INSTITUTE OF RETAIL ECONOMICS

FIRMS' LABOR COST SAVINGS AND RECRUITMENT OF NON-WESTERN IMMIGRANTS: THE UNINTENDED EFFECT OF A PAYROLL TAX REFORM

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HFI WORKING PAPER No5

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Abstract: Immigrants have long faced great challenges in European labor markets, and policymakers in many countries are struggling to improve immigrants' labor market integration. This paper evaluates whether a Swedish youth payroll tax cut 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, this study investigates the effect of these labor cost savings on 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 is largely driven by increased employment of older immigrants who were not targeted by the reform. Within the analyzed sample of firms, 1,100 jobs were created for this group, 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.

Keywords: labor market integration, labor costs, payroll tax cut, non-western immigrants, employment

JEL classifications: H32, J23, J30, J61, L25

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[†] Financial support from the Swedish Retail and Wholesale Council is gratefully acknowledged.

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 influx has put pressure on receiving countries in their attempts to integrate these individuals into their labor markets, which the OECD (2015) argues to be the most crucial part of immigrants' assimilation process. Labor market integration is complicated because many refugees have little education or have an education that is not applicable to 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 (EU) is further obstructed by the difficulties that first-generation immigrants face in establishing themselves in the labor market. In an evaluation of refugees' labor market situation in Europe, the European Commission and OECD (2016) found that non-EU-born immigrants, especially refugees, have higher long-term unemployment rates than native-born within the EU 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 overrepresented in part-time employment and tend to be overqualified for their work tasks, which reflects their particularly problematic labor market situation (OECD, 2014; European Commission and OECD, 2016). The employment rates for native-born and non-EU-born individuals were 69.3 and 64.7 percent, respectively, in 2006. This employment gap of 4.6 percentage points 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 entering the labor market (Martin and Grubb, 2001). Such policies have often been shown to improve employment probabilities, but the majority of previous evaluations have analyzed only the short-term effects of these types of subsidies (Nekby, 2008). Several studies have also argued that such subsidies can potentially displace regular employment, cause deadweight losses and lead to only a small net gain in employment (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 do not directly target 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 and 25 years of age. The magnitude of the payroll tax cut was thus directly related to the number of young employees, meaning that youth-intensive firms received considerable labor cost savings compared to firms that had few young employees. Theoretically, the reform gave rise to two potentially opposing effects. First, firms were incentivized to increase their number of young employees because these employees became less costly to hire, which reflects a substitution effect. Second, the labor cost savings generated by the reform decreased firms' overall production costs, resulting in lower marginal costs of production. This gave rise to a scale effect through which firms were induced to expand their total production and thus to increase their demand for input factors other than youth labor alone. The scale effect is of particular interest in the context of this paper because it suggests that the employment effects of the payroll tax cut were not necessarily limited to the targeted age group.

From a theoretical standpoint, there are multiple reasons why general labor cost reductions may be particularly beneficial for the employment opportunities of immigrants. As firms are provided with larger financial resources, they may become less risk averse in their employment decisions, which promotes the employment of immigrants who were previously considered too risky or costly to hire. Since the payroll tax cut particularly promoted youth employment (Daunfeldt et al., 2019), this might be especially prevalent in cases where immigrants and youths are considered substitutes and when there is a shortage in the youth labor supply. For instance, youths and many immigrant groups have low education levels on average. Another factor is high minimum wage levels, which have been found to disadvantage immigrants' employment opportunities (Jardim et al., 2017). Immigrants may be overrepresented in the pool of unemployed individuals with marginal productivity just below the minimum wage level, which suggests that they might be more likely to be offered employment when firms obtain larger financial resources. In support of this possibility is the fact that reduced payroll taxes have been shown to be particularly supportive of the employment of low-educated individuals, who face

a high labor demand elasticity, meaning that their employment opportunities are more sensitive to labor cost changes (Stokke, 2016).

Using detailed employer-employee data from Statistics Sweden, firms' one-year labor cost savings created by the reform is calculated. All firms that received labor cost savings serve as treated firms. 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 and therefore did not receive any immediate labor cost savings. CEM is used to ensure that the treated and control firms are similar job providers for immigrants in the absence of the reform, meaning that the control firms should resemble 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 have a larger number of employees than firms with no (or small) cost savings and that large firms typically experience 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 possibility, a difference-in-difference-in-difference (DDD) model is used 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 then estimating a DDD model, the immigrant employment effect is isolated. The empirical analysis also includes an industry-level comparison of the effect of reduced labor costs on immigrant employment. Theoretically, one may expect to find large variations since both job skills and educational requirements vary extensively across industries. Specifically, separate estimations are carried out for the retail, hospitality, manufacturing and knowledge-intensive business services industries (henceforth KIBS).

The results show that the youth payroll tax reform increased the employment of first-generation non-western immigrants not explicitly targeted by the reform and that the magnitude of the employment effect increased with the size of firms'

labor cost savings. The average employment increase among firms with the largest savings was more than six times larger than the corresponding increase among firms with the smallest savings. The findings suggest that within the sample of firms, 1,100 jobs were created for first-generation non-western immigrants due to the youth payroll tax cut. This figure is more than proportionate to the immigrant group's population share and indicates that the estimated effect was of economic significance. The industry-level analyses suggest that the positive effect on immigrant employment was not limited to industries that provide low-skilled jobs.

The findings of this paper illustrate that a reform that caused a reduction in firms' total labor costs promoted the recruitment of first-generation non-western immigrants even though this was not the explicit purpose of the reform. This may 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 a broader perspective, 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 and a brief overview of immigrants' labor market situation in Sweden. Section 3 describes the Swedish youth payroll tax reform. Data and descriptive statistics are presented in Section 4, while the empirical methodology is described in Section 5. Section 6 presents the empirical findings. Finally, Section 7 discusses 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. Insiders' market power in turn aggravates 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, which implies that insiders influence firms' employment decisions. 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 insiders. For instance, the limited, or not directly applicable, education and lack of language skills of some immigrant groups indicate that their outsider status on the labor market could be prominent. The insider-outsider theory thus suggests that many immigrants might be outsiders and that firms might be reluctant to employ them. There are several other factors that explain why firms could be less likely to hire foreign-born individuals, e.g., asymmetric information due to these individuals' limited or imperfectly transferable education, their low level of work experience and insufficient language skills (Chiswick and Miller, 2009; Eriksson, 2011; OECD, 2014) as well as discrimination (Phelps, 1972; Arai and Thoursie, 2009), high minimum wage levels (Jardim et al., 2017), and limited social networks (Montgomery, 1991; Behtoui, 2008; Beaman and Magruder, 2012). Such factors may constitute barriers against the employment of immigrants.

Given these theoretical explanations of why firms are less likely 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 both a substitution effect and a scale (output) effect. To see this, consider a firm that produces output y using (for simplicity only) two input factors: z_1 and z_2. Assume that the prices

of z_1 and z_2 are p_1 and p_2 , respectively. The firm's production function is $y = f(z_1, z_2)$. [1] The cost-minimizing input levels can be expressed as

$$\widehat{z_i} = h_i(p_1, p_2, y), \ \text{i} = 1,2 \tag{1}$$

Thus, the cost-minimizing level of each input factor is a function of the input prices and the produced output level. The firm maximizes its profit $\pi = R\big(f(z_1, z_2)\big) - \sum_{i=1}^{2} p_i z_i$, where $R(\cdot)$ denotes a revenue function. The optimal amount of the input factors can be expressed through the input demand functions

$$z_i^* = D_i(p_1, p_2), i = 1,2$$

which simply states the optimal demand for an input factor as a function of all input prices. In turn, the profit-maximizing level of output can be written as

$$y^* = f(z_1^*, z_2^*) = y^*(p_1, p_2) \tag{2}$$

Since profit maximization must imply cost minimization, and by substituting y^* in equation (2) for y in equation (1), we obtain the following expression:

$$z_i^* = D_i(p_1, p_2) = \widehat{z_i} = h_i\big(p_1, p_2, y^*(p_1, p_2)\big)$$

In the context of this paper, we can consider z_1 to be youth labor and z_2 to be other labor, e.g., older non-western immigrants. Consequently, a reduced payroll tax for young employees is represented by a decrease in p_1. To evaluate how a price decrease affects the demand for z_1, differentiate the expression above with respect to p_1.

$$\frac{\partial D_1}{\partial p_1} = \frac{\partial h_1}{\partial p_1} + \frac{\partial h_1}{\partial y}\frac{\partial y}{\partial y^*}\frac{\partial y^*}{\partial p_1} \tag{3}$$

Assuming that the input factors are normal (not inferior) goods, the derivatives have the following signs:

[1] The derivations of the substitution and scale effects are based on Gravelle and Rees (2004).

$$\frac{\partial h_1}{\partial p_1} < 0; \frac{\partial h_1}{\partial y} > 0; \frac{\partial y}{\partial y^*} = 1; \frac{\partial y^*}{\partial p_1} < 0$$

The first derivative on the right-hand side of equation (3) constitutes a substitution effect; for a fixed y, a decrease in p_1 will increase the demand for z_1. The second part on the right-hand side is a scale effect, which consists of three partial derivatives whose product is negative. The third derivative, $\frac{\partial y^*}{\partial p_1} < 0$, is of particular interest. A decrease in the price of z_1 causes the marginal cost curve to shift downwards since $\frac{\partial MC}{\partial p_1} > 0$. In the context of the payroll tax reform, this is where the labor cost savings are generated. The decrease in marginal cost in turn raises the profit-maximizing level of output – y^*– and thus increases the total output produced. The scale effect has two important consequences. It generates an increased demand for z_1 since its demand depends positively on the output level. However, due to the same mechanism, the scale effect also results in an increased demand for z_2.

Thus, through the scale effect, the youth payroll tax reform could have resulted in increased employment of other groups on the labor market. There are reasons why reduced labor costs could particularly benefit the employment of disadvantaged immigrant groups. The previously discussed barriers against immigrant employment could entail large risks for firms that are considering hiring foreign-born individuals. However, the increased resources that were generated by the payroll tax cut could have lowered such barriers and induced firms to become more risk taking, thereby promoting the employment of foreign-born individuals who were previously considered too risky or too costly to employ. Since the reform primarily incentivized the employment of youth, this may be the case especially if there was a shortage in youth labor supply, assuming that youths and immigrants are seen as substitutes on the labor market. Many youths and immigrant groups do, for instance, have low education levels and may therefore qualify for similar jobs. Thus, even if noneligible immigrants are considered substitutes their labor demand may increase due to a restricted youth labor supply. Possibly, youth labor supply may be restricted along both the extensive (number of individuals) and the intensive margin (number of work hours), e.g., if many young prefer part-time work. High minimum wages have

been shown to negatively affect immigrants' employment opportunities (Jardim et al., 2017). If immigrants are overrepresented among unemployed individuals who have a productivity lower than the productivity corresponding to the minimum wage level, their likelihood of entering employment may increase considerably in response to firms' larger financial resources. This possibility is supported by the finding that a payroll tax cut is especially beneficial for low-educated individuals, who have a high labor demand elasticity and whose employment opportunities are thus highly sensitive to changes in labor costs (Stokke, 2016).

2.2. Immigrants on the labor market in Sweden

Immigration to Sweden has undergone substantial changes during recent decades. From the 1950s until the mid-1970s, 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), the employment rate of the foreign-born population was 20 percent lower than that of the native-born population. During recent decades, the composition of the immigrant population has shifted from mainly labor force immigrants to refugee immigrants. This compositional change is also reflected in immigrants' region of birth. In 2000, 28 percent of the foreign-born population was born in another Nordic country, whereas 36 percent was born in a non-European country. The corresponding shares in 2018 were 12 and 56 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 have 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) also

argues 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 and 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, as those arriving during favorable labor market conditions fare better, with higher subsequent employment and earnings.

Rooth and Åslund (2007) emphasize the importance of language skills. They find that poor language acquisition makes 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 that marginalization and socioeconomic status among immigrants in Sweden are 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 with a Southern- or non-European background face a higher risk of unemployment and have lower earnings than comparable native-born individuals. However, having only one parent born abroad leads to a significant reduction in the risk of unemployment. The findings imply that ethnicities that are generally poorly integrated in the first generation will also typically be poorly integrated in the second generation.

Several active labor market policies have specifically targeted or have had specific rules for immigrants (Eriksson, 2011). Among these policies, two of the most extensive are the New Start Jobs (*Nystartsjobb*) and Entry Jobs (*Instegsjobb*) programs.[2] New Start Jobs offers subsidized employment for newly arrived foreign-born and long-term unemployed individuals. A subsidy is given for a maximum of two years and covers approximately 50 percent of the wage cost (Joyce, 2017). Entry Jobs specifically targets newly arrived foreign-born individuals and provides a subsidy of up to 80 percent of the wage cost. This subsidy is also restricted to a maximum of two years. Evaluations have found that of these two policy programs, only New Start Jobs is associated with improved

[2] There is no reason to expect that these policies had different impacts within the treatment and control groups in this paper. Therefore, there is no reason to suspect that they led to biased estimates.

chances of regular employment (Joyce, 2017). The program has, however, also been shown to have crowding-out effects on ordinary employment. Moreover, it is possible that the time-limited nature of these programs makes them less efficient. If a subsidy is perceived as temporary and an immigrant's productivity does not correspond to the non-subsidized wage, employers may remain reluctant to offer employment.

3. THE SWEDISH PAYROLL TAX REFORM

The entirety of the Swedish payroll tax is levied on employers. It is proportional to employees' gross wages and thus is 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 (Swedish Tax Authority, 2019). The current standard payroll tax rate is 31.42 percent and consists of seven different fees, of which six finance social benefits such as pensions, parental leave and sick leave. The seventh fee, representing approximately one-third of the total payroll tax, is unrelated to the financing of social benefits. From a historical perspective, the payroll tax has typically been identical regardless of employees' 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).[3] The targeted age group included all individuals who had turned 18 years of age at the start of the year but were 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 further reducing 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). Parallel to these payroll tax reductions, the corresponding self-employment tax was reduced for self-employed individuals within the same age groups, from 30.71 percent to 20.45 percent (in 2007) and to 15.07 percent (in 2009).

The left-wing political 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 when they were elected into office in 2014. On June 1, 2016, the payroll tax cut for young individuals was completely abolished.

[3] To be precise, the payroll tax was reduced from 32.420 to 21.315 percent. Using two decimal places, the previous government stated that the new payroll tax level was 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 2008.

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

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

Daunfeldt et al. (2019) emphasized that it is savings in absolute (or monetary) terms that determines whether firms are able to hire additional employees, and they therefore used the absolute size of firm-level labor cost savings as their treatment intensity measure. They argued that using a relative measure, a small firm with a high share of young employees could be defined as receiving a higher treatment intensity than a large firm, although the latter firm received considerably larger labor cost savings in monetary terms. Their findings suggested a positive relationship between firm-level savings and firm-level

employment. In total, they concluded that the 2007 payroll tax cut generated 18,100 new jobs over the years 2006-2008, which implies that the net job creation was larger than what had been previously found by Egebark and Kaunitz (2013).

4. DATA AND DESCRIPTIVE STATISTICS

4.1. Data

All data are obtained from the individual-level database LISA (Longitudinal Integration Database for Health Insurance and Labour Market Studies), provided by Statistics Sweden. LISA is built entirely on register data and includes information on all Swedish residents who 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. The information in LISA is used 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 postsecondary 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 industry codes, industry-level differences regarding the effects of the payroll tax reduction on the hiring of non-western immigrants are investigated. Separate estimations are performed for firms active in the retail, hospitality, manufacturing and KIBS industries. See Table A1 (Appendix) for a detailed description of these industries.

To be included in the sample, firms need to exist and have at least one employee in each year during the period 2003-2008, and firms that have an industry code that is equal to zero are excluded from the analysis.[4] This means that firms that entered or exited the market during the study period are not included in the empirical analysis. Out of 744,032 firms that existed during the 2003-2008 period, 199,580 firms satisfy the conditions described above. To prevent firm

[4] Individuals who are hired by a municipality but have an unclear work status (e.g., participation in labor market policy programs) and individuals who receive labor income from a firm not registered in the Statistical Business Register (e.g., a firm operating abroad) are assigned industry code 0. Industry code 0 can also reflect no information on industry affiliation.

outliers from affecting the empirical analysis, firms with extreme annual employment growth are excluded; thus, an additional 1,136 firms are excluded, resulting in 198,444 firms in total.[5] Although restricting the sample in this way is necessary, it also implies that inferences 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 all individuals and their parents. Consequently, it is possible to identify both first- and second-generation immigrants and their regional area of migration.[6] All Swedish residents born in another European country (including all EU and non-EU countries), North America or Oceania are defined as western immigrants, while individuals 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 whose parents were both born abroad, I choose not to define them 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 who 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

[5] Outliers are defined as firms with an annual employment change of more than three standard deviations from the average growth. This implies that all firms that 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, and 11=unknown.

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), two employment definitions based on annual labor earnings are also used to evaluate whether the findings are sensitive to the definition of whom is employed. The first definition requires an individual to have annual labor earnings that exceed one income base amount[7] in addition to being registered as employed in the RAMS register, while the second definition uses two income base amounts as a threshold for being classified as employed. The results presented below rely on the RAMS employment definition, while estimations based on the income-based employment definitions are performed as a robustness check.

In contrast to employment status, which is measured during a measurement week in November, each individual's unemployment status is measured during the last week of November each year. This could result in some individuals being registered as both employed and unemployed in the dataset. Most of these individuals are likely to have a weak labor market position, and it is not possible to conclude with certainty that they are regularly employed. Consequently, they are excluded from the analysis during the years when they are simultaneously registered as employed and unemployed. Over the 2003-2008 period, there are 44,448,725 individual-year observations in total; 25,633,985 of these represent employed individuals, from which 864,630 individual-year observations are excluded.

4.2. Measuring treatment intensity

The 2007 payroll tax reform was not randomized, as it encompassed all firms that had employees within the targeted age group. Consequently, all firms could choose to take part in 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. For instance, self-assignment to treatment could reflect larger resources and thus better possibilities for recruitment. I define treated and

[7] An income base amount is, for instance, used to calculate pension-qualifying income. One income base amount ranges from 40,900 SEK (4,499 USD) to 51,100 SEK (5,621 USD) during the period of study.

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, each firm's total gross wages for young workers in 2006 is used 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 less affected by any self-selection mechanisms.[8] 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, which is shown by Daunfeldt et al. (2019) to be the case for the general firm population. 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, strictly increases with, the amount of labor cost savings. The 2006 $Treatment\ intensity_{i,t}$ of firm i at year t can be expressed as

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

Figures 0.3242 and 0.2132 represent the pre- and post-reform payroll tax levels, respectively. $Gross\ wages, young_{i,t=2006}$ represent the firm-level gross wages paid to 18- to 24-year-olds in 2006, i.e., for the age group about to be covered by the payroll tax cut.[9] Hence, the 2006 treatment intensity measure captures the size of the firm-level labor cost savings that young employees generate during the first post-reform year, provided that they remain employed at the same wage levels.

[8] A potential concern is that firms were aware of the reform in 2006 and altered their employment behavior pre-reform. In the pre-treatment years, the risk of self-selection decreases the further one moves from the reform year 2007. There is, however, a tradeoff involved since the relevancy (the correlation between the assumed and actual labor cost savings) of the treatment intensity measure is largest closest to the reform year.

[9] Specifically, information on firms (e.g., number of employees) is 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 built only 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, all firms receiving initial cost reductions ($Treatment\ intensity_{i,t=2006} > 0$) are divided 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 1 shows the expected labor cost savings for all treated firms, which – using the statistical matching further described in Section 5.1 below - 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 39,200 SEK (4,312 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).[10]

Table 1. Expected one-year labor cost savings by treatment intensity. All treated firms.

Treatment intensity	Min	Max	Mean	Median
>0-20 %	124	9,266	5,681	5,782
>20-40 %	9,278	17,864	13,408	13,318
>40-60 %	17,877	26,179	22,167	22,238
>60-80 %	26,191	39,163	31,379	30,750
>80-100 %	39,175	1,992,242	72,735	55,494

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

Table 2 includes the labor cost savings for firms in the different industry categories based on the general savings distribution of Table 1. This ensures that industry-level differences in immigrant employment are not driven by a variation in the average size of savings across industries. However, there are no upper boundaries on the labor cost savings within the 80-100 treatment intensity range, which is implied by differences in the maximum savings. Despite these differences in maximum savings, the mean and median savings within the industries are relatively similar, meaning that it is possible to analyze whether the link between labor cost savings and immigrant employment was especially strong in some industries. From Table 2, it is also evident that the largest savings was

[10] As of September 2018, 1 SEK is approximately equal to 0.11 USD.

obtained by a firm within the manufacturing industry. Finally, the within-industry shares of firms that belong to the upper >80-100 group vary considerably. Only 11 percent of all hospitality firms (109/1,037) have savings within the upper quintile. In contrast, 24 percent of manufacturing firms belong to this treatment intensity group (641/2,639).

Table 2. Expected one-year labor cost savings by treatment intensity. Treated retail, hospitality, manufacturing and KIBS firms follow the same savings distribution.

Treatment intensity	Min	Max	Mean	Median	# Firms
>0-20					
Retail	161	9,241	5,706	5,807	655
Hospitality	136	9,266	5,445	5,473	341
Manufacturing	642	9,266	5,792	5,868	418
KIBS	309	9,266	5,715	5,930	357
>20-40					
Retail	9,278	17,864	13,553	13,491	688
Hospitality	9,278	17,790	13,133	13,071	285
Manufacturing	9,315	17,864	13,385	13,120	455
KIBS	9,278	17,827	13,098	12,848	329
>40-60					
Retail	17,877	26,179	22,043	22,046	622
Hospitality	17,901	26,031	21,559	21,571	173
Manufacturing	17,938	26,179	22,524	22,880	519
KIBS	17,914	26,129	21,833	21,966	225
>60-80					
Retail	26,191	39,101	31,109	30,095	448
Hospitality	26,216	39,003	32,057	31,553	129
Manufacturing	26,203	39,151	30,734	29,934	606
KIBS	26,203	39,163	31,029	30,441	254
>80-100					
Retail	39,200	377,881	67,313	54,520	441
Hospitality	39,398	187,773	60,889	55,150	109
Manufacturing	39,237	512,679	76,869	60,808	641
KIBS	64,661	227,640	64,661	56,076	171

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

4.3. Descriptive statistics

Table 3 includes descriptive statistics for all treated firms, i.e., firms that received initial labor cost savings, and the matched control firms that did not receive any immediate labor cost savings when the payroll tax reform was implemented.[11] Among the 198,444 firms included in the empirical analysis, 43,019 had young employees in 2006 and were thus about to be treated. The statistical matching method (described in Section 5.1) requires the matched treated and control firms to have similar characteristics on a set of matching variables. In total, this leaves us with 26,599 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 with savings within the lower 20 percent of the savings distribution, whereas the >80-100 group includes firms within top 20 percent in terms of savings. Both the average and median firm size are larger among the treated firms than among the control firms. The average number of employees also increases with treatment intensity, implying that the size of labor cost savings and the average firm size are positively correlated. For instance, each firm within the upper >80-100 treatment intensity has, on average, approximately 16 employees, whereas the average firm within the >0-20 range has approximately 6 employees.

Notably, although the average firm size increases with treatment intensity, the average numbers of first- and second-generation non-western immigrants are more or less unrelated to a firm's labor cost savings and are very minor. Finally, the average share of 18- to 24-year-olds in 2006 – who are about to be targeted by the payroll tax cut – increases with treatment intensity, from 28.8 percent for the >0-20 group to 34.7 percent for the >80-100 group. Thus, approximately one-fourth to one-third of employees at the treated firms belong to the age group that the reform targeted.

[11] The corresponding descriptive statistics for the different industries are available upon request.

Table 3. 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	5.463	3	8.103	1	286	26,599
Dose >0-20 %	6.224	4	7.626	1	241	5,311
Dose >20-40 %	6.336	4	9.508	1	320	5,328
Dose >40-60 %	6.613	5	7.790	1	246	5,313
Dose >60-80 %	7.998	6	8.991	1	209	5,329
Dose >80-100 %	16.466	10	35.731	1	1,442	5,318
# Non-western imm. (1st gen)						
Control	0.088	0	0.372	0	7	26,599
Dose >0-20 %	0.121	0	0.538	0	15	5,311
Dose >20-40 %	0.107	0	0.443	0	6	5.328
Dose >40-60 %	0.093	0	0.423	0	9	5,313
Dose >60-80 %	0.074	0	0.365	0	10	5,329
Dose >80-100 %	0.102	0	0.454	0	11	5,318
# Non-western imm. (2nd gen)						
Control	0.003	0	0.058	0	3	26,599
Dose >0-20 %	0.012	0	0.130	0	3	5,311
Dose >20-40 %	0.011	0	0.117	0	3	5,328
Dose >40-60 %	0.013	0	0.129	0	3	5,313
Dose >60-80 %	0.011	0	0.112	0	3	5,329
Dose >80-100 %	0.020	0	0.153	0	2	5,318
Share of young						
Control	0	0	0	0	0	26,599
Dose >0-20 %	0.288	0.25	0.182	0.007	1	5,311
Dose >20-40 %	0.310	0.25	0.193	0.007	1	5,328
Dose >40-60 %	0.305	0.25	0.193	0.010	1	5,313
Dose >60-80 %	0.297	0.25	0.190	0.013	1	5,329
Dose >80-100 %	0.347	0.333	0.188	0.012	1	5,318

Notes. All firms with immediate labor cost savings (split into treatment intensity groups) vs. control firms with no savings. Firms surviving and having at least one employee per year across the time period 2003-2008 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.

5. METHOD

5.1. 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 without 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 that had hired young individuals at the time of the reform introduction are defined as treated. All firms that had not hired young employees are defined as non-treated and are thus included in the set of potential control firms.

CEM (Blackwell et al., 2009, Iacus et al., 2011; 2012) is used to identify representative control firms. The aim is to find a set of control firms that resemble the counterfactual outcome for the treated firms, indicating that the only important difference between the treated and control firms is the treatment assignment. That is, the treated and control firms would have been identical job providers for immigrants in the 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 $\boldsymbol{X} = (X_1, \dots, X_k)$ that affects both the treatment assignment and the outcome of interest (immigrant employment) is defined. Next, matching is used to locate treated and control firms that have similar values of $\boldsymbol{X}$. In CEM, each matching variable in $\boldsymbol{X}$ is treated separately. First, each variable is coarsened, meaning that similar values of the variable are treated as equal. The degree of coarsening can be set by splitting each covariate into intervals, or bins. For instance, a continuous variable can be coarsened into four equally sized quartiles, whereas a discrete variable ranging from 1 to 4 can be coarsened into two bins ([1-2] and [3-4]). Next, CEM creates strata, with each stratum having the same coarsened values on all matching variables in $\boldsymbol{X}$. To clarify, consider three matching variables, which are all coarsened into four bins. The total number of strata is $4^3 = 64$. All strata that contain at least one treated and one control unit are retained. Finally, treated and control units are matched within each stratum.

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 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 method is different from other matching methods, e.g., propensity score matching, in which improved balance in one covariate might lead to substantial increases in 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 choices of estimation model and model specification (Ho et al., 2007, Iacus et al., 2011). The main advantage of CEM is that one can exactly set the maximum imbalance to be allowed between treatment and control units and 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 carried out first jointly for all firms and then separately for the different industry categories. First, a treatment indicator that is 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. For industry-specific matching, the control firms are also required to belong to the same industry. This is important since it implies that the treated and control firms face the same industry-level labor supply of immigrants. The following firm-level variables are then coarsened and matched using CEM: (i) the trends in each firm's number of western and non-western immigrants during 2003-2006; (ii) the share of employees with no postsecondary education in 2006; and (iii) the shares of western and non-western immigrants in 2006. The reasons for using these particular matching variables are as follows. The matching variables measuring the pre-reform composition of western and non-western immigrants, in terms of both trends and shares, are important since the control firms should be equally frequent employers of immigrants as the treated firms prior to the reform introduction.[12] Moreover, since a large number of immigrants in Sweden

[12] I do not match on the corresponding trends in the total number of employees. The reason is the positive correlation between average firm size and treatment intensity. A high treatment intensity implies a large firm size, which in turn (likely) implies large employment growth in absolute terms. Forcing the treated and control firms to have similar employment growth in the pre-treatment period would therefore exclude firms with large savings. Firm size differences are instead handled by the empirical model.

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. Using 1:1 matching 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. Lastly, the coarsening of each matching variable is as follows. The 2003-2006 trends in the number of western and non-western immigrants are coarsened into bins of approximately +/- two employees. The share without postsecondary education is coarsened into bins of approximately two percent, whereas the corresponding coarsening for the share of western and non-western immigrants is approximately one percent.

Table 4 presents statistics for the distributional differences of the matching variables when the matching includes all industries. In total, 26,599 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 similar not only in terms of means but also across the entire distribution of each matching variable. Consequently, the CEM procedure identifies matched treated and control firms with similar pre-treatment distributions of the matching variables.

Table 4. 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	26,599
Control	-9	-1	0	0	0	1	6	0.003	26,599
Change in # Non-west. imm., 2003-2006									
Treated	-4	0	0	0	0	0	6	-0.003	26,599
Control	-4	0	0	0	0	0	5	0.015	26,599
Share without postsecondary education									
Treated	0	0.333	0.737	1	1	1	1	0.835	26,599
Control	0	0.333	0.737	1	1	1	1	0.835	26,599
Share western imm.									
Treated	0	0	0	0	0	0.3	1	0.048	26,599
Control	0	0	0	0	0	0.3	1	0.048	26,599
Share non-western imm.									
Treated	0	0	0	0	0	0.111	1	0.019	26,599
Control	0	0	0	0	0	0.111	1	0.019	26,599

Notes. Similarity of the distributions of each matching variable for treated and control firms. CEM matching. Firms surviving and having at least one employee per year across the time period 2003-2008 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.

Next, I turn to the industry-specific matching. For brevity, the matching results are presented in the Appendix (Table A2-A5). Importantly, the industry-specific matching procedures generate control firms that have similar characteristics in all underlying matching variables. In total, CEM finds matches for 2,854 retail firms, 1,037 hospitality firms, 2,639 manufacturing firms and 1,336 KIBS firms.

5.2. Empirical model

The empirical analysis relies upon an absolute treatment intensity measure, implying that the size of labor cost savings strictly increases with treatment intensity. This choice is motivated by the fact that the absolute size of savings is likely to be the main determinant of whether a firm will be able to recruit additional employees (Daunfeldt et al., 2019). However, average firm size and treatment intensity are positively correlated. This may pose a problem since previous studies have shown that large firms generally have larger absolute employment growth than small firms (Henrekson and Johansson, 2010). Treated

firms might thus grow more than control firms because of their initially larger size rather than solely because of the payroll tax cut.

The identifying assumption of the empirical analysis is that the treated and control firms would have had identical immigrant employment trends if the reform had not been implemented, i.e., that the control firms constitute the counterfactual outcome for the treated firms. The statistical matching performed in the previous section evidently generated control firms with similar pre-treatment characteristics on the matching variables. However, the selected matching variables may not be the only confounding variables, i.e., the only variables that affect both the treatment assignment and the outcome, suggesting that there still might exist underlying differences that bias the effect of firm-level labor cost savings on immigrant employment.

Therefore, this study utilizes a DDD model (Chetty et al., 2009; Daunfeldt et al., 2019; Gruber, 1994). Unlike an ordinary difference-in-difference (DiD) model, which relies 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.[13]

The implemented DDD model estimates the difference between two DiD models estimated over the periods 2003-2005 and 2006-2008, respectively. The data creation for the 2003-2005 period is identical to that of the 2006-2008 period; a (placebo) treatment intensity is calculated in 2003, based on which the firms are assigned to treatment and control groups. 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 were identical, the DDD and DiD models would generate identical point estimates.[14]

[13] It should be clarified that not all firms in the 2006-2008 period are included in the 2003-2005 period. The treated and control firms are defined in 2006 and are required to have similar pre-treatment characteristics over 2003-2005. It is thus important to exclude firms for which the treatment statuses in 2003-2005 and 2006-2008 differ, e.g., treated in 2006 and (placebo) control in 2003. Consequently, placebo treated firms over 2003-2005 must also be treated over 2006-2008 (same for placebo control). In total, this results in 58,566 firms included over 2006-2008 and 40,063 firms included over 2003-2005.

[14] For clarification, the DDD estimates of Figure 1 (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 1. For instance, the DiD estimates for the >0-20

Hence, under the assumption that employment growth differences between treated and control firms during 2003-2005 correspond to differences in employment growth that would have occurred if the reform had not been implemented, the DDD model isolates the treatment effect from other factors that might have affected the immigrant employment growth in treated and control firms differently.

The DDD model can be expressed as

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

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 (non-western) employees in firm *i*, belonging to group *j*, at time *t*. $Time_t$ is a dummy variable that takes a value of zero for pre-treatment years in both the reform period 2006-2008 and the underlying time period 2003-2005 and a value of one for the corresponding post-treatment years. It is thus equal to zero for 2003 and 2006 and equal to one for 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 (used to account for differences in pre-treatment employment trends) and 2006-2008 (the actual reform period) 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 2007-2008.[15] By deducting employment growth differences between treated and control firms in the underlying period 2003-2005, its parameter δ_{DDD} separates the effect of the payroll tax cut on the employment of non-western immigrants from other factors that could explain differences in immigrant employment. Consequently, the

group in 2006-2008 and 2003-2005 are 0.0109 and -0.00491, respectively. Together, they generate the DDD estimate 0.01581=0.0109-(-0.00491).

[15] Note that 2007 and 2008 are both considered post-treatment years. Thus, the DDD model estimates the average change over 2006-2008 (in relation to the control group and the underlying time period 2003-2005) by calculating joint conditional means over 2007-2008.

potential bias caused by the positive correlation between treatment intensity and average firm size is removed. [16] 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 had no young employees at the reform introduction and thus obtained no initial cost savings. [17]

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 was implemented, all firms had the opportunity to take part in the tax cut by employing young individuals and thus to self-select into treatment. This 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 exogeneous to post-introduction employment. 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 global financial crisis that reached Sweden in 2009 affected the absolute employment growth of treated and control firms differently due to, for instance, their initial differences in size and their different industry belonging.

[16] Analyzing differences in average firm size in year 2003 among all firms (irrespective of industry), it is apparent that that there also exists a positive correlation between average firm size and (placebo) treatment intensity in the underlying time period 2003-2005. Descriptive statistics for 2003 are available upon request.

[17] A potential concern could be that control firms without young employees are also less likely to hire immigrants than treated firms. This could induce bias between treated and control firms and cause the estimates over 2006-2008 to be overestimated if a regular DiD model was used. However, assuming that the DDD model accounts for underlying differences between treated and control firms, this does not constitute a problem.

6. 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, received a labor income corresponding to at least one work hour are defined as employed. However, the more restrictive income-based employment definitions generally yield similar findings, implying that the results are robust to the choice of employment definition.[18]

The results presented below are based on within-firm estimation, which means that any firm-specific heterogeneity that is time invariant is accounted for.[19] The Appendix includes alternative specifications that, additionally, control for fixed effects at both the industry and municipal levels (see Table A6-A8). Accounting for such heterogeneity does not considerably alter the findings. 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. Finally, I assess whether the findings of Figure 1-2 and Figure A2-A3 are robust to the exclusion of young self-employed individuals, who were eligible for a reduced self-employment tax instead of a reduced payroll tax. Specifically, I do this by imposing the restriction of at least two employees in each year. [20]

[18] However, as expected, the average employment of individuals earning at least two income base amounts per year is in most cases somewhat smaller in magnitude than the employment of those earning at least one income base amount. These findings are available upon request.

[19] For the employment of first-generation non-western immigrants, regression tables including other specifications are in the Appendix (see Table A6-A8). From these tables, it is evident that the differences between using fixed effects and using random effects are negligible. The tables including all variables as well as regression tables for second-generation non-western immigrants are available upon request.

[20] For Figure 1, this leads to only minor differences. The point estimates in Figure 2 lose some statistical significance, but the differences are minor except for hospitality firms, for which the magnitude of the point estimate is approximately halved. In Figure A2, the point estimates remain close to zero and statistically insignificant. In Figure A3, there are only minor differences. These robustness checks are available upon request.

6.1. Immigrant employment effect of reduced labor costs among all firms

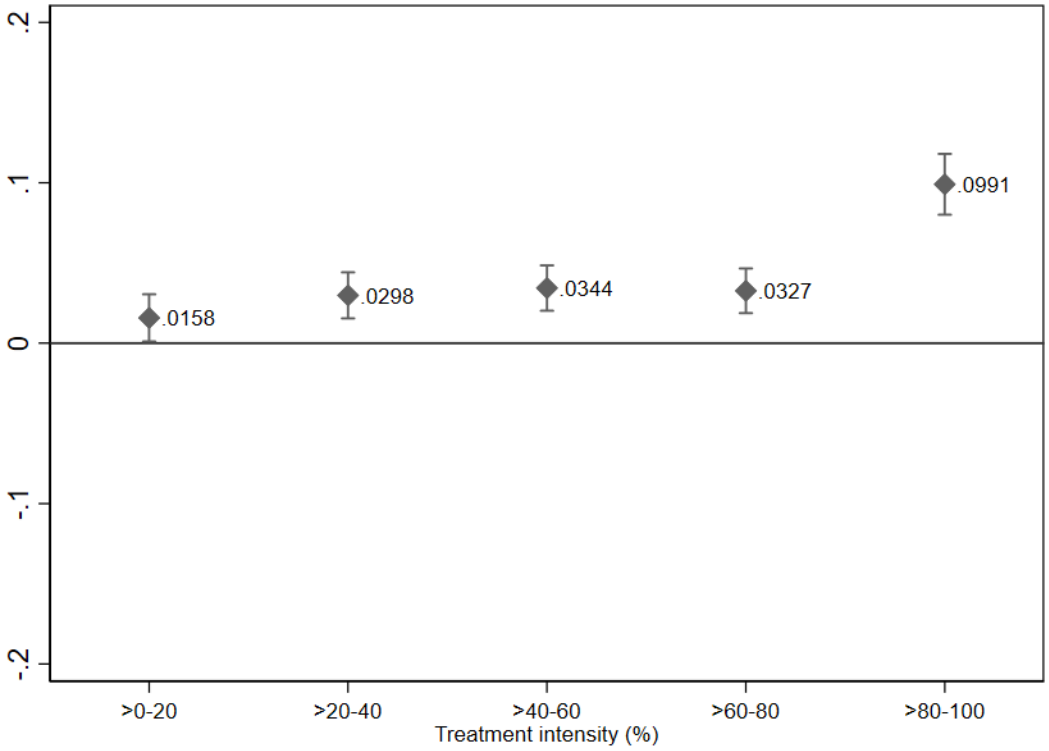
The estimated effects of the generated firm-level labor cost reductions on the hiring of first-generation non-western immigrants are presented in Figure 1. Around each point estimate, a 95 percent confidence interval is included. Thus, an estimate is statistically significant at the 5 percent level if its confidence interval does not cross the x-axis at value zero. The results in Figure 1 show that treated firms on average hired more first-generation non-western immigrants than control firms after the payroll tax cut, but the magnitude of the effect is small for firms that received modest labor cost 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 an initial one-year labor cost savings of at least 39,200 SEK (4,312 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 that 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 an additional 0.10 non-western immigrants due to the youth payroll tax cut may appear to be a small effect, but recall that each firm within this treatment intensity range had on average 0.102 employees of non-western origin prior to the reform (see Table 3), which suggests 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 1,100 jobs for first-generation non-western immigrants in total.[21]

[21] This number is calculated by multiplying the point estimates of Figure 1 by the number of firms within the corresponding treatment intensity groups (see Table 3). That is, 0.0158*5,311+0.0298*5,328+0.0344*5,313+0.0327*5,329+0.0991*5,318=1,127 jobs.

Figure 1. 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 firms surviving and having 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. Point estimates with 95 % confidence intervals.

It might be the case that the positive effect of the payroll tax cut on the hiring of first-generation non-western immigrants was driven by young immigrants, i.e., who were directly targeted by the reform, which suggests that it is age and not region of birth that explains the positive effects presented in Figure 1. This could be the case if the increased employment was primarily caused by a substitution effect that incentivized 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 (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 >0-60 treatment intensity range is solely explained by the hiring of immigrants who were outside the targeted age group and who were thus not targeted by the payroll tax cut. However, for firms within the >60-100 treatment intensity range, the results in Table 5 indicate that the

labor cost savings were primarily, but not solely, used to hire first-generation immigrants outside the targeted age group. Within the >60-80 range, the employment effect for 19-25-year-olds is positive and statistically significant at the 10 percent level. For firms with the top 20 percent savings, the point estimates also suggest employment increases for individuals below, and particularly those within, the reform's targeted age group.

Overall, the findings imply that the payroll tax reform had the unintended effect of promoting the employment of first-generation non-western immigrants who were not explicitly targeted by the reform. This suggests that the increased employment of non-western immigrants was primarily explained by a scale effect, which lowered firms' marginal cost of production and incentivized increased production.

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	-0.0009	0.0027**	0.0002	0.0032*
	(0.0018)	(0.0011)	(0.0013)	(0.0014)	(0.0019)
19-25 years old	-0.0047	-0.0003	0.0014	0.0082*	0.0362***
	(0.0044)	(0.0041)	(0.0042)	(0.0046)	(0.0063)
Min 26 years old	0.0194***	0.0310***	0.0302***	0.0243***	0.0597***
	(0.0060)	(0.0059)	(0.0059)	(0.0054)	(0.0072)

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 firms surviving and having 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.

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

6.2. Industry-level differences

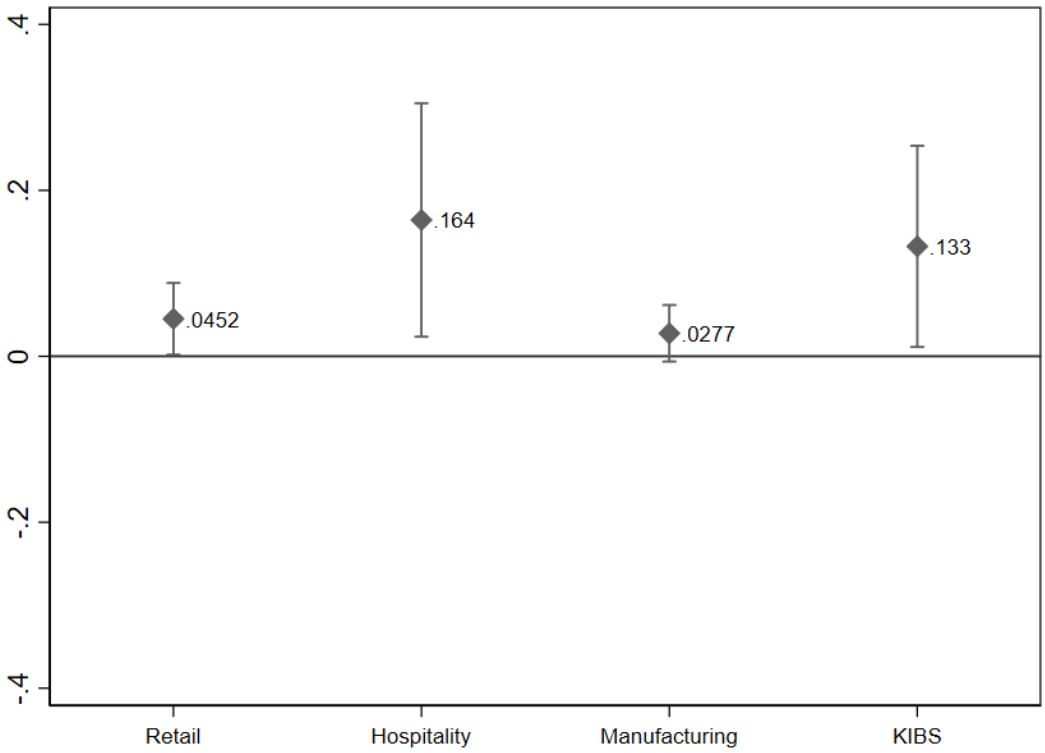
This section analyzes the effects of the firm-level labor cost savings generated by the youth payroll tax cut on the recruitment of first-generation non-western immigrants within the retail, hospitality, manufacturing and KIBS industries. Firms within these industries provide jobs that are characterized by large differences in educational and skill requirements. Recall from Section 4.2 that firms are required to follow the same savings distribution, meaning that industry-level differences are not due to differences in average savings. Instead, they reflect how likely firms within the different industries are to utilize their savings in employing immigrants. [22]

Figure 2 presents the estimated employment effect for non-western immigrants among firms within the upper >80-100 treatment intensity range.[23] The point estimates for the retail, hospitality and KIBS industries are all positive and statistically significant. Based solely on the magnitude of the point estimates, these findings suggest that the immigrant employment effect is considerably smaller within the retail industry. However, the confidence intervals overlap, implying that the estimates are not significantly different from one another. For the manufacturing industry, no statistically significant effect on the employment of first-generation non-western immigrants is found. Considering the positive point estimate for the KIBS industries, an interesting conclusion from Figure 2 is that the positive immigrant employment effect is not limited to industries that generally provide low-skilled jobs.

[22] The maximum savings within the different industries differ, although they follow the same labor cost-saving distribution. I therefore evaluate whether the findings in Figure 2 are robust to the imposition of an upper limit on the savings, which is done by excluding savings above the 99th percentile of the general distribution (the top one percent savings). The estimates for retail and hospitality firms remain similar in magnitude, and their statistical significance is unaltered. For KIBS firms, the estimate becomes smaller and loses its statistical significance (p-value=0.118). From this robustness check, I conclude that the employment effects within the retail and hospitality industries are not driven by firms with the top one percent savings. Within the KIBS sector, the positive employment effect appears to explained by firms with the largest savings. These findings are available upon request.

[23] Corresponding findings for the other treatment intensities are in the Appendix (see the second column of Tables A7-A8). The only point estimate with a p-value below 0.05 is for retail firms within the 60-80 range, and it is equal to 0.041.

Figure 2. 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 firms surviving and having 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. Point estimates with 95 % confidence intervals.

Figure A3 in the Appendix includes the corresponding estimates for second-generation non-western immigrants. The point estimates for all industries are statistically insignificant, suggesting that firms that received large labor cost savings due to the payroll tax reform did not use the savings to hire additional second-generation immigrants in any of the industries under study. [24]

[24] For the other treatment intensities, I only find one statistically significant (10-percent level) point estimate of size -0.0316 for hospitality firms within the >40-60 range. See Table A9 (Appendix).

7. CONCLUSIONS

This paper has investigated whether a Swedish youth payroll tax reform that 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 and general labor cost savings for firms with many young employees. The savings gave rise to both a substitution effect and a scale effect. The substitution effect suggests an increased recruitment of young individuals as they became less costly to hire. However, the scale effect, which implicitly incentivized increased production by reducing firms' marginal cost, may have resulted in firms hiring individuals outside the reform's targeted age group. There are theoretical arguments regarding why this scale effect may have particularly benefitted immigrants' employment opportunities. For instance, it may have lowered the barriers against immigrant employment by making firms less risk averse in their employment decisions, especially in cases where youths and immigrants were considered substitutes. Non-western immigrants might also have been overrepresented in the pool of unemployed individuals with marginal productivity just below the minimum wage level, meaning that they were more likely to become employed when firms obtained larger financial resources and wished to expand.

Three main results emerge from the empirical analyses. First, there is a positive and relatively strong link between the size of firms' savings and firms' subsequent recruitment of first-generation non-western immigrants. The average employment effect is more than six times larger among firms with the largest savings than among 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 of non-western immigrants is to a large extent explained by a scale effect rather than simply by a substitution effect. Third, industry-level analyses suggest statistically significant employment increases within the retail, hospitality and KIBS industries, i.e., industries characterized by large differences in educational and skill requirements. The positive effect found within the KIBS industries implies that the general labor cost reductions did not promote immigrant employment

solely within industries that provide low-qualified jobs. One potential explanation is that labor cost savings induced better job matches between employers and immigrants. For instance, highly educated immigrants who were previously overqualified for their jobs were perhaps considered more employable within the KIBS industries once these firms were provided with greater monetary resources and became less risk averse.

The immigrant employment effect of the 2007 youth payroll tax cut is also of economic significance. The findings suggest that the labor cost savings created approximately 1,100 jobs for first-generation non-western immigrants within the sample of matched firms over the time period 2006-2008. A previous study by Daunfeldt et al. (2019) concluded that the reform created 18,100 new jobs in total. Considering that approximately five percent of the Swedish population in 2006 was 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 provided for young, targeted individuals. This finding, combined with the fact that the employment effects of this paper are mainly driven by an increased recruitment of older non-western immigrants, may suggest that even if these groups are generally considered substitutes, the latter group might benefit from a limited youth labor supply. Regardless, it is evident that the youth payroll tax reform had the unintended effect of promoting the recruitment of individuals outside its target group.

An extensively used labor market policy to improve immigrants' labor market situation has been to offer firms subsidized employment of foreign-born individuals. The Swedish government has, for instance, offered subsidies covering up to 80 percent of the wage cost. However, such subsidies have been offered for only a short period of time, meaning that employers might remain reluctant to hire immigrants. This type of selective policy may also crowd out other groups from the labor market and displace regular employment. It has also been shown that targeted employment subsidies may be stigmatizing for eligible groups and that more general subsidies are likely to prove more efficient (Katz, 1996; Neumark, 2013). The firm-level labor cost savings that were created by the youth payroll tax reform were not limited to a certain group of individuals or

restricted to a certain time period, which means that they were similar to general employment subsidies. Thus, the findings of this paper suggest that to improve the labor market opportunities for marginalized individuals, policy makers should implement reforms that reduce the total labor costs for firms. This is especially urgent considering the recent wave of refugees that has arrived in European countries.

This study is not without limitations. First, the empirical analysis is based on a sample of firms that survived over the years 2003-2008. Thus, inferences should be made only 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 this may be at the expense of 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, the empirical analysis captures the recruitment of all non-western immigrants, irrespective of their previous employment status. It is possible that the labor cost savings promoted not only the recruitment of unemployed immigrants but also the matching and job mobility of immigrants who had already entered employment. To analyze this in greater detail, future research could decompose the employment effects based on immigrants' work history and employment status. This would be of interest considering that immigrants are generally overrepresented in part-time work and are more likely to be overqualified for their work tasks.

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APPENDIX

Table A1. 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 A2. 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	0	2	-0.008	2,854
Control	-2	0	0	0	0	0	2	0.006	2,854
Change in # Non-west. imm., 2003-2006									
Treated	-3	0	0	0	0	0	2	-0.010	2,854
Control	-2	0	0	0	0	0	2	0.001	2,854
Share without postsecondary education									
Treated	0	0.5	0.75	1	1	1	1	0.879	2,854
Control	0	0.5	0.75	1	1	1	1	0.879	2,854
Share western imm.									
Treated	0	0	0	0	0	0.25	1	0.031	2,854
Control	0	0	0	0	0	0.25	1	0.031	2,854
Share non-western imm.									
Treated	0	0	0	0	0	0.111	1	0.021	2,854
Control	0	0	0	0	0	0.111	1	0.021	2,854

Notes. Similarity of the distributions of each matching variable for treated retail firms and non-retail firms. CEM matching. Firms surviving, having at least one employee per year and operating within the same two-digit industry across the time period 2003-2008 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.

Table A3. 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	-3	-1	0	0	0	1	6	0.009	1,037
Control	-3	-1	0	0	0	1	6	0.048	1,037
Change in # Non-west. imm., 2003-2006									
Treated	-4	-1	0	0	0	1	4	0.028	1,037
Control	-4	-1	0	0	0	1	4	0.051	1,037
Share without postsecondary education									
Treated	0	0.5	0.833	1	1	1	1	0.907	1,037
Control	0	0.5	0.833	1	1	1	1	0.907	1,037
Share western imm.									
Treated	0	0	0	0	0.2	0.833	1	0.144	1,037
Control	0	0	0	0	0.2	0.833	1	0.144	1,037
Share non-western imm.									
Treated	0	0	0	0	0	0.833	1	0.105	1,037
Control	0	0	0	0	0	0.833	1	0.105	1,037

Notes. Similarity of the distributions of each matching variable for treated H&R firms and non-H&R firms. CEM matching. Firms surviving, having at least one employee per year and operating within the same two-digit industry across the time period 2003-2008 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.

Table A4. 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	0.019	2,639
Control	-3	-1	0	0	0	1	3	-0.003	2,639
Change in # Non-west. imm., 2003-2006									
Treated	-3	0	0	0	0	0	1	0.009	2,639
Control	-3	0	0	0	0	0	1	-0.002	2,639
Share without postsecondary education									
Treated	0	0.5	0.818	1	1	1	1	0.886	2,639
Control	0	0.5	0.818	1	1	1	1	0.886	2,639
Share western imm.									
Treated	0	0	0	0	0.053	0.25	1	0.050	2,639
Control	0	0	0	0	0.053	0.25	1	0.050	2,639
Share non-western imm.									
Treated	0	0	0	0	0	0.071	1	0.010	2,639
Control	0	0	0	0	0	0.071	1	0.010	2,639

Notes. Similarity of the distributions of each matching variable for treated manufacturing firms and non-manufacturing firms. CEM matching. Firms surviving, having at least one employee per year and operating within the same two-digit industry across the time period 2003-2008 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.

Table A5. 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	3	0.033	1,336
Control	-2	-1	0	0	0	1	2	0.023	1,336
Change in # Non-west. imm., 2003-2006									
Treated	-1	0	0	0	0	0	2	0.011	1,336
Control	-1	0	0	0	0	0	2	0.016	1,336
Share without postsecondary education									
Treated	0	0	0.25	0.5	0.75	1	1	0.497	1,336
Control	0	0	0.25	0.5	0.75	1	1	0.497	1,336
Share western imm.									
Treated	0	0	0	0	0	0.25	1	0.035	1,336
Control	0	0	0	0	0	0.25	1	0.035	1,336
Share non-western imm.									
Treated	0	0	0	0	0	0.063	1	0.011	1,336
Control	0	0	0	0	0	0.067	1	0.011	1,336

Notes. Similarity of the distributions of each matching variable for treated KIBS firms and non-KIBS firms. CEM matching. Firms surviving, having at least one employee per year and operating within the same two-digit industry across the time period 2003-2008 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.

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

Specification: **Tr. intensity**	**1**	**2**	**3**	**4**
0-20	0.0158	0.0158**	0.0158**	0.0162**
	(0.0126)	(0.00751)	(0.00751)	(0.00755)
Observations	169,287	169,287	169,287	169,287
R-squared	0.004	0.003	.	.
20-40	0.0298**	0.0298***	0.0298***	0.0298***
	(0.0122)	(0.00732)	(0.00732)	(0.00734)
Observations	169,299	169,299	169,299	169,299
R-squared	0.004	0.004	.	.
40-60	0.0344***	0.0344***	0.0344***	0.0338***
	(0.0122)	(0.00722)	(0.00722)	(0.00725)
Observations	169,287	169,287	169,287	169,287
R-squared	0.003	0.004	.	.
60-80	0.0327***	0.0327***	0.0327***	0.0322***
	(0.0118)	(0.00710)	(0.00710)	(0.00711)
Observations	169,320	169,320	169,320	169,320
R-squared	0.003	0.004	.	.
80-100	0.0991***	0.0991***	0.0991***	0.0987***
	(0.0129)	(0.00969)	(0.00969)	(0.00969)
Observations	169,299	169,299	169,299	169,299
R-squared	0.008	0.007	.	.
Firm clustered s.e's	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	**1**	**2**	**3**	**4**	**HOSPITALITY**	**1**	**2**	**3**	**4**
Tr. intensity					**Tr. intensity**				
0-20	0.0241	0.0241	0.0241	0.0238	**0-20**	0.0946	0.0946*	0.0946*	0.0964*
	(0.0279)	(0.0152)	(0.0152)	(0.0154)		(0.124)	(0.0500)	(0.0500)	(0.0507)
Observations	18,483	18,483	18,483	18,483	Observations	6,834	6,834	6,834	6,834
R-squared	0.005	0.002	.	.	R-squared	0.008	0.015	.	.
20-40	0.0144	0.0144	0.0144	0.0136	**20-40**	-0.0108	-0.0108	-0.0108	-0.00571
	(0.0258)	(0.0141)	(0.0141)	(0.0142)		(0.117)	(0.0583)	(0.0583)	(0.0588)
Observations	18,582	18,582	18,582	18,582	Observations	6,627	6,627	6,627	6,627
R-squared	0.002	0.002	.	.	R-squared	0.009	0.018	.	.
40-60	-0.00590	-0.00590	-0.00590	-0.00552	**40-60**	0.0750	0.0750	0.0750	0.0788
	(0.0276)	(0.0166)	(0.0166)	(0.0167)		(0.147)	(0.0660)	(0.0661)	(0.0671)
Observations	18,261	18,261	18,261	18,261	Observations	6,072	6,072	6,072	6,072
R-squared	0.002	0.001	.	.	R-squared	0.006	0.016	.	.
60-80	0.0408	0.0408**	0.0408**	0.0388*	**60-80**	0.0940	0.0940	0.0940	0.0928
	(0.0286)	(0.0202)	(0.0202)	(0.0205)		(0.156)	(0.0706)	(0.0706)	(0.0704)
Observations	17,628	17,628	17,628	17,628	Observations	5,904	5,904	5,904	5,904
R-squared	0.002	0.003	.	.	R-squared	0.005	0.017	.	.
80-100	0.0452	0.0452**	0.0452**	0.0440**	**80-100**	0.164	0.164**	0.164**	0.164**
	(0.0300)	(0.0220)	(0.0220)	(0.0223)		(0.159)	(0.0717)	(0.0717)	(0.0731)
Observations	17,574	17,574	17,574	17,574	Observations	5,814	5,814	5,814	5,814
R-squared	0.004	0.006	.	.	R-squared	0.005	0.018	.	.
Firm clustered s.e's	No	Yes	Yes	Yes	Firm clustered s.e's	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	**1**	**2**	**3**	**4**	**KIBS**	**1**	**2**	**3**	**4**
Tr. intensity					**Tr. intensity**				
0-20	-0.0126	-0.0126	-0.0126	-0.0124	**0-20**	-0.0181	-0.0181	-0.0181	-0.0171
	(0.0321)	(0.0176)	(0.0176)	(0.0179)		(0.0395)	(0.0314)	(0.0314)	(0.0319)
Observations	15,999	15,999	15,999	15,999	Observations	8,844	8,844	8,844	8,844
R-squared	0.002	0.003	.	.	R-squared	0.009	0.014	.	.
20-40	-0.0376	-0.0376*	-0.0376*	-0.0388*	**20-40**	0.0246	0.0246	0.0246	0.0247
	(0.0318)	(0.0223)	(0.0223)	(0.0225)		(0.0480)	(0.0298)	(0.0298)	(0.0303)
Observations	16,173	16,173	16,173	16,173	Observations	8,607	8,607	8,607	8,607
R-squared	0.002	0.004	.	.	R-squared	0.010	0.014	.	.
40-60	0.00657	0.00657	0.00657	0.00779	**40-60**	0.0466	0.0466	0.0466	0.0485
	(0.0271)	(0.0143)	(0.0143)	(0.0147)		(0.0451)	(0.0343)	(0.0343)	(0.0347)
Observations	16,623	16,623	16,623	16,623	Observations	8,346	8,346	8,346	8,346
R-squared	0.002	0.003	.	.	R-squared	0.012	0.017	.	.
60-80	0.00186	0.00186	0.00186	0.00113	**60-80**	-0.00290	-0.00290	-0.00290	-0.00163
	(0.0287)	(0.0190)	(0.0190)	(0.0193)		(0.0464)	(0.0417)	(0.0417)	(0.0423)
Observations	16,821	16,821	16,821	16,821	Observations	8,463	8,463	8,463	8,463
R-squared	0.003	0.003	.	.	R-squared	0.015	0.017	.	.
80-100	0.0277	0.0277	0.0277	0.0277	**80-100**	0.133**	0.133**	0.133**	0.133**
	(0.0284)	(0.0174)	(0.0174)	(0.0176)		(0.0563)	(0.0618)	(0.0618)	(0.0630)
Observations	16,995	16,995	16,995	16,995	Observations	8,091	8,091	8,091	8,091
R-squared	0.005	0.007	.	.	R-squared	0.020	0.020	.	.
Firm clustered s.e's	No	Yes	Yes	Yes	Firm clustered s.e's	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

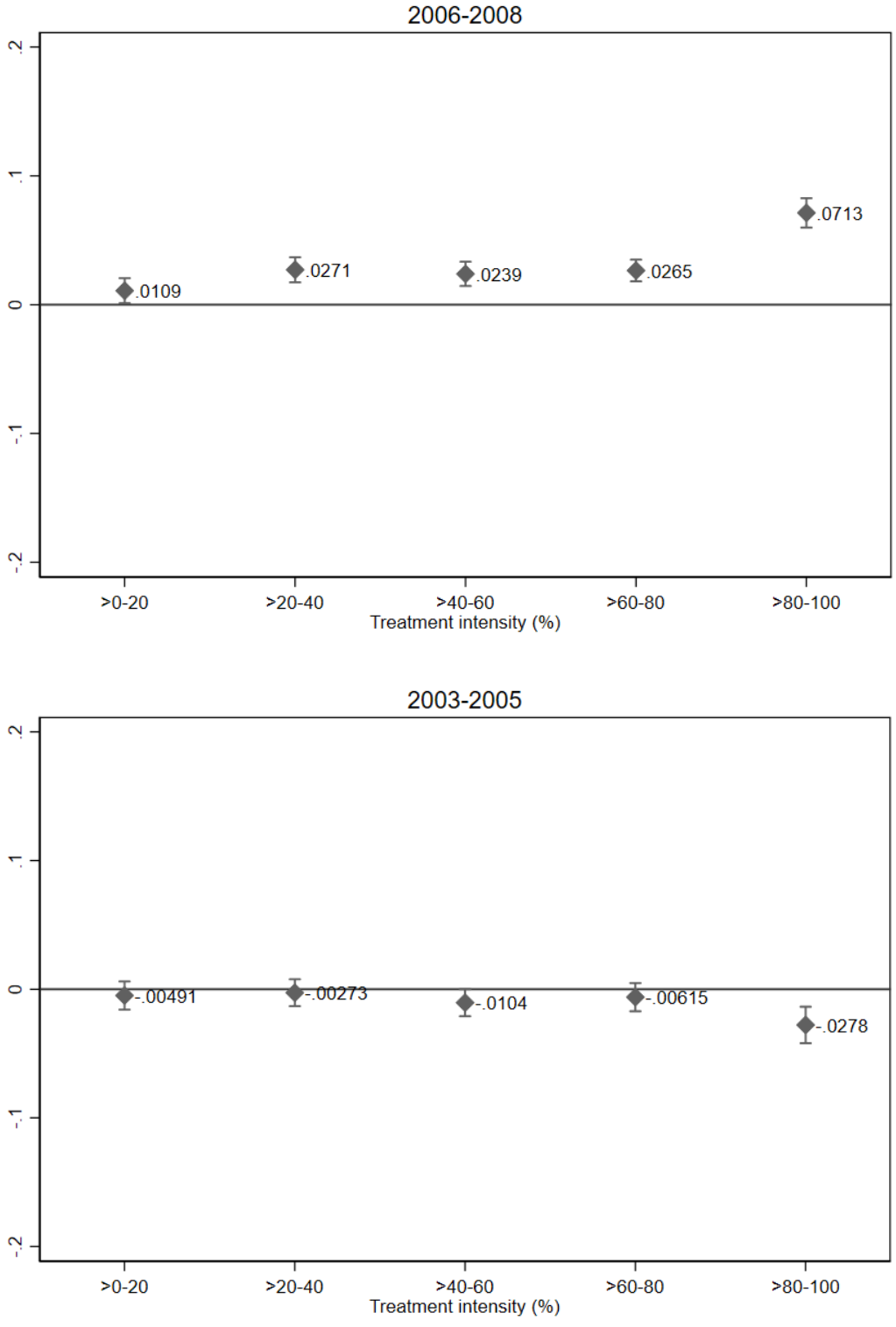
Notes. Dependent variable: Number of first-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Only surviving firms with at least one employee per year are included. Outliers – defined as firms with an annual employment change of more than three standard 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	0.0035	-0.0172	0.0033	0.0082
	(0.0042)	(0.0118)	(0.0057)	(0.0075)
>20-40	-0.0018	0.0150	-0.0013	0.0045
	(0.0053)	(0.0168)	(0.0036)	(0.0067)
>40-60	-0.0047	-0.0316*	0.0026	-0.0134
	(0.0051)	(0.0171)	(0.0025)	(0.0122)
>60-80	-0.0022	-0.0054	-0.0013	0.0031
	(0.0096)	(0.0171)	(0.0015)	(0.0172)
>80-100	-0.0018	0.0332	-0.0043	0.0084
	(0.0098)	(0.0353)	(0.0030)	(0.0136)

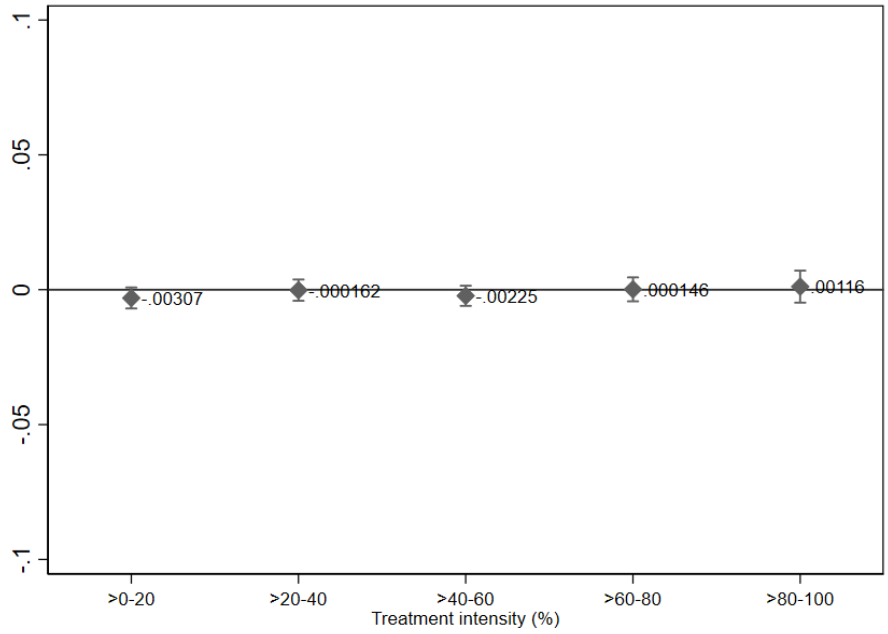
Notes. Dependent variable: Number of second-generation non-western immigrants. Treatment period: 2006-2008. Underlying time period: 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers – defined as firms with an annual employment change of 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 1 decomposed into separate DiD estimates over time periods 2003-2005 and 2006-2008.



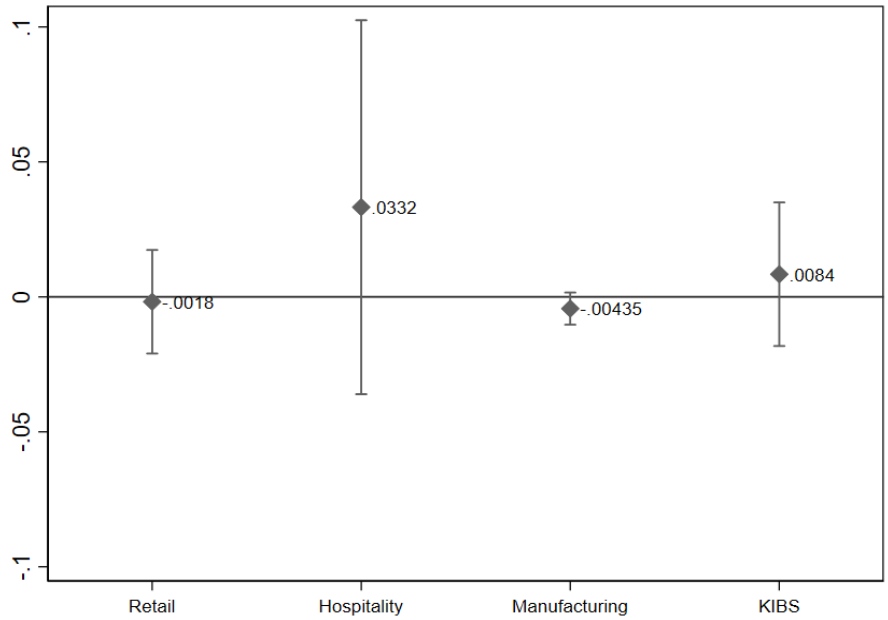
Notes. Dependent variable: Number of first-generation non-western immigrants. DiD estimation over treatment period 2006-2008 and underlying time period 2003-2005. Within-firm estimation. Only surviving firms with at least one employee per year are included. Outliers – defined as firms with an annual employment change of more than three standard deviations (+/- 88 employees) from the average growth – are excluded. Firm clustered standard errors. Point estimates with 95 % confidence intervals.

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. Point estimates with 95 % confidence intervals.

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. Point estimates with 95 % confidence intervals.