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DO REDUCED LABOR COSTS INCREASE EMPLOYMENT AMONG MINIMUM WAGE WORKERS? EVIDENCE FROM A SWEDISH PAYROLL TAX CUT

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We use a youth payroll tax cut in Sweden to investigate whether retail firms that were exposed to substantial labor cost savings increased employment of minimum wage workers more than firms that received smaller labor cost savings. Our dataset includes information on both contracted wages and working hours for most employees in the Swedish retail trade industry. The fact that a large portion of retail employees had contracted wages near the negotiated minimum wage levels at the time of the reform suggests that the minimum wage levels were binding to a great extent. We also find that retail firms with large labor cost savings due to the youth payroll tax cut significantly increased both the number of minimum wage hourly employees *and* their working hours. We observe no such effects for employees with long-term contracts or wages well above the negotiated minimum wages. This suggests that the relatively high minimum wage levels of the Swedish retail industry prevent the employment of workers who are perceived to have low productivity.

Keywords: Retail trade industry, minimum wages, payroll tax reform, natural experiments, collective bargaining

JEL classification: D24, L25, L26.

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1. INTRODUCTION

The academic debate on the employment effects of minimum wages has yielded many publications but seemingly little consensus. Some studies find that minimum wages cause unemployment and create entry barriers for workers who are perceived as low-skilled (Campolieti, 2020; Jardim et al., 2017; Neumark & Wascher, 2006; Wolfson & Belman, 2019), while other studies find a weak relationship between minimum wages and employment levels (Broecke et al., 2017; Card & Krueger, 1994; Doucouliagos & Stanley, 2009; Katz & Krueger, 1992; Schmitt, 2015).

The vast majority of previous studies have investigated the minimum wage effect on the number of jobs (the extensive margin). In contrast, the effect on the number of working hours (the intensive margin) is often ignored, despite being an important adjustment mechanism to labor costs (Jardim et al., 2017). The bulk of empirical evidence is also based on studies that use variation in federal and state minimum wages in the United States (Neumark & Wascher, 2008). Because legal framework and institutions vary significantly across countries, the evidence from these U.S.-based quasi-natural experiments may lack external validity for labor markets elsewhere.

Minimum wages, for example, are higher in Europe than in the United States, although there are substantial variations in minimum wages between countries (Christl et al., 2018). Minimum wages are particularly much higher, on average, in the Scandinavian welfare states (Schulten & Lübker, 2021). It is thus more likely that minimum wages are binding and set above the equilibrium wage in these countries, which, in turn, may cause involuntary unemployment among workers who are perceived to have low skills. Scandinavian minimum wages are also not mandated by law; instead, they are determined by industry-level negotiations between trade unions and employer associations. Policymakers therefore cannot implement minimum wage changes to increase employment among marginalized workers.

An alternative and attainable way to reduce firms' total labor costs for policymakers in these countries is lowering payroll taxes. In contrast to a legislative minimum wage decrease, a payroll tax cut increases firms' demand for labor but does not lower employees' wages. The opportunity cost of labor market entry does not change, which implies that there is no off-setting supply-side reaction. This implies that a payroll tax cut should increase employment for minimum wage workers through an increase in labor demand in situations when minimum wages are binding and set above the market equilibrium.

Using the Swedish youth payroll tax cut in 2007 as a quasi-natural experiment, the purpose of this paper is to use this tax cut to investigate how reduced labor costs for

firms influence minimum wage jobs at the extensive and intensive margins. The Swedish payroll reform implied that firms' labor costs were reduced by 11 percentage points for all employees who were between 19 and 25 years old. As such, the treatment intensity of the reform differed based on firms' total extant wage costs for young employees. Following Daunfeldt et al. (2021), we exploit this firm-level variation in treatment intensity by estimating a difference-in-difference-in-differences (DDD) model. Our empirical setup enables us to analyze whether firms whose labor cost decreased substantially increased the number of minimum wage employees along with their working hours compared to firms that were affected slightly, or were not affected at all, by the youth payroll tax cut.

We use wage statistics from the employers' organization Confederation of Swedish Enterprise (*Svenskt Näringsliv*), covering a large portion of employees in the Swedish retail trade industry. Swedish retail jobs have slight differences in terms of tasks and they typically do not require higher education or previous labor market experience. Minimum wages tend to be binding and are standardized in collective agreements that apply to all workers (Skedinger, 2015). The richness of the data allows us to investigate how lower labor costs affect minimum wage employment at the extensive margin level and the intensive margin level.

Our study makes several contributions to the literature. First, intensive margin effects have previously been overlooked, even though they may represent a crucial adjustment mechanism subsequent to reforms that lower labor costs. Our study is also one of the few studies (exemptions include, e.g., Bossler et al., 2020; Georgiadis et al., 2020) that investigate the effect of firms' labor cost reductions on minimum wage employment. The lack of publications that study reductions is problematic since the effect of minimum wages can be asymmetrical, i.e., the employment effect can have a different magnitude for increases and reductions in labor cost. Finally, our study adds to the literature by investigating how minimum wage employment is affected in an institutional context where minimum wages are high, and policymakers are not able to lower minimum wages by legislation.

We find that firms with large labor cost savings, due to the youth payroll tax cut, hired significantly more minimum wage employees after the reform than firms with smaller savings. We also find a significant positive effect of firms' labor cost savings on the total number of hours worked at the minimum wage rate. The positive effects on minimum wage employees are limited to hourly paid individuals, meaning employees with a monthly salary were unaffected. In a market environment where minimum wages are high and binding, minimum wage employment increases when firms' labor costs decrease. As such, a payroll tax cut for this particular scenario benefits people with a weaker attachment to the labor market.

2. THEORETICAL FRAMEWORK AND PREVIOUS FINDINGS

2.1 Theoretical framework

To illustrate the employment effects of a payroll tax cut that reduces firms' labor costs, we combine and use the queuing hypothesis and the substitution hypothesis (Kellermann, 2017). The implication of the queuing hypothesis is that a high minimum wage will induce labor market entrants who are perceived to have low productivity, such as young low-educated individuals and refugees who prefer direct employment because their opportunity cost of obtaining an education is high. The substitution hypothesis states that workers who are perceived as low-skilled will face a lower probability of employment when the minimum wage is higher than the market clearing wage, and that they will try to accumulate more skills to earn a higher wage in the future.

In a formalized manner, the queuing hypothesis states that all workers' skill levels (q) are perceived as low-skilled by the employer, $q = l$, at time $t = 0$. Moreover, workers can choose between investing in higher skills, $q = h$, through education or seeking employment in a job that pays the low-skilled wage W_t^l. Education costs are dictated by training expenses, CH_t and the absence of the low-skilled wage W_l. Completing an education means that the worker is perceived as high-skilled and is rewarded with a higher wage at W_t^h, i.e., $W_t^h > W_t^l$.

The opportunity cost of education is given by H_0C+$H_0 W_t^l$, where H_0 is the time spent on education and C is the direct cost of education. In addition, $H_0 W_t^l$ is the forgone wage income from the low-skilled job position., Given an investment in education in period t = 0, a market interest rate, r, and a discount factor β , income, Y is given by:

$$Y(H_t) = \sum_{t=1}^{T} \beta^t \left(\frac{W_t^h}{(1+r)^t}\right) H_t - \left(\mathrm{C} + \mathrm{W}_0^{\mathrm{l}}\right)\mathrm{H}_0. \qquad (1)$$

The optimal decision is obtained by maximizing (1) with respect to the time spent on education, leading us to the point where the marginal utility of education equals the marginal cost:

$$\sum_{t=1}^{T} \beta^t \left(\frac{W_t^h}{(1+r)^t}\right) = \left(\mathrm{C} + \mathrm{W}_0^{\mathrm{l}}\right) \qquad (2)$$

When the minimum wage is only binding for low-skilled workers, $W_0^h > \mathrm{MW} > W_0^l$, i.e., set above the market wage, then $MW_t = W_t^l$. This implies that the opportunity cost of education rises for workers who are perceived as low-skilled, which results in an excess supply of workers:

$$\sum_{t=1}^{T} \beta^t \left(\frac{W_t^h}{(1+r)^t}\right) < (\mathrm{C} + \mathrm{MW_t})\mathrm{H_0} \qquad (3)$$

The implication of the queuing hypothesis is that a high minimum wage will induce labor market entrants that are perceived to have low productivity, such as young low-educated individuals and refugees, to prefer direct entry into employment because their opportunity cost of obtaining an education is high. The pool of labor market entrants is thus more likely to consist of a high portion of such workers, as more of them are incentivized to queue for a limited number of job positions. In contrast, a low minimum wage incentivizes entrants to invest in raising their skills to earn a higher wage in the future. In this case, the pool of labor market entrants is more likely to consist of fewer workers that are perceived as low-skilled.

Next, we use the substitution hypothesis and consider a profit function where firms have access to high- and low-skilled workers as inputs, L_t^l, L_t^h; P is the price for output, and output is defined as $F\left(L_t^l, L_t^h\right)$. The profit function is given by:

$$\Pi_t = P_t F\left(L_t^l, L_t^h\right) - \sum_{q=l,h} W_t^q \, L_t^q \qquad (4)$$

The profit maximization can then be written as follows:

$$P_t\left[F'\left(L_t^l\right) + F'\left(L_t^h\right)\right] = W_t^l + W_t^h \qquad (5)$$

If we again consider a situation where minimum wages are binding for low-skilled workers, but not for high-skilled workers, $W_t^h > MW_t$, $> W_t^l$, we have an inequality given by:

$$P_t\left[F'\left(L_t^l\right) + F'\left(L_t^h\right)\right] < MW_t + W_t^h \,, \qquad (6)$$

implying that the firm will substitute high-skilled workers with low-skilled workers whose productivity does not match the higher marginal cost defined by MW_t . The minimum wage thus implies a shift in labor demand from low-skilled workers to employees that are perceived to have high-skills, meaning that L_t^l will decrease and L_t^h will increase.

The probability of being employed is higher for high-skilled workers even in the absence of a minimum wage, $p^h > p^l$. However, the introduction of a minimum wage above the market equilibrium wage means that demand further shifts toward high-skilled workers, i.e., the probability of employment for higher-skilled workers is now even higher, $p_h^{MW} > p_h$, while it is lower for those who are perceived as low-

skilled $p_l^{MW} < p_l$. If we insert these probabilities in equation (3), this leads to the following implication:

$$p_h^{MW} \sum_{t=1}^{T} \beta^t \left(\frac{W_t^h}{(1+r)^t}\right) H_t > p_l^{MW}(\mathrm{C} + MW_t\ )\mathrm{H}_0. \qquad (7)$$

The substitution hypothesis thus states that workers who are perceived as low-skilled will face a lower probability of employment when the minimum wage is higher than the market clearing wage and that they will try to accumulate more skills to earn W_t^h in the future. The implication is that low-skilled workers, to a greater extent, will leave the labor force and that labor market entrants are more likely to consist of workers perceived to have higher skills.

To summarize, equation (3) predicts that less low-skilled entrants will enter the labor market if the legislated minimum wage is reduced, while equation (7) predicts that the probability of being employed for low-skilled workers, p_l^{MW}, increases. As such, the effects on minimum wage employment due to a reduction in legislated minimum wages are ambiguous.

However, from a theoretical perspective, the employment effects from a payroll tax cut are more straightforward than the effects from a legislated minimum wage decrease. The reason is that the wage floor in equation (3) remains at a certain level if firms' labor costs are reduced by a payroll tax cut targeted toward individuals with low perceived productivity. Thus, there is no off-setting force on the opportunity cost of education for low-skilled workers, implying that the demand for low-skilled workers will increase when firms' payroll taxes are lowered.

2.2 Previous findings

Many previous studies have analyzed how minimum wage increases affect employment. Most of these studies are conducted in the United States, and they typically investigate exogenous changes in federal and state minimum wages, using a wide range of methodological approaches and datasets. Most of the early contributions indicated that higher minimum wages reduce employment for low-wage workers (Brown, 1999; Neumark & Wascher, 2006). However, this conventional position has been challenged by several studies finding no effects or even positive employment effects from minimum wage increases (e.g., Card & Krueger, 1994; Katz & Krueger, 1992).

Results from attempts to summarize the minimum wage literature and meta-analyses are also inconclusive. On the one hand, some reviews conclude that there is strong support that minimum wage increases cause negative effects on employment and that vulnerable groups are negatively affected further (Campolieti,

2020; Neumark & Shirley, 2021; Wolfson & Belman, 2019). However, other reviews indicate no relationship or a weak relationship between minimum wages and employment outcomes (Card & Krueger, 2015; Schmitt, 2015). There are also indications of a publication bias towards negative and significant effects on employment due to minimum wage increases, meaning that papers that report significant negative effects are more likely to be published in academic journals (Broecke et al., 2017; Doucouliagos & Stanley, 2009).

However, most studies on the employment effects of minimum wage changes are based on data from the United States. This means that these studies might not be generalizable outside their institutional context. For example, the federal minimum wage per hour in the United States is $ 7.25, which equates to approximately 62 Swedish krona (SEK) at today's exchange rate.[1] This can be compared with the lowest negotiated entry wage in the retail trade industry in Sweden, which is SEK 133.68[2] ($ 15.58), i.e., more than twice as high as the American federal minimum wage.[3] Such large differences in entry wages are important to consider since the effects of small changes in low minimum wages are likely to be modest compared to large changes in high minimum wages (Jardim et al., 2017; Schmitt, 2015).

Previous evidence from Sweden has consistently indicated that workers with a weak attachment to the labor market are disadvantaged by high minimum wages (Calmfors et al., 2016). However, these studies only investigate the effect on extensive margin employment, which means that they ignore that many low-wage jobs are part-time positions (Skedinger, 2015) and that the effect on intensive margin employment may be a more significant adjustment mechanism (Jardim et al., 2017). Furthermore, these studies do not use quasi-natural experiments. Instead, they examine the effect of minimum wage changes on employees with different positions in the wage distribution, making causal inference more questionable.[4] The results indicate that increasing minimum wages are associated with an increase in job separations and a decrease in labor market entry among low-wage and low-skilled workers. There are also studies that find a lower probability of labor market entry for low-wage workers when minimum wages increase but no indications of job separations. However, these results are mostly concentrated on workers in the public sector (Eliasson & Nordström Skans, 2014; Forslund et al., 2014).

[1] Using the 2021 average exchange rate, equivalent to 8.58 SEK/USD.
[2] For employees aged at least 18 years old, with no prior experience.
[3] Considering that the payroll tax level in the USA is 12.4 percent in total compared to 31.42 percent in Sweden, the total minimum wage cost difference is even higher.
[4] This type of analysis has been performed with data from the hotel and restaurant industry (Skedinger, 2006), the retail trade industry (Skedinger, 2015) and among refugee immigrants (Lundborg & Skedinger, 2014).

There is also limited evidence on how payroll tax cuts affect minimum wage jobs. One rare exception is Kramarz & Philippon (2001), who investigated employment effects in France due to changes in labor costs that were partly due to payroll tax changes. They found that increased labor costs decreased employment, while the effect of decreasing labor costs on minimum wage employment was not significantly different from zero. This suggests that the employment effect of labor cost changes is asymmetric.

Numerous studies have investigated the effects of the Swedish youth payroll tax cut in 2007 on, for example, the number of young workers (Egebark & Kaunitz, 2018; Skedinger, 2014), the number of employed immigrants (Gidehag, 2019), and the number of working hours of insiders employed in the retail trade industry (Seerar Westerberg, 2022). Skedinger (2014) has previously used the 2007 payroll tax reform as a quasinatural experiment to investigate the effect of firms' labor cost reductions on low-wage jobs. He uses the same dataset as we do, but his analysis is limited to analyzing the effect of the youth payroll tax cut on the employment of eligible workers compared to marginally older workers. He finds only modest effects on employment.

In contrast, the results from later contributions acknowledging the importance of the aggregated firm-level tax cut windfall (Daunfeldt et al., 2021; Saez et al., 2019, 2021) indicate substantial increases in employment and wage incomes for both younger and older workers. However, these studies lack access to data on hourly wages and number of working hours and therefore cannot investigate the employment effect across hourly wage rates or the effects on intensive margin employment. In summary, Swedish and international evidence indicates that the effects on minimum wage employment from payroll tax reforms reducing labor costs remain unexplored.

3. DATA AND DESCRIPTIVE STATISTICS

3.1 Data

Our study is based on wage statistics from a database compiled by the employers' organization Confederation of Swedish Enterprise (*Svenskt Näringsliv*), covering all firms that were members of the Swedish Trade Federation (*Svensk Handel*) from 2000 to 2015. The data are collected in September each year and include information about individuals' age, contracted wages, performance-related wages, number of working hours, inconvenience allowance, and whether they have a monthly salary or are employed by the hour. The data also include a firm-identification number, making it possible for us to construct an employer-employee dataset.

Initially, our dataset included 2,671,071 individual-year observations, with the number of individuals ranging from approximately 123,000 to 226,000 per year. We restrict our sample to the years 2003-2008 and employees who are 18-65 years old, which leaves us with 907,958 observations in total. We are thus investigating the short-term effects of a reduction in firms' wage costs on the number of hires and working hours near the minimum wage. We refrain from investigating long-term responses because these estimates are likely to accommodate contemporaneous shocks and reforms that may interfere with the results (Mian & Sufi, 2012). Another reason for restricting the post-reform period to 2008, i.e., 2 years, is that the payroll tax cut was lowered once more in 2009, and the age threshold was also extended at that time. Finally, we exclude individual-year duplicates based on the fact that some individuals are registered multiple times with the same employer. After excluding these duplicates, our sample size is reduced to 894,320 individual-year observations (331,039 individuals and 5,802 firms). [5]

As a next step, we restrict the analysis to manual workers – as opposed to nonmanual workers – who follow the retail sector's collective agreement (the so-called Detaljhandelsavtalet).[6]. Approximately 50.5 % – 447,555 observations – of the sample constitute such worker-year observations. Lastf, the firm-level analysis is restricted to firms that existed for the entire period, which reduces the number of firms to 1,182. The reason for only including surviving firms is that we utilize the years 2003-2005 as a placebo period in the estimations (see section 4.3). To ensure that our results are not affected by outliers, we exclude firms that have an employment growth deviating by more than +/- three standard deviations from the average change, leaving us with a total of 1,124 retail firms and 313,585 employees.

3.2 Minimum wages

There are multiple minimum wage levels stipulated in collective agreements, varying both across and within industries. According to *Detaljhandelsavtalet*, the retail sector had six different minimum wages during our study period that depended on the age and tenure of the employees. More specifically, different hourly

[5] We have also performed estimations with the full sample, covering individuals within all collective agreements within retail. Utilizing the full sample, the results remain very similar and are available upon request.

[6] Nonmanuals are employed under different wage setting procedures and are furthermore appearing in the data for a shorter number of years than manual workers. These individuals are therefore excluded from the main sample. The results utilizing the full sample, covering individuals within all collective agreements within retail, remain similar and are available upon request.

minimum wage levels in 2006[7], expressed in USD and in the price level of 2021, apply to:[8]

(a) 18-year-olds with less than one year tenure (12.04 USD).
(b) Individuals who are at least 19 years of age with less than one year tenure (12.31 USD).
(c) Individuals who are at least 19 years of age with at least one year tenure (12.80 USD).
(d) Individuals who are at least 19 years of age with at least two years tenure (13.09 USD).
(e) Individuals who are at least 19 years of age with at least three years tenure (14.18 USD).
(f) Individuals who are at least 19 years of age with at least three years of tenure and five years at the same firm (14.27 USD).

Henceforth, we choose to relate employees' wages to minimum wage (b), as this wage is the most representative for a typical labor market entrant.[9] Figure 1 illustrates how the minimum wage level (b) has evolved for employees covered by retail trade agreements over the 2002-2015 period. The corresponding developments for GDP per capita and the median wage level within the retail industry are also displayed in the figure.

Figure 1 shows that the minimum wage level has increased by 39.3 % during the study period, while the median wage has increased by 32.8 %, implying that the negotiated minimum wage has increased approximately 20 % more than the wage of a representative employee within the retail trade industry. The general productivity growth, represented by growth in GDP per capita, has also increased at a slower pace than the minimum wage in the retail trade industry.

Note that the illustrated growth of minimum wages will only cause involuntary unemployment if the minimum wage exceeds the equilibrium wage. If this is the case, the negotiated minimum wage level constitutes a binding condition for becoming employed. We cannot observe the equilibrium wage, but wages are likely above market equilibrium if they cluster around the negotiated minimum wages (Skedinger, 2015).

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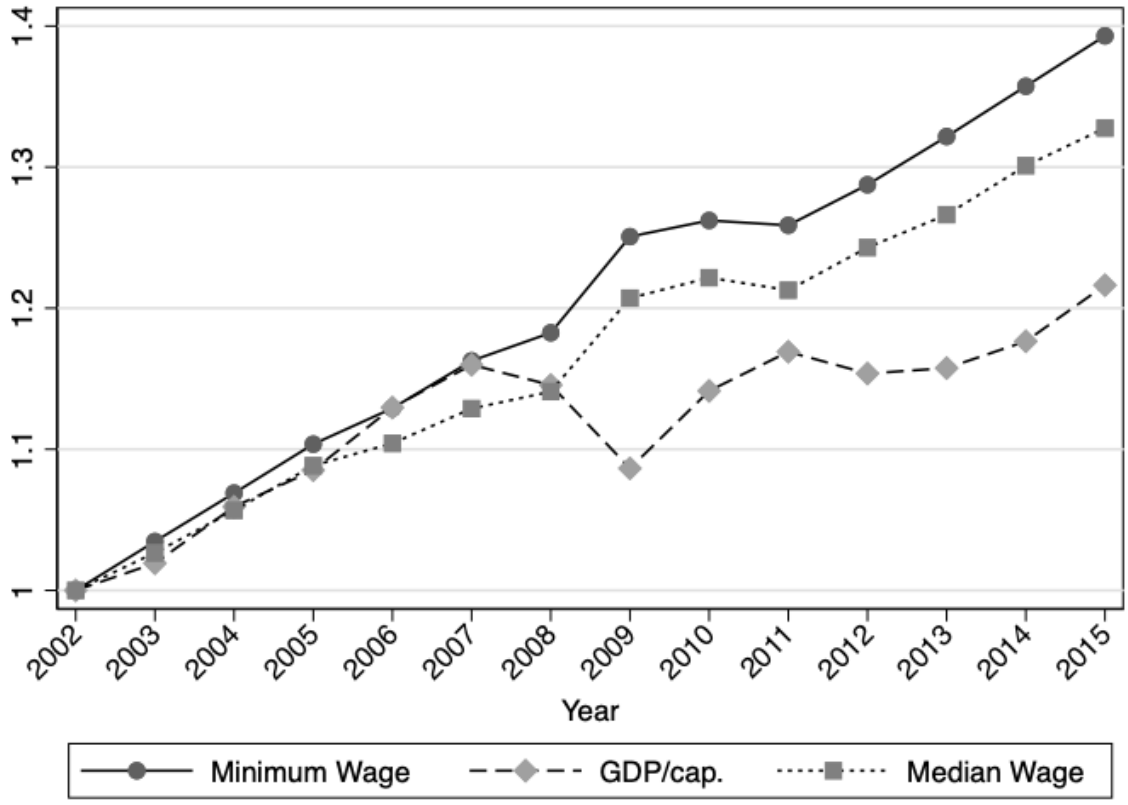
[7] We present the 2006 wage levels because this is the last pre-reform year, i.e., the year before the youth payroll tax cut was implemented.
[8] Note that we use the average 2021 USD/SEK exchange rate to convert the minimum wages to USD. The negotiated minimum wages in the retail trade industry have increased as of 2022 to (in USD) a) 14.68, b) 14.89, c) 15.28, d) 15.50, e) 16.35, e) 16.37.
[9] Minimum wage level (a) is only applicable for employees that are 18 years old, and these individuals constitute a very low portion of all new hires in the retail trade industry (e.g., 6.6 % in 2006).

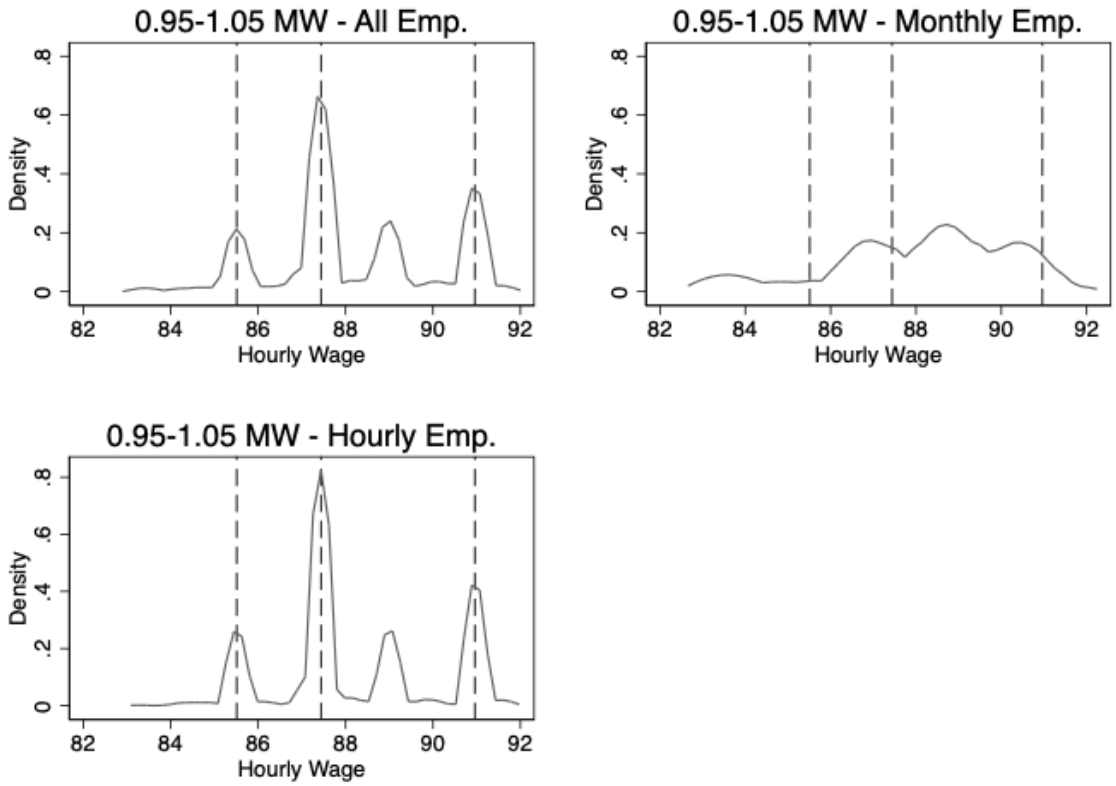
Figure 1. Percentage growth in the minimum wage level over 2002-2015.



Note. Minimum wage and median wage levels are measured in the price level of 2021. Real GDP per capita (constant prices). The reference year is 2002 (=1) for all variables.

Figure 2 shows spikes exactly at the minimum wage thresholds for all employees who work within this wage interval (upper-left), revealing a clear overrepresentation of employees who work at one of the minimum wage rates. The wage distribution for employees working per hour (bottom-left) closely resembles the joint distribution, while the wage distribution for those employed on a monthly wage includes no spikes in the vicinity of the minimum wage thresholds (upper-right). Employees on a monthly wage are more established in the labor market, while those paid by the hour tend to have a looser attachment. The negotiated minimum wage levels thus seem to exceed the equilibrium wage for workers with less experience but not for employees with more seniority.

Figure 2. 2006 hourly wage distribution around the three lowest minimum wages.



Note. The distributions include hourly wages in the interval 95-105 % around the minimum wage applicable for individuals aged 19 or above who have less than one year tenure, illustrated by the dashed vertical line in the middle. The left and right dashed lines represent minimum wages for 18-year-olds with less than one year tenure and individuals aged 19 or above with at least one year tenure, respectively. The hourly wage rate is measured in Swedish krona (SEK).

4. EMPIRICAL METHOD

4.1. Treatment intensity

To investigate the effects of lower payroll taxes on minimum wage employment, we use the Swedish youth payroll tax cut in 2007 as a quasi-natural experiment. This reform reduced firms' payroll taxes by 11.1 percentage points, from 32.42 % to 21.32 %, for all employees who had turned 18 but were younger than 25 years at the beginning of the year (Proposition 2006/07: 84).[10] It thus created firm-level variation in labor cost savings that were proportional to firms' total wage costs for employees aged 19-25 years.

Following (Daunfeldt et al., 2021), we use the youth payroll tax cut to construct a treatment intensity measure that is related to firms' labor cost savings in absolute (monetary) terms. The reason for using an absolute measure is that hiring decisions are likely to be based on how much the firm saves in monetary terms rather than in percentage terms.[11] More specifically, we define our treatment intensity measure as:

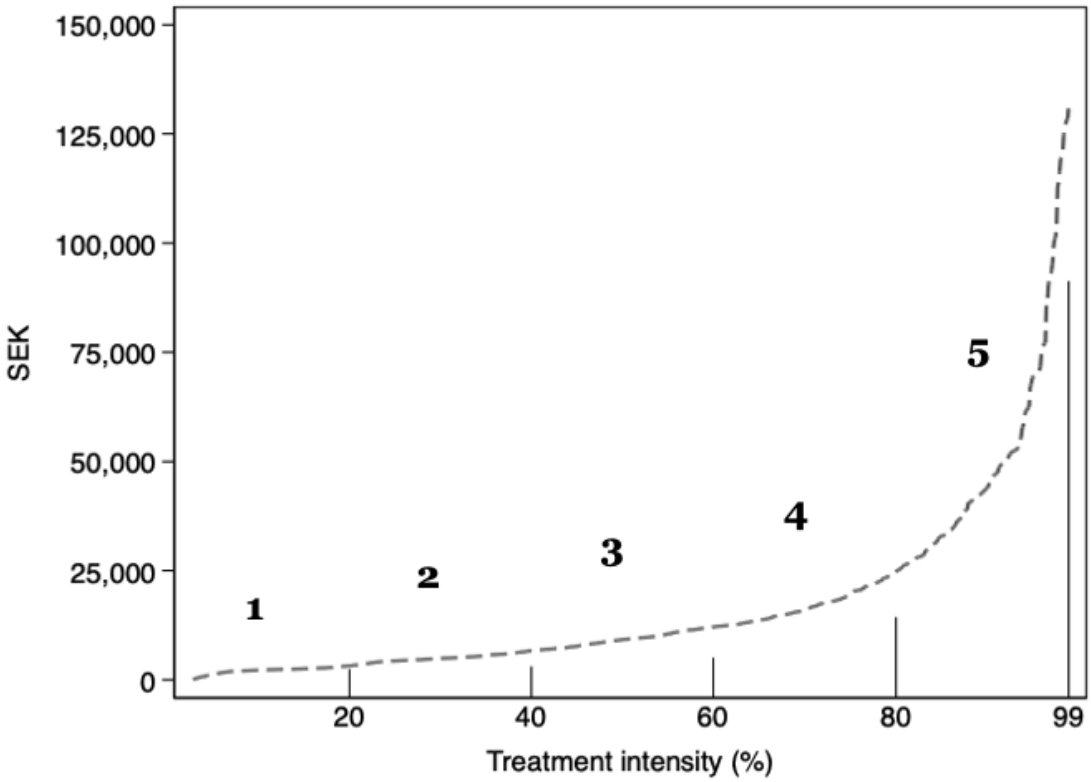
$$Treatment\ intensity_{i,t=2006} = 0.111 * W\_young_{i,t=2006} \qquad (8)$$

where indices i and t denote firm and year, respectively; the 11.1 percentage point reduction is multiplied by the firms' total gross wages paid to the eligible age group of young employees during September 2006 ($W\_young_{i,t=2006}$), i.e., a year before the reform was implemented. Hence, if the wage sum of all young employees in September 2006 is of similar size in September 2007, the treatment intensity measure is predetermined and captures the size of firms' labor cost savings induced by the reform.

[10] A further reduction of six percentage points was implemented in 2009 (Proposition 2008/09: 7), when the payroll tax cut was also expanded to include all individuals under 27 years of age. The payroll tax was incrementally adjusted back to a uniform rate for all workers, at 31.42 percent, in 2016.

[11] Daunfeldt et al. (2021) have shown that the correlation between absolute and relative treatment intensities of the Swedish youth payroll tax cut is low, indicating that, for the most part, large labor cost savings in monetary terms are not equivalent to large labor cost savings in percentage terms.

Figure 3. Labor cost savings (Swedish krona, SEK) by continuous treatment intensity (%). Year 2006.



Note. Labor cost savings in SEK on the vertical axis. Percentiles 1-99 of the treatment intensity measure on the horizontal axis. For illustrative purposes, savings that exceed the 99th percentile are excluded. Groups 1-5 correspond to percentiles in the range of 0-20, 20-40, 40-60, 60-80 and 80-99, respectively, including firms that existed over the 2003-2008 period. Outliers are excluded. Figures are measured in the price level of 2021.

Next, we categorize all firms into five treatment intensity groups based on their rank in the treatment intensity distribution. The first group (0-20) includes the 20 % of the firms with the lowest labor cost savings, while the fifth treatment intensity includes those 20 % of firms with the highest labor cost savings due to the youth payroll tax cut in 2007 (>80-100). The percentiles of the treatment intensity measure and the corresponding labor cost savings in Swedish krona are displayed in Figure 3. It is noticeable that most firms obtained small labor cost savings, while the yearly savings for firms within the highest treatment intensity group (>80-100), on average, equals 65,700 SEK.

Table 1 provides a more detailed examination of the labor cost savings induced by the youth payroll tax cut. We notice that 80 % of the firms obtained first-year savings of up to 24,500 SEK, with the average savings in the treatment intensity groups ranging from approximately 2,200 SEK to 16,900 SEK. The average savings amount to 65,700 SEK in the highest treatment intensity group (>80-100), and the maximum savings are as much as 955,700 SEK.

Table 1. Labor cost savings (Swedish krona, SEK) by treatment intensity group. Year 2006.

Group	Savings	Mean	Median	# Firms
>0–20 %	69–3,224	2,204	2,354	180
>20–40 %	3,228–6,674	4,951	4,908	180
>40–60 %	6,681–12,125	9,224	9,281	179
>60–80 %	12,176–24,526	16,885	16,201	180
>80–100 %	25,059–955,734	65,719	43,936	179

Note. Includes firms that existed over the 2003-2008 period. Outliers are excluded. Figures are measured in the price level of 2021.

4.2. Wage intervals

To investigate the effects of the youth payroll tax cut on minimum wage jobs, we define four wage intervals around negotiated minimum wage (b). Table 2 presents these wage intervals and the negotiated minimum wages with which they overlapped from 2006 to 2008. The 95-105 % wage interval covers the three lowest minimum wages every year, while wage intervals 105-115 % and 115-125 % cover the minimum wages of workers with at least two years of tenure, i.e., at least one of the three highest minimum wages. The highest wage interval, i.e., 125-150 %, exceeds the negotiated minimum wage levels for all study years.[12]

Table 2. Wage intervals, their range in Swedish krona (SEK), and how they relate to the six negotiated minimum wages 2006-2008.

Wage intervals	2006	2007	2008
0.95-1.05	83.07–91.81	87.44–96.64	91.99–101.67
1.05-1.15	91.81–100.56	96.64–105.8	101.67–111.35
1.15-1.25	100.56–109.30	105.8–115.05	111.35–121.04
1.25-1.5	109.30–131.16	115.05–138.06	121.04–145.25

Note. Measured in SEK. The wage range departs from the minimum wage of an individual at least 19 years of age and with no prior experience, which corresponds to 87.44 SEK in 2006, 92.04 SEK in 2007, and 96.83 SEK in 2008.

[12] We ensure that the wage intervals do not overlap by defining them as 95-105, >105-115, >115-125 and >125-150 %. Since the differences between the minimum wages vary over time, the minimum wage coverage of the wage intervals also varies. The 95-105 % interval consistently covers minimum wages (a)-(c) over our period of study 2006-2008, whereas the 125-150 % interval does not cover any minimum wage. In 2007, the wage interval 105-115 % covers minimum wages (d)-(e), and 115-125 % covers minimum wage (f). In 2008, the 105-115 % interval covers minimum wages (d)-(f).

4.3. EMPIRICAL METHOD

We estimate a Difference-in-Difference-in-Differences model, henceforth DDD model (Chetty et al., 2009; Daunfeldt et al., 2021; Gruber, 1994) to capture the effect of reduced labor costs on minimum wage jobs within the retail industry. The DDD model, which is an extension of the standard difference-in-differences model (DiD), adds a third difference by including an additional pretreatment period over which the outcomes of treated and control firms are compared. This means that we compare the outcomes of treated and control firms across 2003-2005 and 2006-2008. The years 2003-2005 are considered a placebo period, where we categorize the firms into placebo groups by calculating the hypothetical labor cost savings that firms would have received if the reform had been introduced in 2004 instead of 2007.

The identifying assumption of our DDD model is that the underlying differences between treated firms and control firms that could have affected employment outcomes are identical in both 2003-2005 and 2006-2008. Thus, existing differences between treated and control firms in the 2003-2005 period should constitute the differences that would have been the materialized if the reform had not been implemented. We know, for example, that there is a positive correlation between firms' labor cost savings and the number of employees, i.e., firms with high treatment intensities tend to have many employees. Previous research also shows that large firms tend to grow more in absolute terms than smaller firms (Delmar et al., 2003), suggesting that firms with a high treatment intensity hire more employees because of their size rather than because of their reduced labor costs. The positive correlation between firm-level savings and number of employees constitutes one potentially confounding factor that our empirical model accounts for.

The estimated DDD model can be expressed as:

$$H_{ijt} = \alpha + \beta_1 Time_t + \beta_2 Group_j + \beta_3 Treat_i + \sum_{n=4}^{6} \beta_n \left(Group_j * Time_t\right) + \delta_{DDD}\left(Group_j * Treat_i * Time_t\right) + \eta_i + \varepsilon_{ijt}, \quad (9)$$

where the indices i, j and t denote firm, group classification (treated or control) and year, respectively. The outcome variable H_{ijt} represents either the number of hires or the number of work hours in the vicinity of the minimum wage. $Time_t$ is a binary variable that is equal zero for the pre-treatment years of both time periods and equals one for the corresponding post-treatment years. Hence, $Time_t$ is equal zero in 2003 and 2006 and equals one in 2004-2005 and 2007-2008. The variable $Group_j$ is an indicator variable for firms' group classification and is equal zero for the control firms and equals one for the treated firms in both time periods. $Treat_i$

separates all firms included in the period 2003-2005 from those included in the period 2006-2008 by taking the value zero for the former and one for the latter.

The main variable of interest is the interaction term of these three binary variables - $Group_j * Treat_i * Time_t$ – which is equal one for the treated firms in the actual posttreatment years, i.e., in 2007-2008. Its parameter, δ_{DDD}, isolates the effect of reduced labor costs on minimum wage jobs by deducting underlying differences between treated firms and control firms in the 2003-2005 period, i.e., in the pre-reform years.[13] Last, η_i accounts for time-invariant firm-specific factors, whereas ε_{ijt} is an idiosyncratic error term.

Our DDD model is estimated separately for our treatment intensity groups >20-40, >40-60, >60-80 and >80-100. The control group consists of firms with either no reductions or small reductions, i.e., with a treatment intensity in the range of 0-20. To estimate the effects for minimum wage workers, we estimate this model separately for the four different wage intervals in the study (see Table 2). We thus estimate the relationship between reduced labor costs and minimum wage jobs and, in addition, examine whether the effect varies with the magnitude of the labor cost reductions.

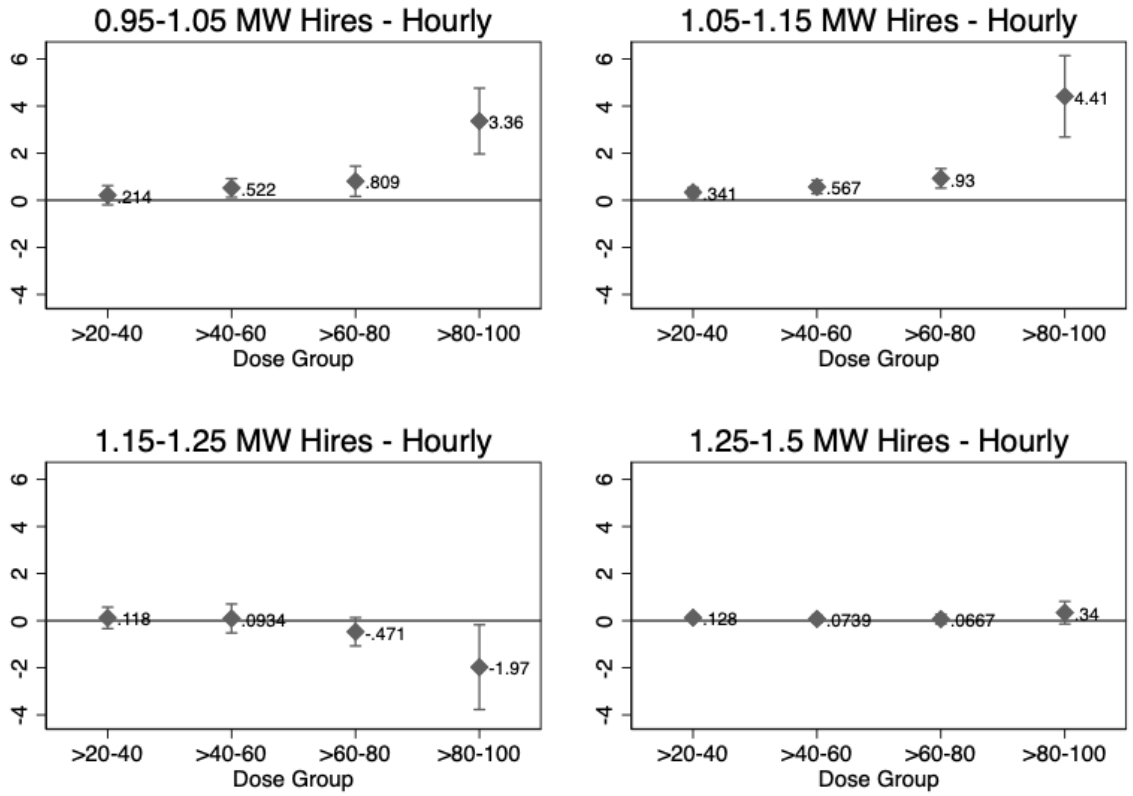
5. FINDINGS

5.1. Effect on the number of minimum wage hires

The estimated effects on the number of hires who are employed hourly are presented in Figure 4. The results show that retail firms with large labor cost reductions hired significantly more hourly employees than firms with small labor cost reductions. The point estimates for firms with the smallest labor cost reductions (>20-40) are only statistically significant within one of the four wage intervals (1.05-1.15), while we can observe positive and statistically significant effects on the number of hires within the two lowest wage intervals among firms with larger labor cost reductions. A firm with labor cost savings in the range >40-60 increased its number of hires near the minimum wage (0.95-1.05) by 0.52 individuals, while the corresponding estimate for firms in the two highest treatment intensity groups (i.e., >60-80 and >80-100) amounts to 0.81 and 3.36 hires, respectively.

[13] The parameter δ_{DDD} is equivalent to the difference between two DiD models over the periods 2006-2008 and 2003-2005. Specifically, $\delta_{DDD} = \delta_{DiD,2006-2008} - \delta_{DiD,2003-2005}$.

Figure 4. DDD estimations. The effect on number of hourly employed hires over treatment intensity, by wage intervals relative the minimum wage rate.



Note. Firms that existed over the 2003-2008 period are included. Firms with extreme annual employment changes are excluded (+/- 3 std. dev. from mean). Point estimates correspond to 95 % confidence intervals. Within-firm estimation, standard errors are clustered at the firm level.

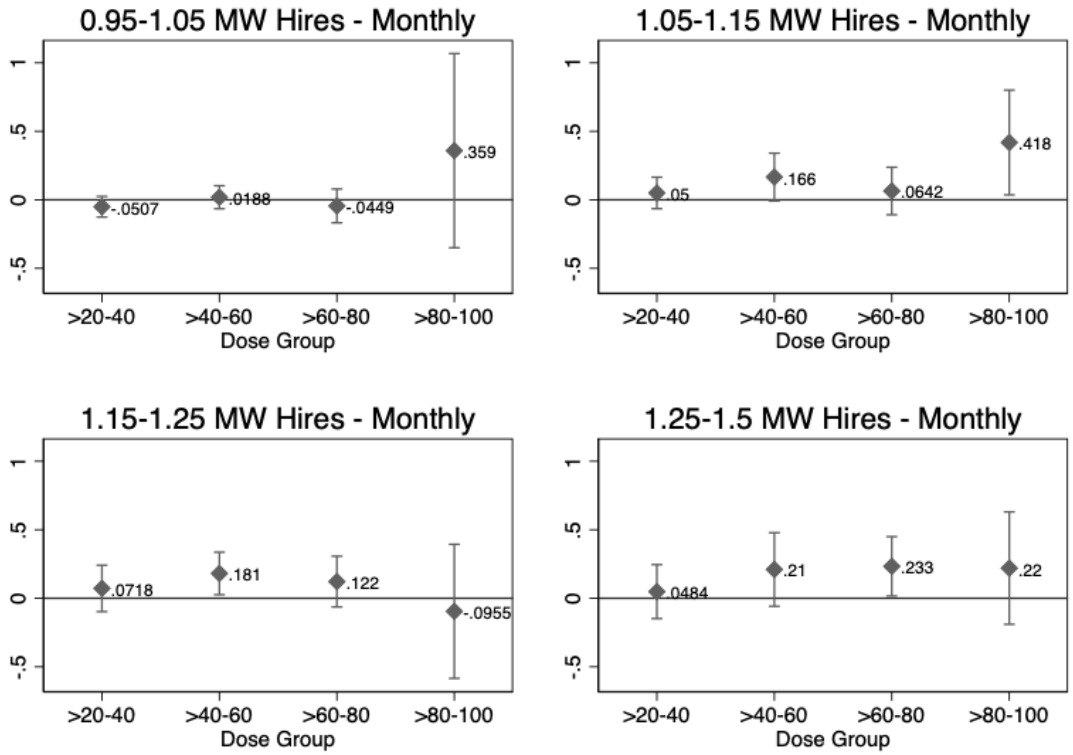
An even stronger effect of firms' labor cost savings can be observed on employees in the 1.05-1.15 wage interval, showing an average increase of 4.41 employees for firms in the >80-100 treatment intensity range. Note that the 95 % confidence interval for firms in the >80-100 group does not overlap the confidence intervals for the lower treatment intensity groups, suggesting that firms with the largest labor cost savings hire significantly more employees than firms with smaller labor cost savings. The increase in the number of minimum wage workers who are employed by the hour is thus significantly higher among firms with large labor cost savings than for firms with smaller labor cost savings.

In contrast, we find no positive and statistically significant effects on the number of hires at higher wages, as seen in the lower panels. The positive effect on hires of firms' labor cost reductions thus primarily holds for low-wage jobs. For wage levels that are 15-25 % above the minimum wage (i.e., wage interval 1.15-1.25), we instead obtain one negative and statistically significant point estimate. This can be explained by a substitution effect, i.e., that firms with large labor cost reductions are incentivized to hire low-wage workers instead of workers with higher wages.

However, the total number of hires within the two lower wage intervals outweighs the negative effect found in wage interval 1.15-1.25, implying that the total number of hires at these wage levels has increased due to the payroll tax cut.

To test whether reduced labor costs also benefit individuals with a more permanent position in the labor market, we perform separate estimations for individuals with a monthly salary. These individuals tend to have permanent and more stable job positions through long-term contracts than employees who are contracted by the hour, and who are commonly replaced by young and part-time employees (Skedinger, 2015). If reduced labor costs primarily increase the number of jobs for individuals who generally have more difficulties entering the labor market, we expect to find no significant estimates for hires that are employed monthly.

Figure 5. DDD estimations. The effect on the number of monthly employed hires over treatment intensity by wage intervals relative to the minimum wage rate.



Note. Firms that existed over the 2003-2008 period are included. Firms with extreme annual employment changes are excluded (+/- 3 std. dev. from mean). Point estimates correspond to 95 % confidence intervals. Within-firm estimation standard errors are clustered at the firm level.

From Figure 5, it is apparent that the point estimates suggest small changes in the number of monthly employed hires and that most estimates are statistically insignificant. Our findings thus indicate that reduced labor costs cause an increase in the number of hourly employees hired and have wages in the vicinity of the

negotiated minimum wages. No such effect can be observed for employees who have monthly wages and a more stable position in the job market.

5.2. Effect on the number of working hours of minimum wage workers

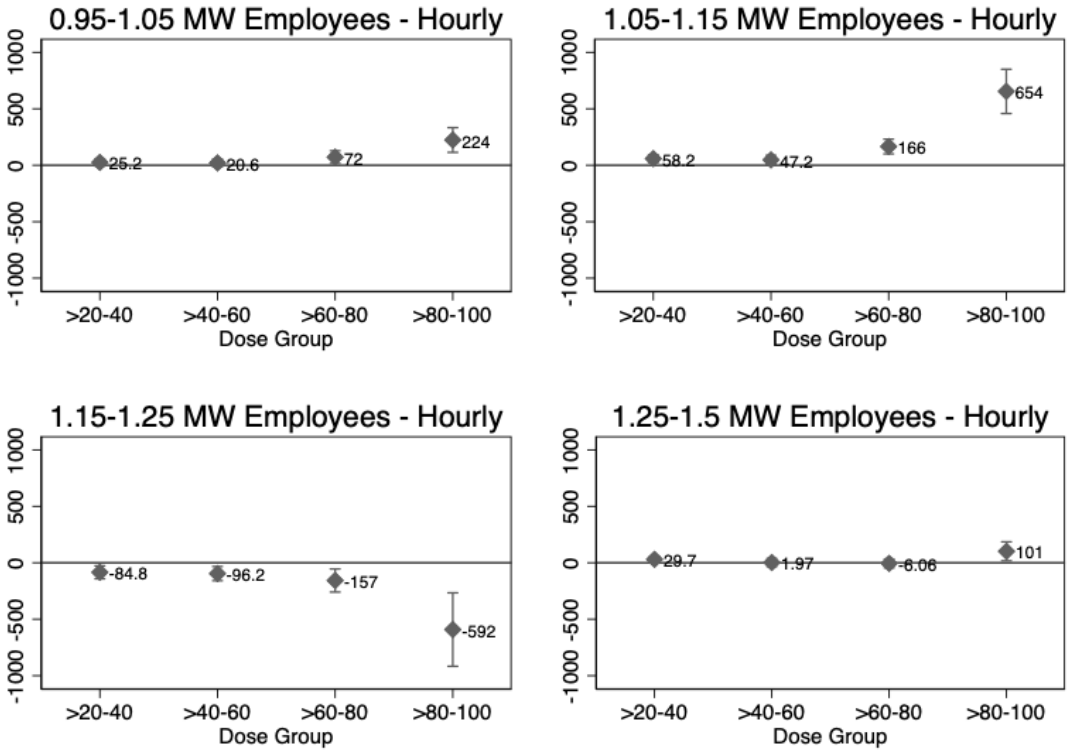
Studying the number of hires does not necessarily capture the total employment effect around the minimum wage since firms can also adjust the number of working hours among their existing personnel. We therefore proceed by investigating the effect of labor cost savings on the total number of working hours near the minimum wage. This measure captures changes made to both the total number of employees and to the number of working hours among those already employed, i.e., it combines the extensive and intensive margin employment effect in one measure.

We present the estimated effects on the total number of working hours among employees that were employed hourly in Figure 6. The results show that firms with large labor cost savings significantly increased the number of working hours for employees with wages in proximity to the minimum wage. Within the two lowest wage intervals, firms with savings in the >60-80 and >80-100 treatment intensity groups, on average, increased the number of hours worked per month for employees paid by the hour for 72 and 224 hours in the 0.95-1.05 wage interval and 166 and 654 hours in the 1.05-1.15 wage interval. The corresponding increases in the number of employees were 0.81-0.93 and 3.36-4.41, respectively (see Figure 5).

If we combine the estimates above, the increase in the number of work hours per new employee amounts to 89 and 178 hours per month for firms with savings in the >60-80 treatment intensity range (72/0.809 and 166/0.93) and amounts to 67 and 148 hours per new employee and month (224/3.36 and 654/4.41) in the >80-100 treatment intensity range. This means that the increase in the number of hours for firms that received the largest labor cost savings is close to, or exceeds, the number of hours that corresponds to a full-time job. However, hourly employed retail employees rarely work full time. For example, hourly employed retail employees worked, on average, 89 hours in September 2006. This suggests that the effect of firms' labor cost savings on the number of working hours near the minimum wage is partly explained by an employment effect on the intensive margin, i.e., that incumbent employees in the proximity of the minimum wage worked more hours.

Note also that we obtain negative and statistically significant point estimates within the wage interval 1.15-1.25, suggesting negative effects on the number of work hours. This could potentially be explained by the substitution effect discussed in section 5.1. Within the wage interval farthest away from the minimum wage (1.25-1.5), we find some indications of a positive effect on the number of work hours.

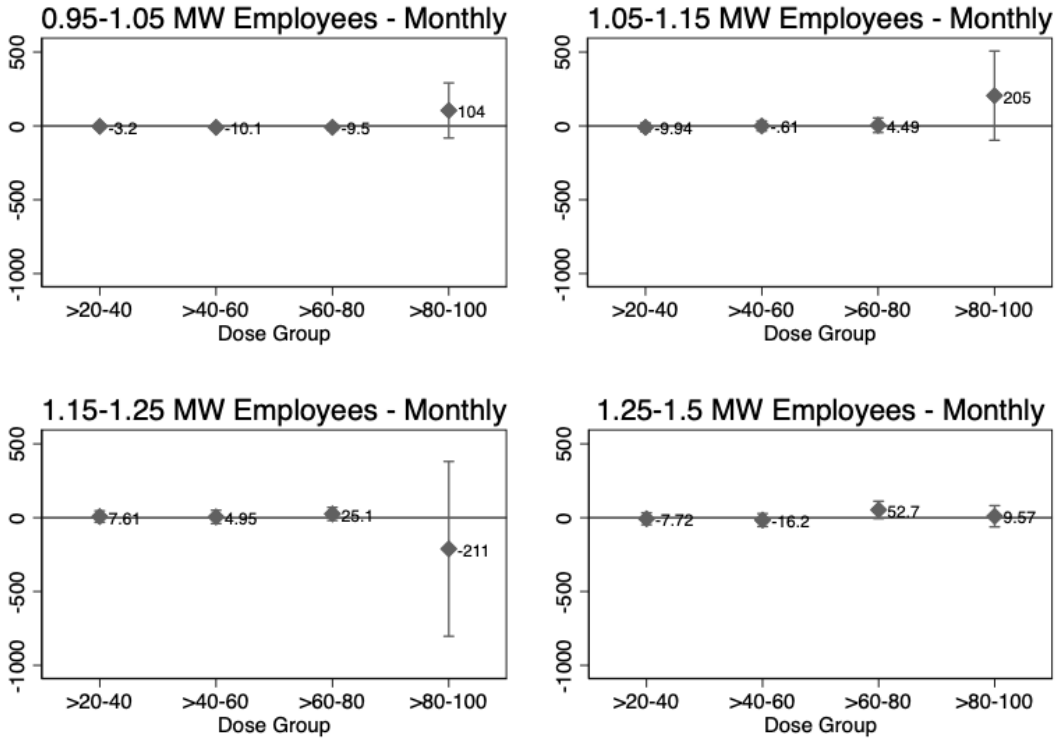
Figure 6. DDD estimations. The effect on number of hours worked for hourly employed employees over treatment intensity, by wage intervals relative to the minimum wage rate.



Note. Firms that existed over the 2003-2008 period are included. Firms with extreme annual employment changes are excluded (+/- 3 std. dev. from mean). Point estimates correspond to 95 % confidence intervals. Within-firm estimation standard errors are clustered at the firm level.

The estimated effects of firms' labor cost savings on the number of hours worked for employees that are employed monthly are presented in Figure 7. We do not obtain any statistically significant point estimate. Thus, the findings in Figures 6-7 indicate that labor cost savings increased the number of work hours among low-wage workers paid by the hour, while workers with a more permanent labor market position were not affected.

Figure 7. DDD estimations. The effect on number of hours worked for monthly salaried employees over treatment intensity, by wage intervals relative the minimum wage rate.



Note. Firms that existed over the 2003-2008 period are included. Firms with extreme annual employment changes are excluded (+/- 3 std. dev. from mean). Point estimates correspond to 95 % confidence intervals. Within-firm estimation standard errors are clustered at the firm level.

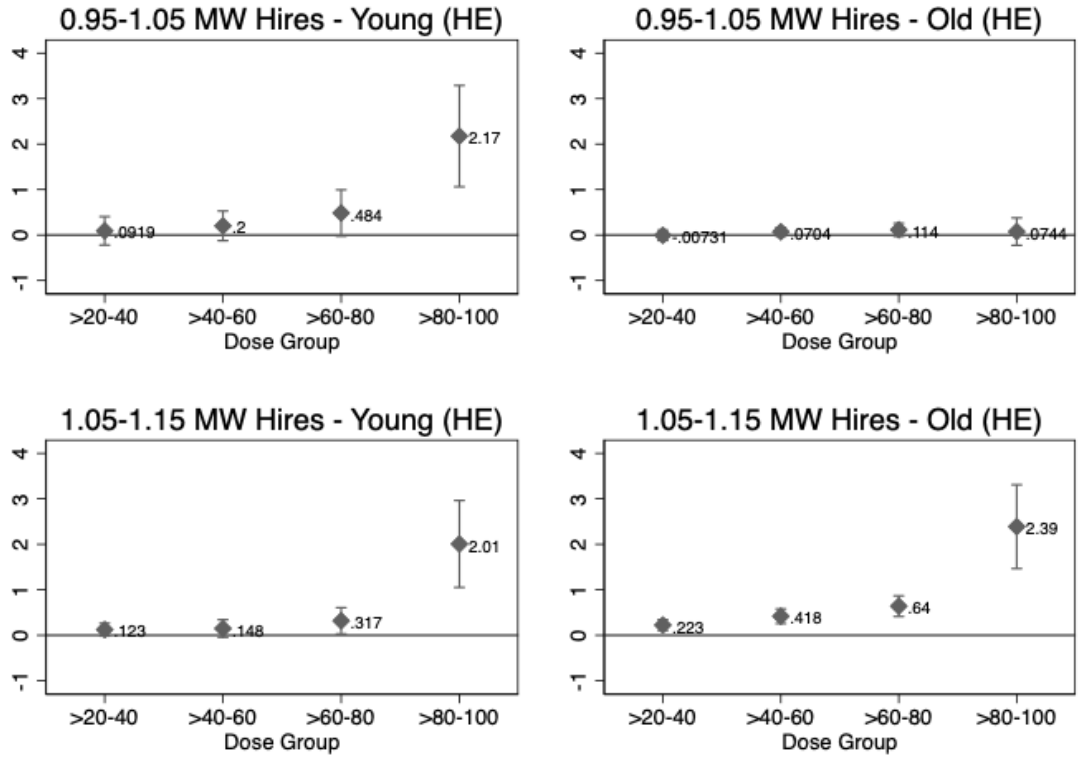
5.3. ROBUSTNESS CHECK

Earlier contributions have found that the Swedish youth payroll tax cut in 2007 increased the employment of young individuals who were targeted by the reform (Daunfeldt et al., 2021; Saez et al., 2019). A potential concern is that the employment effects on minimum wage jobs that we observe are entirely explained by a positive effect within the reform's target group, i.e., 19- to 25-year-olds. We therefore perform a robustness check where we estimate the effect on minimum wage hires separately for the reform's age group (age 19-25) and nontargeted individuals (aged at least 26). These estimates are presented in Figure 8.

We find positive and statistically significant point estimates for the reform's targeted age group 19-25-year-olds within the wage interval closest to the minimum wage (0.95-1.05), while the point estimates for older employees are insignificant. This is expected since young employees are overrepresented in low-wage positions, while older individuals seldom receive a wage in the 0.95-1.05 wage interval. However, when investigating the employment effects in the wage interval 1.05-1.15, we find

positive effects on hires of both young and older individuals. In the highest treatment intensity (>80-100), the average firm hired 2.01 more individuals aged 19-25 years and 2.39 more individuals who were above the age of 25 years. Firms that received large labor cost reductions thus hired both young and old individuals in the vicinity of the minimum wage, suggesting that the positive effect of firms' labor cost savings on minimum wage employment is not limited to employees that were targeted by the reform.

Figure 8. DDD estimations. The effect on the number of hourly employed hires over treatment intensity by age group and wage intervals 95-105 % (upper) and 105-115 % (lower) relative to the minimum wage rate.



Note. Young employees are defined as 19- to 25-year-olds. Old employees are at least 26 years old. Note. Firms that existed over the 2003-2008 period are included. Firms with extreme annual employment changes are excluded (+/- 3 std. dev. from mean). Point estimates correspond to 95 % confidence intervals. Within-firm estimation standard errors are clustered at the firm level.

Thus far, our control group has consisted of firms that received either no labor cost savings, or minor, labor cost savings following the introduction of the youth payroll tax cut (treatment intensity 0-20). To assess whether our findings are sensitive to this choice, we use two alternative control groups. First, we only include firms that received small savings in our control group (>0-20), thereby excluding firms without any savings. Firms without any young employees and thus not exposed to any savings might be systematically different from firms in the treatment groups in terms of recruitment, suggesting that they might not be a valid comparison group.

Second, for comparison, we do the opposite and only include firms without any savings in the control group. All results remain qualitatively similar when we use these two alternative control groups. This indicates that there are no systematic differences between control group firms in the main estimations, in terms of small or no savings.[14]

6. DISCUSSION

The most influential publications in the minimum wage literature are based on quasi-natural experiments in the United States, estimating employment effects on the extensive margin of small increases in relatively low minimum wages. Although this large body of literature is yet to reach consensus, little is known about the adjustment mechanisms in other institutional contexts. European labor market thresholds are higher than those of the United States, and the effects of a lower minimum wage may be different, which is the departure point of this paper.

Our aim has been to investigate the effects of a youth payroll tax cut in Sweden on minimum wage employment – both at the extensive and intensive margins, utilizing the fact that the youth payroll tax cut created firm-level variation in labor cost savings based on firms' total wage costs for their young employees. Contrary to a reduction in legislated minimum wages, a payroll tax cut reduces firms' labor costs but not employees' wages. The opportunity cost of labor market entry is therefore constant, and there is theoretically no offsetting supply side reaction. However, researchers presently have limited knowledge on how payroll tax cuts affect minimum wage jobs. We therefore use the labor cost savings created by the payroll tax cut to sort firms into five different treatment intensity groups. Using wage statistics from the Swedish retail trade industry, including data on contracted hourly wages and number of hours worked, we then investigated the effects of the reform on minimum wage employment by estimating a difference-in-difference-in-differences model.

We find clear indications that minimum wages in the Swedish retail industry were binding when the youth payroll tax cut was implemented. As much as 89 % of hourly employees had a contracted hourly wage in a five percent interval around one of the negotiated minimum wages in the pre-reform year. Furthermore, firms that received major labor cost savings due to the youth payroll tax cut significantly increased their number of employees and working hours near the minimum wage compared to firms with smaller savings. No major effects could be observed for individuals who earned considerably more than the minimum wage. The positive effects of labor cost savings on minimum wage employment were also, to a great extent, limited to employees who worked by the hour, i.e., who had less stable job

[14] These results are available upon request from the authors.

positions. Our results thus suggest that retail firms primarily increased the employment of low-wage workers when their labor costs were reduced due to the payroll tax reform.

Our results are relevant considering the high unemployment rates among first-generation immigrants and low-educated youths across Europe (Bruno et al., 2014; Daunfeldt et al., 2019). The retail trade industry has many job positions that do not require higher education or extensive training (Skedinger, 2015), and retail firms are therefore expected to provide jobs for workers that are typically perceived as low-skilled. However, our results suggest that high binding minimum wages deter retail firms from employing workers with difficulties entering the labor market. This is in accordance with (Jardim et al., 2017), who found a negative impact from a relatively large increase in minimum wages in Seattle on low-wage labor market entry and a decrease in intensive margin employment.[15] This suggests that the longstanding debate on the employment effects of minimum wage changes can benefit from considering the initial level of the minimum wage, if minimum wages are binding, and the magnitude of the minimum wage change.

Our study also adds to the literature on the employment effects of the youth payroll tax cut in Sweden. The efficiency of this reform was first questioned because studies found a negligible effect of the youth payroll tax cut on employment, implying an extensive loss in government revenues per created job (Egebark & Kaunitz, 2018; Skedinger, 2014). However, these studies likely underestimated the employment effects of the reform because they did not consider that firms received different treatment intensities and that the firms were able to use their labor cost savings to hire both younger and older employees. Recent studies have consequently found larger effects on employment on the extensive margin and thereby lower government revenue losses per created job (Daunfeldt et al., 2021; Saez et al., 2019). Recent findings have also documented a positive employment effect on the intensive margin and small spillover effects on incumbent workers' wages (Seerar Westerberg, 2021). Our findings thus provide more evidence on the positive effects of the youth payroll tax cut, showing that the reform, to a large extent, benefitted low-wage workers who had more difficulties obtaining stable positions on the labor market.

Like any study, ours has its limitations. First, we lack information on the human capital characteristics of the employees. As discussed in section 2, there might be a trade-off between minimum wages and employment among those who are perceived as low-skilled, which might induce substitution toward high-skilled workers

[15] The post-reform minimum wage level in Seattle also corresponds quite closely to the lowest minimum wage levels in the Swedish retail trade industry. The investigated post-reform minimum wage in Seattle was USD 13, while the lowest negotiated minimum wage in Sweden was USD 14.73 in 2021.

(Jardim et al., 2017; Kellermann, 2017; Skedinger, 2015). However, the substitution between workers that differ in their actual or perceived skills cannot be explicitly tested without information on characteristics such as educational attainment, unemployment history or test scores. A fruitful area for further research is therefore to study the dynamics between payroll tax changes and transitions in and out of employment among groups that are typically perceived as low-skilled and high-skilled.

The payroll tax reform in 2007 targeted young employees, meaning that the results might not be applicable for a general payroll tax cut, which might shift the tax incidence toward workers since all firms are affected in a similar manner. Under such circumstances, trade unions may be more able to use centralized collective agreements to raise wages among insiders, thereby crowding out the positive employment effect that we observe (Daunfeldt et al., 2021; Holmlund, 1983; Seerar Westerberg, 2021). Employment may furthermore respond differently to payroll tax cuts in other countries and industries. An interesting avenue for further research would therefore be to explore the effects on minimum wage jobs of general payroll tax cuts and the effects of payroll tax reforms in less centralized wage bargaining contexts. Finally, employment and wages may respond asymmetrically to payroll tax increases and decreases, respectively. This constitutes another fruitful area for further studies.

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