Firms' labor cost savings and recruitment of non-western immigrants
- The unintended effect of a payroll tax reform

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Abstract

Immigrants have for a long time faced great challenges on European labor markets and policymakers in many countries are struggling to improve their labor market integration. This paper evaluates whether a Swedish youth payroll tax reform had the unintended effect of promoting employment of non-western immigrants. The reform generated firm-level labor cost savings, which were proportional to the number of young employees at the time of the reform implementation. Utilizing matched employer-employee data, I calculate firms' one-year labor cost savings and investigate the link between these savings and the recruitment of non-western immigrants. The findings suggest a strong and positive link between firms' labor cost savings and their subsequent hiring of first-generation non-western immigrants, which to a large extent is driven by an increased employment of older immigrants who were not targeted by the reform. In total, the findings suggest that 1,200 jobs were created for this group within the analyzed sample of firms, which corresponds to a net job creation that is more than proportionate to the group's population share. The youth payroll tax reform thus had employment-promoting effects outside its target group, illustrating that general labor cost reductions can lower barriers against immigrant employment and enhance the labor market opportunities for non-western immigrants.

1. INTRODUCTION

Immigration to Europe has increased dramatically during the past decade, reaching a peak in 2016 when almost 1.4 million individuals applied for asylum in Europe (EASO, 2016). This has put pressure on the receiving countries in their attempts to integrate these individuals on their labor markets, which OECD (2015) argues to be the most crucial part of immigrants' assimilation process. The labor market integration is complicated due to the fact that many refugees have little education or education that is not applicable on the receiving country's labor market (OECD, 2014; European Commission and OECD, 2016).

The integration of the recent wave of immigrants into the European Union is further obstructed by the difficulties that first-generation immigrants in general face in establishing themselves at the labor market. In an evaluation of refugees' labor market situation in Europe by the European Commission and OECD (2016), it is found that non-EU born immigrants and, especially, refugees have higher long-term unemployment rates that native-born within the European Union, and that it takes on average 20 years for a refugee cohort to achieve the same employment rate as the native-born population. Refugees are also shown to be overrepresented in part-time employment and tend to be overqualified for their work tasks, reflecting their particularly problematic labor market situation (OECD, 2014; European Commission and OECD, 2016). The employment rate for native-born and non-EU born amounted to 69.3 and 64.7 percent, respectively, in 2006. This implies an employment gap of 4.6 percentage points, which has widened to 10 percentage points in 2017 (Eurostat, 2019).

An active labor market policy that has been frequently implemented by European policymakers is to provide employment subsidies for immigrants and other groups that have difficulties to enter the labor market (Martin and Grubb, 2001). Such policies have often been shown to facilitate employment probabilities, but the majority of previous evaluations have only analyzed the short-term effects of this type of subsidies (Nekby, 2008). Several studies have also argued that such subsidies potentially can displace regular employment, cause deadweight losses and that the net gain in employment is small (Martin and Grubb, 2001; Kluve, 2006; Nekby, 2008).

Another way to facilitate the integration of foreign-born individuals is to implement policies that reduce the labor costs for employers, but that are not targeted directly towards

immigrants. Swedish policymakers implemented such a reform in 2007 when they lowered the payroll tax from 32.42 to 21.32 percentage points for all employees between 19-25 years of age. The magnitude of the payroll tax cut was directly related to the firms' number of young employees, meaning that youth-intensive firms received considerable labor cost savings in contrast to firms that had few young employees. These savings generated both a substitution effect and an income effect for the firms. As the reform made 19-25-year-olds relatively less costly to hire, the substitution effect should have promoted the employment of this group. However, the reduction in labor costs also gave rise to an income effect, suggesting that the firms could use the labor cost savings to recruit individuals who were not directly targeted by the reform, e.g. first-generation immigrants. In contrast to specific employment subsidies for immigrants, these savings were created due to the firms' initial workforce composition and, thus, not tied to a certain time period.

The purpose of this paper is to investigate if the payroll tax cut in Sweden facilitated the labor market integration of immigrants and thus had a consequence not intended by the policymakers. Previous literature has found that a limited, or not perfectly transferable, education level, little work experience and lacking skills in the native language could prevent immigrants from entering employment (Chiswick and Miller, 2009; Eriksson, 2011; OECD, 2014). This suggests that problems related to asymmetric information might be especially large between employers and foreign-born job candidates. Moreover, this does in turn imply that there is a larger risk involved in hiring immigrants. Such risks are likely to be particularly high in a country like Sweden with strict labor market legislations (Coad et al., 2014). Problems related to asymmetric information is partly reflected in the fact that immigrants who enter employment are generally overrepresented in part-time work and face a higher risk of being overqualified (OECD, 2014; European Commission and OECD, 2016). Some studies have also found that high minimum wage levels could prevent employment of low-productivity individuals, i.e. individuals near the minimum wage threshold, and that reduced labor costs improve their employment opportunities (Eriksson, 2011; Jardim et al., 2017). Due to the income effect, the labor cost savings generated by the payroll tax reform could have reduced the risk which firms face when hiring foreign-born individuals, thereby enhancing the labor market mobility of - and improving the job matching for - immigrants.

Using detailed employer-employee data from Statistics Sweden, firms' one-year labor cost savings, created by the reform, is calculated. All firms which received labor cost savings are considered to be treated. Coarsened Exact Matching (CEM) is then used to identify control

firms (Iacus et al., 2011; 2012) that lacked young employees when the payroll tax cut was implemented. This implies that the control firms did not experience an immediate reduction in their labor costs when the payroll tax reform was implemented. CEM is used to ensure that the treated and control firms are as similar job providers for immigrants as possible in absence of the reform, meaning that the control firms represent the counterfactual outcome. Each treated firm is considered to receive different doses, or treatment intensities, contingent on the size of their labor cost savings.

An empirical challenge is that firms with large savings generally hold a larger number of employees than firms with no (or small) cost savings, and that large firms typically experience a higher absolute employment growth than small firms (Henrekson and Johansson, 2010). Thus, firms with large cost savings might have grown more than firms receiving no initial cost savings for reasons independent of the reform. To account for this, I rely on a difference-in-difference-in-difference (DDD) model to estimate the employment effects over the 2006-2008 period (Chetty et al., 2009; Daunfeldt et al., 2019; Gruber, 1994). The DDD model accounts for differences in firm size by deducting underlying differences in employment growth. By first using statistical matching to reduce heterogeneity between treated and control firms and in addition relying on a DDD model, the immigrant employment effect is isolated. The empirical analysis is also carried out separately within the retail, hospitality, manufacturing and knowledge-intensive business services industries (henceforth, KIBS). These industries provide jobs of different qualifications and different skills. Consequently, this industry-level comparison makes it possible to analyze whether some industries were especially prone to hire non-western immigrants.

The results show that the youth payroll tax cut increased the employment of first-generation non-western immigrants not targeted by the reform, and that the employment effect increases with the size of firm-level labor cost savings. The average employment effect among firms with the largest labor cost savings is five times larger than the corresponding effect among firms with the smallest savings. The estimated effects are also of economic significance; within the sample of firms, 1,200 jobs were created for first-generation non-western immigrants. This net job creation is more than proportionate to the immigrant group's population share. Hence, the findings of this paper illustrate that a reform which caused a reduction in firms' total labor costs promoted the recruitment of first-generation non-western immigrants even though it was not the purpose of the reform. This could imply that labor cost

savings remove the barrier that prevents firms from hiring first-generation immigrants, whose skills and previous work experience might be difficult to assess. In addition, the findings highlight the importance of reduced labor costs for the improvement of immigrants' labor market opportunities.

The outline of this paper is as follows. The next section provides a theoretical background, a brief overview of immigrants' labor market situation in Sweden and a background on the payroll tax reform. Data and descriptive statistics are presented in Section 3, while the empirical methodology is described in Section 4. Section 5 includes the main findings and a robustness analysis. Lastly, Section 6 provides a discussion of the findings and concludes the paper.

2. EMPLOYMENT OPPORTUNITIES FOR NON-WESTERN IMMIGRANTS

2.1. THEORETICAL BACKGROUND

The insider-outsider theory by Lindbeck and Snower (1989; 2001) suggests that insiders on the labor market, i.e. incumbent workers, gain market power and push wages above the market-clearing level. The insiders' market power does in turn aggravate outsiders' labor market position by decreasing their chances of becoming employed. Insiders' market power arises from the labor turnover costs associated with hiring and firing personnel and which implies that insiders have an impact on a firm's employment decision. Increased labor costs, as well as high costs tied to job learning and firing workers, make firms less prone to hire new personnel (Lindbeck and Snower, 2001). The insider-outsider theory suggests an especially troublesome labor market situation for low-skilled outsiders, whose marginal productivity might not correspond to the wage level, driven up by the insiders. For instance, the limited, or not directly applicable, education, and lacking language skills of some immigrant groups indicate that their outsider status on the labor market could be prominent. In conclusion, the insider-outsider theory suggests that many immigrants might be outsiders and, thus, that firms might be reluctant to employ them.

In addition, there are several other theoretical factors that explain why firms in general are less prone to hire foreign-born individuals. Problems related to asymmetric information

between employers and employees are likely to be larger for foreign-born job seekers. This is for instance due to the fact that firms are struggling in validating immigrants' educational background and previous work experience (Chiswick and Miller, 2009; OECD, 2014). Asymmetric information entails the risk of immigrants being overqualified for their work tasks. OECD (2014) finds that highly educated immigrants face a 50 percent higher risk of being overqualified than corresponding native-born individuals. Asymmetric information does also introduce risks for firms that are considering hiring immigrants since they cannot do a complete skills assessment. This might cause firms to utilize informal rather than formal methods in the recruitment process, e.g. social networks (Montgomery, 1991; Beaman and Magruder, 2012). Recruitment based on social networks could further limit the employment of immigrants, who generally have small and limited social networks (Behtoui, 2008). It is moreover probable that problems related to asymmetric information are particularly prominent in countries with strict labor market legislations, since the cost of making the "wrong" recruitment is high (Coad et al., 2014). Two other factors that are likely to obstruct immigrant employment are high minimum wages (Jardim et al., 2017) and language barriers (OECD, 2014). Lastly, both statistical discrimination (Phelps, 1972) and preference-based discrimination (Arai and Thoursie, 2009) could influence the decision to hire foreign-born individuals.

Given these theoretical explanations of why firms are less prone to hire immigrants, there are several channels through which labor cost savings could promote firms' employment of foreign-born individuals. First, the generated labor cost savings are associated with an income effect and increase the resources that can be invested in further recruitment. Since the firms have larger resources, they could hire individuals who otherwise would have not become employed. This implies, for instance, that the savings could facilitate the employment opportunities for immigrants who are outsiders, or are struggling, on the labor market. The fact that asymmetric information introduces a particularly large risk for firms that are considering hiring immigrants suggests that there could be a barrier against immigrant employment. This employment barrier is reflected in the fact that many employed immigrants tend to be in part-time work and are more likely to be overqualified for their work tasks (OECD, 2014; European Commission and OECD, 2016). By lowering the risk for the employing firm, the labor cost savings could arguably enhance immigrants' labor market mobility and improve their job matching, incentivizing the employment of individuals who are struggling to compete on the labor market.

Fast-growing firms have been shown to be frequent employers of marginalized immigrants and other outsiders on the labor market (Coad et al., 2014). A plausible explanation is that such firms become less restrictive in their recruitment decisions during phases of rapid growth and prioritize to hire low-cost employees, whose skills are improved through on-the-job training. Since a reduction in firms' labor costs provide incentives for employment growth, this constitutes an additional channel through which labor cost savings could promote immigrant employment. The labor cost savings could also, at least partly, compensate firms for having to pay the above market-clearing wage (caused by insiders' market power) and thereby reduce the negative impact on outsider's employment status by inducing firms' employment. A reduction in firms' labor costs could also increase the employment of immigrants whose productivity levels do not correspond to the pre-reform hiring cost, caused by for instance high minimum wage levels.

2.2. IMMIGRANTS ON THE LABOR MARKET IN SWEDEN

The immigration to Sweden has gone through substantial changes during the recent decades. During the 1950's up until the mid-1970's, the foreign-born population had a higher employment rate than the native-born population, but since then the employment gap between foreign-born and native-born has gradually become negative and has continued to expand (Ekberg, 2009). In 2006 (the year before the reform implementation), foreign-born had an employment rate that was 20 percent lower than among the native-born population. During the recent decades, the immigrant composition has shifted from mainly labor force immigration to refugee immigration. This compositional change is also reflected in immigrants' region of birth. In 2000, 27.9 percent of the foreign-born population were born in another Nordic country whereas 36 percent were born in a non-European country. The corresponding shares in 2018 were 12 and 55.8 percent, respectively (Statistics Sweden, 2019:1). The recent refugee immigration wave reached its peak in 2015, when almost 163,000 individuals applied for asylum (Swedish Migration Agency, 2015).

A large number of studies has evaluated immigrants' labor market situation in Sweden. In a research survey, Eriksson (2011) finds the key determinants of the low employment rate among immigrants in Sweden to be: (i) lack of language skills, (ii) limited access to informal networks, (iii) high employability requirements from employers, and (iv) ethnic

discrimination. The research survey implies that a major share of all Swedish job vacancies is filled by individuals identified through informal networks, which is problematic for immigrants due to their limited access to such networks. Eriksson (2011) does also argue that immigrants' employment opportunities could be disadvantaged by the ongoing structural change in Sweden, which is characterized by a shrinking industry sector and a growing service sector, since it has resulted in higher skills requirements. Åslund and Rooth (2007) find that immigrants' integration success crucially depends on the labor market situation upon their time of arrival, where those arriving during favorable labor market conditions fare better, with higher employment and earnings subsequently.

The importance of language skills is emphasized by Rooth and Åslund (2007). They find poor language acquisition to make foreign-born individuals less employable on the Swedish labor market and that improved language skills significantly increases the likelihood of employment and earnings. Previous research has also found marginalization and socioeconomic status among immigrants in Sweden to be inter-generational (Ekberg and Hammarstedt, 2002; Rooth and Ekberg, 2003). Rooth and Ekberg (2003) evaluate the employment outcomes and earnings of second-generation immigrants in Sweden. They find that second-generation immigrants having a Southern or non-European background face a higher risk of unemployment and have lower earnings than comparable native-born. Having however only one parent born abroad leads to significant reduction in the risk of entering unemployment. The findings imply that ethnicities which are generally poorly integrated in the first generation will also typically be poorly integrated in the second generation.

Several active labor market policies have been targeted specifically at, or have had specific rules for, immigrants (Eriksson, 2011). Among these, two of the most extensive policy programs are New Start Jobs (*Nystartsjobb*) and Entry Jobs (*Instegsjobb*).[1] New Start Jobs offers subsidized employment of both newly arrived foreign-born and long-term unemployed individuals. The subsidy is given for a maximum of two years and covers approximately 50 percent of the wage cost (Joyce, 2017). Entry Jobs is targeted specifically at newly arrived foreign-born and provides a subsidy of up to 80 percent of the wage cost. This subsidy is also restricted to a maximum of two years. Of these two policy programs, evaluations have found

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[1] There is no reason to expect that these policies would have had different impacts within the treatment and control groups of this paper. Therefore, there is no reason to suspect that they would lead to biased estimates.

that only New Start Jobs is associated with improved chances of regular employment (Joyce, 2017). The program has however also been shown to have crowding-out effects on ordinary employment. It is moreover possible that the fact these programs are time limited make them less efficient. If a subsidy is perceived as temporary and an immigrant's productivity does not correspond to the non-subsidized wage, employers might remain reluctant to offer employment.

2.3. THE SWEDISH PAYROLL TAX REFORM

The Swedish payroll tax is in its entirety levied on the employers. It is proportional to the employees' gross wages, and is thus part of their total labor costs. During the past 50 years, the payroll tax level has increased significantly; from 11.65 percent in 1970 to approximately 30 percent from 1994 and onwards (Ekonomifakta, 2019). The current standard payroll tax rate is 31.42 percent and consists of seven different fees, financing social benefits such as pensions, parental leave and sick leave. From a historical perspective, the payroll tax has typically been identical regardless of an individual's age or geographic location.

The previous center-right government in Sweden reduced the payroll tax level from 32.42 to 21.32 percent for young individuals on July 1, 2007. (Swedish Government, 2006).[2] The targeted age group included all individuals who at the start of the year had turned 18 years of age, but not yet 25. Thus, all individuals born in 1982-1988 were targeted in 2007. The aim of the reform was to decrease the high and growing youth unemployment rate in Sweden at that time. On January 1, 2009, the reform was extended by an additional reduction of the payroll tax level to 15.49 percent and by encompassing all individuals who had not turned 26 by the start of 2009 (Swedish Government, 2008). The political left-wing parties, which were in opposition at the time, criticized the reform for being inefficient and costly in terms of foregone tax revenues. Consequently, the left-wing parties decided to implement a stepwise abolishment of the payroll tax reduction once they were elected into office in 2014. On June 1, 2016, the payroll tax cut for young individuals was completely abolished.

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[2] To be precise, the payroll tax was reduced from 32.420 and 21.315 percent. Using two decimals, the previous government stated the new payroll tax level to be 21.31, corresponding to a reduction of 11.11 percentage points (Swedish Government, 2006). However, the reduction was limited to 9.71 percentage points during the second half of 2007 and, therefore, the reform was not fully implemented until the start of year 2008.

Several studies have evaluated the Swedish payroll tax reform. Egebark and Kaunitz (2013; 2014) and Skedinger (2014) have focused on how the reform affected individuals close to the reforms' age threshold, finding relatively small employment effects of the youth payroll tax cut. Egebark and Kaunitz (2013) estimated the total net job creation to amount to 6,000-10,000 jobs per year. Furthermore, they concluded that the intensive-margin employment effect (number of work hours) of the reform to be limited. Skedinger (2014) focused specifically on the retail industry and also found small employment effects, although it appeared stronger for individuals close to the minimum wage threshold.

Three studies have acknowledged that the number of young employees hired at the time of the reform is linked to firm-level labor cost savings (Egebark and Kaunitz, 2017; Saez et al. 2019; Daunfeldt et al., 2019). More specifically, they do all consider firms to have received different doses – or treatment intensities – of the reform, varying with the size of the savings. Both Egebark and Kaunitz (2017) and Saez et al. (2019) utilized treatment intensities measured in relative rather than absolute terms. Egebark & Kaunitz (2017) defined the wages paid to young employees in 2006 as a share of total turnover as a treatment intensity measure and analyzed how firm performance varied with this measure. They found no evidence that firms with a high treatment intensity experienced a relative improvement in performance. Similarly, Saez et al. (2019) exploited the share of firms' 2006 total wages that was devoted to young employees, finding that the firm-level employment increased more among firms that had an initially high treatment intensity.

Daunfeldt et al. (2019) emphasized that it is the savings in absolute terms that determine if firms decide to hire additional employees, and they therefore used the absolute size of firm-level labor cost savings as a measure of treatment intensity. They argued that using a relative measure, a small firm having a high share of young employees could be defined as receiving a higher treatment intensity than a large firm, although the latter firm received a considerably larger labor cost saving in monetary terms. Their findings suggested a positive relationship between the firm-level savings and firm-level employment. In total, they concluded that the 2007 payroll tax cut generated 16,400 new jobs over the years 2006-2008, which implies that the net job creation was larger than what had been found earlier by Egebark and Kaunitz (2013). In this study, I follow the work of Daunfeldt et al. (2019) and define a treatment intensity measure in absolute terms, which is further described in section 4.2.

In comparison to the labor market policies which were discussed in the previous section, the 2007 payroll tax reform differs in many regards. The most obvious difference is that it targeted young individuals rather than immigrants. But the labor cost savings that were created by the reform gave rise to both a substitution effect and an income effect. Theoretically, the substitution effect should have incentivized the employment of young individuals since they became relatively less costly to hire. The income effect however could have increased the employment of individuals who did not belong to the reform's target group. For instance, immigrants with limited productivity levels and skills that are difficult to evaluate. This paper does not focus on the payroll tax reform itself but rather on the labor cost savings it created and which – due to the income effect - could have helped firms to overcome barriers against the employment of immigrants. As discussed, unlike the time-limited subsidies for immigrant employment which have been implemented earlier, the savings were a direct consequence of the firms' initial workforce composition and therefore not limited to a specific time period. Moreover, the abolishment of the payroll tax reform was not initiated until 2015, implying that youth-intensive firms continuously obtained savings over a longer time period.

3. DATA AND DESCRIPTIVE STATISTICS

3.1. DATA

All data is obtained from the individual-level database LISA (Longitudinal Integration Database for Health Insurance and Labour Market Studies), provided by Statistics Sweden. LISA is entirely built on register data and includes information on all Swedish residents that are 16 years or older, e.g., individuals' employment status, incomes and educational backgrounds. LISA also includes an identification number for each employer, making it possible to match all employees with their employer (if any) during the month of November. I exploit this information to create a panel of all Swedish firms and their employees from 2003 to 2008. In total, the dataset includes 8,181,219 individuals and 744,032 firms.

By aggregating individual-level variables, firm-level measures of, for example, total gross wages, the share of employees with post-secondary education, and the average age of the employees are constructed. Each employer, and thus employee, is also assigned an industry code from the SNI2002 (Swedish Standard Industrial Classification) system, which consists

of 776 industries at the most detailed (five-digit) level, and 60 industries at the most aggregated (two-digit) level. By exploiting the industry codes, it is possible to investigate industry-level differences regarding the effects of the payroll tax reduction on the hiring of non-western immigrants. I use this information to do separate estimations for firms active in the retail, hospitality, manufacturing and KIBS sectors.[3]

I impose some conditions which need to be fulfilled for a firm to be included in the sample. More specifically, the firm needs to exist and have at least one employee in each year during the time period 2003-2008. I do moreover exclude firms that have a two-digit industry code equal to 0 (zero).[4] These conditions are necessary because in addition to the analyzed reform period of 2006-2008, the years 2003-2005 are included in the empirical analysis. These restrictions also imply that those firms which entered or exited the dataset during the study period are excluded from the empirical analysis. Out of 744,032 firms that existed during the 2003-2008 period, 202,638 firms satisfy the conditions described above. To prevent firm outliers from affecting the empirical analysis, I exclude firms that have had an extreme annual employment growth. This excludes additionally 1,137 firms, resulting in 201,501 firms in total.[5] Although it is necessary to restrict the sample, it also implies that inference can be made only for surviving firms with at least one employee and not for the overall firm population.

LISA provides information on the region of birth for each individual and their parents. Consequently, it is possible to identify both first- and second-generation immigrants and their regional area of migration.[6] I distinguish between western and non-western immigrants. All residents in Sweden that were originally born in another European country (including all EU and non-EU countries), North America or Oceania are defined as western immigrants, while,

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[3] For definitions and further information, see Table A5 (appendix). KIBS firms are defined in accordance with Eurostat (2012). SNI2002 builds on the European industry classification NACE (rev 1.1) and they are identical up to the four-digit level.

[4] Individuals who are either hired by a municipality but whose work status is unclear (e.g. participation in labor market policies) or receive labor income from a firm not registered in the Statistical Business Register (e.g., a firm operating abroad) are assigned the two-digit industry code 0.

[5] Outliers are defined as firms which have had an annual employment change of more than three standard deviations from the average growth. This implies that all firms which have changed their size by more than 88 employees over one year, are excluded.

[6] Specifically, the LISA variables FodGrEg3, FodGrFar3 and FodGrMor3 are used, which all contain the following regional birth categories: 00=Sweden, 01=Nordic countries (excl. Sweden), 02=EU27 (excl. Denmark, Finland and Sweden), 03=Europe (excl EU27 and Nordic countries), 04=Africa, 05=North America, 06=South America, 07=Asia, 08=Oceania, 09=Soviet, 11=unknown.

individuals originally born in Asia, Soviet, Africa or South America are considered non-western immigrants. Similarly, a Swedish-born individual is considered to be a western or non-western second-generation immigrant if both parents were born within the same regional group in accordance with the definitions above. Since previous studies have shown that individuals with one native-born parent generally do not face the same integration difficulties as those with both parents born abroad, they are not defined as second-generation immigrants (Rooth and Ekberg, 2003). Immigrants are a heterogeneous group, consisting of both refugees and labor immigrants. However, the vast majority of all immigrants in Sweden that are born in non-western countries are refugees or relatives to refugees, and they generally face greater difficulties in establishing themselves on the labor market than western immigrants (Eriksson, 2011; Lundborg, 2013). Therefore, the analysis of this study focuses on the hiring of non-western immigrants.

Each individual's employment status in LISA is measured during the month of November and is derived from the RAMS (Labour Statistics based on Administrative Sources) register. To be registered as employed in RAMS, an individual's income needs to correspond to at least one hour of work during a measurement week in November. This employment variable captures a very heterogeneous sample of individuals, including both part-time and full-time employees. Following Mörk et al. (2014), I also construct two employment definitions based on annual labor earnings to evaluate whether the findings are sensitive to who is defined as employed in the dataset. The first definition requires an individual (in addition to being registered as employed in the RAMS register) to have had annual labor earnings of more than one income base amount[7], whereas the second definition uses two income base amounts as a threshold for being classified as employed. The main findings are not sensitive to the choice of employment definition.[8] Since I am primarily interested in the total immigrant employment effect, the results in this paper rely on the RAMS employment definition. Estimations based on the income-based employment definitions are however available upon request.

In contrast to the employment status, which is measured during a measurement week in November, each individual's unemployment status is measured specifically during the last week of November each year. This could result in some individuals being registered as both

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[7] An income base amount ranges from 40,900 SEK (4,499 USD) to 51,100 SEK (5,621 USD) during the period of study.
[8] See footnote 16.

employed and unemployed in the dataset. Most of these individuals are likely to have a weak labor market position and it is moreover not possible to conclude that they are regularly employed with certainty. I therefore choose to exclude all individuals who are simultaneously registered as employed and unemployed. This excludes 570,386 out of 8,181,219 individuals in total (or 864,630 out of 44,448,752 observations) over the 2003-2008 period.

3.2. DESCRIPTIVE STATISTICS

Table 1 includes descriptive statistics for all treated firms, i.e. those firms which received initial labor cost savings, and the matched control firms which did not receive any immediate labor cost savings when the payroll tax reform was implemented.[9] Among the 201,501 firms included in the empirical analysis, 43,073 firms had young employees in 2006 and were, thus, about to be treated. The statistical matching method, which is further explained in section 4.1, requires the matched treated and control firms to have similar characteristics on a set on matching variables. In total, this leaves us with 26,650 treated firms matched to the same number of control firms. The treated firms are split into five treatment intensity groups. The lowest >0-20 group includes firms having savings within the lower 20 percent of the savings distribution, whereas the >80-100 group includes firms with the top-20 percent savings. Both the average and median firm size is larger among the treated firms compared to the control firms. The average number of employees is also increasing with treatment intensity, implying that the size of labor cost savings and average firm size are positively correlated. For instance, each firm within the upper >80-100 treatment intensity has on average approximately 17 employees whereas the average firm within the >0-20 range has approximately 6 employees.

Note that the average share of non-western employees among treated firms ranges from 1 to 2 percent, indicating that only a minor share of all employees is born in a non-western country. The average share of second-generation non-western immigrants is even smaller. Interestingly, although average firm size is increasing with treatment intensity, the average

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[9] The corresponding descriptive statistics for the different industries are available upon request. Compared to the findings of Table 1, hospitality firms have larger shares of 18-24-year-olds and non-western immigrants. For instance, the average share of first-generation non-western immigrants ranges between 2 and 10 percent, whereas the average share of 18-24-year-olds are in-between 33.4 and 50.4 percent. Retail firms within the higher treatment intensity ranges are also more youth-intensive than firms in general. On the contrary, manufacturing and KIBS firms are characterized by typically having lower shares of both first-generation non-western immigrants and 18-24-year-olds. They do also, on average, have a larger number of employees.

number of non-western immigrants is more or less unrelated to firm's labor cost savings. Finally, the average share of 18-24-year-olds in 2006 - who are about to be targeted by the payroll tax cut - is increasing with treatment intensity; from 28.8 percent for the >0-20 group to 34.8 percent for the >80-100 group. Thus, approximately in-between one-fourth and one-third of the employees in the treated firms belong to targeted age group of the reform.

Table 1. Descriptive statistics. All firms with cost savings vs. control firms with no labor cost savings. Year 2006.

ALL FIRMS	Mean	Median	Std.dev.	Min	Max	# Firms
Firm size (# employees)						
Control	5.474	3	8.088	1	286	26,650
Dose >0-20 %	6.269	4	7.834	1	241	5,337
Dose >20-40 %	6.384	5	9.741	1	320	5,327
Dose >40-60 %	6.562	5	7.138	1	206	5,325
Dose >60-80 %	8.050	6	9.209	1	232	5,331
Dose >80-100 %	16.564	10	35.716	1	1,442	5,330
# Non-western imm. (1st gen)						
Control	0.088	0	0.374	0	7	26,650
Dose >0-20 %	0.127	0	0.545	0	15	5,337
Dose >20-40 %	0.105	0	0.444	0	6	5,327
Dose >40-60 %	0.091	0	0.417	0	9	5,325
Dose >60-80 %	0.074	0	0.365	0	10	5,331
Dose >80-100 %	0.103	0	0.463	0	11	5,330
# Non-western imm. (2nd gen)						
Control	0.003	0	0.056	0	3	26,650
Dose >0-20 %	0.013	0	0.126	0	3	5,337
Dose >20-40 %	0.011	0	0.118	0	3	5,327
Dose >40-60 %	0.013	0	0.127	0	3	5,325
Dose >60-80 %	0.009	0	0.102	0	2	5,331
Dose >80-100 %	0.020	0	0.166	0	3	5,330
Share of young						
Control	0	0	0	0	0	26,650
Dose >0-20 %	0.288	0.25	0.182	0.007	1	5,337
Dose >20-40 %	0.310	0.25	0.194	0.006	1	5,327
Dose >40-60 %	0.306	0.25	0.194	0.010	1	5,325
Dose >60-80 %	0.297	0.25	0.190	0.013	1	5,331
Dose >80-100 %	0.348	0.333	0.190	0.019	1	5,330

Notes: All firms with immediate labor cost savings (split into treatment intensity groups) vs. control firms with no savings. Including firms surviving and having at least one employee per year across the time period 2003-2008. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/-88 employees) from the average growth - are excluded. Variables are measured in year 2006.

4. METHOD

4.1. COARSENED EXACT MATCHING

To accurately estimate the effect of the payroll tax cut on the hiring of non-western immigrants, a set of control firms lacking initial labor cost savings needs to be identified. Since a firm's labor cost savings are directly related to its number of young employees, i.e. 19-25-year-olds, firms which had young individuals hired at the time of the reform introduction are defined as treated. All firms which did not have young employees are consequently defined as non-treated and are, thus, included in the set of potential control firms.

I rely on the statistical matching method Coarsened Exact Matching (CEM) (Blackwell et al., 2009, Iacus et al., 2011; 2012) to identify representative control firms. My aim is to find a set of control firms which resembles the counterfactual outcome for the treated firms, indicating that the only important difference between the treated and control firms is the treatment assignment. This implies that the treated and control firms would have been identical job providers for immigrants in absence of treatment, i.e., if the treated firms never had received any labor cost savings due to the payroll tax reform.

First, a k-dimensional vector of covariates $X = (X_1, \ldots, X_k)$ that affect both the treatment assignment and the outcome of interest (immigrant employment) is defined. Next, matching is used to locate treated and control firms which have similar values on X. In CEM, each matching variable in X is treated separately. First, each variable is coarsened, meaning that similar values on the variable are treated as equal. The degree of coarsening is set by the researcher by splitting each covariate into intervals, or strata. For instance, a continuous variable could be coarsened into four equally sized quartiles, whereas a discrete variable ranging from 1 to 4 could be coarsened into two strata (1-2 and 3-4). Next, treated and control units that for each covariate are within the same strata across the full set of matching covariates, are matched. Since matching occurs within intervals across the distribution of each covariate, the matched treated and control units are likely to be similar not just in terms of means but also in higher-order moments, e.g. the variance.

CEM belongs to a class of matching methods called Monotonic Imbalance Bounding (MIB), introduced by Iacus et al. (2011). A common characteristic of MIB methods is that an improved balance, i.e. similarity, in one covariate, does not affect the maximum imbalance, i.e. dissimilarity, of other covariates since the maximum imbalance between treated and control units is pre-determined (Iacus et. al, 2011; 2012). This is in contrast to other matching methods, e.g. Propensity Score Matching, in which an improved balance in one covariate might lead to substantial increases of imbalance in other covariates. Furthermore, MIB matching methods have been shown to reduce model dependence, implying that empirical findings will be more robust to the choice of estimation model and model specification (Ho et al., 2007, Iacus et al., 2011). The main advantage of CEM is the possibility to exactly set the maximum imbalance between treatment and control units that is to be allowed and, furthermore, that this is done separately for each covariate. Setting these limits does, however, lead to a tradeoff because more coarsening (fewer strata) will increase imbalance but generate a larger number of matched units.

The matching procedure is first carried out for all firms jointly and then separately for the different industry categories. First, a treatment indicator being equal to one for treated firms, i.e. with positive savings, and equal to zero for firms without immediate labor cost savings at the time of the reform, is created.[10] The following firm-level variables are then coarsened and matched upon using CEM: (i) the trends in each firms' number of western and non-western immigrants during 2003-2006; (ii) the share of employees having no postsecondary education in 2006; and, lastly, (iii) the shares of western and non-western immigrants in 2006. The matching variables measuring the pre-reform composition of western and non-western immigrants are important since the control firms should be equally frequent employers of immigrants as the treated firms prior to the reform introduction. Moreover, since a large number of immigrants in Sweden lack higher education (Eriksson, 2011), a variable for the share of employees without postsecondary education is included to ensure that treated and control firms provide job opportunities with similar skill requirements. The CEM is set so that it generates 1:1 matching, meaning that the number of matched treated and control firms are equal. This ensures that differences in matching variables between the matched treatment and control groups are not due to the groups containing a different number of firms.

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[10] For the industry-specific matching, an additional requirement is that the control firms do not belong to the same industry. The reason is that I am interested in industry-level differences, i.e. how immigrant employment varies across industries.

Table 2 presents statistics for the distribution of the matching variables when the matching includes all industries. In total, 26,650 treated firms are matched to the same number of control firms. Considering the three-year trends in employment of western and non-western immigrants, one can notice that the median treated and control firms have not changed their employment at all, whereas the average firms have had only very moderate changes. On average, the treated and control firms have relatively low educational requirements; approximately 84 percent of the employees lacked postsecondary education in 2006. Turning to the share of immigrant employees born in a western or non-western country, one can notice that the average shares among treated and control firms are identical and, moreover, that approximately 7 percent of the workforce is constituted by individuals born in another country. Importantly, the treated and control firms are not only similar in terms of means but across the entire distribution of each matching variable. Consequently, the CEM procedure identifies representative control firms, which is implied by the treated and control firms having similar pre-treatment distributions of each matching variable.

Table 2. All firms with labor cost savings vs. control firms without labor cost savings. Balance on matching variables. Year 2006.

	Min	5th	25th	50th	75th	95th	Max	Mean	# Firms
Change in # West. imm, 2003-2006									
Treated	-9	-1	0	0	0	1	6	0.009	26,650
Control	-9	-1	0	0	0	1	6	0.029	26,650
Change in # Non-west. imm, 2003-2006									
Treated	-4	0	0	0	0	0	6	-0.003	26,650
Control	-4	0	0	0	0	0	5	0.017	26,650
Share without postsecondary education									
Treated	0	0.333	0.737	1	1	1	1	0.835	26,650
Control	0	0.333	0.737	1	1	1	1	0.835	26,650
Share western imm.									
Treated	0	0	0	0	0	0.308	1	0.048	26,650
Control	0	0	0	0	0	0.308	1	0.048	26,650
Share non-western imm.									
Treated	0	0	0	0	0	0.111	1	0.019	26,650
Control	0	0	0	0	0	0.111	1	0.019	26,650

Notes: Similarity of the distributions of each matching variable for treated and control firms. CEM matching. Including firms surviving, having at least one employee per year across the time period 2003-2008. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Variables measured in year 2006.

Next, I turn to the industry-specific matching. For brevity, the matching results are presented in Appendix (Table A1-A4). The industry-specific matching procedures do, importantly, generate control firms which have similar characteristics on all of the underlying matching variables. In total, CEM finds matches for 4,297 retail firms, 1,955 hospitality firms, 3,346 manufacturing firms and 1,502 KIBS firms.

4.2. MEASURING TREATMENT INTENSITY

The 2007 payroll tax reform was not randomized as it encompassed all firms which had employees within the targeted age group. Consequently, all firms could choose to take part of the reform by hiring young individuals and thereby assign themselves into the treatment group. This might give rise to a selection bias because it is likely that firms that assigned themselves into treatment differ from firms that did not. I define treated and control firms in the pre-treatment period to avoid any problems associated with self-selection and, therefore, the treatment assignment should be pre-determined. More specifically, I use each firm's total gross wages for young workers in 2006 to construct a proxy for the labor cost savings that each firm received during the first year after the payroll tax cut. By defining the labor cost savings in the pre-treatment year of 2006, the results should be unaffected by any self-selection mechanisms. The 2006 labor cost savings should also be strongly correlated with the actual 2007 cost savings, i.e. the 2006 measure should be a relevant proxy. The relevance of the 2006 proxy is further facilitated by the fact that it is defined in the month of November 2006, and the reform was implemented shortly thereafter in mid-2007.

Each firm's treatment intensity is positively related to, and strictly increasing with, the amount of labor cost savings. The 2006 $Treatment\ intensity_{i,t}$ of firm i at year t can be expressed as

$$Treatment\ intensity_{i,t=2006} = (0.3242 - 0.2132) \times Gross\ wages, young_{it=2006}$$

The figures 0.3242 and 0.2132 represent the pre- and post-reform payroll tax levels, respectively. $Gross\ wages, young_{it=2006}$ represent the firm-level gross wages paid to 18-24-year-olds in 2006, i.e. for the age group about to be covered by payroll tax cut.[11] Hence, the 2006 treatment intensity measure captures the size of the firm-level labor cost savings that the young employees will generate during the first post-reform year, provided that they remain employed at the same wage levels.

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[11] Specifically, information on firms (e.g., number of employees) are measured in the month of November, whereas gross wages are collected from each individual's primary income source during a given year. Hence, the gross wage sum for each firm, observed in November, is only built upon individuals who did have that firm as their primary employer. This implies that the actual firm-level gross wage sum will be underestimated if there are individuals who have had another primary employer during the same year.

Next, I split all firms receiving initial cost reductions ($Treatment\ intensity_{i,t=2006} > 0$) into five equally sized quantiles across the cost savings distribution. These groups constitute the treated firms which are analyzed in the regression analyses. The lowest quantile contains firms with labor cost savings within the >0-20 percent range, while the highest quantile captures firms within the >80-100 range. Recall that the control firms did not receive an initial labor cost reduction, meaning that $Treatment\ intensity_{i,t=2006} = 0$.

Table 3 shows the expected labor cost savings for all treated firms which are matched to control firms without initial cost savings. From the statistics, it is apparent that the initial one-year labor cost savings were small for most treated firms; 80 percent saved approximately 39,000 SEK (4,290 USD) or less. However, there is a large variation within the upper >80-100 range, in which one firm obtained a cost reduction of 1,992,242 SEK (219,150 USD).[12]

Table 3. Expected one-year labor cost savings by treatment intensity. All treated firms.

Treatment intensity	Min	Max	Median
>0-20 %	124	9,266	5,782
>20-40 %	9,278	17,790	13,269
>40-60 %	17,803	26,129	22,238
>60-80 %	26,142	38,973	30,700
>80-100 %	39,003	1,992,242	58,053

Notes: Inflation adjusted with base year 2016. Measured in SEK.

Table 4 includes the savings intervals for the different industry categories. To evaluate whether some industries were particularly prone to use the savings in hiring non-western immigrants, the industries follow the general savings distribution of Table 3. There are however no upper boundaries on the labor cost savings within the 80-100 treatment intensity range, which is implied by differences in the maximum savings.[13] From Table 4, it is also evident that the largest saving was received for a firm within the retail industry.

[12] As of September 2018, 1 SEK is approximately equal to 0.11 USD.
[13] Although there are differences in maximum savings, the median savings within the different industries remain similar. This allows for an analysis of whether the link between labor cost savings and immigrant employment was especially important in some industries.

Table 4. Expected one-year labor cost savings by treatment intensity. Treated retail, hospitality, manufacturing and KIBS firms following the same savings distribution.

Treatment intensity	Min	Max	Median	# Firms
>0-20				
Retail	161	9,241	5,844	822
Hospitality	136	9,266	5,473	484
Manufacturing	284	9,266	5,881	515
KIBS	309	9,266	5,874	392
>20-40				
Retail	9,278	17,790	13,343	882
Hospitality	9,278	17,790	13,201	464
Manufacturing	9,315	17,790	13,256	553
KIBS	9,278	17,778	12,978	350
>40-60				
Retail	17,815	26,117	22,065	842
Hospitality	17,815	26,031	21,583	308
Manufacturing	17,803	26,129	22,744	621
KIBS	17,803	26,129	21,842	267
>60-80				
Retail	26,142	38,990	30,379	691
Hospitality	26,190	38,978	31,386	276
Manufacturing	26,154	38,990	30,144	743
KIBS	26,203	38,447	30,453	269
>80-100				
Retail	39,003	1,212,303	62,500	1,060
Hospitality	39,003	432,944	61,574	423
Manufacturing	39,003	875,166	62,735	914
KIBS	39,064	472,836	58,356	224

Notes: Inflation adjusted with base year 2016. Measured in SEK. Treated retail, hospitality, manufacturing and KIBS firms.

4.3. EMPIRICAL MODEL

The empirical analysis relies upon an absolute treatment intensity measure, implying that the size of labor cost savings is strictly increasing with treatment intensity. This choice is motivated by the fact that the amount of cost savings is likely to determine a firm's employment decision. However, average firm size and treatment intensity are positively correlated. This could constitute a problem since previous studies have shown that large firms in general have a larger absolute employment growth than small firms (Henrekson and Johansson, 2010). Treated firms might thus grow more than the control firms because of their initially larger size, rather than solely because of the payroll tax cut.

To handle this empirical challenge, it is important that the empirical model accounts for the possibility of non-parallel employment trends among treated and control firms in the pre-treatment stage. The identifying assumption of the empirical analysis is that the treated and control firms would have had identical employment trends in the counterfactual state of the reform not being implemented. A difference-in-difference-in-difference (DDD) model (Chetty et al., 2009; Daunfeldt et al., 2019; Gruber, 1994) is therefore used. Unlike an ordinary difference-in-difference (DiD) model, which builds on the assumption of parallel pre-treatment trends, the DDD model instead accounts for any (potential) non-parallel trends in the pre-treatment years. This is done by deducting differences in employment growth between treated and control firms in the pre-treatment period of 2003-2005 from employment growth differences in the reform period 2006-2008.

The implemented DDD model estimates the difference between two DiD models estimated over the periods 2003-2005 and 2006-2008, respectively. By deducting the 2003-2005 estimate, it is assumed that any non-parallel trends in the pre-treatment years are accounted for. If the 2003-2005 trends for the treated and control firms would be identical, the DDD and DiD models generate identical point estimates.[14] Hence, under the assumption that employment growth differences between treated and control firms during 2003-2005 correspond to the differences in employment growth that would have been if the reform had not been implemented, the DDD model isolates the treatment effect from other factors that might have had affected the employment growth in treated and control firms differently.

The DDD model can be expressed as

$$
\begin{aligned}
Size_{ijt} = {} & \alpha + \beta_1 Time_t + \beta_2 Group_j + \beta_3 Treat_i + \beta_4 \big(Group_j * Time_t\big) \\
& + \beta_5 (Treat_i * Time_t) + \beta_6 \big(Group_j * Treat_i\big) \\
& + \delta_{DDD} \big(Group_j * Treat_i * Time_t\big) + \varepsilon_{ijt}
\end{aligned}
$$

Where indices i, j and t denote firm, group (treated or control) and year, respectively. The dependent variable $Size_{ijt}$ is defined as the number of employees in firm i, belonging to group j, at time t. $Time_t$ is a dummy variable that takes on the value zero for pre-treatment

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[14] For clarification, the DDD estimates of Figure 2 (below) are split into separate DiD estimates in Figure A1 in Appendix. It is apparent that the differences between the DiD estimates are identical to the corresponding DDD estimates of Figure 2. For instance, the DiD estimates for >0-20 group over 2006-2008 and 2003-2005 are 0.0121 and -0.00701, respectively. Together, they generate the DDD estimate 0.01911=0.0121-(-0.00701).

years of both the reform period 2006-2008 and the underlying time period 2003-2005, and the value one for the corresponding post-treatment years. It is thus equal to zero for the years 2003 and 2006 and equal to one for the years 2004, 2005, 2007 and 2008. $Group_j$ is an indicator for group belonging and is equal to zero for the control groups of both time periods and equal to one for the corresponding treatment groups. The variable $Treat_i$ distinguishes between all firms included in the time periods 2003-2005 and 2006-2008, by being equal to zero for firms included in the former group and equal to one for firms included in the latter group. The variable of primary interest is $Group_j * Treat_i * Time_t$, which is equal to one for the treated firms in the post-treatment years of 2007-2008. By deducting employment growth differences between treated and control in the underlying period 2003-2005, its parameter δ_{DDD} separates the effect of the payroll tax cut on employment from other factors that could explain differences in employment growth. Consequently, the otherwise potential bias caused by the positive correlation between treatment intensity and average firm size is removed. [15] The DDD model is estimated separately for each treatment intensity group and it is therefore possible to analyze how the employment of non-western immigrants varied with the amount of labor cost savings. Recall that the control group consists of matched firms that obtained no initial cost savings.

This paper focuses solely on short-run employment effects of the reform and thus disregards the 2009 reform extension. There are three main reasons motivating this choice. First, once the reform had been implemented all firms had the opportunity to take part of the tax cut by employing young individuals and, thereby, self-select into treatment. Such self-selection is likely to become more prominent with time. The object of interest is not the reduced payroll tax cut itself but rather the immediate cost savings that were created for firms that had young employees at the time of the reform introduction. These savings can be considered to be exogenous. Furthermore, it has been shown that estimates become noisier the longer the period of study, which makes it harder to separate the true treatment effect (Mian and Sufi, 2012). In this context, it is also likely that the financial crisis which reached Sweden in 2009

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[15] Analyzing the average firm size in the underlying pre-treatment year 2003, it is apparent that the average firm size of the treatment and control groups is similar in the pre-treatment years of the underlying and reform periods of 2003-2005 and 2006-2008, respectively. Importantly, the differences in mean size are similar in both years meaning that the 2003-2005 differences between treated and control firms are representative of the counterfactual differences, i.e. if the payroll tax reform had not been implemented. Descriptive statistics for the year 2003 are available upon request.

affected the absolute employment growth of treated and control firms differently due to for instance their initial differences in size and different industry belongings.

5. FINDINGS

The findings presented in this section indicate how the payroll tax cut affected the hiring of both first-generation and second-generation non-western immigrants. The estimations rely on the RAMS employment definition, meaning that all individuals who, during a measurement week in November, have received a labor income corresponding to at least one work hour, are defined as employed. However, the more restrictive income-based employment definitions yield very similar findings, implying that the results are robust to the choice of employment definition.[16]

The results presented below are based on within-firm estimation, which means that I account for any firm-specific heterogeneity that is time-invariant.[17] The appendix includes alternative specifications which additionally account for fixed effects at both the industry and municipal level (see table A6-A8). Accounting for such heterogeneity does not alter the findings considerably. Note that the firm-level fixed effects absorb all heterogeneity across industries and locations as long as the firms operate within the same industry and regional area during the entire period of study.

5.1. IMMIGRANT EMPLOYMENT EFFECT OF REDUCED LABOR COSTS AMONG ALL FIRMS

The estimated effects of the generated firm-level labor cost reductions on the hiring of first-generation non-western immigrants are presented in Figure 2. Around each point estimate, a 95 percent confidence interval is included. Thus, an estimate is statistically significant at the 5 percent level if its confidence interval does not cross the x-axis at value zero. The results in

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[16] However, as expected, the average employment of individuals earning at least two income base amounts per year is typically somewhat smaller in magnitude. These findings are available upon request.

[17] For the employment of first-generation non-western immigrants, regression tables including other specifications are in Appendix (see table A6-A8). From these tables, it is evident that the differences between using fixed effects and random effects are negligible. Tables including all variables as well as regression tables for second-generation non-western immigrants are available upon request.
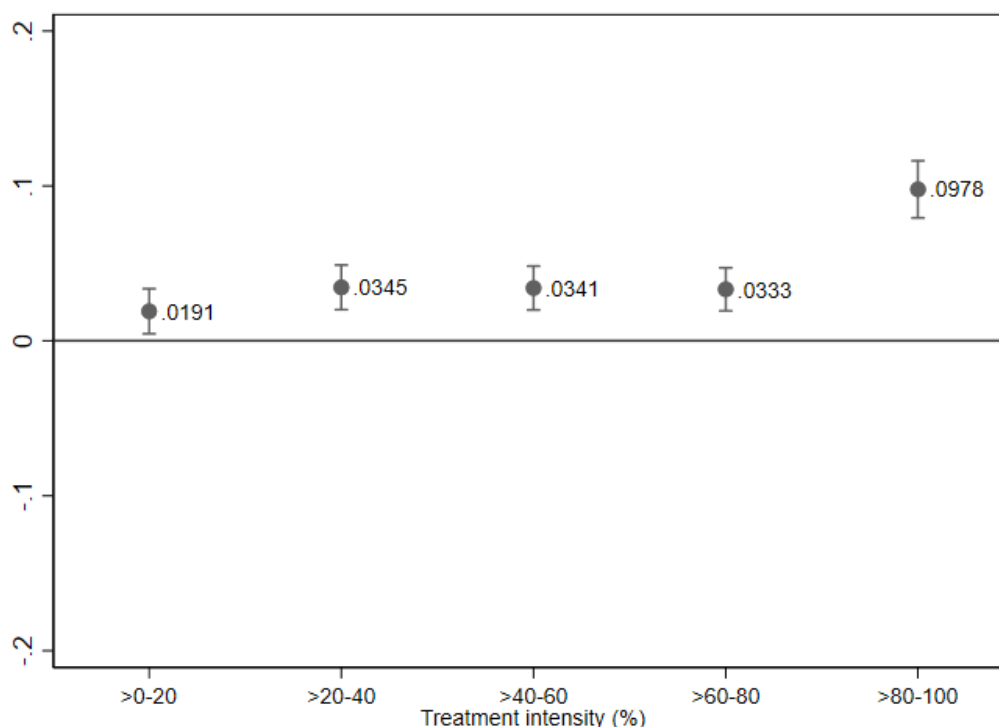
Figure 2 show that treated firms on average hired more first-generation non-western immigrants than the control firms after the payroll tax cut, but that the size of the effect is small for firms that received relatively modest labor costs savings. Note also that the confidence intervals for the lower treatment intensity groups overlap, suggesting that the estimates are not significantly different from one another.

However, firms within the highest >80-100 treatment intensity range - corresponding to initial one-year labor cost savings of at least 39 000 SEK (4,290 USD) - hired a significantly larger number of first-generation non-western immigrants than firms with smaller labor cost savings. Each firm within this group hired on average 0.10 first-generation non-western immigrants over the time period 2006-2008 as a result of the generated labor cost savings. This finding thus suggests that a reform which lowered the labor costs for young employees had the unintended consequence of increasing the recruitment of first-generation immigrants originating from non-western countries.

Recruiting additionally 0.10 non-western immigrants due to the youth payroll tax cut might appear to be a small effect, but recall that each firm within this treatment intensity range on average had 0.103 employees of non-western origin prior to the reform (see Table 1), suggesting that the effect is of large economic significance. Within the matched firm sample, the results suggest that the labor cost savings generated by the payroll tax reform created approximately 1,200 jobs for first-generation non-western immigrants in total.[18] Since approximately 5 percent of the Swedish population in 2006 was born in non-western countries (Statistics Sweden, 2019), the job creation among these immigrants appear to be more than proportionate to their population share.

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[18] This number is calculated by multiplying the point estimates of Figure 2 by the number of firms within the corresponding treatment intensity groups (see Table 1). That is;
0.0191*5337+0.0345*5327+0.0341*5325+0.0333*5331+0.0978*5330=1,166 jobs.

Figure 2. Total employment of first-generation non-western immigrants, by treatment intensity. DDD estimation.

Notes. Dependent variable: Number of first-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.001.

It might be the case that the positive effect of the payroll tax cut on the hiring of first-generation non-western immigrants is driven by young immigrants who are directly targeted by the reform, suggesting that it is age and not region of birth that explains the positive effects presented in Figure 2. This could be the case if the increased employment is merely caused by a substitution effect, which incentivizes recruitment within the target group. To evaluate whether this is the case, the total employment effect of the youth payroll tax cut on the hiring of first-generation non-western immigrants is decomposed into age groups below, within and above the targeted age group of 19-25-year-olds in Table 5.

The results in Table 5 show that the increased employment of first-generation non-western immigrants within the >0-80 treatment intensity range is solely explained by the hiring of immigrants that are above 25 years old, and who are thus not targeted by the payroll tax cut. However, for firms that received the largest labor cost savings due to the payroll tax cut, the results in Table 5 indicate that the labor costs savings were primarily used to hire first-

generation immigrants outside the targeted age group, although the effect is also positive and statistically significant for non-western immigrants of ages 19-25. This implies that the payroll tax reform had the unintended effect of promoting the employment of first-generation non-western immigrants who were not targeted by the reform, suggesting that the increased employment is primarily explained by an income effect.

Table 5. Total employment of first-generation non-western immigrants, by age group and treatment intensity. DDD estimation.

Tr. intensity:	>0-20	>20-40	>40-60	>60-80	>80-100
Max 18 years old	0.0010	-0.0008	0.0018	0.0010	0.0032*
	(0.0018)	(0.0010)	(0.0013)	(0.0014)	(0.0018)
19-25-year-olds	-0.0047	0.0033	0.0014	0.0074	0.0395***
	(0.0044)	(0.0041)	(0.0042)	(0.0046)	(0.0060)
Min 26 years old	0.0228***	0.0320***	0.0309***	0.0249***	0.0550***
	(0.0059)	(0.0059)	(0.0058)	(0.0054)	(0.0070)

Notes. Dependent variables: Number of first-generation non-western immigrants within the age intervals maximum 18 years, 19-25 years and minimum 26 years. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Firm clustered standard errors within parentheses. *p<0.1, **p<0.05, ***p<0.001.

Lastly, the estimated employment effect for second-generation non-western immigrants is presented in Figure A2 (in Appendix). The findings suggest that the initial labor cost savings had no effect on the recruitment of individuals belonging to this group. A possible explanation is that second-generation immigrants are better integrated on the Swedish labor market and, consequently, that their employment outcomes are less dependent on a reduction in firms' labor costs.

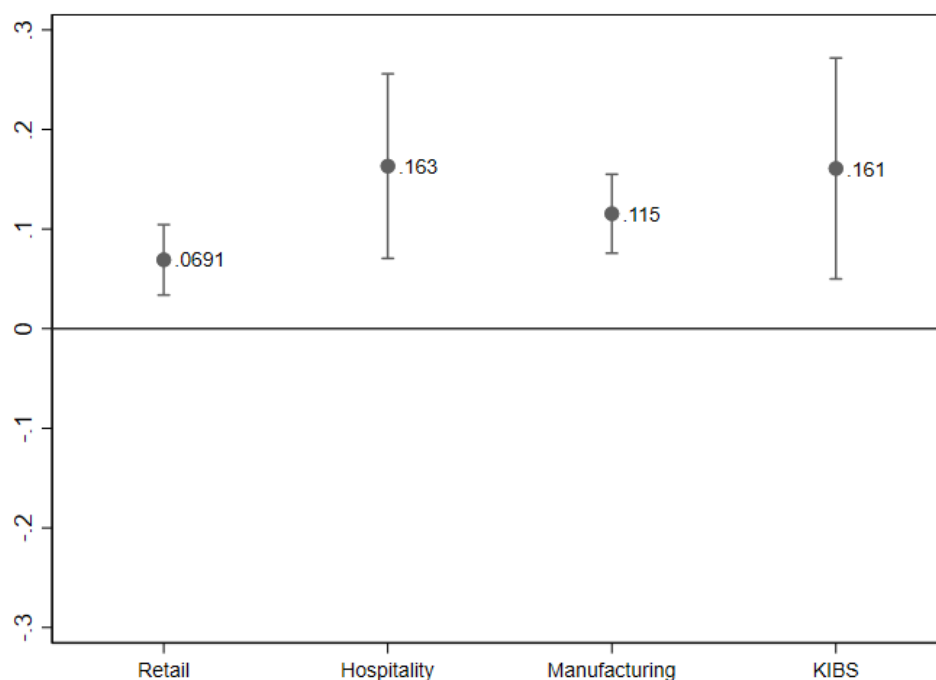## 5.2. IMMIGRANT EMPLOYMENT EFFECT – INDUSTRY-LEVEL DIFFERENCES

This section analyzes the effects of the firm-level labor cost savings, generated by the youth payroll tax cut, on the recruitment of first-generation non-western immigrants within the retail, hospitality, manufacturing and KIBS industries. Firms within these industries provide jobs that are characterized by large differences in educational and skills requirements. To ensure that any industry-level differences can be related to differences in proneness to recruit non-western immigrants rather than differences in average savings, the firms are required to follow the same cost savings distribution (see Table 4). [19]

Figure 4 presents the estimated employment effect for non-western immigrants among firms within the upper >80-100 treatment intensity range.[20] The point estimates for all industries are positive and statistically significant, with the estimated effect being considerably smaller among retail firms. For hospitality, manufacturing and KIBS firms, the estimated effects are relatively similar in magnitude and larger than the general effect found in Figure 2. Hence, based solely on the point estimates' magnitude, these findings suggest that the average retail firm was less prone to utilize its savings in hiring non-western immigrants than firms in general, whereas the opposite holds for hospitality, manufacturing and KIBS firms. However, the confidence intervals for all industries do overlap, which implies that the estimates are not significantly different from one another. One can thus conclude that the youth payroll tax cut had a positive effect on the recruitment of first-generation non-western immigrants in all industries under study.

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[19] The maximum savings within the different industries differ although they follow the same labor cost saving distribution. I therefore evaluate if the findings are robust to imposing an upper limit on the savings, which is done by excluding savings above the 99th percentile of the distribution (the top-one percent savings). Generally, the point estimates become slightly smaller in magnitude when imposing this restriction but their statistical significance is unaltered. An exception is found for KIBS firms, for which the point estimate in Figure 4 becomes statistically insignificant. From this robustness check, I thus conclude that industry-level differences regarding the recruitment of non-western immigrants are not due to differences in the size of labor cost savings. These findings are available upon request.

[20] Corresponding findings for the other treatment intensities are in appendix (see the second column of tables A7-A8). The only point estimate with a p-value below 0.05 is for hospitality firms within the 60-80 range and it is equal to 0.103.

Figure 4. Employment of first-generation non-western immigrants. Industry-level differences. Treatment intensity >80-100 %. DDD estimation.

Notes. Dependent variable: Number of first-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.001.

Figure A3 in appendix includes the corresponding estimates for second-generation non-western immigrants. The point estimates for all industries are statistically insignificant, suggesting that firms which received large labor cost savings due to the payroll tax reform did not use it to hire more second-generation immigrants in any of the industries under study. [21]

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[21] For the other treatment intensities, I do only find one statistically significant, but very minor, point estimate of size 0.0052 for manufacturing firms within the >40-60 range. See Table A9 (appendix).

6. CONCLUSIONS

Many European countries are facing great challenges in integrating foreign-born individuals on their labor markets, and in facilitating their labor market opportunities. The integration process is obstructed by the fact that a substantial share of the immigrants from non-western countries has little education and skills not directly transferable to the labor market of the receiving country. An extensively used labor market policy to improve immigrants' labor market situation has been to offer employers subsidized employment of foreign-born individuals. The Swedish government, for instance, has offered subsidies covering up to 80 percent of the wage cost. Usually, these subsidies are only available during a short period of time, which means that employers might remain hesitant to recruit first-generation immigrants. This type of selective policies might also crowd-out other groups from the labor market and displace regular employment.

The purpose of this paper has been to investigate if a Swedish payroll tax reform targeted at young individuals and which generated labor cost savings for youth-intensive firms, had the unintended consequence of promoting the recruitment of non-western immigrants. Unlike previous subsidies aimed directly at immigrants, this reform created immediate labor cost savings for firms with many young employees. The savings created both an income effect and a substitution effect. The substitution effect suggests an increased recruitment of young individuals as they became less costly hire. However, the income effect could have resulted in firms hiring individuals outside the reform's targeted age group. Moreover, since these savings were contingent on the firms' initial workforce composition, they were not tied to a certain time period, nor to a specific group, in the same way as wage subsidies for foreign-born individuals.

Three main results emerge from the empirical analyses. First, there is a positive and relatively strong link between the size of firms' savings and their subsequent recruitment of first-generation non-western immigrants. The average employment effect is five times larger among firms with the largest savings compared to the firms with the smallest savings. Second, a substantial fraction of the employment effect is driven by an increased recruitment of older, non-targeted immigrants. This implies that the increased recruitment to a large extent is explained by an income effect rather than simply by a substitution effect. Third, industry-level analyses suggest statistically significant employment increases within all of the four industries

under study, which implies that the positive effect of the payroll tax reform on immigrant employment is not limited to industries that provide low-qualified jobs.

Thus, although the payroll tax reform was implemented to promote youth employment, it also had the unintended effect of promoting the recruitment of individuals outside its target group. Specifically, the findings illustrate that labor cost savings can enhance the labor market opportunities for non-western immigrants. A potential explanation behind this finding is that such savings lower the barrier that hinders firms from hiring first-generation immigrants, whose previous skills and experience might be difficult to observe and evaluate.

The economic significance of my findings implies that the labor cost savings created approximately 1,200 jobs for first-generation non-western immigrants within the sample of matched firms over the time period 2006-2008. In a previous study by Daunfeldt et al. (2019), it is concluded that the reform created 16,400 new jobs in total. Considering that approximately five percent of the Swedish population in 2006 were born in non-western countries, it seems as if the employment effect for non-western immigrants was more than proportionate to their population share. Daunfeldt et al. (2019) also found that the majority of the created jobs were for young, targeted individuals. Combining this finding with the fact that the employment effects of this paper are mainly driven by an increased recruitment of older non-western immigrants, suggests that there is a tendency that young native-born and older non-western immigrants are seen as substitutes when firms make their recruitment decisions.

Lastly, I should stress that this study is not without limitations. First, the empirical analysis relies on a sample of firms which are existent over the years 2003-2008. Thus, inference should only be made for surviving firms and not for firms entering or exiting during the period of study. Importantly, although the statistical matching generates a representative control group of firms, it limits the number of firms included in the empirical analysis. The internal validity of the findings is likely to be high but, possibly, at the expense of a limited external validity. Furthermore, due to data limitations, this study is not able to optimally separate labor market immigrants from refugees. Although non-western immigrants are overrepresented within the refugee population, the ideal would be to exploit information on the reason for immigration.

Finally, I want to emphasize that the empirical analysis does capture the recruitment of all non-western immigrants, regardless of their previous employment status. Thus, the labor cost savings do not only promote the recruitment of unemployed immigrants but also the matching of, and job mobility for, immigrants who have already entered employment. This is of interest considering that immigrants are generally overrepresented in part-time work and are more likely to be overqualified for their job. However, decomposing the employment effects based on immigrants' work history and employment status constitute interesting questions for future research.

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APPENDIX

Table A1. Retail firms with labor cost savings vs. non-retail firms without labor cost savings. Balance on matching variables. Year 2006.

	Min	5th	25th	50th	75th	95th	Max	Mean	# Firms
Change in # West. imm, 2003-2006									
Treated	-2	-1	0	0	0	1	3	0.018	4,297
Control	-2	-1	0	0	0	1	3	0.002	4,297
Change in # Non-west. imm, 2003-2006									
Treated	-5	0	0	0	0	1	3	0.008	4,297
Control	-4	0	0	0	0	1	3	0.034	4,297
Share without postsecondary education									
Treated	0	0.5	0.769	0.917	1	1	1	0.862	4,297
Control	0	0.5	0.769	0.917	1	1	1	0.862	4,297
Share western imm.									
Treated	0	0	0	0	0	0.222	1	0.037	4,297
Control	0	0	0	0	0	0.222	1	0.037	4,297
Share non-western imm.									
Treated	0	0	0	0	0	0.167	1	0.024	4,297
Control	0	0	0	0	0	0.167	1	0.024	4,297

Notes: Similarity of the distributions of each matching variable for treated retail firms and non-retail firms. CEM matching. Including firms surviving, having at least one employee per year and operating within the same 2-digit industry across the time period 2003-2008. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Variables measured in year 2006.

Table A2. Hospitality firms (hotels and restaurants (H&R)) with labor cost savings vs. non-H&R firms without labor cost savings. Year 2006.

	Min	5th	25th	50th	75th	95th	Max	Mean	# Firms
Change in # West. imm, 2003-2006									
Treated	-4	-1	0	0	0	1	3	0.079	1,955
Control	-3	-1	0	0	0	1	4	0.033	1,955
Change in # Non-west. imm, 2003-2006									
Treated	-4	-1	0	0	0	1	6	0.071	1,955
Control	-3	-1	0	0	0	1	5	0.054	1,955
Share without postsecondary education									
Treated	0	0.5	0.789	0.929	1	1	1	0.871	1,955
Control	0	0.5	0.786	0.929	1	1	1	0.870	1,955
Share western imm.									
Treated	0	0	0	0	0.143	0.667	1	0.119	1,955
Control	0	0	0	0	0.143	0.667	1	0.119	1,955
Share non-western imm.									
Treated	0	0	0	0	0	0.5	1	0.081	1,955
Control	0	0	0	0	0	0.5	1	0.081	1,955

Notes: Similarity of the distributions of each matching variable for treated H&R firms and non-H&R firms. CEM matching. Including firms surviving, having at least one employee per year and operating within the same 2-digit industry across the time period 2003-2008. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Variables measured in year 2006.

Table A3. Manufacturing firms with labor cost savings vs. non-manufacturing firms without labor cost savings. Year 2006.

	Min	5th	25th	50th	75th	95th	Max	Mean	# Firms
Change in # West. imm, 2003-2006									
Treated	-3	-1	0	0	0	1	3	0.041	3,346
Control	-4	-1	0	0	0	1	3	0.023	3,346
Change in # Non-west. imm, 2003-2006									
Treated	-3	0	0	0	0	0	3	-0.013	3,346
Control	-3	0	0	0	0	0	3	0.005	3,346
Share without postsecondary education									
Treated	0	0.5	0.8	0.921	1	1	1	0.872	3,346
Control	0	0.5	0.8	0.923	1	1	1	0.872	3,346
Share western imm.									
Treated	0	0	0	0	0.071	0.25	1	0.054	3,346
Control	0	0	0	0	0.071	0.25	1	0.054	3,346
Share non-western imm.									
Treated	0	0	0	0	0	0.077	1	0.011	3,346
Control	0	0	0	0	0	0.077	1	0.011	3,346

Notes: Similarity of the distributions of each matching variable for treated manufacturing firms and non-manufacturing firms. CEM matching. Including firms surviving, having at least one employee per year and operating within the same 2-digit industry across the time period 2003-2008. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Variables measured in year 2006.

Table A4. Knowledge-intensive business services (KIBS) firms with labor cost savings vs. non-KIBS firms without labor cost savings. Year 2006.

	Min	5th	25th	50th	75th	95th	Max	Mean	# Firms
Change in # West. imm, 2003-2006									
Treated	-3	-1	0	0	0	1	5	0.083	1,502
Control	-4	-1	0	0	0	1	5	0.033	1,502
Change in # Non-west. imm, 2003-2006									
Treated	-2	0	0	0	0	0	2	0.004	1,502
Control	-2	0	0	0	0	0	2	0.019	1,502
Share without postsecondary education									
Treated	0	0	0.286	0.5	0.786	1	1	0.532	1,502
Control	0	0	0.286	0.5	0.792	1	1	0.532	1,502
Share western imm.									
Treated	0	0	0	0	0.042	0.25	1	0.041	1,502
Control	0	0	0	0	0.042	0.25	1	0.041	1,502
Share non-western imm.									
Treated	0	0	0	0	0	0.1	1	0.013	1,502
Control	0	0	0	0	0	0.1	1	0.013	1,502

Notes: Similarity of the distributions of each matching variable for treated KIBS firms and non-KIBS firms. CEM matching. Including firms surviving, having at least one employee per year and operating within the same 2-digit industry across the time period 2003-2008. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Variables measured in year 2006.

Table A5. Description of industry categories. NACE Rev 1.1.

Industry category	NACE Rev 1.1	Description
Retail	52.1	Retail sale in non-specialized stores
	52.2	Retail sale of food, beverages and tobacco in specialized stores
	52.3	Retail sale of pharmaceutical and medical goods, cosmetic and toilet articles
	52.4	Other retail sale of new goods in specialized stores
	52.5	Retail sale of second-hand goods in stores
	52.6	Retail sale not in stores
	52.7	Repair of personal and household goods
H&R	55.1	Hotels
	55.2	Camping sites and other provision of short-stay accommodation
	55.3	Restaurants
	55.4	Bars
	55.5	Canteens and catering
Manufacturing	15	Manufacture of food products and beverages
	16	Manufacture of tobacco products
	17	Manufacture of textiles
	18	Manufacture of wearing apparel; dressing and dyeing of fur
	19	Tanning and dressing of leather; manufacture of luggage, handbags, saddlery, harness and footwear
	20	Manufacture of wood and of products of wood and cork, except furniture; manufacture of articles of straw and plaiting materials
	21	Manufacture of pulp, paper and paper products
	22	Publishing, printing and reproduction of recorded media
	23	Manufacture of coke, refined petroleum products and nuclear fuel
	24	Manufacture of chemicals and chemical products
	25	Manufacture of rubber and plastic products
	26	Manufacture of other non-metallic mineral products
	27	Manufacture of basic metals
	28	Manufacture of fabricated metal products, except machinery and equipment
	29	Manufacture of machinery and equipment n.e.c.
	30	Manufacture of office machinery and computers
	31	Manufacture of electrical machinery and apparatus n.e.c.
	32	Manufacture of radio, television and communication equipment and apparatus
	33	Manufacture of medical, precision and optical instruments, watches and clocks
	34	Manufacture of motor vehicles, trailers and semi-trailers
	35	Manufacture of other transport equipment
	36	Manufacture of furniture; manufacturing n.e.c.
	37	Recycling
KIBS	72.1	Hardware consultancy
	72.2	Software consultancy and supply
	72.3	Data processing
	72.4	Database activities
	72.5	Maintenance and repair of office; accounting and computing machinery
	72.6	Other computer related activities
	73.1	Research and experimental development on natural sciences and engineering
	73.2	Research and experimental development on social sciences and humanities
	74.1	Legal, accounting, book-keeping and auditing activities; tax consultancy…
		…market research and public opinion polling; business and management consultancy; holdings
	74.2	Architectural and engineering activities and related technical consultancy
	74.3	Technical testing and analysis
	74.4	Advertising

Notes: The KIBS sector is defined in accordance with Eurostat (2012). NACE Rev 1.1 and SNI2002 are identical up to (and including) the four-digit level. H&R refers to hotels and restaurants (hospitality firms).

Table A6. All firms. Employment of first-generation non-western immigrants. DDD estimation.

Specification: Tr. intensity	1	2	3	4
0-20	0.0191	0.0191**	0.0191**	0.0198***
	(0.0126)	(0.00742)	(0.00742)	(0.00747)
Observations	170,016	170,016	170,016	170,016
R-squared	0.004	0.004	.	.
20-40	0.0345***	0.0345***	0.0345***	0.0350***
	(0.0122)	(0.00734)	(0.00734)	(0.00737)
Observations	169,971	169,971	169,971	169,971
R-squared	0.004	0.005	.	
40-60	0.0341***	0.0341***	0.0341***	0.0341***
	(0.0121)	(0.00721)	(0.00721)	(0.00723)
Observations	169,965	169,965	169,965	169,965
R-squared	0.003	0.004	.	.
60-80	0.0333***	0.0333***	0.0333***	0.0333***
	(0.0118)	(0.00709)	(0.00709)	(0.00711)
Observations	169,998	169,998	169,998	169,998
R-squared	0.003	0.004	.	.
80-100	0.0978***	0.0978***	0.0978***	0.0978***
	(0.0128)	(0.00942)	(0.00942)	(0.00942)
Observations	169,986	169,986	169,986	169,986
R-squared	0.008	0.007	.	.
Firm clustered s.e	No	Yes	Yes	Yes
Firm FE	No	Yes	No	No
Firm RE	No	No	Yes	Yes
Industry FE	No	No	No	Yes
Municip FE	No	No	No	Yes

Notes. Dependent variable: Number of first-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Standard errors within parentheses. Point estimates in figures are represented by the estimate in the second column. *p<0.1, **p<0.05, ***p<0.001.

Table A7. Retail and Hospitality firms. Employment of first-generation non-western immigrants. DDD estimation.

RETAIL Tr. intensity	1	2	3	4	HOSPITALITY Tr. intensity	1	2	3	4
0-20	-0.00254	-0.00254	-0.00254	-0.00291	**0-20**	0.0679	0.0679*	0.0679*	0.0684
	(0.0280)	(0.0159)	(0.0159)	(0.0161)		(0.0864)	(0.0403)	(0.0403)	(0.0416)
Observations	26,952	26,952	26,952	26,952	Observations	12,627	12,627	12,627	12,627
R-squared	0.005	0.007	.	.	R-squared	0.008	0.006	.	.
20-40	0.00774	0.00774	0.00774	0.00674	**20-40**	0.0267	0.0267	0.0267	0.0254
	(0.0270)	(0.0166)	(0.0166)	(0.0169)		(0.0848)	(0.0478)	(0.0478)	(0.0491)
Observations	27,204	27,204	27,204	27,204	Observations	12,456	12,456	12,456	12,456
R-squared	0.004	0.007	.	.	R-squared	0.012	0.010	.	.
40-60	-0.00644	-0.00644	-0.00644	-0.00645	**40-60**	0.0733	0.0733	0.0733	0.0766
	(0.0277)	(0.0155)	(0.0155)	(0.0157)		(0.0989)	(0.0537)	(0.0537)	(0.0547)
Observations	26,919	26,919	26,919	26,919	Observations	11,739	11,739	11,739	11,739
R-squared	0.004	0.008	.	.	R-squared	0.007	0.008	.	.
60-80	0.0346	0.0346*	0.0346*	0.0325*	**60-80**	0.103	0.103**	0.103**	0.103*
	(0.0277)	(0.0183)	(0.0183)	(0.0184)		(0.0953)	(0.0523)	(0.0523)	(0.0537)
Observations	26,637	26,637	26,637	26,637	Observations	11,715	11,715	11,715	11,715
R-squared	0.005	0.008	.	.	R-squared	0.008	0.008	.	.
80-100	0.0691***	0.0691***	0.0691***	0.0696***	**80-100**	0.163**	0.163***	0.163***	0.166***
	(0.0260)	(0.0180)	(0.0180)	(0.0184)		(0.0807)	(0.0473)	(0.0473)	(0.0486)
Observations	28,305	28,305	28,305	28,305	Observations	12,348	12,348	12,348	12,348
R-squared	0.012	0.007	.	.	R-squared	0.006	0.013	.	.
Firm clustered s.e	No	Yes	Yes	Yes	Firm clustered s.e	No	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Firm FE	No	Yes	No	No
Firm RE	No	No	Yes	Yes	Firm RE	No	No	Yes	Yes
Industry FE	No	No	No	Yes	Industry FE	No	No	No	Yes
Municip FE	No	No	No	Yes	Municip FE	No	No	No	Yes

Notes. Dependent variable: Number of first-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Standard errors within parentheses. Point estimates in figures are represented by the estimates in the second column. *p<0.1, **p<0.05, ***p<0.001.

Table A8. Manufacturing and KIBS firms. Employment of first-generation non-western immigrants. DDD estimation.

| MANUFACTURING Tr. intensity	1	2	3	4	KIBS Tr. intensity	1	2	3	4
0-20	0.00683	0.00683	0.00683	0.00696	**0-20**	-0.00803	-0.00803	-0.00803	-0.00466
	(0.0305)	(0.0181)	(0.0181)	(0.0184)		(0.0459)	(0.0291)	(0.0291)	(0.0300)
Observations	20,010	20,010	20,010	20,010	Observations	9,807	9,807	9,807	9,807
R-squared	0.006	0.006	.	.	R-squared	0.006	0.008	.	.
20-40	0.00182	0.00182	0.00182	0.00151	**20-40**	0.0400	0.0400	0.0400	0.0411
	(0.0299)	(0.0206)	(0.0206)	(0.0209)		(0.0538)	(0.0303)	(0.0304)	(0.0315)
Observations	20,193	20,193	20,193	20,193	Observations	9,540	9,540	9,540	9,540
R-squared	0.006	0.006	.	.	R-squared	0.007	0.008	.	.
40-60	0.0280	0.0280*	0.0280*	0.0287*	**40-60**	0.0884	0.0884*	0.0884*	0.0850*
	(0.0259)	(0.0148)	(0.0148)	(0.0152)		(0.0577)	(0.0494)	(0.0494)	(0.0460)
Observations	20,688	20,688	20,688	20,688	Observations	9,324	9,324	9,324	9,324
R-squared	0.006	0.006	.	.	R-squared	0.011	0.021	.	.
60-80	0.0353	0.0353*	0.0353*	0.0354*	**60-80**	0.0108	0.0108	0.0108	0.0142
	(0.0266)	(0.0187)	(0.0187)	(0.0190)		(0.0526)	(0.0406)	(0.0406)	(0.0421)
Observations	21,129	21,129	21,129	21,129	Observations	9,369	9,369	9,369	9,369
R-squared	0.006	0.006	.	.	R-squared	0.009	0.014	.	.
80-100	0.115***	0.115***	0.115***	0.119***	**80-100**	0.161***	0.161***	0.161***	0.163***
	(0.0257)	(0.0202)	(0.0202)	(0.0208)		(0.0616)	(0.0566)	(0.0566)	(0.0588)
Observations	21,978	21,978	21,978	21,978	Observations	9,156	9,156	9,156	9,156
R-squared	0.015	0.014	.	.	R-squared	0.013	0.016	.	.
Firm clustered s.e	No	Yes	Yes	Yes	Firm clustered s.e	No	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Firm FE	No	Yes	No	No
Firm RE	No	No	Yes	Yes	Firm RE	No	No	Yes	Yes
Industry FE	No	No	No	Yes	Industry FE	No	No	No	Yes
Municip FE	No	No	No	Yes	Municip FE	No	No	No	Yes

Notes. Dependent variable: Number of first-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Standard errors within parentheses. Point estimates in figures are represented by the estimates in the second column. *p<0.1, **p<0.05, ***p<0.001.

Table A9. Employment of second-generation non-western immigrants. Industry-level differences. DDD estimation.

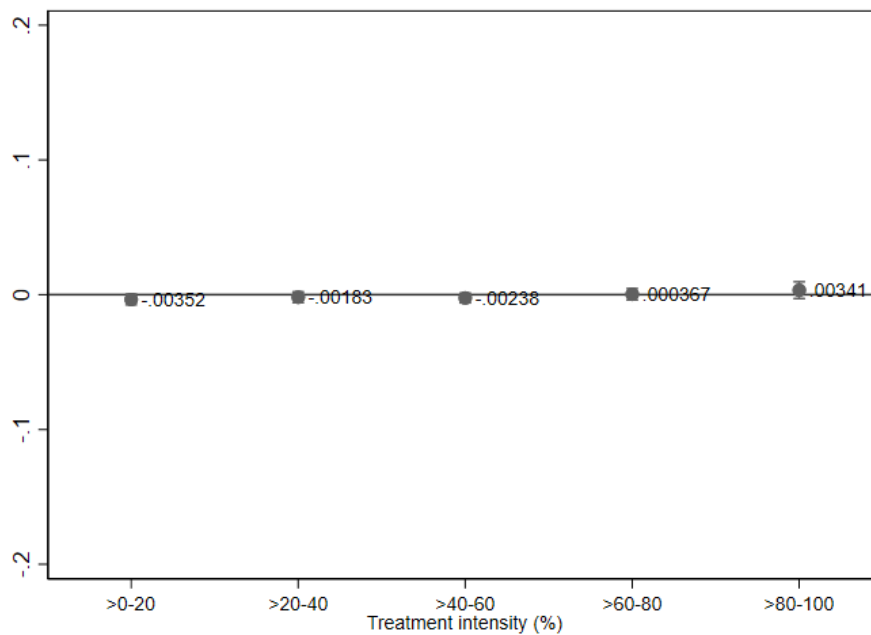| Tr. intensity	Retail	Hospitality	Manufacturing	KIBS
>0-20	0.0029	-0.0154	0.0052	0.0070
	(0.0038)	(0.0099)	(0.005)	(0.0069)
>20-40	-0.0077	0.0179	-0.0013	0.0095
	(0.0052)	(0.0116)	(0.0037)	(0.0074)
>40-60	-0.0051	-0.0160	0.0052**	-0.0177
	(0.0053)	(0.0152)	(0.0027)	(0.0113)
>60-80	0.0031	0.0092	0.0012	0.0080
	(0.0070)	(0.0150)	(0.0042)	(0.0158)
>80-100	0.0015	0.0056	-0.0047	-0.0010
	(0.0081)	(0.0168)	(0.0044)	(0.0155)

Notes. Dependent variable: Number of second-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Firm clustered standard errors within parentheses. *p<0.1, **p<0.05, ***p<0.001.

Figure A1. Figure 2 decomposed into separate DiD estimates over time periods 2003-2005 and 2006-2008.
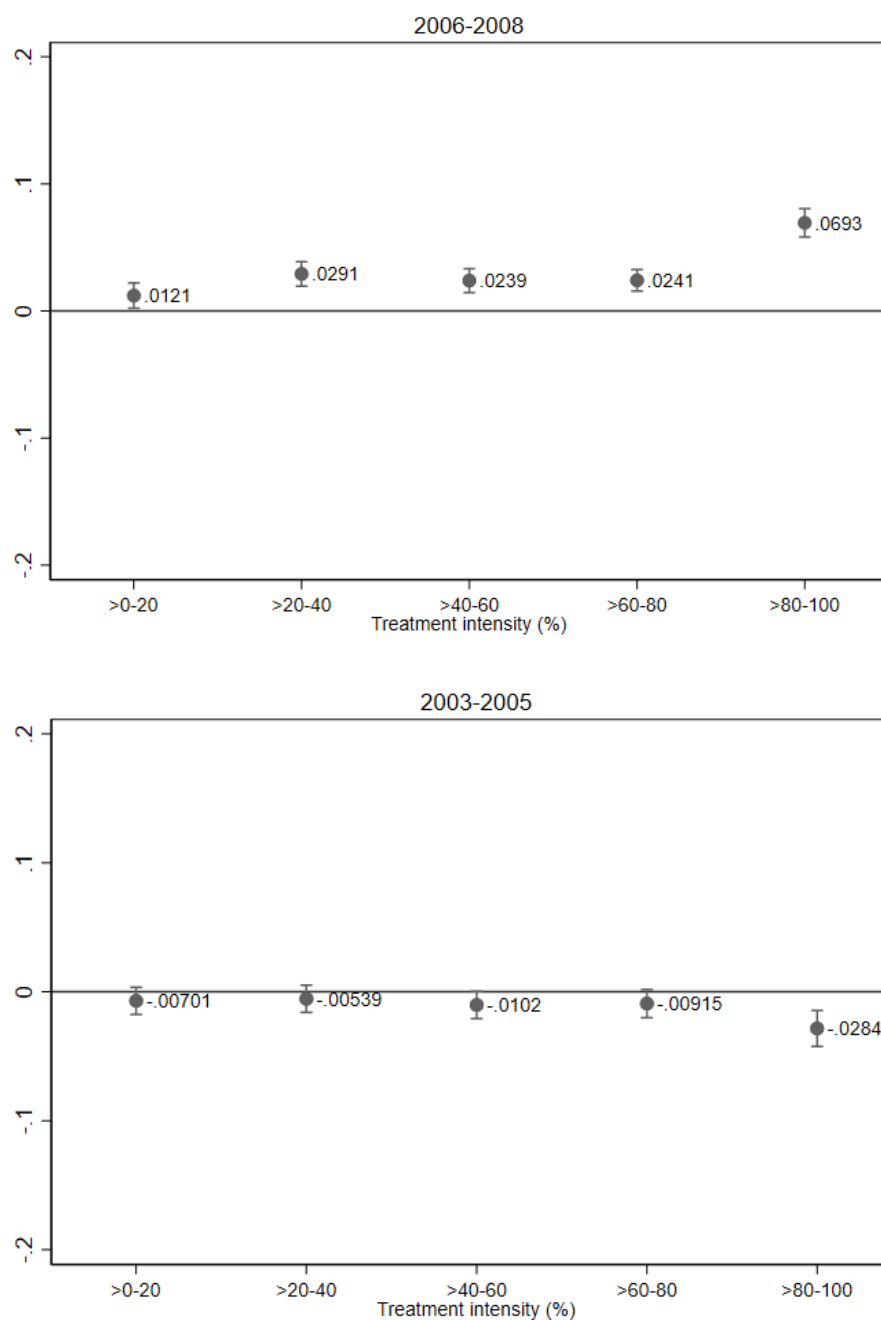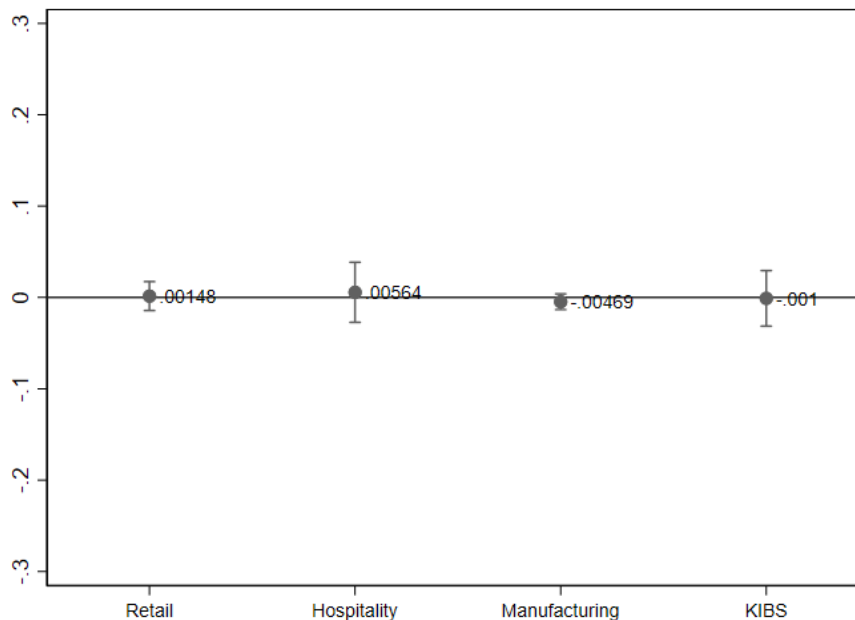

Notes. Dependent variable: Number of first-generation non-western immigrants. DiD estimation over treatment period 2006-2008 and underlying time period 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.001.

Figure A2. Total employment of second-generation non-western immigrants, by treatment intensity. DDD estimation.

Notes. Dependent variable: Number of second-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.001.

Figure A3. Employment of second-generation non-western immigrants. Industry-level differences. Treatment intensity >80-100 %. DDD estimation.

Notes. Dependent variable: Number of second-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers - defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth - are excluded. Firm clustered standard errors. *p<0.1, **p<0.05, ***p<0.001.