effect on hourly wages, indicating that the salaries in treated occupations grow more modestly after the LMT exemption which was unclear in occupation-region level analysis. The observed negative effect in hourly wages raises two possibilities. First, the LMT exemptions might have had an impact on wage bargaining at the local level. Second, it is also possible that individuals would change firms, and as a result, earn less due to being newer employees.

In Panel B of Table 5, 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. Their earnings fall by €1,400 annually, and while they are less likely unemployed, they are also less likely full-employed. Oldest group is also less likely to transit to better paid occupations and works less hours. The negative effect on monthly working hours would likely be even stronger if we looked at the lowest salary quartile of workers, as that was the group that was observed to have a negative effect on working hours in the occupation-region level estimates. It is also likely that the occupations in the lowest salary quartile are ones with more part-time workers (compared to higher quartiles of occupations) and thus the negative effect on working hours is likely to be more driven by the lowest quartile.

Interestingly, for the youngest group (those under 30), the results in column 7 indicate that the risk of leaving the labor force increases nearly twofold 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 A11 in the Online Appendix. Transitions to education also increase for young workers, which is perhaps a positive funding. We also observe that young workers are more likely to transit to worse occupations than their previous occupation in terms of average salary in the occupation.

Panels B.B2 and B.B3 of Table 5 repeat the analysis step-by-step by gender and education. Females and those with secondary degrees are, on average, less likely to benefit from the rule change. While the results for females are understandable (because they are more likely to work in service sector jobs than men), we would have expected that the lowest educational category, those without a degree, would bear the observed cost. Nevertheless, this might be explained by the fact that those with a degree can negotiate more working hours during tight labor markets, while those without a degree have fewer options.

Table 5: Mechanisms and heterogeneity, individual level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	earnings	native earnings	unemployment months	long-term unemployment	12 months employed	outflow to education	outflow outside of labor force	outflow to better occupation	outflow to worse occupation	working hours	overtime hours	hourly wage	moving
Panel A: Pooled individual level estimates (standard TWFE specification)													
1.D	-314.0**	-334.9**	-0.0995***	-0.00386***	0.0198***	-0.000700	0.000319	-0.00590	0.00280	-1.272***	-0.118*	-0.385***	-0.000940*
	(139.5)	(139.0)	(0.0231)	(0.00102)	(0.00447)	(0.000660)	(0.000645)	(0.00420)	(0.00286)	(0.235)	(0.0619)	(0.142)	(0.000550)
Panel B: 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	0.898***	-0.144**	0.00681	-0.00268**
	(150.8)	(146.9)	(0.0239)	(0.00117)	(0.00617)	(0.00200)	(0.00116)	(0.00663)	(0.00426)	(0.298)	(0.0624)	(0.159)	(0.00118)
1.D # 1.over 50	-1738.4***	-1766.9***	0.0986***	0.00337***	-0.0385***	-0.00449***	-0.000720	-0.00914***	-0.00232	-2.171***	0.00217	0.175	-0.000948*
	(291.9)	(290.4)	(0.0190)	(0.000678)	(0.00982)	(0.00168)	(0.00101)	(0.00259)	(0.00157)	(0.342)	(0.0416)	(0.263)	(0.000501)
1.D # 1.under 30	352.0	349.5	0.00292	0.000366	0.0484***	0.0202**	0.00708***	-0.0154***	0.00874***	-0.345	-0.0221	0.348	0.00493***
	(227.1)	(234.4)	(0.00991)	(0.000549)	(0.0100)	(0.00938)	(0.00252)	(0.00531)	(0.00311)	(0.350)	(0.0521)	(0.288)	(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	0.286	-0.105*	0.153	-0.00203
	(190.7)	(186.6)	(0.0229)	(0.000933)	(0.00743)	(0.00220)	(0.00183)	(0.00717)	(0.00450)	(0.276)	(0.0609)	(0.206)	(0.00152)
1.D # 1.man	506.2***	485.1***	-0.0153	0.00264**	0.00177	-0.00301	-0.00373**	0.0224***	0.00731**	0.160	-0.130	-0.208	0.0000540
	(151.4)	(151.4)	(0.0330)	(0.00130)	(0.00473)	(0.00205)	(0.00168)	(0.00565)	(0.00357)	(0.313)	(0.0896)	(0.248)	(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	0.306	-0.164	0.735	-0.00119
	(229.1)	(240.8)	(0.0353)	(0.00187)	(0.00654)	(0.00293)	(0.00566)	(0.00776)	(0.00716)	(0.455)	(0.131)	(0.463)	(0.00189)
1.D # 2.education	-344.8**	-335.4**	-0.0217	-0.000169	0.00125	0.000373	-0.00819	-0.00431	0.00343	-0.0189	0.0784	-0.705**	-0.000306
	(151.9)	(142.7)	(0.0315)	(0.00126)	(0.00488)	(0.00230)	(0.00526)	(0.00410)	(0.00424)	(0.385)	(0.103)	(0.357)	(0.00124)
1.D # 3.education	-243.0	-258.3	-0.0215	-0.00224	0.0000925	-0.00661*	-0.0102*	0.0181**	-0.0176***	0.170	-0.0742	-0.553	-0.00241
	(194.2)	(212.4)	(0.0380)	(0.00138)	(0.00563)	(0.00360)	(0.00571)	(0.00737)	(0.00642)	(0.487)	(0.132)	(0.424)	(0.00197)
N	6403800	5975170	6403800	6403800	6403800	6403800	6403800	6403800	6403800	3450234	3450234	3435744	6403800
Outcome mean	27770.23	28013.52	0.5611837	0.0086906	0.8108202	0.0072244	0.0031838	0.054993	0.0474856	159.4977	1.809405	15.86893	0.0128727

Notes. 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.

5.4 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. The figure indicates 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 6 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.

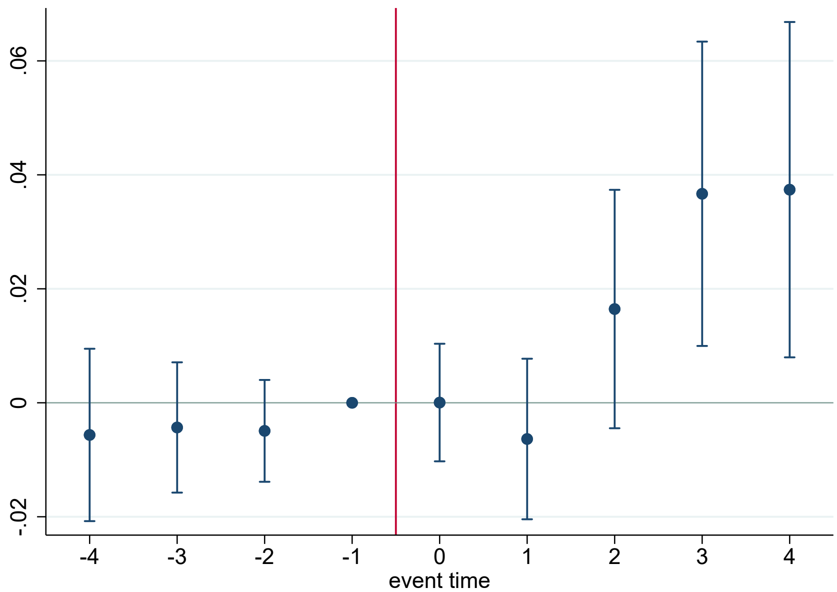


Figure 11: Effect on the number of non-EU workers employed by the firm

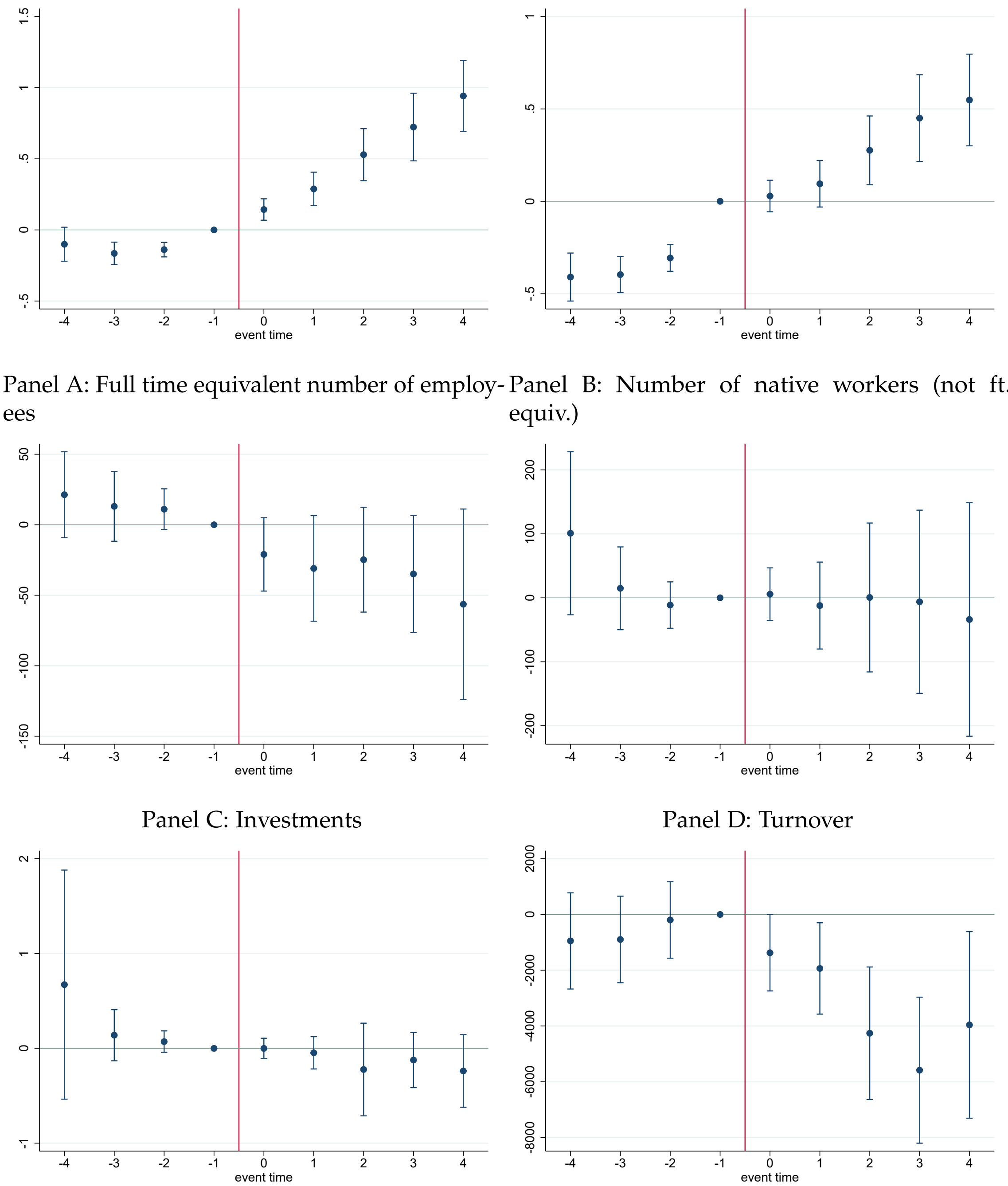
We also study other outcomes besides the stock of non-EU employees. In addition to basic variables such as firm size and turnover, we also investigate effects on profit share, investments, and labor productivity (value added per worker). Table 6 shows pooled difference-in-differences estimates on various outcomes. The key observation from Table 6 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 Figure 12 shows some evidence of pre-trends, especially for the number of native workers.

Regarding other firm-level outcomes, results in Table 6 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 Figure 12, but one should still be cautious when interpreting these estimates. It should be noted that 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				Investments, €1,000							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	nr workers FT equiv.	nr native workers	nr non-EU	nr EU	all	buildings	machines	IT	labor share	turnover, €1,000	profit ratio	labor productivity
Panel A: All matched firms												
Treatment effect	0.275***	0.282***	0.00133	0.00722	-31.64*	-28.64*	-2.599	-0.399	0.0250	-20.05	-0.214	-1475.3*
	(0.0565)	(0.0587)	(0.00634)	(0.00571)	(16.90)	(15.97)	(3.729)	(0.281)	(0.324)	(42.87)	(0.230)	(800.5)
N	126077	126077	126077	126077	126077	126077	126077	126077	123758	126077	124609	124103
Outcome mean	25.78507	25.67418	0.3090909	0.3046061	191.3294	47.27018	135.6696	8.389582	0.7170176	7596.564	0.052119	68133.14
Panel B: Firms with 2-10 employees												
Treatment effect	0.241***	0.223***	0.00970**	0.00925**	-4.638	-4.323	-0.271	-0.0434	-0.0490	-19.95	-0.284	-1427.7*
	(0.0320)	(0.0362)	(0.00467)	(0.00449)	(5.429)	(2.629)	(3.958)	(0.0654)	(0.434)	(47.36)	(0.307)	(865.2))
N	100191	100191	100191	100191	100191	100191	100191	100191	98741	100191	99111	99032
Outcome mean	5.720105	5.821998	0.0727017	0.0708255	56.31264	26.00087	30.31158	0.0001893	0.7238818	967.7059	0.0659245	65693.59
Panel C: Firms with 10-50 employees												
Treatment effect	0.588***	0.560***	-0.0136	0.00668	-90.47	-81.32	-7.805	-1.350	0.266	65.35	-0.00228	-2253.3
	(0.194)	(0.201)	(0.0202)	(0.0178)	(56.65)	(54.56)	(8.874)	(1.069)	(0.337)	(86.15)	(0.0402)	(1704.3)
N	25844	25844	25844	25844	25844	25844	25844	25844	24975	25844	25456	25029
Outcome mean	20.83527	20.31585	0.2652808	0.3201986	168.8348	38.73596	128.258	1.840792	0.7096341	4455.55	0.0356983	67792.78
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. 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



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 12: Firm-level event studies

6 Robustness

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.

Choice of method and specification

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 first stage and earnings estimates, where not-yet-treated units are included, are presented in Appendix D.

Dropping seasonal worker occupations

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.9.

Heterogeneous treatment effects by treatment cohort

We show in Online Appendix that there is heterogeneity between groups (i.e., units treated at different times). We show the by-group estimates for the main outcomes. This 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. Online Appendix C shows treatment effects by group, i.e., treatment effects estimated separately for each cohort receiving treatment.

In Online Appendix A.8, 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.

Placebo treatment

We use a placebo treatment timing test for 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 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 J2, 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 J3, the number of observations would drop so much that such a specification is not sensible to use.

7 Government transfers and distributional implications

7.1 Government transfers

To shed some light on the fiscal impact of removing labor market testing requirements, we estimate the causal effects of the exemptions on transfers received and taxes paid at the occupation-region level. The analysis is conducted for three different groups of workers: natives, non-EU workers, and EU/EEA workers. In this subsection, we first estimate the effect of LMT exemptions on transfers for natives, EU nationals and non-EU nationals by quartile (bottom fourth vs the rest). We subsequently provide a calculation on the total net transfers in a scenario where all bottom quartile occupation-regions were to be exempted from LMT, assuming that our estimates would remain relevant to such a large expansion of exemptions.

Table 7 shows these estimates by the salary quartiles. 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 7: Taxes, transfers and the number of workers, pooled estimates

	Pooled ATT	**Std. Error**	**t value**
Lowest quartile			
nr workers, native	-51.6091	65.6416	-0.79
nr workers, non-EU workers	28.1886	22.7658	1.24
nr workers, EU workers	12.1594	7.816	1.56
taxes, native	1.2424	770.3363	0.002
taxes, non-EU workers	-46.6641	187.6036	-0.25
taxes, EU workers	-414.4989	142.3759	-2.91***
transfers, native	372.7319	77.2309	4.83***
transfers, non-EU workers	-51.2001	210.8537	-0.24
transfers, EU workers	140.964	145.6654	0.97
Top 3 quartiles			
nr workers, native	-0.3178	16.2945	-0.020
nr workers, non-EU workers	10.2337	4.1288	2.48**
nr workers, EU workers	7.024	2.3862	2.94***
taxes, native	-112.3092	67.1113	-1.67*
taxes, non-EU workers	290.5031	170.5377	1.70*
taxes, EU workers	44.0401	414.9268	0.11
transfers, native	-176.7483	35.5198	-4.98***
transfers, non-EU workers	-45.9032	195.1869	-0.24
transfers, EU workers	-188.4238	118.4388	-1.59

Notes. Table shows ATT estimates. Significance levels: (*) 0.1 (**) 0.05 (***) 0.01

Table 7 shows that in the lowest salary quartile of occupations, natives receive more transfers, which could result, e.g., from increased unemployment months among native workers or from an increase in income-complementing transfers such as the adjusted unemployment benefit aimed at part-time workers or the housing benefit. Taxes paid by natives are not affected, but the estimate is very imprecise, so we cannot rule out even large decreases in taxes paid. The reason why we observe no effects on taxes while observing a positive impact on transfers received may be that in Finland, taxes paid on transfers received are often larger than taxes paid on small earnings. This is due to deductions that employed workers receive, but benefit recipients do not. For example, income-dependent unemployment benefits may be taxed at 25% while labor earnings higher than those benefits can be taxed at a much lower rate, such as 2%.

Taking the point estimates on the effect on taxes minus the effect on transfers, we get a drop of €372 in net transfers paid by the natives following an LMT exemption. Assuming that all bottom-fourth occupation-regions were to be exempted from LMT and that these estimates would remain relevant to such a large expansion of exemptions, it would suggest a negative fiscal impact of 372 * 396,434 = 147 million euros from native net transfers. For simplicity, we exclude EU workers from this calculation. This calculation does not take into account the possible fiscal impact of the new non-EU entrants themselves.

This simple exercise, undeniably, has some significant limitations. First, it focuses mainly on increased immigration in the lowest quartile of occupations. Second, it does not take into account indirect taxation, such as the value-added tax. Second, if the program was expanded to cover all professions and regions, there could be general equilibrium effects that we cannot measure with our research design. Third, 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, which 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.

7.2 Implications for income inequality

As we find a negative effect on the mean annual earnings in occupations that belong to the lowest salary quartile, our occupation-region level results suggest that removing exemptions may increase income inequality. In this subsection, we try to assess whether this is the case by simulating the Gini coefficient under the complete removal of *all exemptions*. Thus, these calculations try to assess what would happen under a nationwide policy change instead of assessing the income inequality impacts of the exemptions currently made. These simulation results rely on causal estimates shown in the previous subsections.

Table 8 shows the simulation results. The results show that the decrease in native wages increases the Gini Index by 2.05%, but the Index does not increase further if we take into account the increase in the number of foreign workers.

This analysis has some significant limitations because our results suggest most of the increase in labor supply comes from immigrants that already live in Finland but don't have work authorization. There is a limited number of these individuals, so our estimates would not necessarily generalize if the policy was expanded nationwide. However, one could expect that firms would increase their efforts in hiring people from abroad in that case. The estimates in Table merely indicate that our findings would have implications for income inequality if the effects for each occupation were of the same magnitude in the context of a nationwide removal of labor market testing.

Table 8: Income Inequality Implications of Nationwide Expansion

	Gini Index (disposable income)	S90/S40 ratio (disposable income)
Baseline	0.293	1.084
Decreased Earnings for Lowest Quartile	0.299 (+2.05%)	1.150 (+6.1 %)
Increase in the number of non-EU Workers	0.299 (+2.05%)	1.154 (+6.5 %)

Notes. Table shows simulation results indicating how income inequality would change if the policy was expanded to all occupations in all regions, assuming the effect would be similar to those in our causal estimates. The S90/S40 ratio is also calculated using disposable income, and thus, the numbers differ somewhat from the PALMA ratios conventionally estimated for Finland.

8 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 10.7% drop in earnings from the pre-treatment mean relative to the control group by year 5. 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%. Even though the bottom quartile is able to attenuate the effect through individual adaptation, we do not find a positive earnings effect in any earnings bracket, unlike, e.g., Foged and Peri (2016) in Denmark, Beerli et al. (2021) in Switzerland and East et al. (2023). Our findings are more in line with the findings of, e.g., Dustmann et al. (2017), Bratsberg and Raaum (2012), and Kuosmanen and Meriläinen (2022). The zero effect for the top quartile is expected since the LMT exemptions have a negligible 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 increases in the number of immigrants similar to the bottom quartile. But, contrary to the lowest quartile, in this quartile, we do not see any effect on average native earnings at the occupation-region level.

In terms of welfare-maximizing policy design, the top three quartiles pose a puzzle. We could consider the effects of lifting immigration restriction 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 slightly 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. The 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. In case immigration policy is to target the bottom quartile of earnings, explicit rules for minimum earnings as a condition for work permits might be more appropriate and less bureaucratic than LMT. And, in case LMT is the chosen policy tool, the policy would probably benefit from being more accurately targeted at areas with tightening labor markets, as intended.

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 in the income distribution. For the following discussion, we assume that all the observed occupation-region level earnings effect arises 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 percent change in labor demand relative to a percent 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 12.5% in the earnings of lowest salary quartile employees by the fifth year after treatment while increasing their employment by approximately 8% in years 3-5 (based on the event study shown in Appendix A, Figure A6). The log specification used in Figure A6 drops units with 0 employment in some years. These estimates would imply a medium-run labor demand elasticity of 8%/-12.5% = -0.64. 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 -€500 (1.7 %) and an effect of 7% on the number of employees (again using a log-specification), suggesting an elasticity of -7%/-1.7% = -4.2. 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, i.e., compare its size to the size of the immigration shock. Our base estimates suggest an increase of at least 20% in immigration in years 3 to 5 after treatment (depending on the group). When the negative effect we observe for native workers is around two percent, this would imply a wage elasticity of immigration of -2%/20% = -0.1. This means that for every one percent increase in immigration, native wages would be negatively impacted by 0.1 %. This is roughly in line with estimates in the previous literature (Bratsberg and Raaum, 2012; Borjas, 2013; Edo and Rapoport, 2019).

Moreover, when generalizing our results, one has to be cautious. First, Finland is a very 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.

9 Conclusion

We have documented that lifting labor market testing requirements for non-EU workers increased the inflow of non-EU workers, and most of this increase is due to immigrants already in Finland getting employed in the exempted occupation-region units. Lifting LMT also had a negative effect on the average annual earnings in the affected occupation-region units. Further analyses showed that the most vulnerable to these negative effects are older workers and workers in low-paying occupations. For policymakers, our results reveal costs that should be considered when considering a more liberal policy for less-skilled labor immigration.

The set of outcomes we study is not completely exhaustive, and there can be some benefits to increased immigration that we are not able to identify. It should also be noted that the results presented in this paper are short-term effects of economic immigration under an employer-based system of labor migration. Long-term and general equilibrium effects of labor market testing rules are out of the scope of this paper. Despite this, it is still important to understand what the immediate effects of immigration are for incumbent workers. Further, our research lends a hand to the discussion of optimal immigration policies. Our results suggest that removing LMT requirements may increase immigration and help firms grow, but this may come at a cost of increasing income inequality if removing LMT requirements lowers wages at the bottom of the income distribution but not elsewhere.

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Regulating Labor Immigration: The Effects of Lifting Labor Market Testing

Supplementary Appendix for Online Publication

A Online Appendix: Other outcomes (occupation-region level estimates)

A.1 Occupation-region level V/U, V & U by quartile

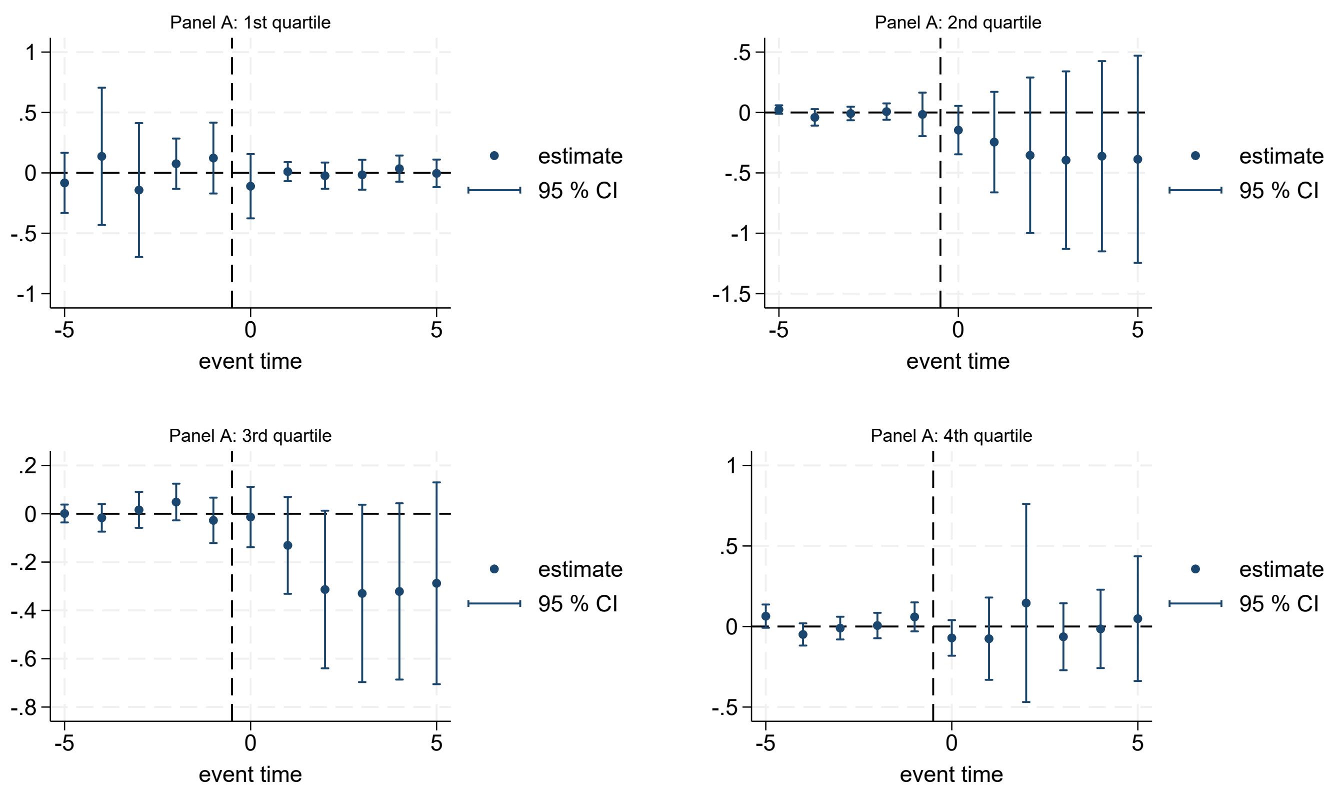

Figure A1: Occupation-region level V/U by quartile

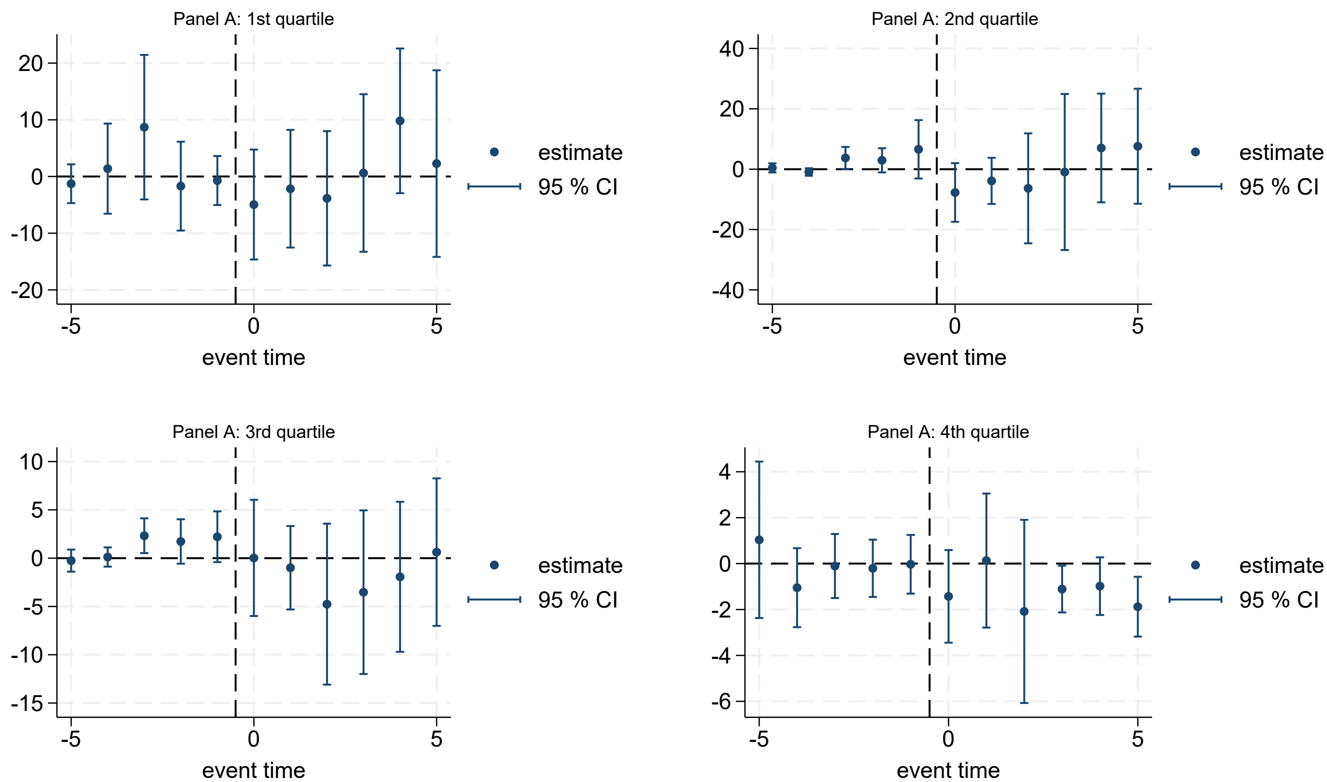

Figure A2: Occupation-region level V by quartile

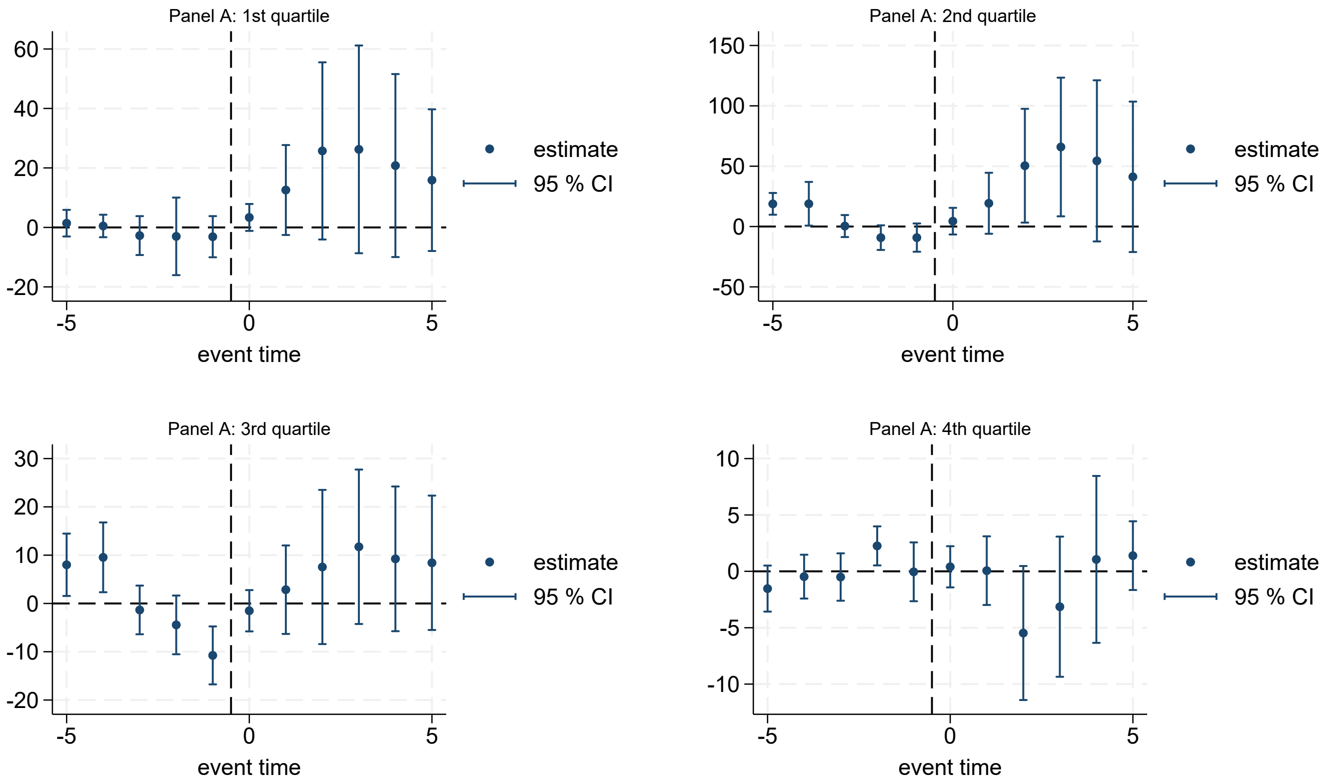

Figure A3: Occupation-region level U by quartile

A.2 Effect on the earnings of new workers

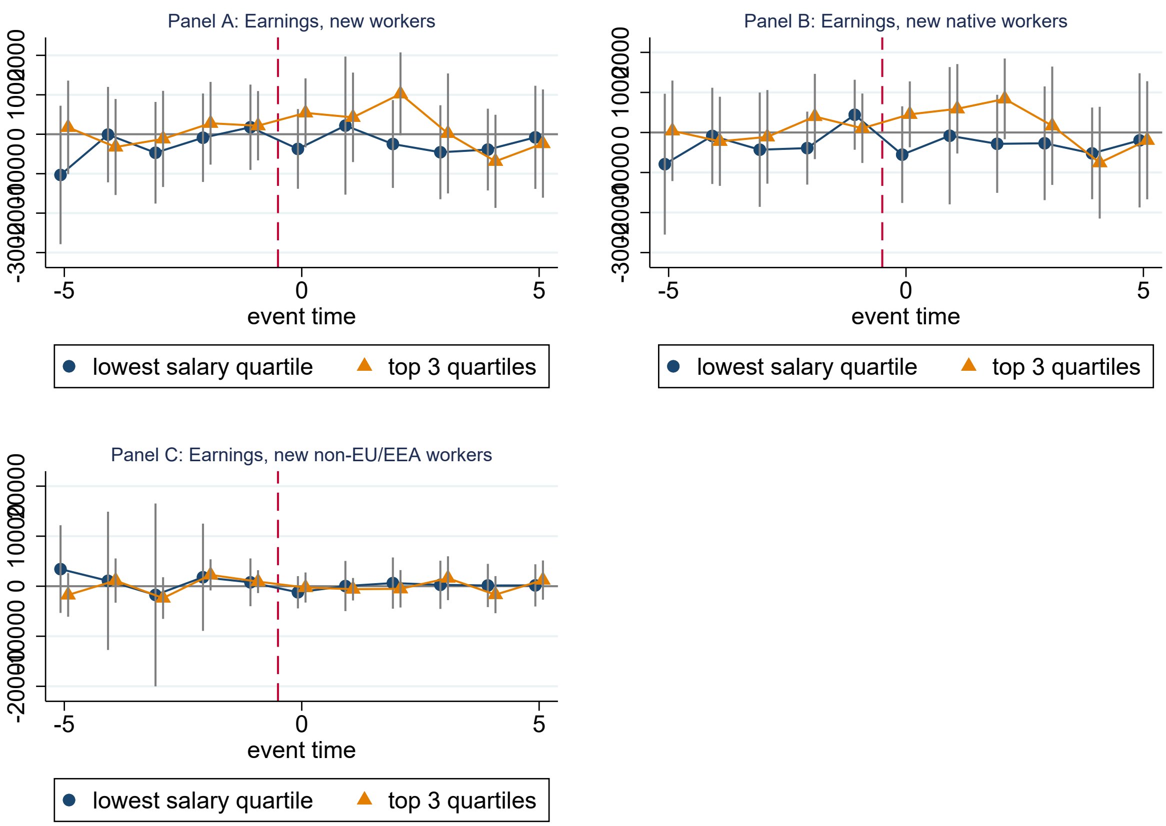

Figure A4: Effects on annual earnings, new workers

Notes. 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.3 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 A5 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 A5: Log(number of workers)

Notes. 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.

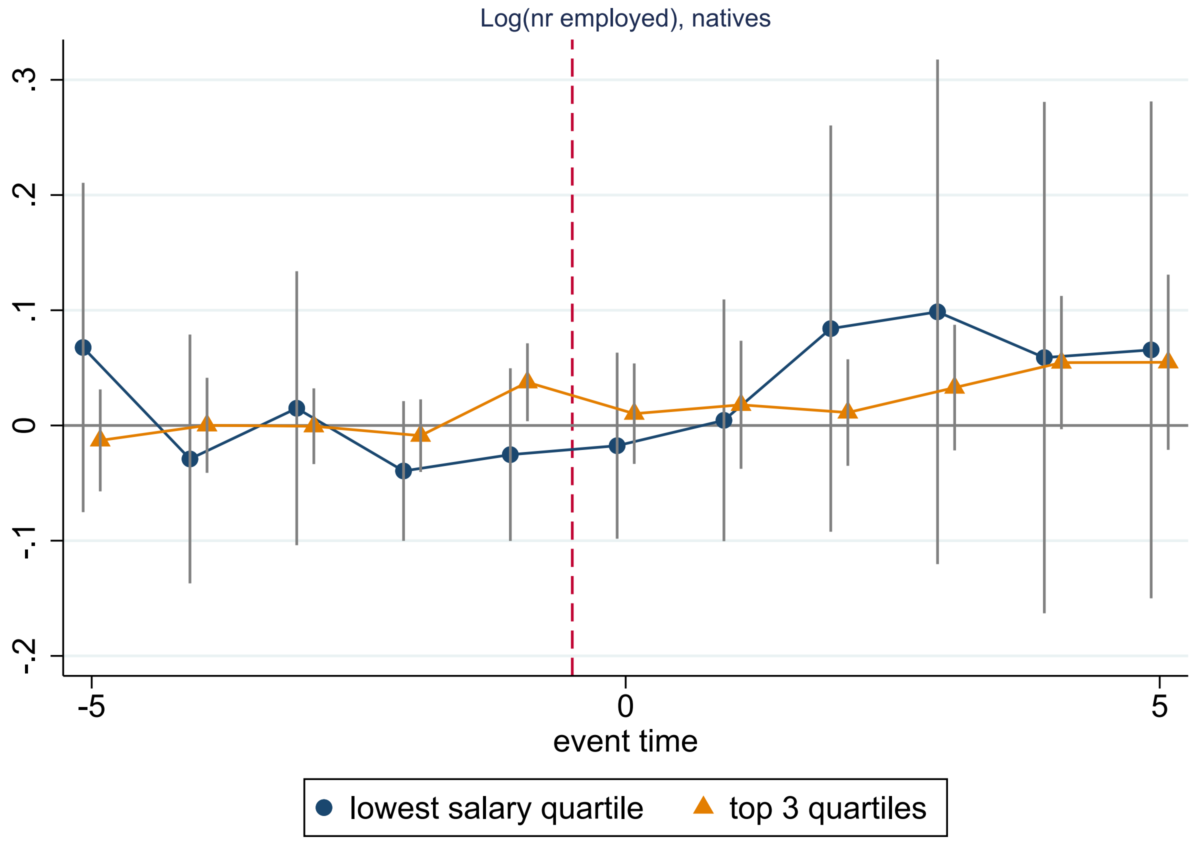

Figure A6: Log(number of native workers)

Notes. 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.4 Working hours and overtime

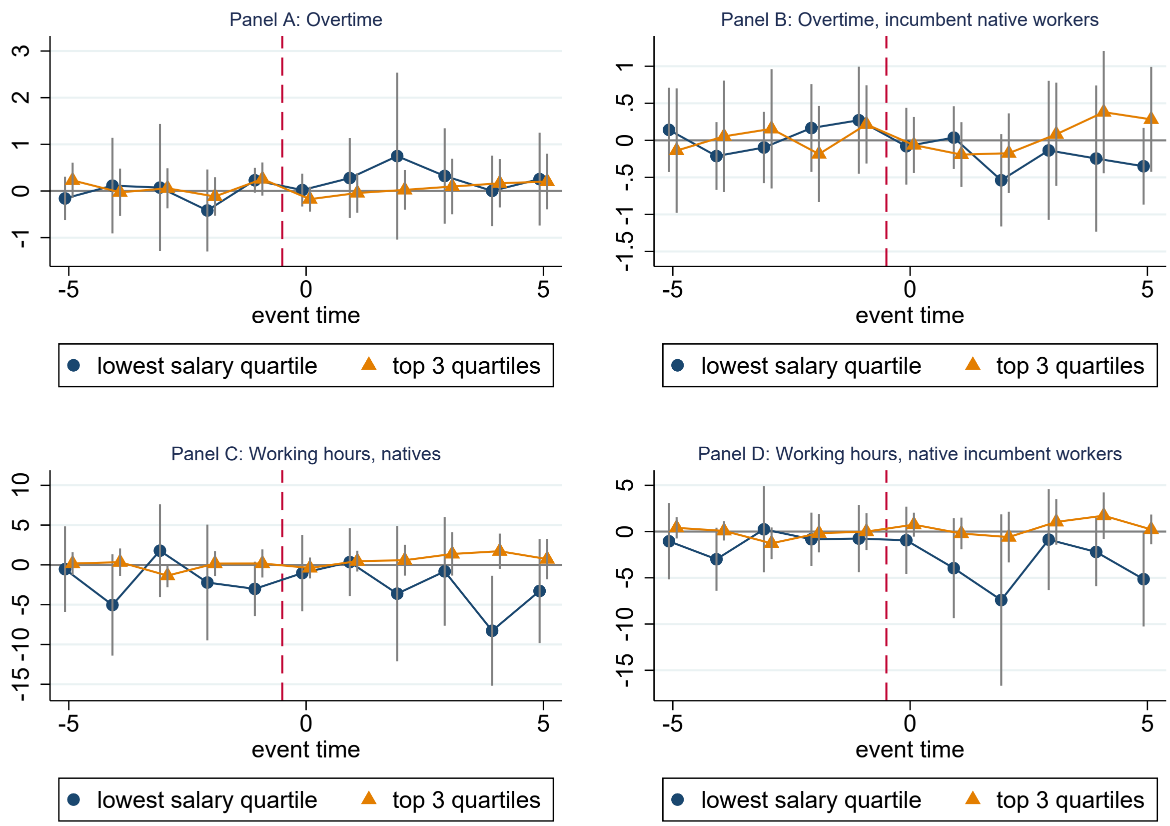

Figure A7: Effects on overtime and total working hours

Notes. 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.

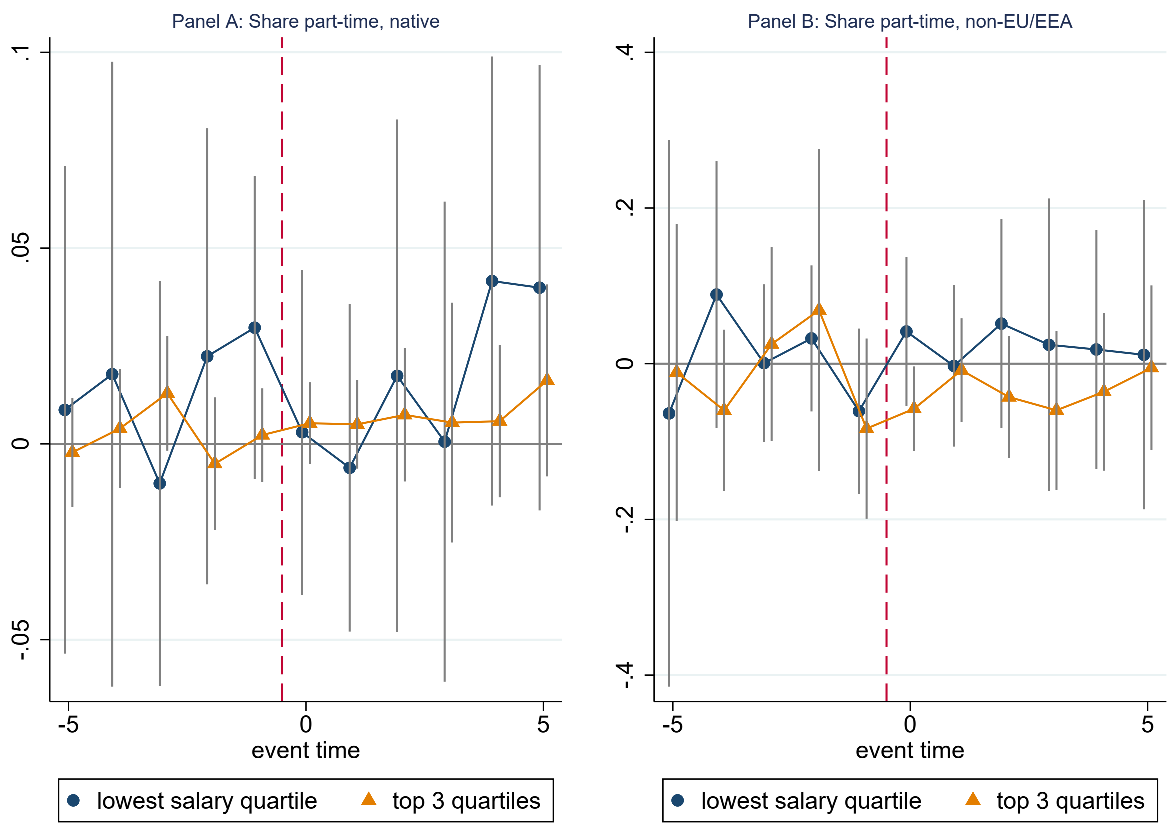

Figure A8: Effects on the share of part-time workers for natives and non-EU workers

Notes. 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.5 Outflows

Figure A9 shows the effects of lifting labor market testing requirements on the outflow of workers from treated professions. Figure A10 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 A11 shows outflows to education, unemployment, outside of the labor force, and pension.

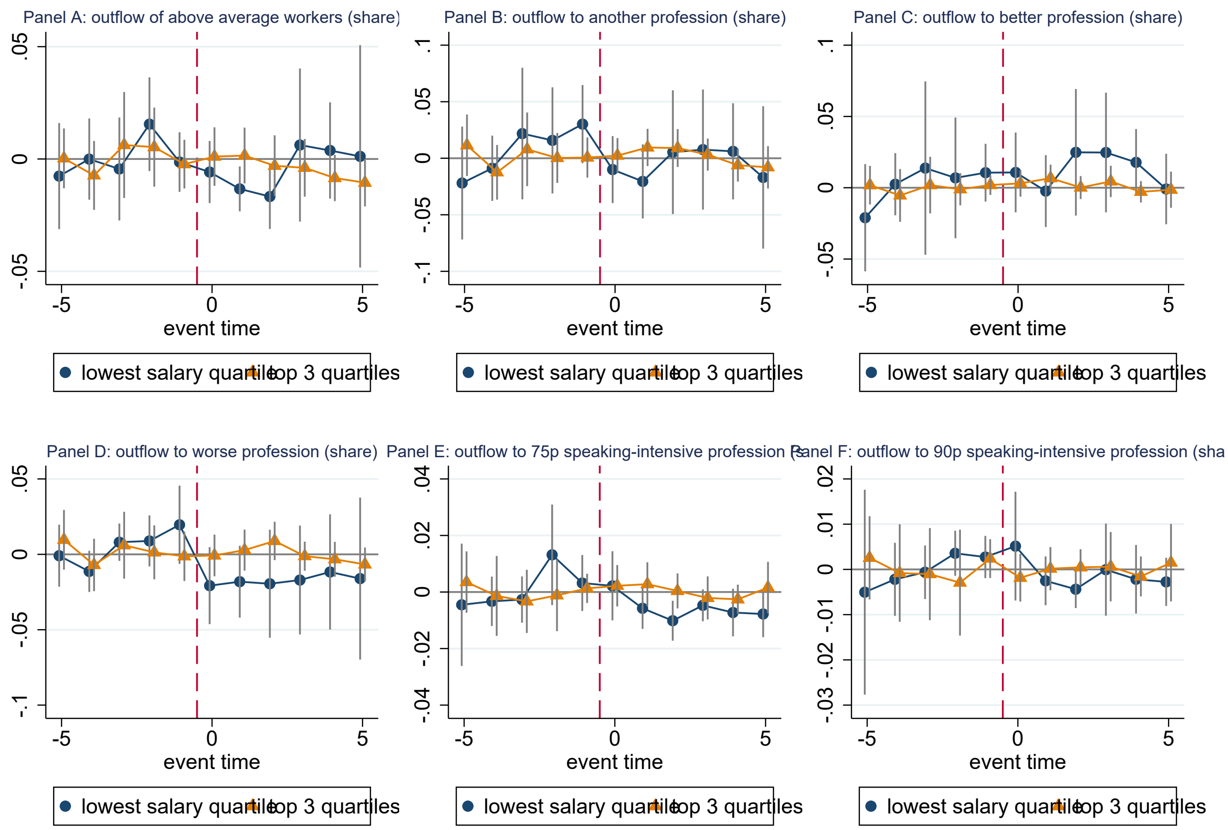

Figure A9: Effects on outflow to other professions

Notes. 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.

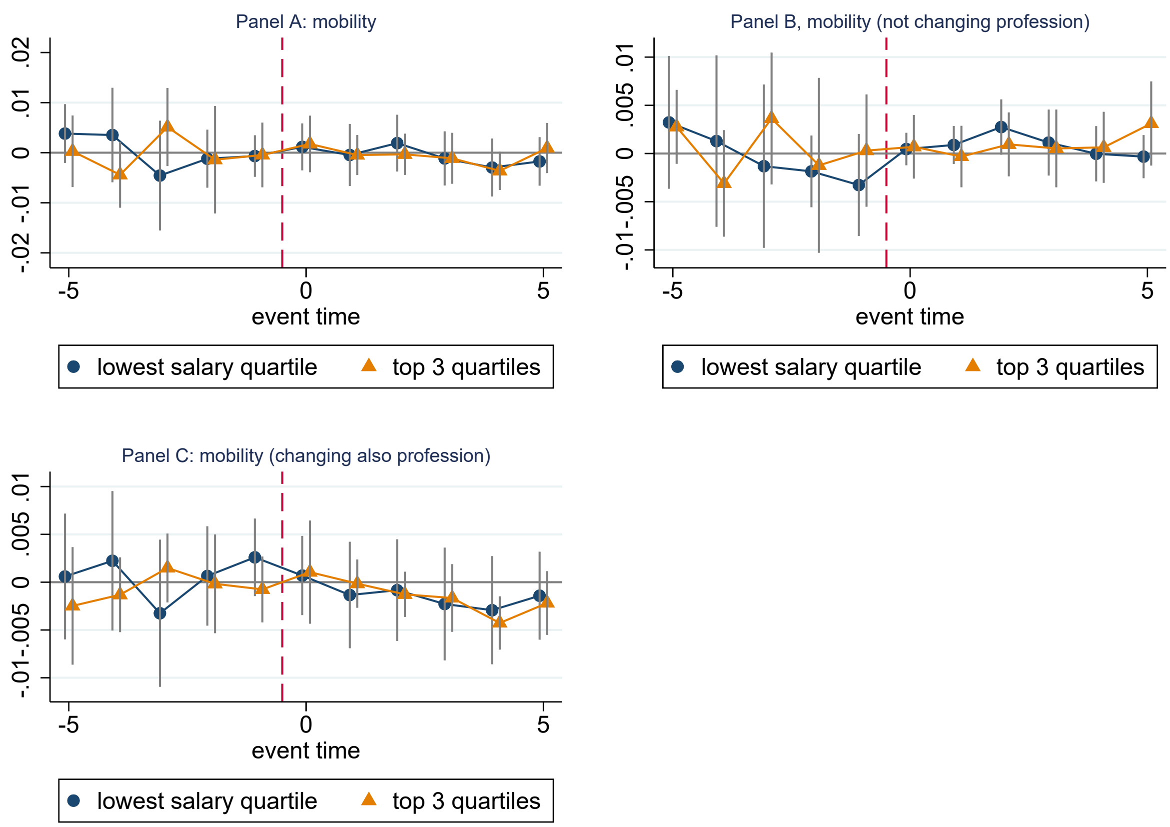

Figure A10: Mobility

Notes. 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.

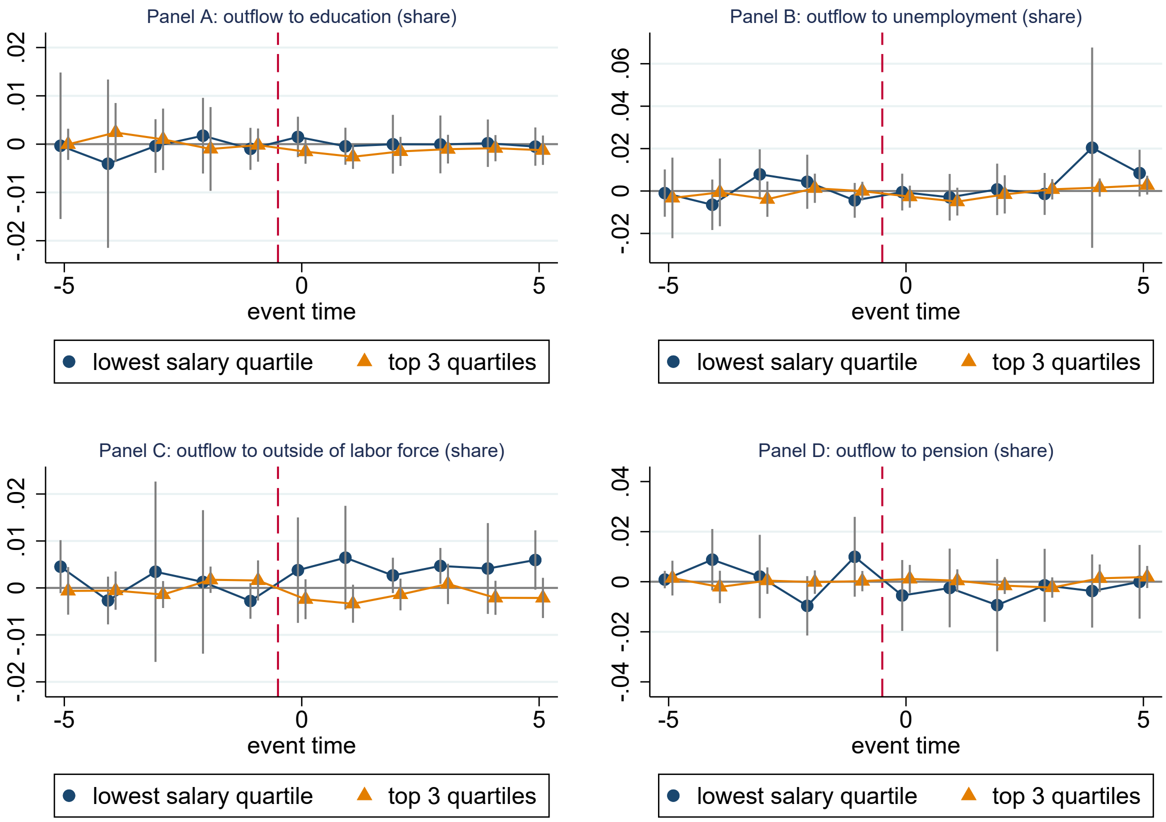

Figure A11: Effects on other outflows

Notes. 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.6 Heterogeneity by public/private sector and gender

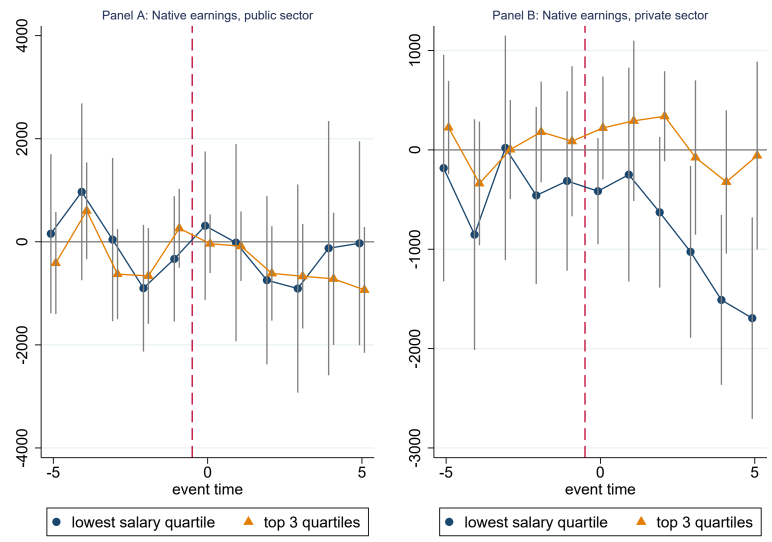

Figure A12: Heterogeneity of the earnings effect, public vs. private sector jobs

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

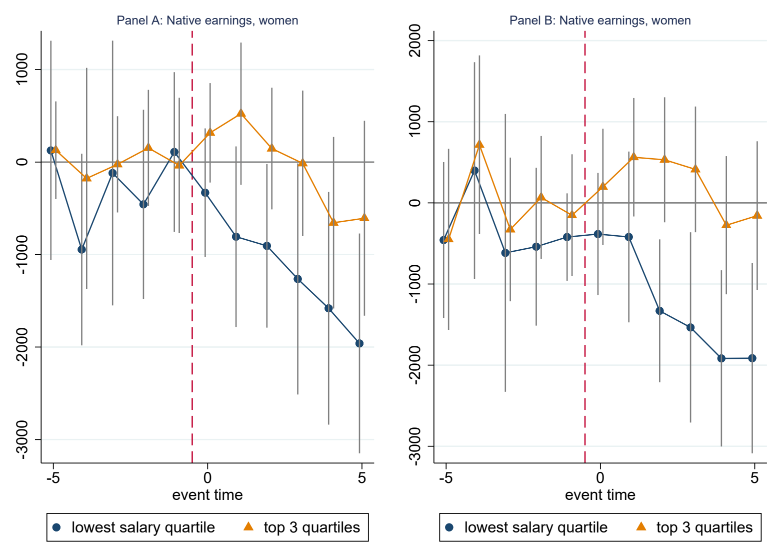

Figure A13: Heterogeneity of the earnings effect by gender

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

A.7 Effect on annual earnings: heterogeneity by profession group (1-digit level)

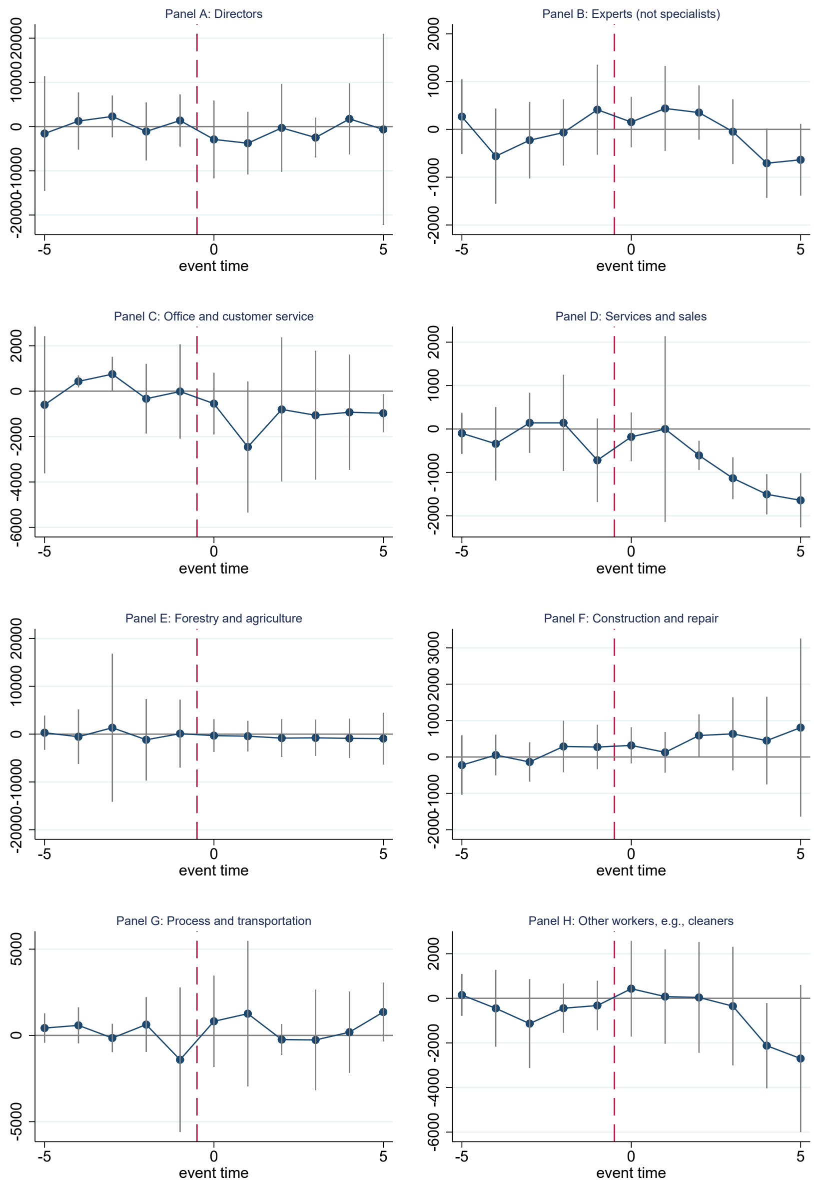

Figure A14: Effect on native earnings, heterogeneity by profession group

Notes. 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.8 Heterogeneity between cities and countryside

Stock of foreign workers

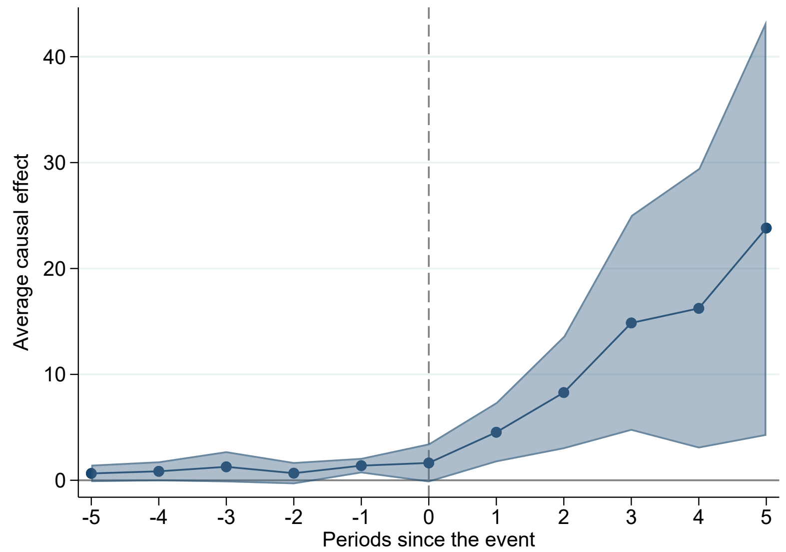

Figure A15: Stock of foreign workers in cities

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

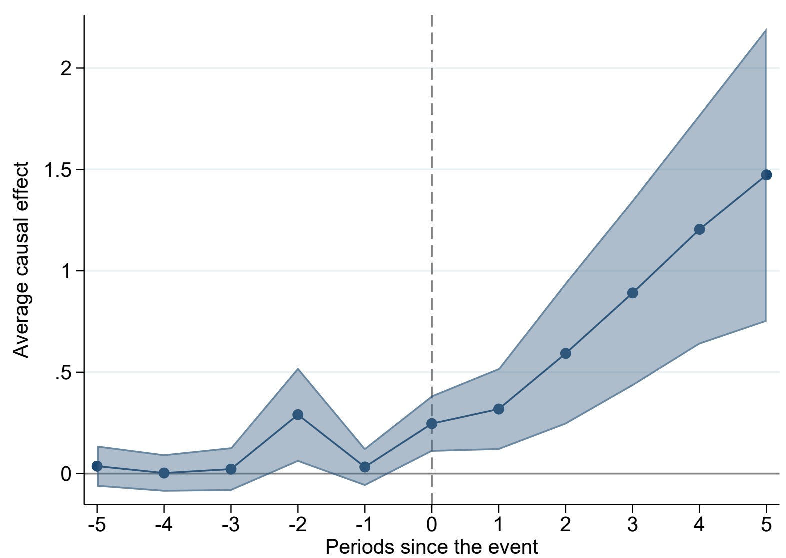

Figure A16: Stock of foreign workers in non-city urban areas

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

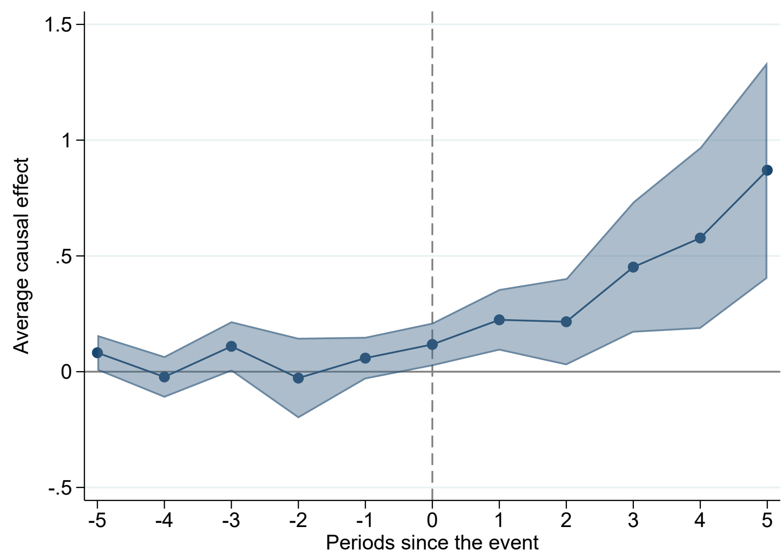

Figure A17: Stock of foreign workers in rural areas

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

Earnings effect

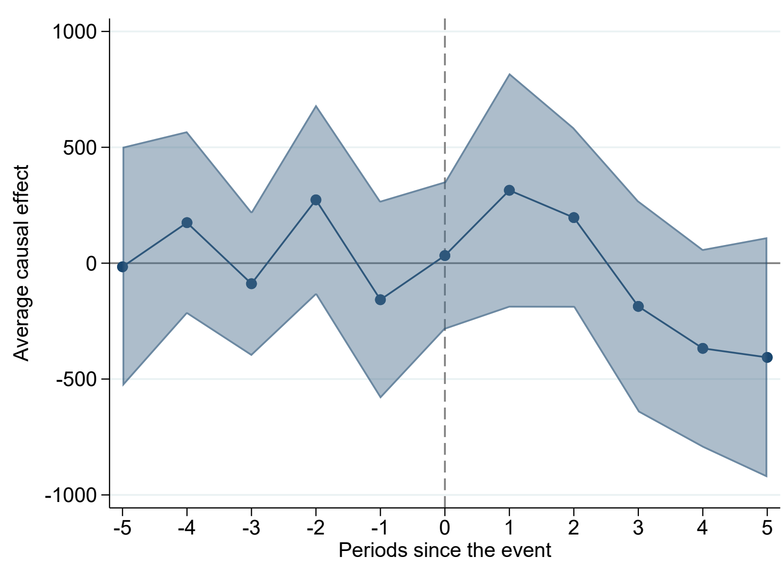

Figure A18: Earnings effect in cities

Notes. Figure shows the estimate of the earnings effect in cities.

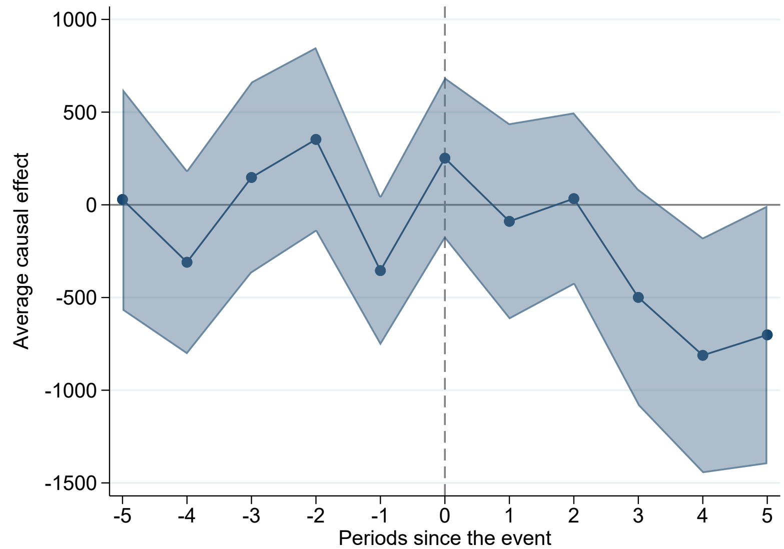

Figure A19: Earnings effect in non-city urban areas

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

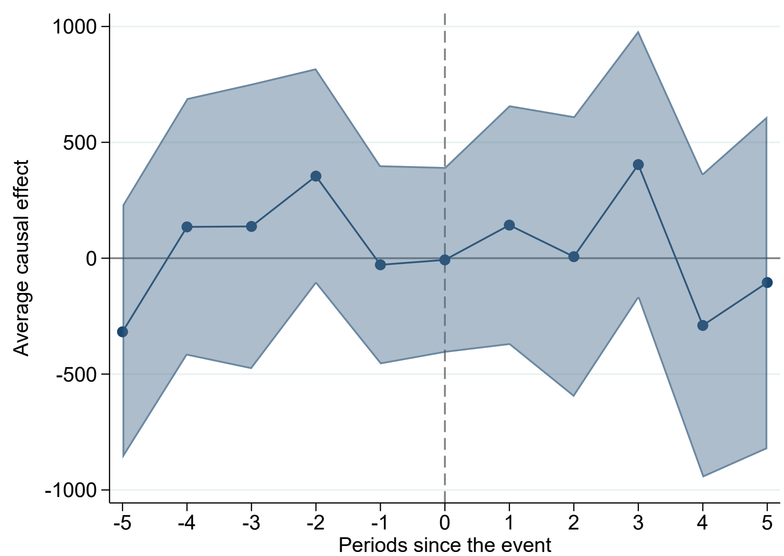

Figure A20: Earnings effect in rural areas

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

A.9 Robustness to dropping Lapland and seasonal worker occupations

Stock of foreign workers

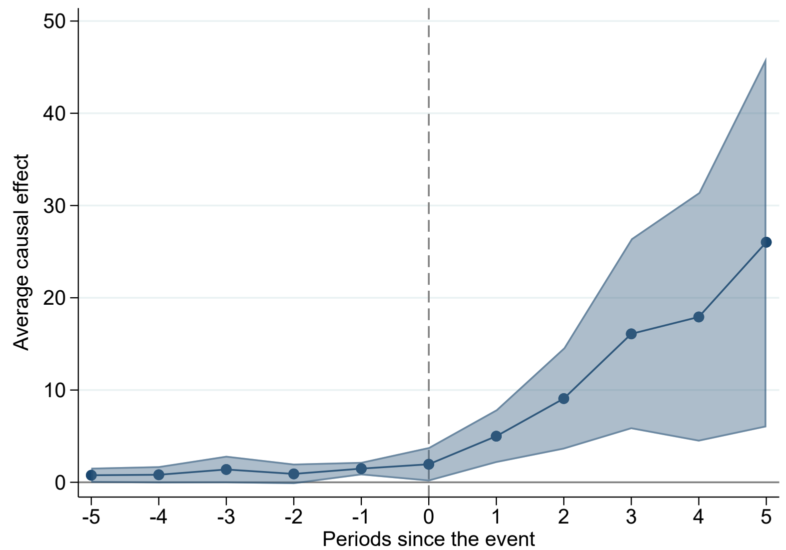

Figure A21: Stock of foreign workers, seasonal workers dropped (definition 1)

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

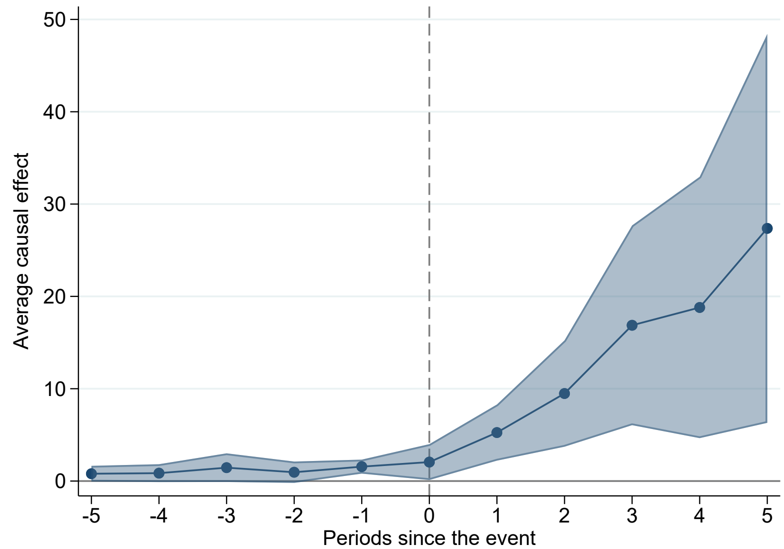

Figure A22: Stock of foreign workers, seasonal workers dropped (definition 2)

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

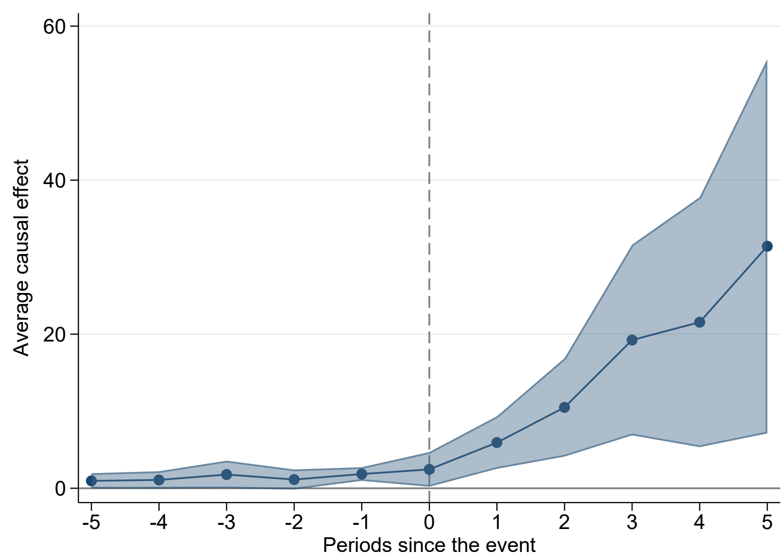

Figure A23: Stock of foreign workers, without Lapland

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

Earnings effect

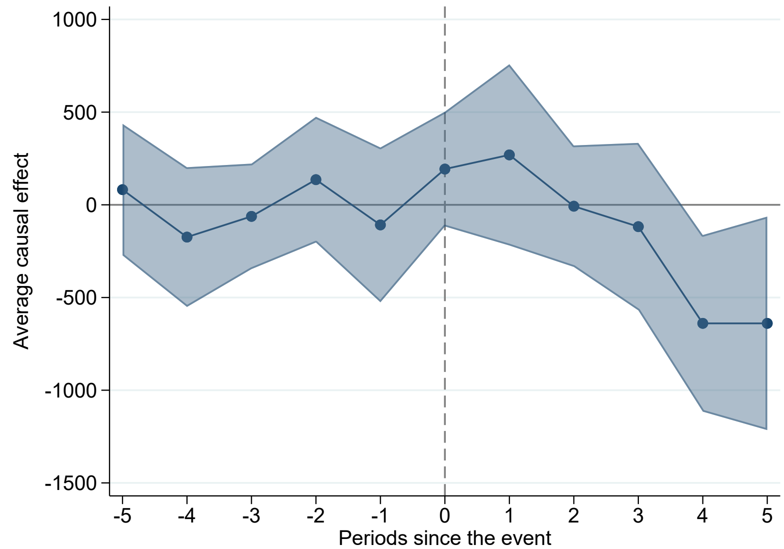

Figure A24: Earnings effect, seasonal workers dropped (definition 1)

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

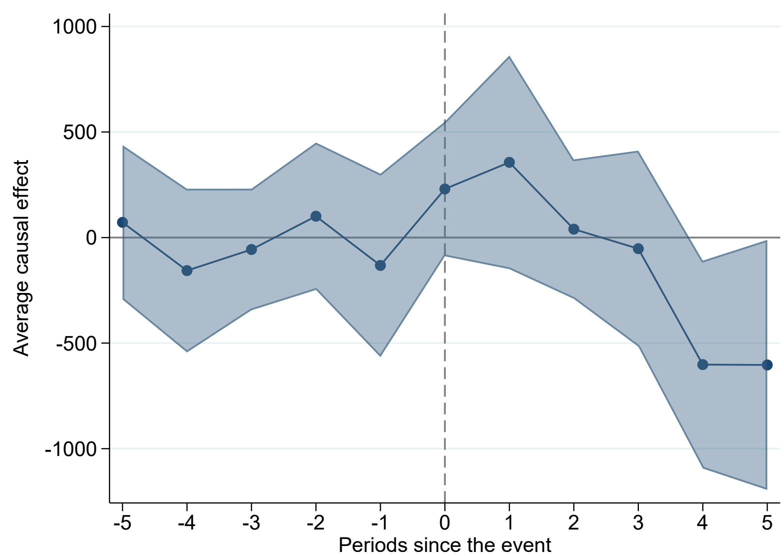

Figure A25: Earnings effect, seasonal workers dropped (definition 2)

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

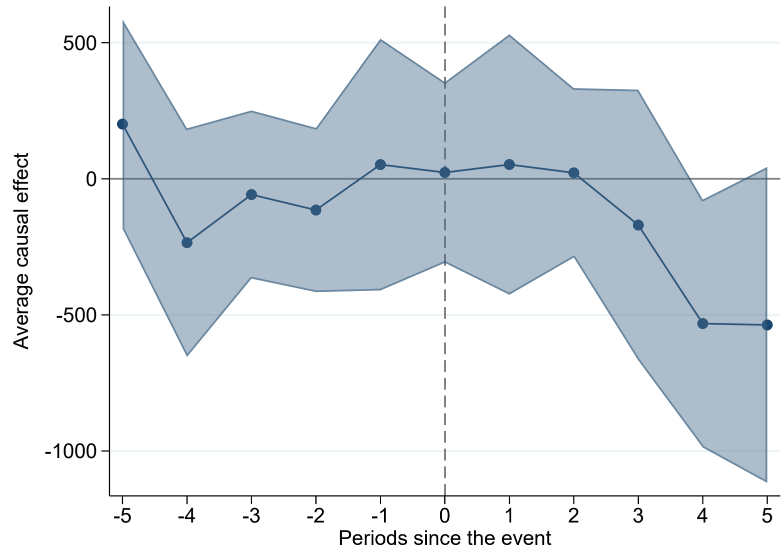

Figure A26: Earnings effect, without Lapland

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

A.10 Decomposing the inflow effect into components based on previous employment: those coming from other occupations and those entering from non-employment

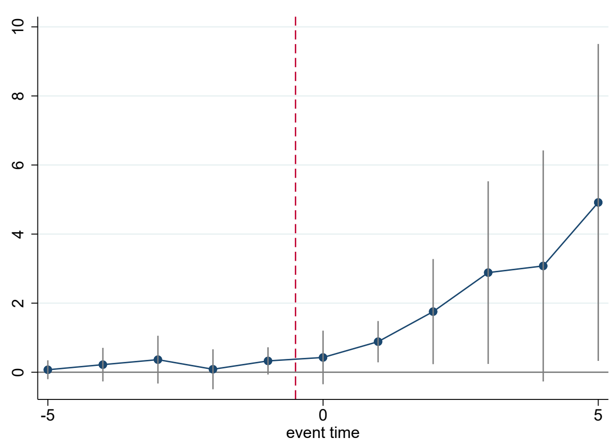

Figure A27: Inflow of non-EU workers who already worked in some occupation during the previous year

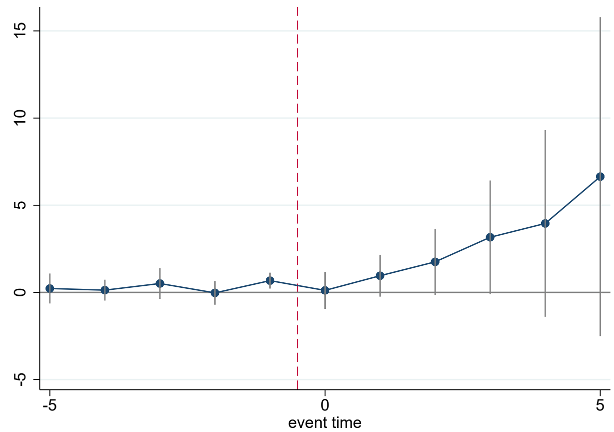

Figure A28: Inflow of non-EU workers who did not work in any occupation during the previous year

A.11 Heterogeneity by establishment size (effects on occupation-region level averages in specific types of establishments)

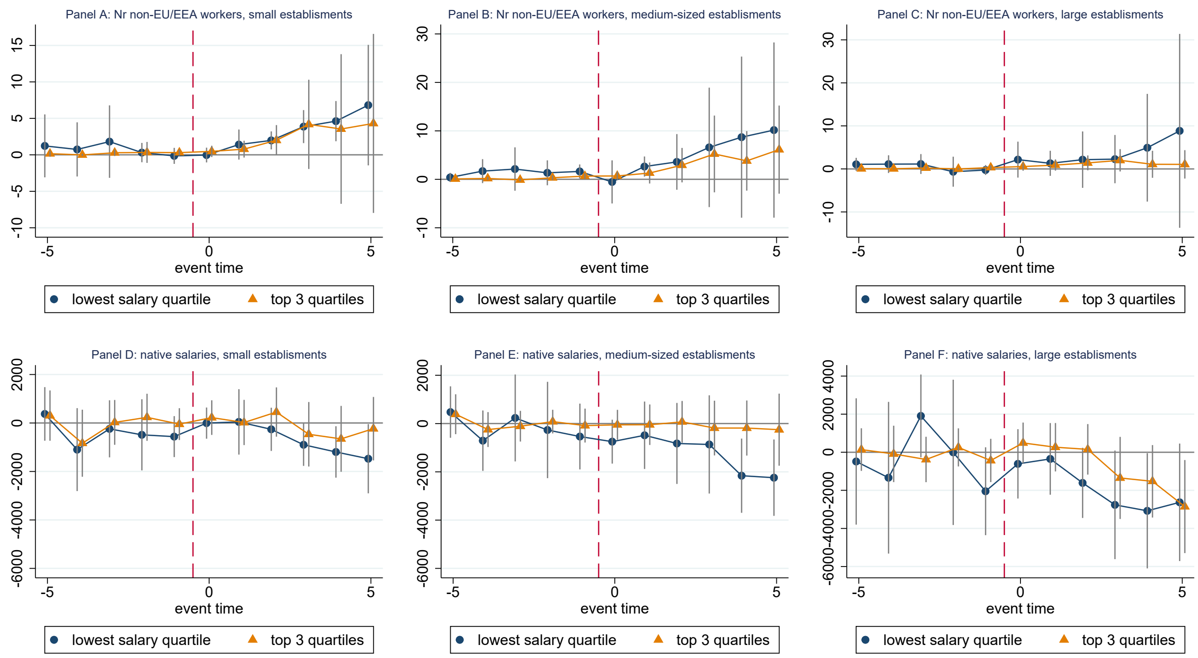

Figure A29: Effect on the earnings of native workers by the size of firm establishment

Notes. 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.

B Online Appendix: Expansion of treatment, 2012-2021

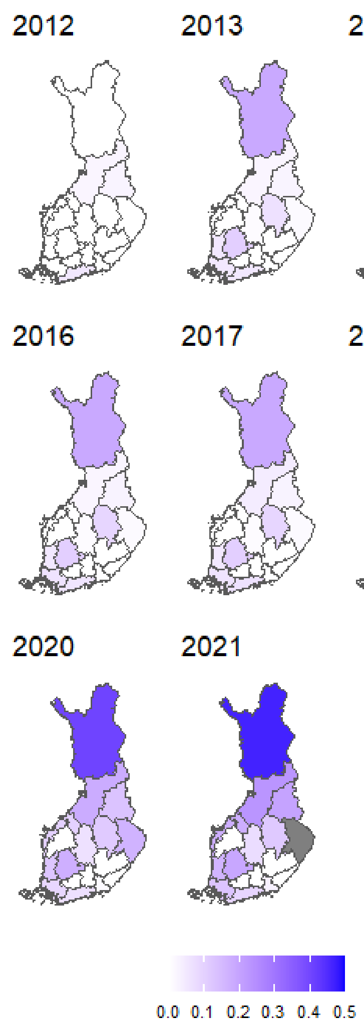

Figure B1: Staggered treatment: share occupations that are treated, 2012-2021

Notes. 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).

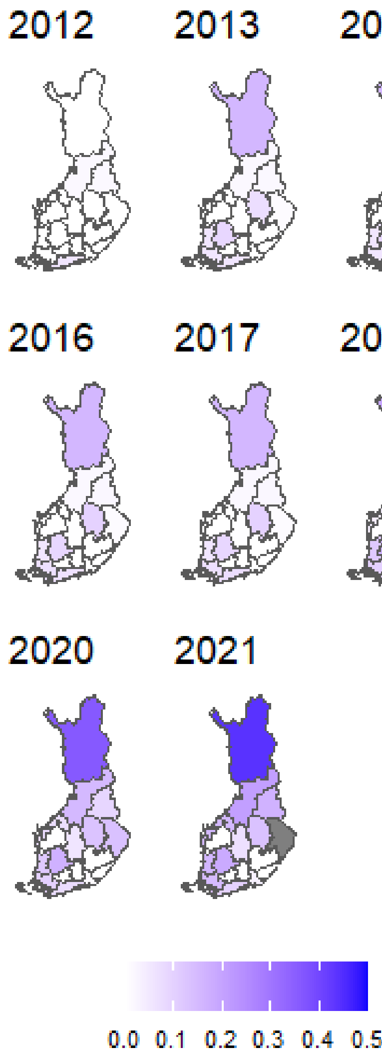

Figure B2: Staggered treatment: share of non-specialist occupations that are treated, 2012-2021

Notes. 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.

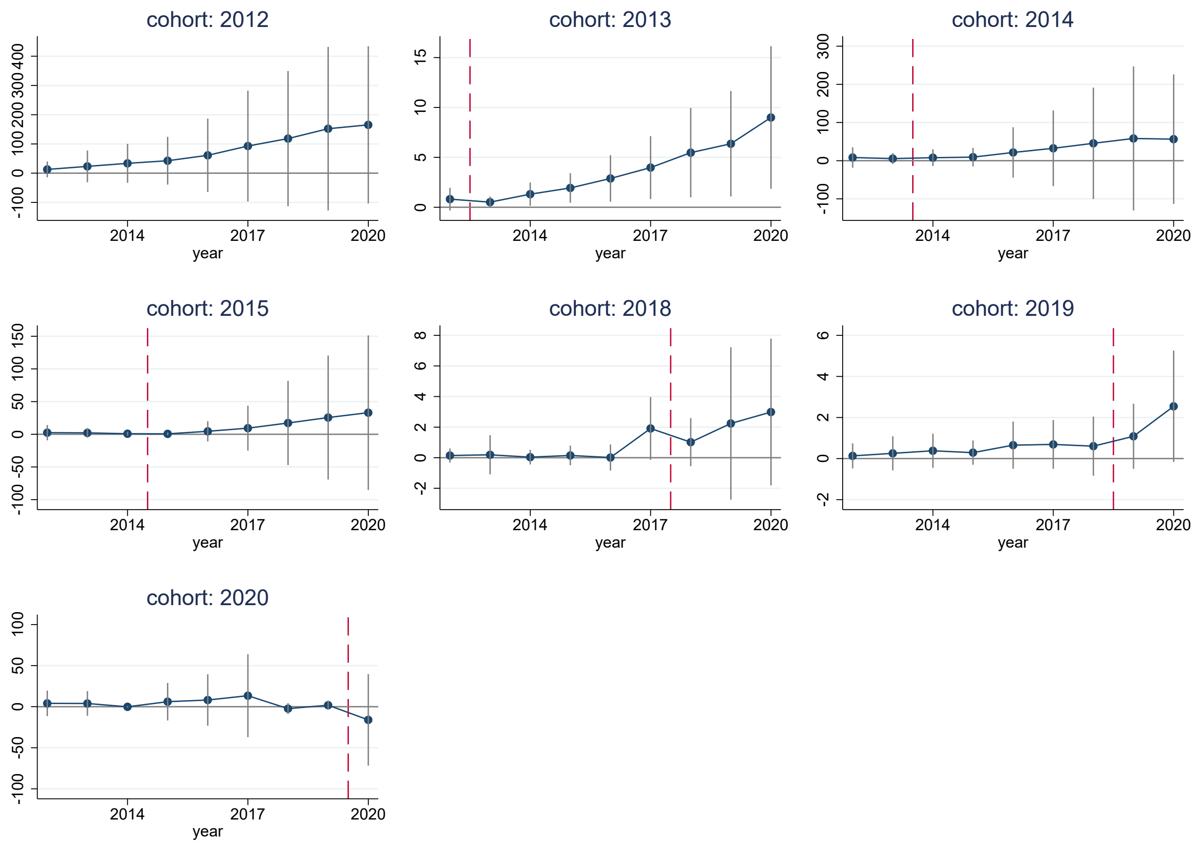

Figure C1: Number of non-EU workers

Notes. Figure shows event study plots for each treated cohort.

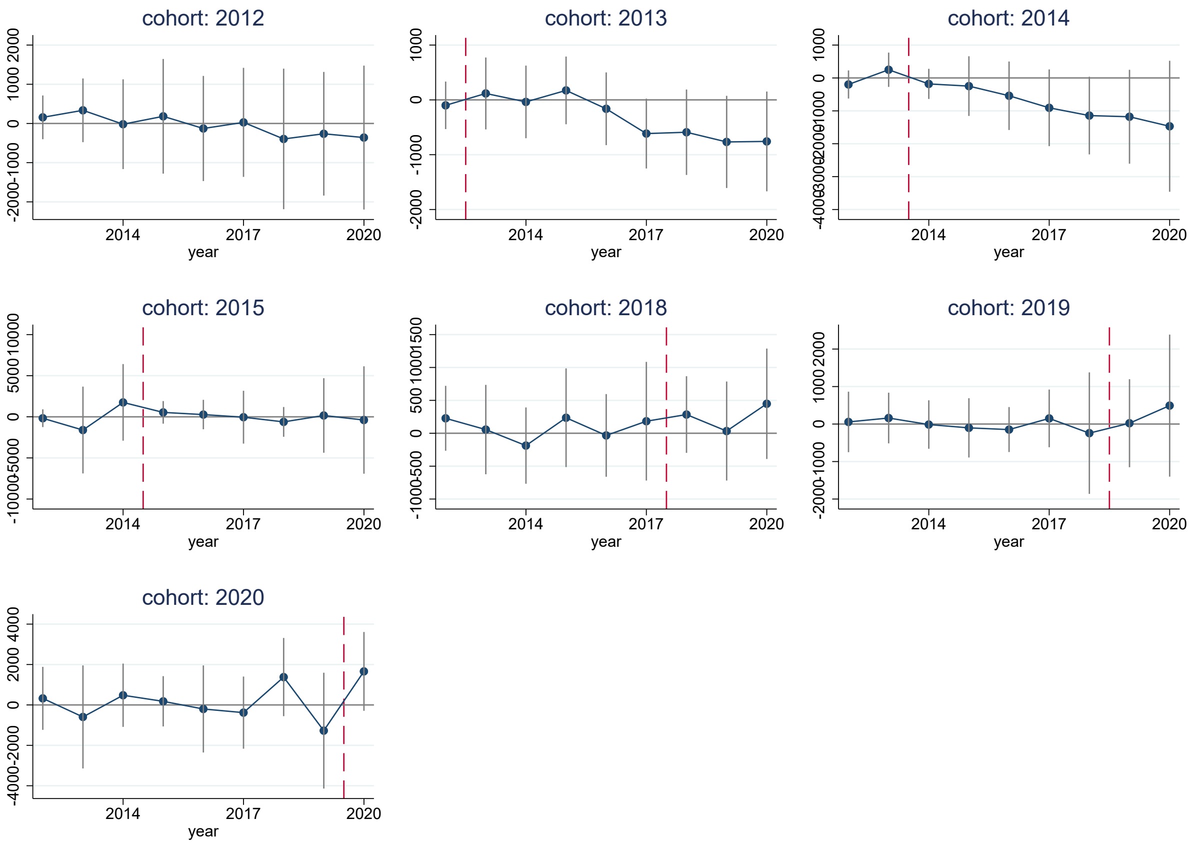

Figure C2: Annual earnings

Notes. Figure shows event study plots for each treated cohort.

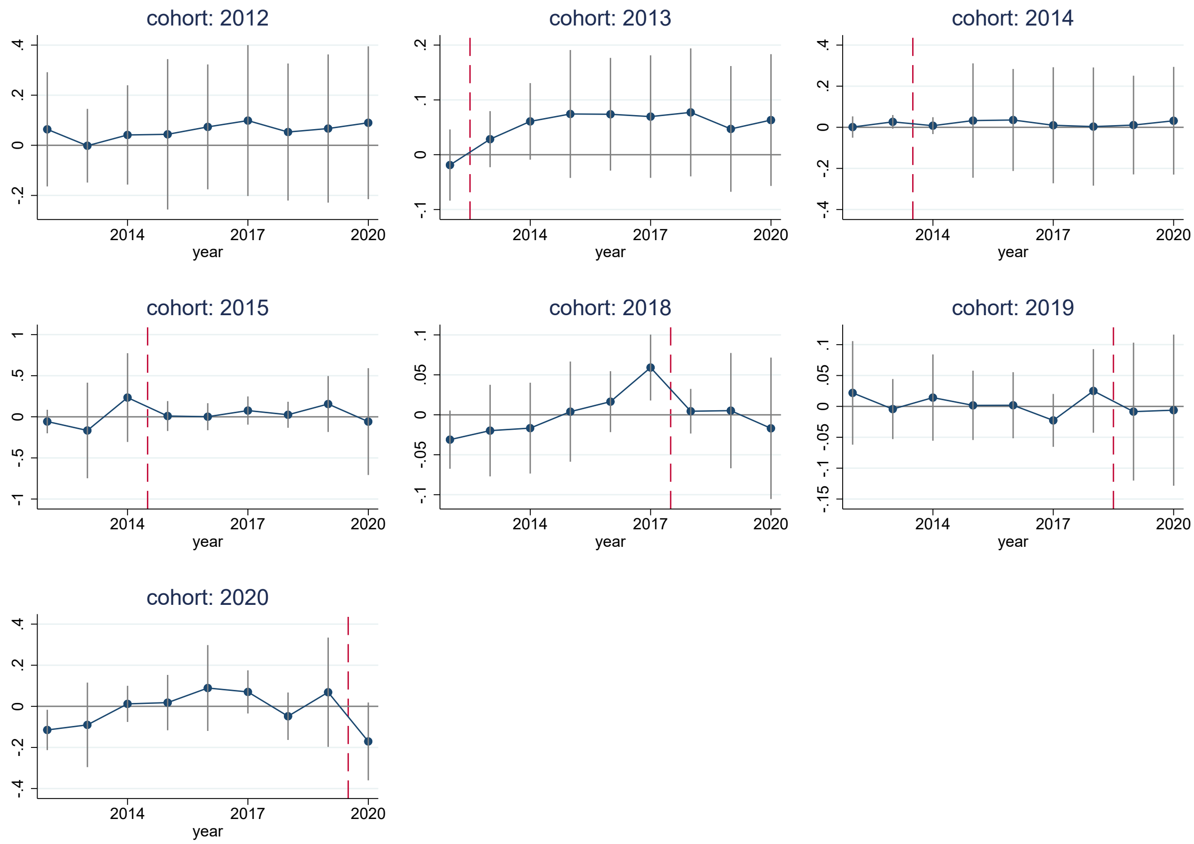

Figure C3: Number of workers

Notes. Figure shows event study plots for each treated cohort.

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

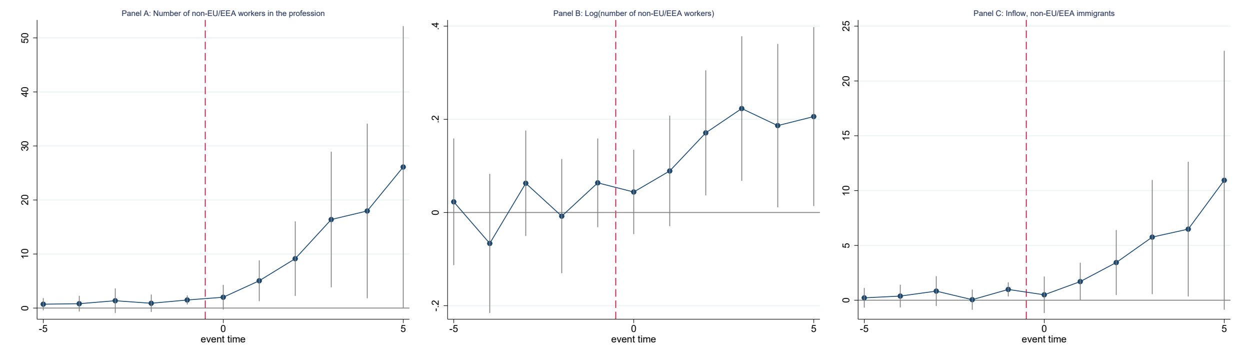

Figure D1: Stock and inflow of foreign workers, not yet treated

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

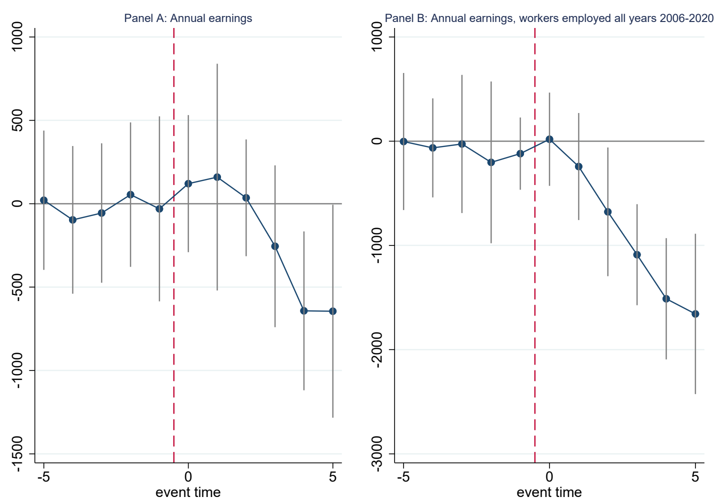

Figure D2: Earnings

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

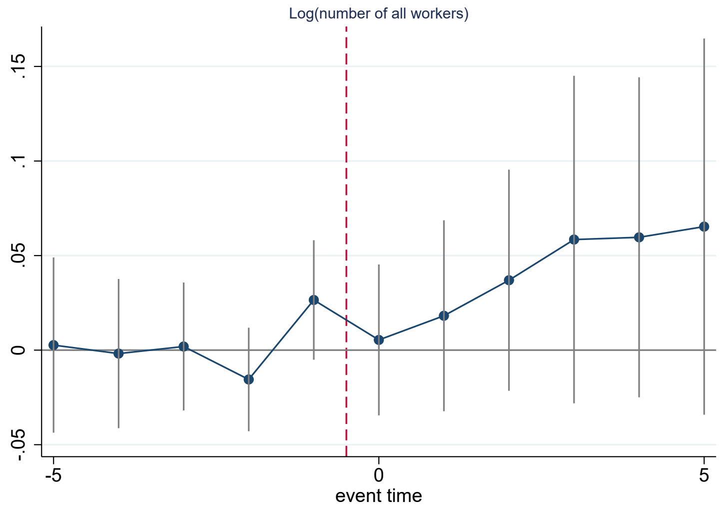

Figure D3: Log(nr all workers)

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

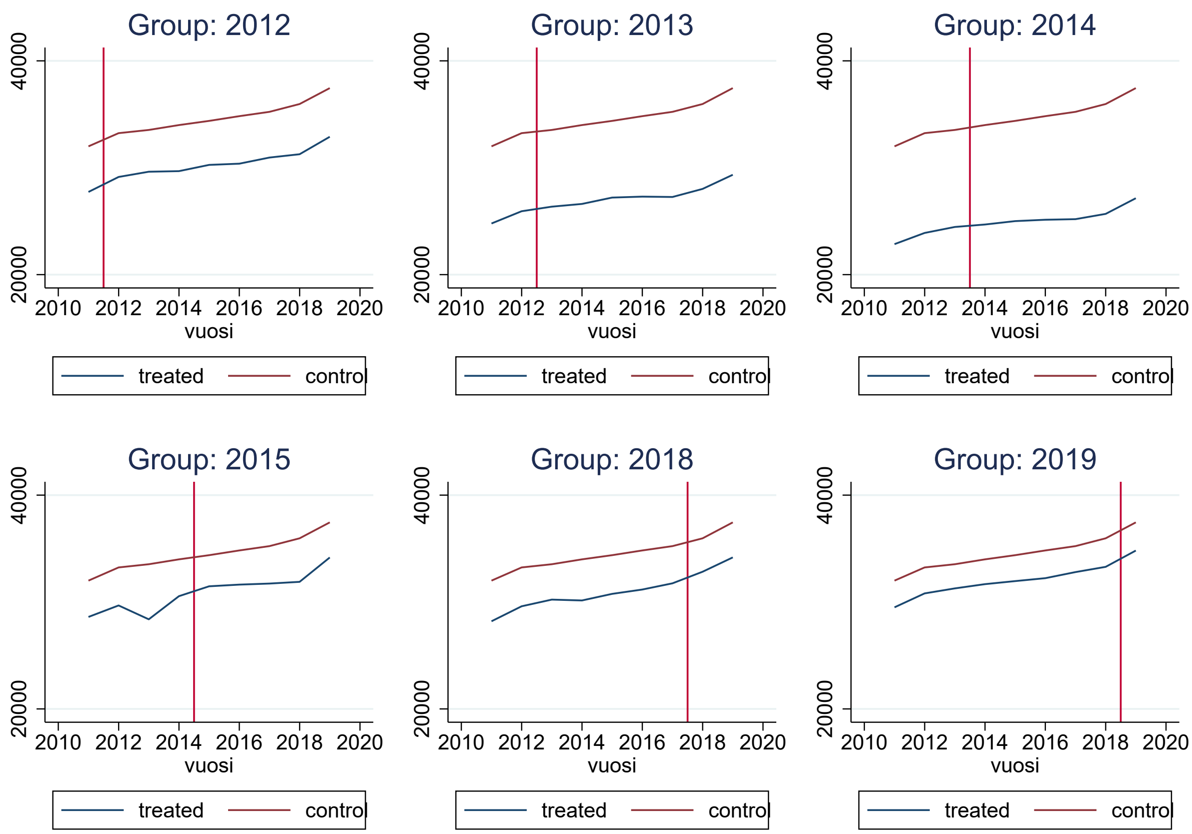

Figure E1: Salaries in treated vs. control professions

Notes. 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. 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

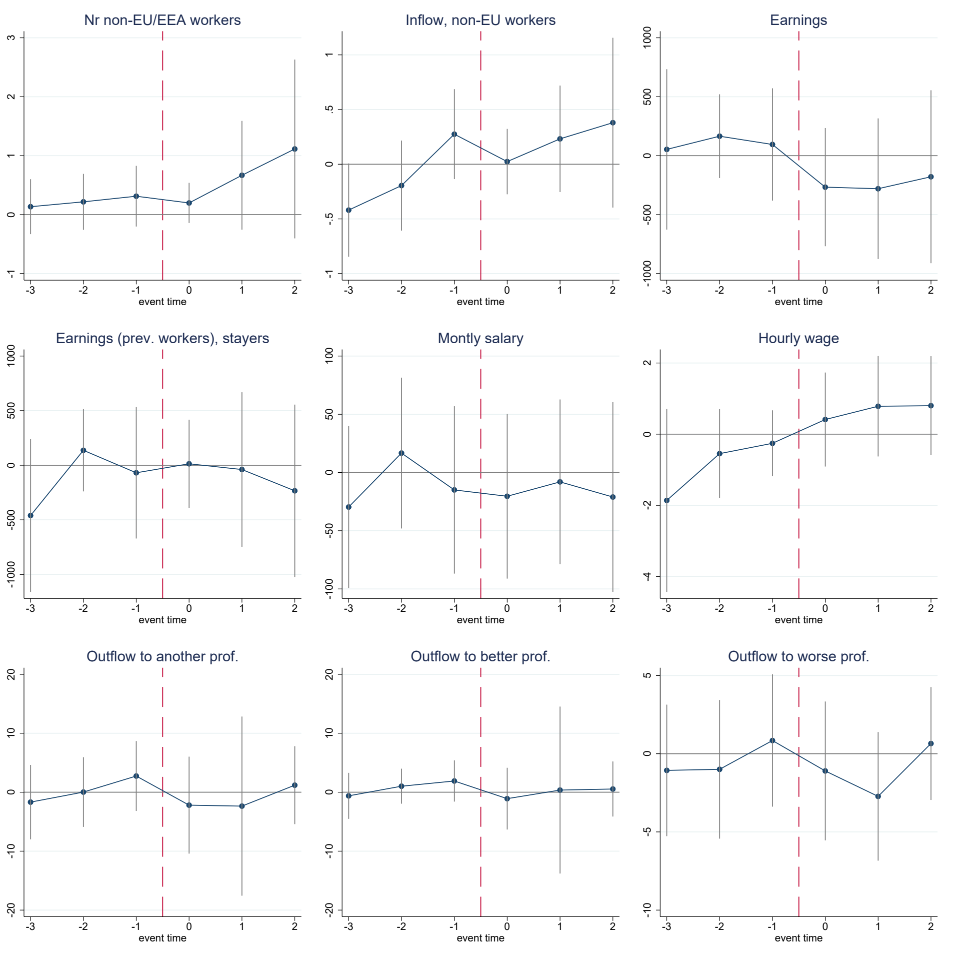

Figure F1: Placebo

Notes. Figure shows placebo estimates for different outcome variables.

G Online Appendix: Main results using other event study estimators

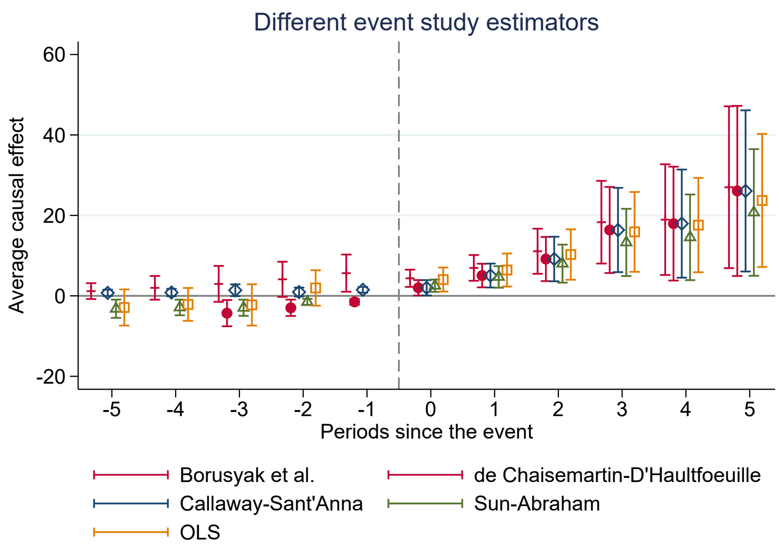

Figure G1: Effect on the number of non-EU workers

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

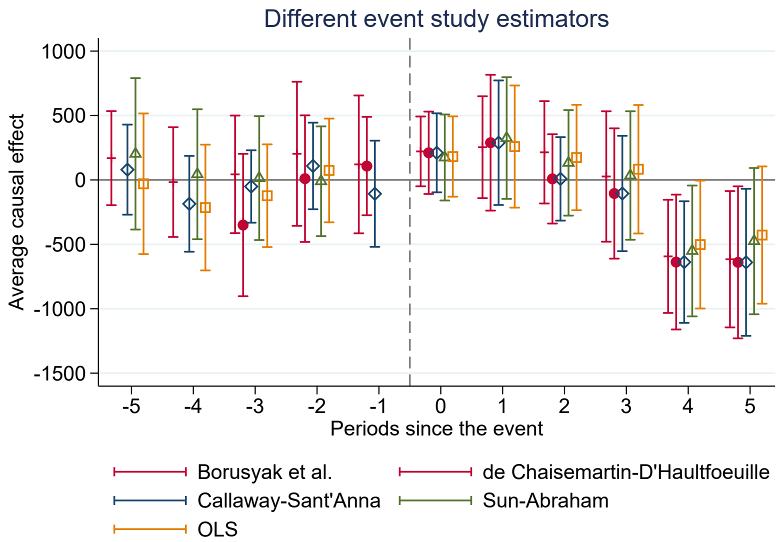

Figure G2: Effect on annual earnings of native workers

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

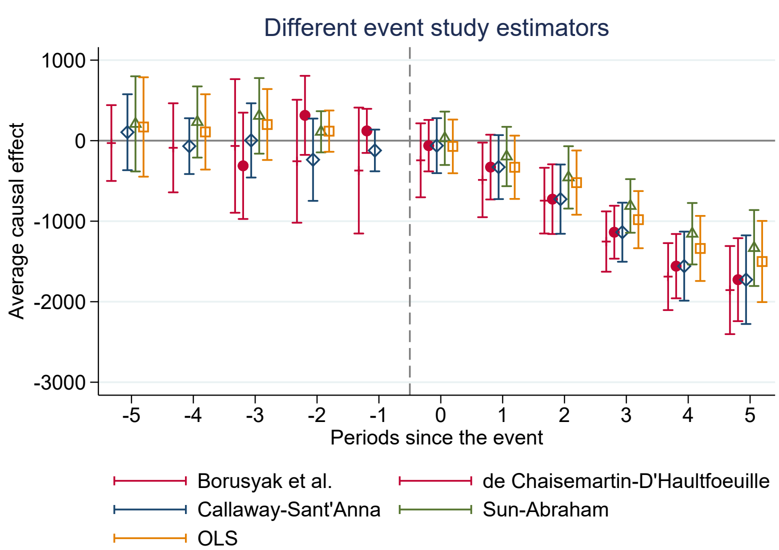

Figure G3: Effect on annual earnings of incumbent native workers

Notes. Effect on annual earnings of incumbent natives. Figure shows 5 different event study estimates.

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

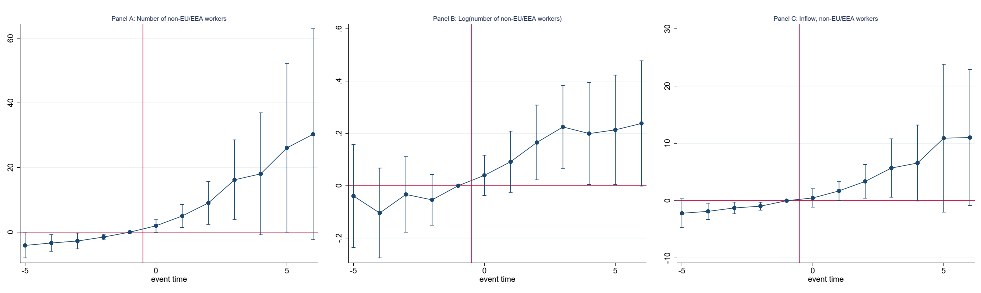

Figure H1: Effect on the stock and inflow of non-EU workers

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

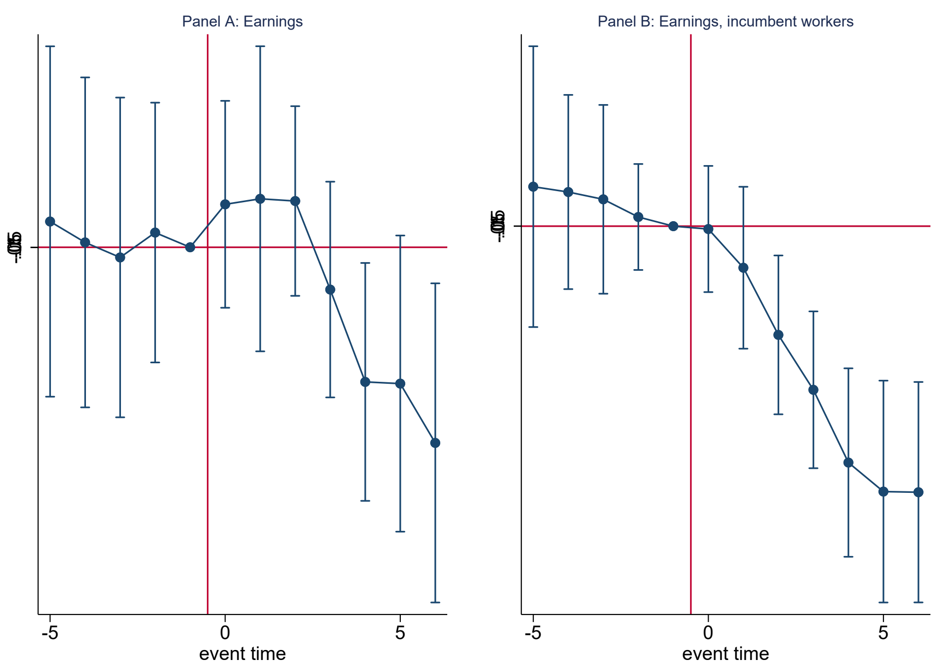

Figure H2: Effect on annual earnings

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

I Online Appendix: Earnings effects by occupation type

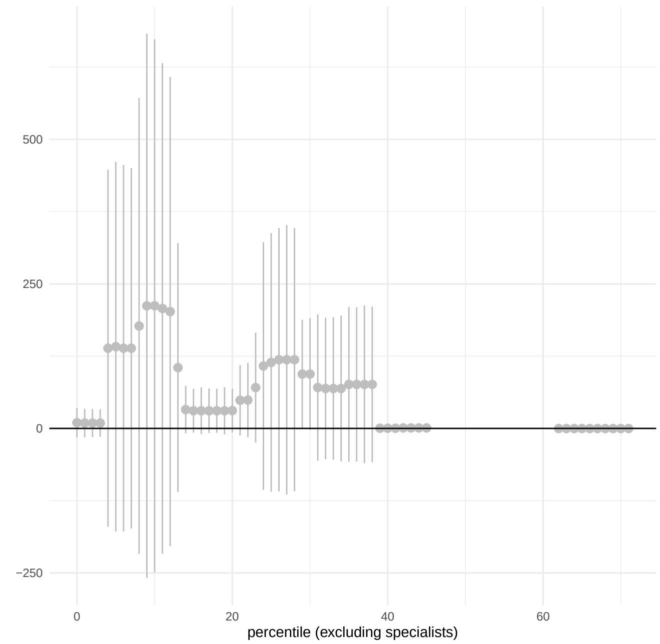

Figure I3: Service workers (groups 5 and service occupations in group 9), nr immigrants

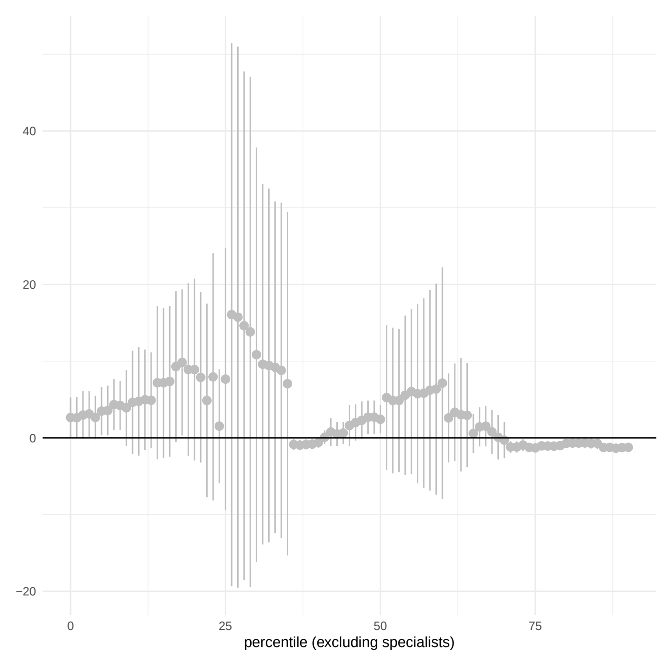

Figure I4: Non-service workers, nr immigrants

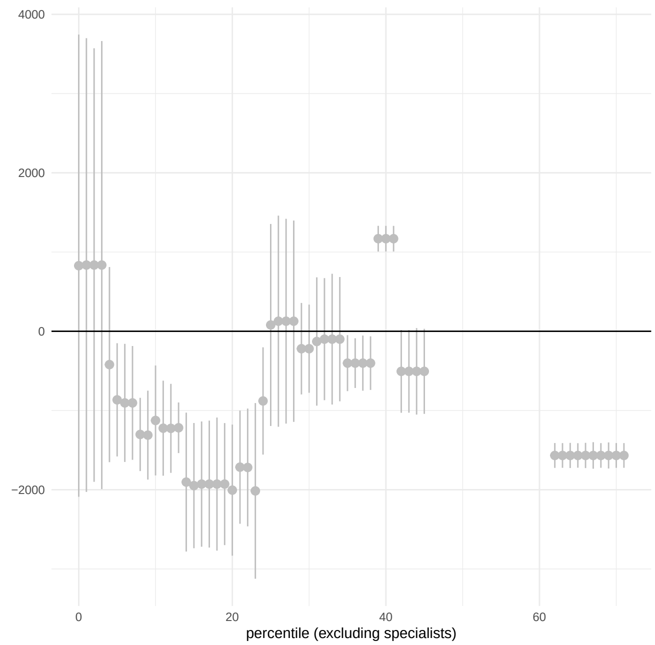

Figure I5: Service workers (groups 5 and selected occupations in group 9), earnings

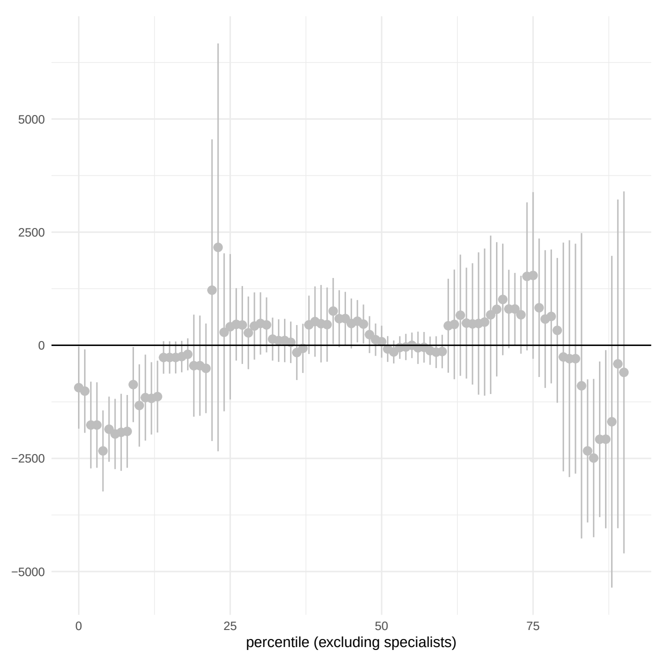

Figure I6: Non-service workers, earnings

J Online Appendix: Additional firm-level figures and tables

	control	treat	difference
nr non-EU workers b	0.457	0.535	0.078
	(3.324)	(2.513)	(0.050)
Number of workers	16.951	16.911	-0.040
	(82.527)	(32.084)	(1.060)
Sales	4.859e+06	3.808e+06	-1.050e+06**
	(3.521e+07)	(1.394e+07)	(453484.281)
Taxes paid	51,643.684	39,378.613	-12265.071*
	(505471.188)	(202498.156)	(6,520.979)
Investments	221517.125	117104.734	-1.044e+05
	(6.762e+06)	(1.124e+06)	(82,091.250)
Share foreign	0.028	0.028	0.000
	(0.106)	(0.096)	(0.002)
Nr establishments	1.401	1.178	-0.223***
	(3.957)	(1.106)	(0.049)
Profits	281814.594	148698.094	-1.331e+05**
	(4.341e+06)	(1.425e+06)	(54,707.230)
Value added per worker	71,844.531	68,914.539	-2,954.215
	(116907.148)	(96,563.344)	(1,830.219)
Observations	6,973	6,973	13,946

Table J1: Balance table for firms after matching

Table J2: Pooled firm-level DiD estimates with coarsened exact matching (MATCH 2)

	Size and personnel					Investments, €1,000							
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	size of firm	nr native workers	nr non-EU	nr EU		all	buildings	machines	IT	labor share	turnover, €1,000	profit ratio	labor productivity
Panel A: 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
Panel B: Firms with 2-10 employees													
Treatment effect	0.180***	0.164***	0.00907*	0.0103**		-7.005	-4.894*	-2.054	-0.0571	-0.440	52.68**	-0.318	-931.2
	(0.0320)	(0.0362)	(0.00467)	(0.00449)		(5.429)	(2.629)	(3.958)	(0.0654)	(0.434)	(47.36)	(0.307)	(865.2))
N	76167	76167	76167	76167		76167	76167	76167	76167	75316	76167	75473	75541
Panel C: Firms with 10-50 employees													
Treatment effect	0.368	0.289	0.00374	-0.00120		-39.54**	-27.56*	-11.20	-0.207	-0.207	34.35	0.00771	-2101.7
	(0.346)	(0.353)	(0.0301)	(0.0252)		(19.21)	(16.19)	(7.455)	(0.741)	(0.138)	(151.3)	(0.00916)	(2700.3)
N	11151	11151	11151	11151		11151	11151	11151	11151	10912	11151	11082	10933
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. 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 (MATCH 3)

	Size and personnel				Investments, €1,000							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	size of firm	nr native workers	nr non-EU	nr EU	all	buildings	machines	IT	labor share	turnover, €1,000	profit ratio	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. 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: Alternative control group, earnings estimates by quartile and percentile

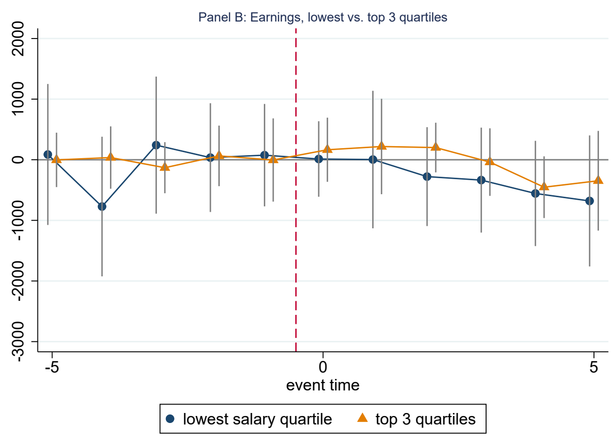

Figure K1: Earnings by quartile, changing also the control group

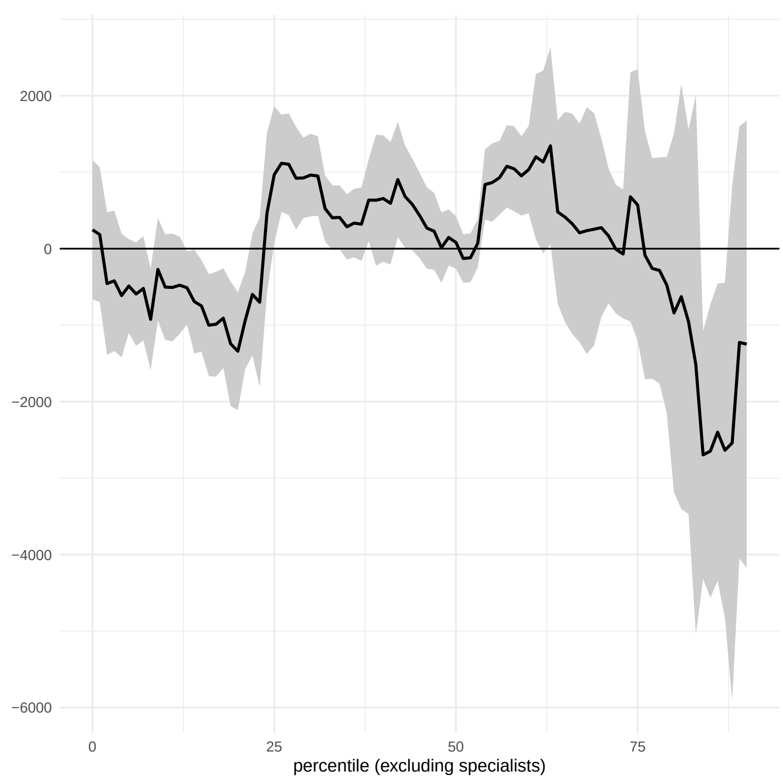

Figure K2: Percentile figure showing the pooled estimate (whole post period) for annual earnings, changing also the control group

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