The wage and employment effects of minimum wages on low-wage industries: Evidence from the Bill 148 Act in Ontario

Bachelor Thesis
Erasmus School of Economics
International Bachelor Economics and Business Economics

Author: Alkim Cetinkaya
Number: 523353
Supervisor: Ana Gomes Figueiredo Varatojo dos Santos
Second Assessor: Sacha Kapoor
Date: 13-12-2022

This thesis attempts to assess the wage and employment effects of a higher minimum wage following the Bill 148 Act. In 2018, the reform introduced an increase of $2.6 to Ontario's hourly minimum wage which was initially set at $11.40. By using a difference-in-difference design, I exploit the variation in which certain industries were heavily affected by the reform while some industries did not experience an increase in their average wage floors. I find that the minimum wage policy was indeed binding for low-paying industries relative to higher-paying industries, raising the hourly wages of low-wage workers significantly. I derive some heterogeneity in the wage responses across treated and control industries based on a triple difference-in-difference strategy. My analysis on the employment effects shows that a higher minimum wage induces low-wage industries to cut back on employment, without increasing the weekly hours worked. This adverse employment effect appears to be not driven by a lower share of part-time workers relative to full-time workers. Although my estimates on an extensive margin of employment conform to the textbook theory on a competitive model, the overall analysis raises doubts on its implications. I show that my estimates are robust to a variety of alternative specifications and tests which further corroborate that parallel trends assumption is likely to hold.

1. Introduction

The effect of minimum wage increases on employment has been widely studied and debated by economists whose estimates point toward a substantial negative effect, to a positive effect or no effect. A large body of empirical studies have aimed at understanding and reconciling these divergent results to reach a consensus on the effects of introducing or increasing hourly minimum wages. The difficulty in reaching a consensus is that the impact depends not only on the market dynamics of low-wage jobs (due to different elasticity of demand), but also on the trade-off it poses. As it may seem like a higher minimum wage benefits the low-income individuals by increasing their hourly wages, it could be well that an increase results in higher costs for employers, potentially leading them to reduce their labour force. However, it is also possible that a higher minimum wage results in significantly lower incidence in poverty by improving the welfare of low-income earners. Although the effect on poverty is not the focus of this research, investigating the wage and employment response of low-wage industries would help to understand the direction of welfare effects of a higher minimum wage.

Minimum wage policies have featured prominently in recent policy debates in Canada, in particular Ontario initiating the discussion on its potential implications to businesses, employment growth and overall poverty levels. In 2018, Ontario implemented a set of unprecedented and unforeseen amendments to its labor market regulations, namely the Bill 148 Act. This reform brought about an increase of $2.6 in hourly minimum wages which were initially set at $11.40 Canadian dollars. Many representatives who voted for the bill were highly criticized by the incumbent provincial premier who was especially vocal in his belief that such a substantial increase would curb employment growth by putting financial burden on employers.

This paper aims to resolve some ambiguities about the employment effects of a higher minimum wage by using a difference-in-difference design and exploiting the extent to which some industries were heavily affected while some were barely affected. Using Labour Force survey data between June 2017 and March 2018, I compare the changes in hourly wages, the number of employees and weekly hours worked across heavily and weakly affected industries. I provide further insights into the heterogeneity in wage responses and the full/part-time labor-labor substitution.

As the cost of employing a worker increases for a low-wage firm, I expect to find a lower number of full-time equivalent employees at heavily affected industries after the reform. Based on a traditional labor market model characterized by perfect competition, a higher minimum wage induces employers to substitute away from part-time employees and increase the number of hours worked for the remaining workers. Hence, I hypothesize that the Bill 148 indeed had such an impact on extensive and intensive margins of employment. However, since my analysis only investigates the short-run effects of a higher minimum wage for low-wage industries, it is also likely that firms did not reduce their labor demand and dismiss their employees in a short-time interval.

I use an event-study analysis to estimate separate treatment effects for each month prior the reform, and confirm the presence of parallel trends in the outcome variables 2017. Conditional on being employed, the mean hourly wages of low-wage workers increase by 6.7 log points relative to high-wage workers following the treatment period. I also use the number of extra hours worked per week as my independent variable to probe the robustness of my estimates. I further calculate an alternative measure which indicates how much an industry must increase its average hourly wages to meet the new minimum wage rate. The model using this gap measure as an independent variable yields consistent results pointing towards a positive wage effect of the Bill 148. I employ a triple difference-in-difference method to explore the heterogenous wage responses across industries with respect to workers' age, education level, gender and firm size. Then, I practice the same empirical design to investigate the effect on intensive and extensive margins of employment. Conditional on being employed in 2017, workers in low-wage industries experience their weekly working hours go down by 3.5 log points on average. This finding already contradicts with my expected outcome, and the predicted by the competitive model theory. Nevertheless, I detect a significant disemployment effect in industries with a low minimum-to-median wage which is then reinforced by the alternative gap measure. My benchmark point estimate implies a negative effect by 14 to 16 log points, depending on the specification. I derive no significant full-time labor substitution at low-wage industries after the reform, which raises a puzzling question on the reallocation of disemployed full-time workers. Although it is not feasible to directly test the main identifying assumption of parallel trends, I believe that I provide sufficiently strong evidence to buttress the presence of similar trends across two groups of industries before the reform was effective. Given that a bulk of research is dedicated to the United States, I extend the existing literature by leveraging the variation in minimum wage exposure across industries in Canada. The cross-industry design is surprisingly uncommon when comparing to the abundance of cross-region designs dominating the minimum wage literature (Card & Krueger, 1994; Kim & Taylor, 1995). I believe it is more challenging with the cross-region design to avoid any unobservable time-varying characteristics which would affect regions differentially.

The remainder of the article proceeds as follows. Section II presents the related literature and background information on the Bill 148 amendments introduced in Ontario. Section III explains the data, sample selection and methodology used throughout the analysis. Section III provides the cross-industry analysis on the wage and employment effects followed by a variety of robustness checks. I supplement the findings from the previous section with a cross-region analysis in Section IV where I also include a set of validity checks. Section V discusses the results, compares them to the existing literature and proposes any improvements for future research. Section V concludes.

2. Related Literature

My article relates to several strands of minimum wage literature. To begin with, most of the existing work has largely concentrated on the effects of minimum wages on employment with principal focus on teenagers and young adults, who are viewed as a proxy for low-skilled labor given that they constitute a large segment of low-wage workers. (Card, 1992; Allegretto et al., 2017; Neumark et al., 2014). In Canada, youth minimum wage workers account for slightly more than half (52.3%) of the total minimum wage earners in 2018. Hence, the aggregate employment effects of minimum wages are more likely to be evident for this group than for other demographic groups. Whether minimum wages induce adverse or positive employment effects on teen employment remains unclear due to several papers with contradictory results. For example, Card and Krueger (1993) challenge the textbook theory that minimum wages reduce employment in a perfectly competitive labor market (George J. Stigler, 1946) by presenting no evidence on the adverse effects of a higher minimum wage on employment at fast-food restaurants in the United States. In a further analysis, they detect an upward trend in teenage employment in New Jersey where minimum wage was raised by 80 cents relative to Pennsylvania. However, the study has been criticized for the adequacy of an unreliable counterfactual, small sample size and short-time frames. Although the debate has mainly evolved around the employment effects on teen employment, and on workers in specific sectors most vulnerable to a change in hourly wages (Dube, Lester, and Reich 2010) or on workers earnings wages below the minimum wage (Clemens and Wither 2019), this paper attempts to identify the overall impact on the low-wage industries with an average hourly wage falling below the minimum wage. This article attempts to resolve the concerns over the reliability of studies which intend to control spatial heterogeneity by including state-specific linear time trends and narrowing the scope of the geographic areas used for controls, by using cross-industry variation to estimate the employment effects of the reform. The legislation Bill 148 came as a large shock to treated industries in Ontario which were mandated to raise their hourly wage rates from $11.40 to $14.00. Similar to our approach, Derenoncourt and Montialoux (2020) compared the newly versus previously covered industries before and after the 1967 minimum wage rise to quantify the wage and employment effects of the 1967 reform. I can argue that the time-varying unobservable factors within treated, and control industries or within industries are unlikely to exist within a time frame of 5 months, and conditional on fixed differences between workers and industries, employment outcomes in the treated and control industries would have followed a similar trend as in the control industries.

Exploiting the industry-level differences in the coverage of minimum wage, Derenoncourt and Montialoux (2020) show that earnings rose sharply for workers in the newly covered industries such as agriculture, restaurants, nursing homes which were previously uncovered. They analyze the employment effects of the 1967 expansion by first, using the cross-state design and comparing the strongly treated states which had no minimum wage law as of 1966 versus weakly treated states which did. Second, they use a bunching method proposed by Cengiz et al. (2019) to quantify the cross-industry change in number of workers employed below and above the minimum wage after the reform. Derenoncourt and Montialoux (2020) find evidence on substantial wage effect and small, negligible employment effects which are highly consistent with Cengiz et al. (2019) and recent literature on policy changes. I will conduct an analysis similar to the cross-industry design, comparing strongly treated industries which had a low minimum-to-median wage with weakly treated industries with a high minimum-to-median wage hourly in 2017. Concerned with the differing labor market dynamics across industries which could potentially hinder the causality of my estimates, I run a heterogeneity analysis for the wage and employment effects to check if differences in market (labor demand elasticity, monopsony, perfect competition, number of sellers etc.) play a role in the employment and wage response of certain industries.

There exists sufficient number of studies focusing on the minimum wage impact on youth employment in Canada (Sen et al. 2010; Baker et al. 1999; Campolieti et al. 2005, 2006), but few on the short-run effects for low-wage industries in Canada. Baker et al. (1999) examines the effects of more frequent legislative changes over shorter periods on teen employment, concluding small and insignificant estimates. In a recent paper, Cengiz et al. (2019) estimate the overall employment impact on low-wage workers in the US. The superiority of their analysis, namely a bunching design, stems from the ability to capture an overall employment effect without having to focus on a subsegment to isolate the effects. They achieve this by assessing the localized employment changes around the minimum wages, shifting the wage distribution and creating a "bunching" at and slightly above the minimum wage. I will estimate the overall employment effects on low-wage industries with a cross-industry analysis, as well as show the extent to which teenagers and low-educated workers are influenced by the Bill 148 reform. Given that price adjustments on consumer goods following a rise in minimum wage is a short-run phenomenon (Aaronson, 2001), my focus on short-run effects remains highly beneficial to understand the changes in various labor market indicators (hours worked, extra hours worked, employment). Another advantage of focusing on short-run effects is that time-varying unobservables are less likely to render my DiD design invalid.

Finally, my paper expands on the minimum wage literature which focuses on the reallocation effects, by implementing an identification design which exploits variation in exposure across industries. I provide strong evidence on the reallocation effects of the minimum wage by introducing the "gap measure" in my cross-industry analysis which has often been used in the minimum wage literature. (Card and Krueger 1994; Dustmann et al. 2021; Draca, Machin, and Van Reenen 2011). This measure depends on the share of individuals in the industry who earn less than the minimum wage, as well as on how much a worker's wage is below the minimum wage. It measures an industry's exposure to the minimum wage as the industry's gap measure. The advantage of using this measure is that it can pick up potential reallocation of workers from more exposed industries (low-wage) to less exposed (higher-wage) industries, rather than the displacement of low-wage workers or on overall decline in employment. To the best of my knowledge, this paper is the first to attempt applying the gap measure on an industry-level analysis.

3. Institutional Details

3.1 The Bill 148

On November 2, 2017, the province Ontario passed a new employment legislation namely "Fair Workplaces, Better Jobs Act" under Bill 148 to bring fairness, security and opportunity into workplaces for vulnerable workers and their families. The Act came into force as a response to the Changing Workplaces Review, which reported in 2014 that more than 30 % of employees in Ontario were struggling financially and mentally under "precarious jobs", which describes a type of employment that is temporary, unprotected, insecure and insufficiently paid to support a household. The Act enforces a 23% increase in hourly minimum wage across Ontario, which goes up to $14 from $11.4 as of January 1, 2018. This becomes the largest minimum wage jump in Canada since the Great Recession, expected to impact 55% of all retail workers in the province. Additionally, the legislation expands paid leave duration for all employees regardless of their contract type, granting an additional two paid sick days annually to each person who is employed for at least a week. The provisions of this legislation mainly target part-time and contract workers by mandating an hourly rate equal to the rate of full-time workers. The passage of the bill was a landmark initiative of Kathleen Wynne who is a member of the Ontario Liberal party. Many representatives who voted in favor of a 23% rise in hourly minimum wage argued that it would help low-income households by alleviating poverty and increase business productivity by reducing employee turnover. The sweeping reform was highly successful in serving the interests of unions, anti-poverty activists and in general, employees who sought more flexible labor conditions. However, when the incumbent Progressive Conservative premier, Doug Ford took the office in June 2018, he revoked the changes introduced by the Liberals, in particular freezing the province's minimum wage at $14 an hour until 2020 while the hourly minimum wage was set to rise to $15 in 2019 as a result of the Liberals' labor laws. Cutting two paid personal leaves for workers was among the amendments to Bill 148 that the new regime adopted. The Progressive Conservatives stated that the Bill 148 was a "no-sign" reform, which lacked a comprehensive economic impact analysis and consultation with the Ontario business community before getting legislated. They argued that the Act put an unanticipated financial burden on businesses, curbing the job growth in the province. My endeavor with this paper is to empirically test whether these arguments can be held accountable.

3.2 The Macroeconomic Context

The Bill 148 was enacted in Ontario during a period when the province was experiencing a decreasing unemployment rate with a rising employment-to-population ratio. (Figure 1) Between the plotted years 2013-2018, Ontario was governed by the Liberals with Kathleen Wynne as the premier. During that period, the unemployment rate shrank from 7.6% to 5.6% (Panel A) while the stock of employed workers exhibited a steady increase reaching 7.4 million in 2018 from 6.6 million in 2013 (Panel C). Nonetheless, with a growing population across Canada, the relative share of employed individuals in Ontario compared to Quebec increased at a slower rate. Besides, there was a rapid decline in the overall number of employees earning below $12.00 per hour after 2018, suggesting that this trend was mainly driven by Ontario's Bill 148 reform which shifted the wage distribution above the $12.00 threshold (Panel D). Given that Ontario was not characterized by an economic downturn, yet by a positive macroeconomic environment at the time of the reform, one can interpret the legislation as an exogenous amendment to raise the minimum wage rather than a policy response to tackle an economic subject or ideal. This provides strong evidence against the possibility of simultaneity bias, or endogeneity problem which would by definition threaten my identification strategy. However, the endogeneity problem might also emerge when policy makers decide on a higher minimum wage at times with stable and strong economy which denotes a correlation between the policy and the outcome variable, employment. Such this be the case; parallel trends assumption is again violated because in counterfactual, the treatment group would have diverged anyway regardless of the treatment. I provide my insights about this potential issue in Section VI.

4. Data

The data used is from the Labour Force Survey (LFS), derived from Statistics Canada. LFS is the most comprehensive, publicly available survey conducted nationwide every month in each Canadian province and territories in order to capture labor market indicators as well as demographic and family-relationship information for all household members aged 15 and above. The LFS survey started collecting cross-sectional information on a wide variety of variables from the selected representative samples since 1952. Respondents are asked to report their employment status, hourly rate of pay before taxes and other deductions, job duration, industry, occupation, reason for absence or presence in the labor market, and many more defining labor market characteristics. The survey uses a rotating panel sample design so that selected dwellings remain in the sample for six consecutive months. LFS provides rich data on each worker's industry, occupation, size of workplace and permanence of jobs. There are in total 21 industry codes, and 40 detailed occupation categories conditioning on being employed. Although immigration, union, and marital status are available, there is missing individual-level information on race. Beginning of January 2017, information is collected on the usual wages or salary of employees at their main job. Respondents are asked to report their wage/salary before taxes and other deductions, and include tips, commissions and bonuses. Weekly and hourly wages/salaries are calculated in conjunction with usual paid work hours per week. The main advantage of using LFS is that I can observe hourly wages per worker which is the outcome of interest when estimating the wage effect following a rise. Data on each province, industry and metropolitan per province allows me to perform a cross-industry difference-in-difference design.

There are two limitations involved in using LFS to analyze the Bill 148 reform. The first limitation is the lack of information on income or annual earnings per individual. Given that the main political debate between the Liberals and Conservatives in Ontario concerns the consequences of a higher minimum wage on poverty and whether it could potentially alleviate low-income families out of poverty, data on annual earnings would have allowed me to corroborate such arguments empirically. Second, the main limitation throughout this paper is the lack of sample weights available in the requested LFS files. Even though the sample data are weighted to enable tabulations of estimates at the regional level of aggregation, there was no published document with final weights included for my sample. As reported by Statistics Canada, the sampling for the labor force surveys were done such that in total of 56,000 households which best meet the need for reliable estimates were selected. Hence, the sample of the LFS consists of independent samples taken from different regions to best represent the demographics of the whole population.

4.1 Sample Selection

My sample covers data from June 2017 – March 2018. The "pre-policy" period refers to June – December 2017 while the "post-policy" period is from December 2017 onwards until March 2018. The sample includes 50,163 observations of all workers aged 17-62 residing in Ontario who were reported to be employed at an industry. Throughout my analysis, unpaid family workers and self-employed individuals are excluded from the sample. Only when I investigate the employment effects, I further eliminate individuals expecting or with children from the sample of analysis. This is because workers with children, or expecting children, were subject to an extended parental leave scheme which came into force as of December 2017, a month prior to the reform of interest. The aim of making amendments to the Employment Insurance (EI) was to grant more flexibility for working parents, and it is likely that such improvements had an impact on the labor market decision/status of employees with children, or on pregnancy leave. Using a sample with working parents included would hinder the identification of the causal impact of the reform on employment by leading to spurious estimation which would also pick up the effect of EI improvements on employment. I examine the impact of a hike in provincial minimum wage on hourly earnings across two sets of industries. As suggested by Neumark et al. (2013), one should empirically confirm the choice of control group by applying synthetic control method and find which units (states, industries etc.) best resemble or match the treatment unit. Due to the lack of firm-level information, this exercise will not be employed but highly recommended for future studies.

4.2 Outcome Variable

I estimate the effect of the reform on three main outcome variables: (i) log average hourly wages in Canadian dollars, (ii) log average number of hours worked at the main job and (iii) log number of full-time equivalent employees per industry. I also run the regression with the number of extra hours worked, the share of part-time workers as my dependent variable to support my estimations. In an alternative model, I run the regression on the gap measure which quantifies the proportional increase in wages necessary for an industry j to meet the new minimum rate.

4.3 Treatment and Control Groups

The treatment group consists of the industries with low minimum-to-median wage; accommodation and food services, retail trade and agriculture, which together employed about 13.5% of the workforce in Ontario. Expectedly, these industries have the highest proportion of workers earning at or less than the minimum wage which implies that the wage increase is primarily concentrated on the treated industries. The identification of minimum wage effects requires a critically constructed and valid counterfactual "control group" for how the dependent variable would have evolved absent the increase. (Neumark et al., 2013). The control group is formed by weakly treated industries with relatively higher minimum-to-medium wages, whose wage floor remained unaffected by the policy reform: finance and insurance, public administration and educational services. The underlying idea is that minimum wage hardly affects the wages of workers employed in the control industries, because it is not binding on the higher-wage industries. I expect that high and low-wage workers are unlikely to be substitutes of each other given that they are employed in different sectors requiring different skills and background.

4.4 Summary Statistics

The descriptive statistics suggest that there are baseline differences in individual-level and labor market characteristics across treated and control group (see Table I). Before the hike in January 2018, workers in the strongly treated industries were paid 55% lower than workers in the weakly treated industries. As expected, the percentage of teenagers employed in the strongly treated industries is much greater (34%) than the weakly treated industries (7%). Female workers are overrepresented in the industries heavily affected by the increase in minimum wages. Given the age composition of workers in the treatment group, there are more individuals who were never married. In the control group, 60% of all employees have obtained a college degree while 20% have gone beyond the bachelor's degree. The fraction of workers working full-time relative to part-time is lower in the treated group, confirming the idea that low-wage workers tend to couple their studies or external activities with part-time work. Most workers in the treatment group are not a union member (90%) whereas less than half remain uncovered by a union contract in the control group. Industries in both groups are mainly characterized by larger firms with more than 500 employees. The usual hours worked per week in both sets of industries are on average similar, yet 3 hours more in the control group. The difference in the average job tenure amounts to 50 months, which do not come as a surprise given that low-wage industries provide unstable, short-term jobs viewed as a steppingstone to higher-paid positions. The number of employees in the control group is by definition equal to the number of observations for the control group because I report the effects of minimum wage on individuals conditional on being employed at one of the industries of focus. The difference in observations across groups (number of workers) isn't large enough to imbalance the model. The last row in Table I illustrates the gap measure averaged across each set of industries in 2017. The gap measure is equal to 0.0047 in the weakly affected industries which is substantially lower compared to the heavily affected industries with 0.077 gap measure, as expected. The calculation of this measure will be explained in more detail in the next chapter. The observable individual- and industry-level variables in Table I are added as controls in my benchmark estimation.

5. Methodology

5.1 The Wage Effect

I start by studying the effect of the Bill 148 Act on hourly wages using LFS data. The aim is to demonstrate that the reform was binding and significantly raised hourly wages for workers employed in low-wage industries relative to workers employed in high-wage industries. The baseline empirical approach is a cross-industry difference-in-differences research design where I compare the dynamics of average hourly wages in the industries which had a low minimum-to-median wage versus weakly treated industries with a high minimum-to-median wage hourly in 2017. The identifying assumption is that absent the minimum wage increase, wages in more and less affected industries would have evolved at the same rate absent the rise in minimum wage.

The advantage of using an industry-level approach instead of individual or regional is that it can reveal potential decline in employment from hiring of unemployed individuals. Hence, the employment effect is not merely driven by workers who were employed when the minimum wage was raised to $14.00 and who were partially shielded from the adverse impacts of the policy, but also by those who were not employed prior. Another benefit of a cross-industry design is that one can rule out the potential unobserved macroeconomic differences across regions, which vary over time. Otherwise, the unobserved time-varying characteristics which differentially impact the treatment and control groups violate the baseline identification assumption of parallel trends. As shown in Table I, treatment and control industries are allowed to differ in terms of baseline time-invariant characteristics or be subject to certain time-variant sectoral shocks as long as the shocks evolve in parallel for both groups. I provide supporting evidence that there are no time-varying factors which influence the wage dynamics differently for both groups. Firstly, I provide graphical evidence that hourly wages in treated industries versus control industries followed a similar parallel trend before January 2018, lending support to the baseline identification assumption of DiD method. (see Figure II) Second, I further bolster the validity of my estimations by including a wide range of controls, time-varying effects and sensitivity analysis to render it unlikely that the wage effects are confounded by contemporaneous changes diverging the trends across both groups. (see Table II)

The following difference-in-difference model is constructed to compare the dynamics of wages in the strongly and weakly treated industries, before and after January 2018 when the Bill 148 came into force;

$$\log w_{ijm} = \alpha + \beta_1 * Post_m + \beta_2 * Strongly\ treated_j + \beta_3 * Strongly\ treated_j * Post_m + \beta_4 X_{ijm} + \delta_m + \delta_j + \delta_{CMA} + \varepsilon_{ijm} \tag{1}$$

where $logw_{ijm}$ denotes the hourly wages measured in logs for an individual i in industry j, and in month m; $Post_m$ is an indicator variable that takes the value 1 from January 2018 onwards; δ_m and δ_j are year-month and industry-specific fixed effects respectively, in order to capture the baseline differences across industries. I also include the city fixed effects captured in δ_{CMA}, which allow me to take into account time-invariant city characteristics which might affect wages. Throughout my cross-industry analysis, I control for the following individual-level characteristics contained in the vector X_{ijm}: gender, age, immigration status, years of schooling, marital status, working experience. Since an individual can only report their hourly wages conditional on being employed, there are additional worker-level characteristics controlled and included in this analysis: union membership, private/public worker, usual hours worked at the main job, part-time or full-time status, temporary or permanent job status, and occupation. The coefficient of interest β_2 measures the difference in the hourly wages expressed in logs between the treated and control industries before and after the reform. For the post-reform period the coefficient estimates β_2 yield causal effects of raising minimum wage by \$2.6 in the most affected industries in comparison to the least affected industries, relative to the pre-policy period. This holds true under the parallel trends assumption.

A complementary approach to measure the change in hourly wages is by exploiting the variation in exposure to the minimum wage across industries. An advantage of this approach is that the employment effects will not be driven merely by individuals who were employed when minimum wage was raised by \$2.6 and who were possibly shielded from adverse effects of the policy, but also by those who were not in employment prior to 2018 (Dustmann et al., 2021). Thus, the approach can pick up potential declines in employment from reduced hiring of unemployed workers.

The gap measure is computed as follows:

$$\overline{GAP_{Jt}} = \frac{\Sigma h_{it} \max(0; MW - w_{it})}{\Sigma h_{it} w_{it}} \tag{2}$$

where h_{it} denotes the weekly hours worked of worker i (employed at industry j), MW is the minimum wage, and w_{it} refers to the worker's hourly wage reported by LFS. The measure (if multiplied by 100) reflects the average wage increase (in percent) necessary to bring all workers in the industry up to the minimum wage. The measure is averaged over each month per industry to obtain a time-constant measure. The gap measure for every industry can be found in Appendix. The following equation is formulated to assess the wage response of more or less exposed industries following the increase.

$$logw_{jt} = \alpha_j + \beta\, \overline{GAP_j} \times Post_t + \rho_t + \delta_j time_t + \varepsilon_{jt} \tag{3}$$

where $Post$ is an indicator variable equal to 1 for the post policy year (2018) and $time$ captures a linear time trend that is allowed to vary across industries. Industry baseline characteristics (occupation, establishment and firm size) are included separately as well as interacted with time fixed effects to account for differential pre-trends across heavily and barely affected industries. The outcome variable is the number of workers employed in the industry, expressed in logs.

The standard errors are clustered at the industry-level in both specifications to allow for arbitrary dependence of error term and to account for potential shocks within industry j over time. I also include two-way cluster at the individual and industry-level in Table II as an additional check.

5.1.1 Heterogeneity Analysis

I run a separate analysis with respect to effects by age, education, firm size and gender (see Table III). Descriptive statistics in Table I suggest that low-wage industries are mainly composed of younger workers, and with low levels of education. I expect to derive a larger increase in hourly wages for younger and low-educated workers relative to the rest across the treatment and control groups. To study this hypothesis and examine the heterogeneous wage effects of the reform, a triple difference-in-difference methodology is used as below:

$$\log w_{jm} = \alpha + \beta_1 Strongly\ treated_j * Post_m * Educ_i + \beta_2 * Strongly\ treated_j * Post_m + \beta_3 * Post_m * Educ_i + \beta_4 * Strongly\ treated_j * Educ_i + \delta_m + \delta_j + \beta_5 * X_{ijm} + \varepsilon \quad (4)$$

where the coefficient of interest becomes β_1 to capture the difference in wage effects between low and highly educated workers in treated industries versus control industries. I create a separate dummy variable, $Educ_i$, which takes 1 if an individual has completed post-secondary level education and 0 otherwise. To further explore the heterogenous effects with respect to age, firm size and gender, I replace the dummy variable ($Educ_i$) with another dummy indicating whether the worker belongs to the category of interest. For example, I create a dummy variable $Male_i$ which is equal to 1 if an individual is male. The variables δ_m, δ_j denote month and industry-fixed effects while worker-level characteristics are captured by X_{ijm}.

5.2 The Employment Effects

I repeat the same analysis to elucidate the employment responses of treated industries following the Bill 148 Act. First, the exact specification of DiD design as in equation 1 is performed to compare the extensive and intensive margin employment outcomes across industries. I demonstrate the parallel trends in labor force characteristics outcomes prior to the reform for both groups by employing an event-study design. (see Figure III and IV) Parallel trends assumption implies that in the absence of the reform, employment would have evolved according to the general macroeconomic trend between years 2017 and 2018 across groups. The estimations can be interpreted as causal if this assumption is empirically validated. Then, I exploit the same gap measure constructed in the previous analysis to provide a corresponding analysis for the employment responses of heavily and less exposed industries. This measure captures the proportional increase in average hourly wages for industry j to meet the new minimum rate. As a final practice, I discuss the validity of my results and address the endogeneity issue.
The regression model to assess the change in employment across treated and control group in the post-policy months relative to pre-policy is as follows:

$$logemp_{jm} = \alpha + \beta_1 * Post_m + \beta_2 * Strongly\ treated_j + \beta_3 * Strongly\ treated_j * Post_m + \beta_4 X_{ijm} + \delta_m + \delta_j + \delta_{CMA} + \varepsilon_{ijm} \quad (5)$$

which is similar to the equation 1 except that the outcome is log number of full-time equivalent employees at industry j in month m. Full-time equivalent (FTE) employment is calculated as the number of full-time workers plus 0.5 times the number of part-time workers. The coefficient of interest, β_2, measures the difference in log employment across strongly and weakly treated industries before and after 2018. When investigating the policy impact on an intensive margin, the dependent variable becomes log number of hours worked at the main job per week, $loghrs_{ijm}$. I control for the same observable time-variant and invariant covariates which may be correlated with the treatment by including same worker's characteristics, time, city and industry dummies. Standard errors are clustered at the industry-level to allow for arbitrary serial correlation in error terms within industries over time as individuals reoccur in the sample.

The model below with the gap measure is the same as equation 3 except that the outcome variable is now (1) log number of FTE employees at industry j in month m (extensive margin) and (2) log number of hours worked per week at industry j in month m (intensive margin). The same variants of regression explained for equation 3 apply to the model below.

$$Outcome_{jm} = \alpha_j + \beta\, \overline{GAP_j} \times Post_t + \rho_t + \delta_j time_t + \varepsilon_{jt} \quad (6)$$

6. Results

6.1 The Wage Effect

As a first step, I estimate event-study regressions of the equation 1 by estimating separate treatment effects for each month (Figure II). The graphical representation of event-specific estimates allows me to confirm the existence of parallel trends assumption both empirically and visually as the point estimates of pre-reform months are statistically insignificant and hover around 0. It also allows me to assess the presence of any anticipation effects before the policy or dynamic treatment effects after the policy. Figure II illustrates that the average log hourly wage falls in month prior to the reform for low-wage industries relative to high-wage and follows an upward trend from 2018 onwards. This upward trend justifies that the hike in minimum wage was binding and had an immediate effect, raising the average hourly wages in strongly treated industries in comparison to weakly affected industries. Since the upward trend from plotted point estimates persist after January, I predict that the reform had a dynamic wage effect for low-wage employees increasing hourly earnings over time. The fall in log hourly earnings in December 2017 could be explained by low-wage firms anticipating the economic burden caused by the hike in minimum wages from 2018 and thus, reducing it further to smooth out the future costs.

The difference-in-difference estimates of the wage effect are presented in Table II. The table provides evidence on the average change in log hourly earnings of treated workers relative to control workers in the post-reform period. The model in column (1) exhibits a statistically significant, positive effect of the reform on log average hourly wages, excluding any covariates. As displayed in Column (2), adding individual-level controls and time fixed effects decreases the estimated coefficient of interest by a small amount which means that sorting on observables is not part of the response of the reform, at least in the short run. Column (4) provides a complete estimation of the wage effects, indicating that the hourly wages increased by 6.4 log points on average for workers in the low-wage industries relative to the workers in the high-wage industries. The magnitude and statistical power of the point estimate are amplified when month, industry and city fixed-effects as well as worker-level controls are included in the model as shown in column (2-4). The difference in point estimates between the specifications is not substantial to raise doubts on the presence of sorting on observables. Including fixed effects and controls to the regression is necessary since it accounts for all time-invariant industry and metropolitan characteristics and any potential observable confounders which could explain the variation in hourly wages. To limit the possibility that the estimations channel a general macroeconomic trend which pushed up hourly wages for all industries over time regardless of the reform, I replace the month dummies/fixed effects in equation 2 with linear time trends as shown in column (5). The treatment coefficient is similar to the main analysis in column (4), implying that average hourly wages for workers in low-wage industries increase by 6.4 log points relative to workers in high-wage industries. To check the sensitivity of my results, I extend the model in column (5) by including a linear time trend which differs for both groups instead of a general linear trend in column (6). This results in a statistically significant point estimate equal to 7.4 log points at 5% significance level and slightly smaller standard error compared to the main model. Finally, I add industry-specific time trends in column (7) to allow every industry to have a distinct linear trend. This yields a decrease in the coefficient by 2.3 log points and a significantly smaller standard error compared to previous models. Adding time trends in the regression relies on the assumption that any preexisting trends in outcomes between groups have been linear and would have continued at the same rate in the absence of the reform. I believe that over shorter-periods, the linear restriction is less likely to lead to nonsensical results and need not be justified. Given that the coefficient of interest goes up by 0.04 log points and becomes statistically significant at 5% significance level after the introduction of time trends, it is worth pointing out that the average hourly wages per group indeed follow a growth pattern which renders de-trending a necessary practice to derive a causal interpretation of estimates. All specifications indicate that the minimum wage raised hourly wages: being employed at low-wage industries leads to an increase in average hourly wages by 5 to 7 log points (5-7%), depending on the model.

I further perform a heterogeneity analysis using triple difference-in-difference approach in Table III. As presented in column (1) row (2), mean hourly earnings increase by 13 log points for teenagers relative to non-teenagers across industries at 1% significance level. This point estimate is 5 log points greater than the main estimate in Table II which measures the average hourly wage change for all ages across industries before and after the reform. The largest wage effect is reported in column (2) with 20 log point increase in hourly wages among low-educated employees at 1% significance level. According to column (3), low-wage workers at firms with more than 500 employees experienced their average hourly wages go up by 1 log point after the reform relative to high-wage workers in the control group at 5% significance level. This result is consistent with the fact that minimum wage employees became increasingly concentrated in large firms. (Statistics Canada, 2018) Column (4) suggests no significant difference between hourly wages between genders in treatment and control groups before and after the reform. The estimated coefficient of positive 1 log point is also greater than the main point estimate. These results buttress the idea that the reform did target low-wage workers, and my empirical design captures the intended wage effect of this reform rather than a general macroeconomic trend affecting all workers in the treatment group.

Finally, Table IV provides complementary evidence on the wage effect by using the gap measure as the treatment indicator. In the first column, I display simple difference-in-difference estimate based on equation 3 with only industry and time fixed effects included. In column (2), I add industry-specific linear time trends in the regression to de-trend a possible growth pattern in hourly wages per industry. This yields a similar point estimate equal to 53 log points with smaller standard error at 1% significance level. In column (3), I probe the robustness of my findings by adding controls for industry baseline characteristics (establishment and firm size, occupation) interacted with a linear time trend. This enables me to account for any linear time trends differing within industry due to differences in baseline characteristics. The addition of the interaction effects amplifies the estimated wage effect by 150 log points, further substantiating my results. One explanation for this change is the presence of a seasonal pattern in employee wages in an industry depending on industry-level fixed baseline characteristics. If some of the true variation in the wage growth and gap measure can be attributed to these differential pre-trends, the inclusion of interaction terms will lead to a change in point estimate. In sum, all specifications confirm the presence of a substantial wage effect in low-wage industries following the policy change: a 1 percentage point increase in the gap measure leads to an increase in average hourly wages by 53 log points. This translates into 53% increase in hourly wages at industries with a stronger exposure to the minimum wage reform.

6.1.1 Robustness Checks

In addition to providing several specifications in the analysis above, I report the following robustness tests to further support my evidence on the wage effect. Firstly, I restrict my sample to full-time workers only. In Table V, column (1) exhibits a point estimate similar to my baseline estimation of 6.6 log points. I then run the same regression on a subsample with only part-time workers which increases the magnitude of the wage effect to 8.5 log points. This result implies that the reform affected full-time and part-time workers' hourly wage differentially. Because the difference of 1.9 log points is driven by the substantially higher fraction of part-time workers in treated industries compared to control industries. In column (3), I test whether the precision of my findings is robust to an alternative way of clustering standard errors. I believe that the intensity of treatment varies by census metropolitan areas, making it possible that unobserved components of the hourly wage for workers are correlated within those areas. Hence, I implement two-way clustering at the industry and CMA levels. The precision of my results remains unchanged.

In general, one of the potential concerns is that a higher minimum wage induced wage spillovers to the control industries which also responded to the reform by raising their hourly earnings. In this case, the direct effect of the minimum wage on hourly earnings in low-wage industries would be biased downwards by the indirect effect of wage spillovers in high-wage industries. This would yield a treatment estimate lower than the actual wage effect. However, the potential wage spillovers are unlikely to occur and threaten my short-run analysis in Ontario where the average hourly wage in high-wage industries were already 55% higher than the low-wage ones. Even the research which studies the impact of minimum wage changes in 138 states over 37 years (Cengiz et al. 2019) finds only modest wage spillovers.

6.2 The Employment Effects

6.2.1 Intensive Margin

Figure III depicts the event study estimates for β_3 on the log average hours worked at the main job per week (equation 5) for each event/month. The point estimates β_3 trace out how the outcomes in strongly treated industries evolve in comparison to less affected industries by the minimum wage, relative to the pre-policy months. This practice is also crucial to best visualize the parallel trends prior to the reform by plotting the separate treatment effects when “leads” are included in the model. Except October, the line hovers around 0 suggesting that there are no differential dynamics in the outcome variable across groups before treatment takes place. The coefficient β_3 in October might be due to a sectoral labor market shock for low-wage workers whose weekly usual working hours went up relative to high-wage workers on average. I address this issue in my robustness checks (Table IX) by excluding a certain industry (agriculture) from the treatment group to see whether the baseline results still hold. In the treatment month January, working hours per week drop significantly by approximately 7 log points for low-wage workers followed by a sharp increase in the later months. Based on Figure III, one can detect dynamic treatment effects characterized by a gradual change in the outcome variable months after the treatment is introduced.

These findings are aligned with the baseline DiD estimates reported in Table VI. Panel A shows that weekly working hours decrease by 4 to 6 log points for low-wage workers relative to high-wage workers in the post-reform months. To test the sensitivity of my estimates, I include linear and differing time trends as well as industry-specific non-linear time trends in Table VII. All specifications yield negative point estimates at 5% significance level, implying that individuals at heavily affected industries worked less hours per week on average, in comparison to weakly affected industries after the minimum wage raise. My preferred model in column (7) accounts for industry-specific quadratic time trends, allowing each industry follow a non-linear time trend given the differences in market dynamics, elasticity of labor demand and sectoral characteristics. Moreover, I run a separate regression with log additional hours worked per week as my outcome variable in Panel B. The coefficient estimates point toward a similar conclusion of reduced extra hours worked for low-wage industries. I provide the results from a placebo test in column (8) where the treatment month is taken as August.

6.2.2 Extensive Margin

I now investigate how the hike in minimum wage in 2018 affected the employment prospects of individuals working or wish to work at low-wage industries. The cross-industry approach compares the average employment increase or decline induced by the reform through capturing both the potential decline (rise) in hiring and job dismissal across groups. Figure IV visually highlights that parallel trends assumption holds given that the treatment coefficients do not deviate from 0 in the preceding months. As illustrated by the graph, there is indeed a reduction in the number of employees at strongly treated industries in 2018. The adverse employment effect persists over time since the point estimates remain below 0. According to the trend as of January, low-wage industries cut back on employment by more than 10% compared to high-wage industries. This finding suggests that low-wage industries are likely to be characterized by perfect competition with elastic demand.

I document the corresponding DiD estimates based on equation 5 in Panel C. Each column represents a specification with particular covariates, quantifying the log average change in number of employed in strongly treated industries following the reform which brought about a \$2.6 increase in minimum wages in Ontario. I infer from the negative point estimates in column (4-6) that the reform reduced mean employment of workers at industries with low minimum-to-median wage. These estimates for the DiD coefficient of interest exhibit statistical significance at 1% level with R-squared equal to 0.9. Given that I previously identify the positive wage effect, one expects that the employment has become more appealing for low-wage workers driving up the labour supply marginally. However, the results imply that the presence of significant disemployment effect for low-wage workers is mainly driven by firms which respond by reducing the rate at hiring new workers or the rate at retaining existing workers. Coefficient estimates for the placebo period are close to zero, lending additional support to the main identification assumption of parallel trends: in the absence of the reform, there would be no deviations in employment trends across treatment and control group.

In line with the evidence above, Table VIII demonstrates negative point estimates for the DiD regression using the gap measure as an indicator of treatment. The results remain similar at 1% significance level when I account for differential pre-trends in various ways in column (2-4). The estimate from my preferred specification in column (2) indicates that I cannot reject the hypothesis that employment in the heavily exposed industries (with a relatively higher gap measure) declined relative to the less exposed industries at 1% significance level. In other words, a 1 percentage point increase in the gap measure yields a decrease in log number of employed by

2 log points on average. This evidence conforms to the textbook theory of labor market behavior in a competitive setting in which a rise in worker wages renders the firm unprofitable, threatening its survival and growth in the industry.

6.2.3 Robustness Checks

In an attempt to derive a causal inference of a higher minimum wage on employment, Table IX presents some alternative specifications to probe the robustness of my conclusion. Each column reproduces a point estimate of the coefficient β_3 for a particular model, except the first column with the baseline estimate. Column (2) shows estimation result obtained from a subsample that excludes individuals working at agriculture industry while in column (3), the employment is redefined to exclude management employees. The exclusion of agriculture industry is to rule out any possible seasonal patterns different from other industries. These modifications yield similar results to my baseline specification, suggesting a decrease in employment by 15 log points. I restrict my data to workers living in Toronto, Ontario in column (4). This change has no effect on my main outcome either. In column (5), I construct an alternative control group composed of industries with the second highest minimum-to-median wage and lowest gap measure. The employment pre-trends for industry groups are illustrated in Figure V as a prerequisite for a valid DiD design. Despite that the estimated coefficient conforms to the sign of my baseline estimate, its interpretation is not appropriate as the pre-trends differ across both groups, violating the underlying assumption.

One might still be concerned about the possible endogeneity issue which is commonly raised by minimum wage studies (Neumark and Wascher 2006; Sen et al. 2010; Millar et al. 2005; Addison and Blackburn 1999). An empirical method to surmount this issue is an instrumental variable design proposed by Besley and Case (2000). This allows to relax the assumption that amendments to the Bill 148 were exogenous to low-wage industries because certain labor market policies are closely identified with a specific ideology and party which enacts their policies when in power. Likewise in other countries, left wing political parties in Canada possess ideologies that promote social policies more than right wing parties. Hence, if a left leaning party forms a provincial government, it follows that minimum wage hikes are more likely to occur than in provinces or periods governed by right leaning parties. In this respect, an instrument based on the political party in power per province/period is sufficiently valid to avoid any direct correlation with any outcome variable such as employment. Nevertheless, there is no need to empirically employ this strategy based on three main arguments about the exogeneity of this reform. First, the reform came into force during the final year of Wynne's governance. Wynee is known for her unprecedented policies first ones ever to be implemented across Canada such as a universal basic income pilot or a large increase in minimum wages. According to the poles, it was evident that the governing Liberals were losing votes necessary to win the next provincial election. During five years of her governance, Wynne only announced these labor market improvements in a year with unstable political power of her party. Hence, it makes it very plausible that the amendments were not announced to target any economic concern in the province but rather to regain enough support from the lower income households and workers to maintain her weakening popularity. This establishes no correlation between the reform and any outcome variables of interest. Secondly, the incumbent premier Ford referred to the Bill 148 amendments as a “no sign” act putting unanticipated pressure on businesses. This implies that it was an unforeseen policy change with a no clear reason. Lastly, I provide Figure 1 which represents the macroeconomic environment leading up to the reform. As final evidence on the exogeneity of the Bill 148, it depicts a more positive macroeconomic environment in Ontario than the second most populated province, Quebec. This then raises the question of why the hourly minimum wage jumped to $14.00 in Ontario while stayed at $12.00 in Quebec, lending additional support to the exogeneity of the reform.

6.3 Full-time and Part-time Substitution

Throughout my analysis, I have focused on the change in the number of FTE employees without paying attention to the possible changes in the distribution of full- and part-time workers across heavily and weakly affected industries. Since the evidence has so far established that the market behavior of the treated industries complies with the perfect competition, I surmise to find an increase in full-time relative to part-time employment following the hike in minimum wages. One reason is that it induces employers to substitute skilled workers and capital for minimum wage workers in a conventional labor model. Full-time workers in a low-wage industry are typically older and may well possess higher skills due to more hours of working experience than part-time workers (Card and Krueger, 1994). Based on my sample, the mean age of full-time workers in the control industries is 39 years whereas it is 29 for part-time workers. Hence, it is likely that firms respond to the reform by increasing the proportion of full-time workers and reducing the hiring of part-time workers. Before the treatment in 2017, a full-time worker employed in a treated industry receives on average 19 euros per hour while this reduces to 11 euros per hour for a part-time worker. This wage difference reinforces the view that full-time workers are relatively more skilled and productive inducing low-wage firms to pay a higher hourly wage. I test the hypothesis that low-wage firms would naturally want to hire a greater proportion of full-time workers compared to the pre-policy months in Table X. In column (1) and (2), the outcome variable corresponds to the log number of part-time employees and the total number of employees with both full-and part-time included. Column (3) represents the mean changes in the proportion of part-time employees in low-wage industries relative to high-wage industries before and after the policy change. The model accounts for time, industry fixed effects with worker- and industry-level controls. The results illustrate that the fraction of part-time employees in treated group relative to control group increased by 3.7% in the post-reform year. The point estimate is statistically significant at 10% level. Together with the findings in the previous section, I can conclude that low-wage firms dismissed more full-time employees than part-time which is reflected on the positive point estimates in column (1) and (3) while negative point estimates in Table X. As opposed to my intuition, there is a significant shift in the fraction of part-time workers instead of full-time. This shift is also contradicting the result that the wage effect was bigger for part-time workers relative to full-time workers (see Table III) which should suggest that firms bear more costs when increasing the hourly wages of part-time workers after the reform.

As an additional analysis, I show the policy impact on the proportion of teenagers employed in low-wage industries relative to high-wage industries in Table X, column (4). One would expect low-wage firms to first eliminate the least experienced, hence the youngest employees as a response to a minimum wage hike. Contrarily, I detect no significant change in the fraction of teenagers employed in the treated group compared to the control group after the reform since the estimated coefficient is not statistically significant and close to 0.

7. Discussion

My findings indicate that the Bill 148 was a binding reform for low-wage industries, pushing the hourly wages up by more than 2.7 log points relative to high-wage industries. Additionally, the reform induced low-wage industries to cut back on full-time employment without increasing the number of weekly hours worked. The wage and employment effects are interpreted as causal under the parallel trends assumption which implies that in the absence of treatment, two groups of industries would have developed in a similar way. Although one cannot test this assumption directly, I present several pieces of evidence such as adding linear time trends, employing event-study design and explaining the exogeneity of the policy change. Thus, I cannot reject my hypothesis that employment decreases for low-wage workers relative to high-wage workers. Although my evidence on disemployment effects is consistent with the prediction of a perfectly competitive model, it contradicts with the conventional theory because of the findings on a lower number of hours worked per week and a higher share of part-time workers in low-wage industries relative to the pre-reform months. Ultimately, the analysis points toward a decline in the number of only full-time employees following the increase in minimum wages. This is an unexpected and ambiguous result since my hypothesis posits that a higher minimum wage leads to a higher fraction of skilled, full-time workers who work more hours to replace the efforts of part-time workers. One possible explanation for my contrasting findings is that firms might offset the effect of the minimum wage by reducing the share of full-time employees who are entitled to more non-wage compensation or benefits. It could be such that part-time contract was less costly for the firm or full-time employees simply switched to higher-paying industries. The latter possibility would hinder my identification strategy as the control group of high-wage industries would also be affected by the reform. I believe that in a time span of three post-policy months, the reallocation of low-wage workers from low to high-paying industries is unlikely because it would take more time for a low-wage worker to acquire skills and knowledge necessary in order to be employed in a higher-paying industry. In the presence of reallocation effects of a higher minimum wage, I would find no employment effects (Dustmann et al., 2022).

The main limitation of my analysis is that it does not capture the reallocation effects of a higher minimum wage. As suggested by the evidence of lower full-time employees relative to part-time, minimum wage might have led to an increase in the allocational efficiency of workers (Dustmann et al., 2022), thereby improving the quality of establishments in the economy. Another limitation is the design of the available data. Information on firm-level data such as business exits, consumer prices, non-wage compensation would be helpful in explaining the discrepancy between the estimated effects on intensive and extensive margin. Although perfect competition model can provide a potential explanation for the estimated disemployment effects of the Bill 148, it cannot explain the increase in hourly worked as well as the increase in the share of part-time workers. These findings can be rationalized in a context where the minimum wage strongly induces low-wage full-time workers to switch to higher paying industries. This is a hypothesis to be tested in future studies. As one might argue that the short-run analysis fails to fully capture the dynamics of employment response of firms, I would complement my findings with a future study on the long-term effects of the Bill 148. Lastly, with a more detailed dataset on the firm-specific characteristics, I would perform a synthetic control design to empirically determine which industry/firm forms the most appropriate and valid control group. This method is widely encouraged (Neumark et al. 2021) for any cross-unit analysis.

My empirical findings are in line with most literature on the wage effects of a higher minimum wage (Derenoncourt and Montialoux, 2020) while contradicting some recent studies on the employment effects (Cengiz et al., 2019). Using a novel bunching method, Cengiz et al. (2019) concludes that the minimum wage leads to no disemployment effects. Nevertheless, my estimates support the findings on low-wage employment from Baiman et al. (2007) who estimate -0.75 FTE employment elasticity with -0.85 for hours worked. They also match with the study by Campolieti et al. (2006) who find an employment elasticity equal to -0.17 for teenagers in Canada. Most research on the effects of minimum wage employs a difference-in-difference design as in this article.

8. Conclusion

This paper examines the wage and employment effects of a 23% increase in minimum wages in Ontario, implemented by the Bill 148 Act in 2018. As this reform has fueled many political disputes on its potential consequences on employment, I attempt to elucidate the employment effect of a higher minimum wage on low-wage workers by comparing the change in the number of employees across heavily and weakly affected industries before and after the reform, conditional on being employed before the reform. To assess whether the reform was successful at pushing up the wages for workers in lower-paying industries, I employ a difference-in-difference design after plotting the event-study estimates to corroborate the parallel trends assumption. The reform had a significant impact on the hourly wages of low-wage industries. There is some heterogeneity in wage responses across industries with a subsegment of low-wage employees such as the lower educated, teenagers, ones working at big firms experienced a relatively larger increase in their hourly wages. I explore the policy impact on both intensive and extensive margins of labor market response by using the same identification strategy. I find that a higher minimum wage reduced the number of weekly hours worked on average as well as the number of full-time equivalent employees in low-wage industries. Although the evidence on intensive margin response is inconsistent with my expectation, the adverse employment effect can be explained by the standard competitive model with a negative elasticity of labor demand. As I attempt to rationalize the unambiguous disemployment by low-wage firms substituting away from part-time employees, I detect no evidence that the share of part-time workers decreased. I provide a wide variety of alternative specifications to probe the robustness of my conclusion. My employment analysis only captures the disemployment from reduced hiring and job dismissal. Taken as a whole, the results are ambiguous suggesting that policymakers should not use "minimum wage" as part of their propaganda but rather as a tool to understand the market dynamics and labor response of low-wage employers. Although every policy has winners and losers regardless, my analysis falls short to fully cover the minimum wage effects on the economy in Ontario. For future research, one should explore whether there exists a reallocation of the disemployed full-time workers to unemployment or higher-paying industries.

References

Aaronson, D., French, E., and MacDonald, J. (2008). The Minimum Wage, Restaurant Prices, and Labor Market Structure. *Journal of Human Resources*, 43(3), 688–720.

Alvarez, J., Benguria, F., Engbom, N., and Moser, C. (2018). Who Pays for the Minimum Wage? *American Economic Journal: Macroeconomics*, 1(10).149–89.

Bailey, M. J., DiNardo, J., and Stuart, B. A. (2021). The economic impact of a high national minimum wage: Evidence from the 1966 fair labor standards act. *Journal of Labor Economics*, 39(2), 329–367.

Baker, M., Benjamin, D., and Stanger, S. (1999). The Highs and Lows of the Minimum Wage Effect: A Time-Series Cross-Section Study of the Canadian Law. *Journal of Labor Economics*, 17(2), 318–350.

Brown, C., Gilroy, C., and Kohen, A. (1982). The Effect of The Minimum Wage on Employment and Unemployment. *Journal of Economic Literature*, 20(2), 487–528.

Campolieti, M. (2015). Minimum Wages and Wage Spillovers in Canada. *Canadian Public Policy / Analyse de Politiques*, 41(1), 15–34.

Card, D., and Krueger, A. B. (2000). Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Reply. *The American Economic Review*, 90(5), 1397–1420.

Cengiz, D., Dube A., Lindner A., Zipperer B. (2019). The Effect of Minimum Wages on Low-Wage Jobs. *The Quarterly Journal of Economics*, 134(3), 1405–1454

Chamber, O. (n.d.). Bill 148, The Fair Workplaces Better Jobs Act (2017). Retrieved from Ontario Chamber of Commerce: https://occ.ca/bill-148/

Derenoncourt, E. and Montialoux C. (2021). Minimum Wages and Racial Inequality. *The Quarterly Journal of Economics*, 136(1), 169–228.

Dustmann C., Lindner A., Schönberg U., Umkehrer M., Philipp vom Berge. (2022). Reallocation Effects of the Minimum Wage. *The Quarterly Journal of Economics*, 137(1), 267–328.

Maximum insights on minimum wage workers: 20 years of data (2018). Statistics Canada.

Giuliano, L. (2013). Minimum Wage Effects on Employment, Substitution, and the Teenage Labor Supply: Evidence from Personnel Data. *Journal of Labor Economics*, 155-194.

Gruber, J. (1994). The Incidence of Mandated Maternity Benefits. *The American Economic Review*, 84(3), 622–641

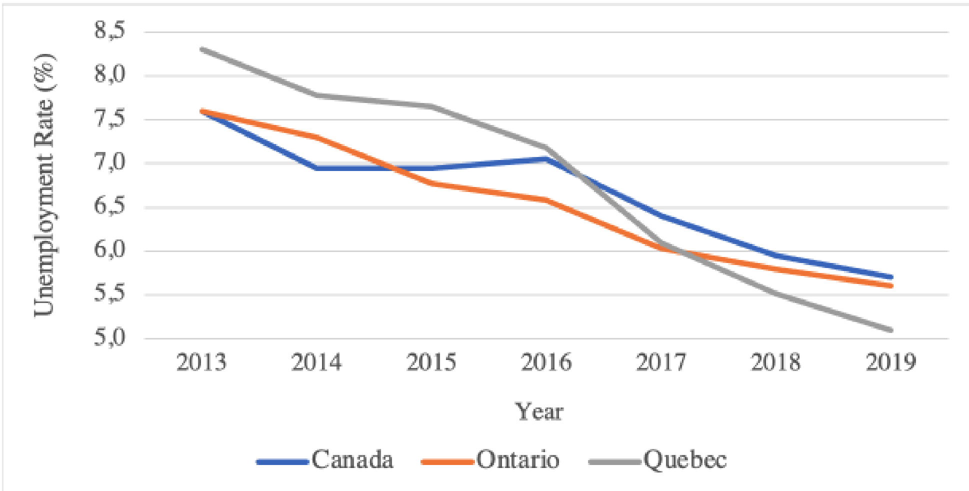
Neumark, D., and Shirley, P. (2021). Myth or Measurement: What Does the New Minimum Wage Research Say about Minimum Wages and Job Loss in the United States? *National Bureau of Economic Research*, 28388.

MaCurdy, T. (2015). How Effective Is the Minimum Wage at Supporting the Poor? *Journal of Political Economy*, 123(2), 497–545.

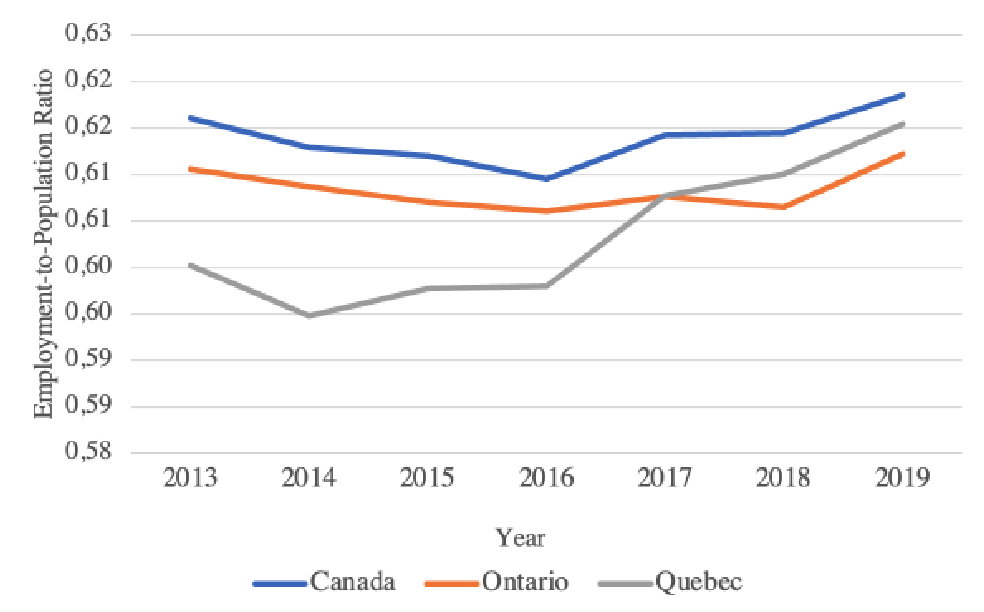
Stigler, G. J. (1946). The Economics of Minimum Wage Legislation. *The American Economic Review*, 36(3), 358–365.

Figure 1. Macroeconomic context of the minimum wage increase (2018)

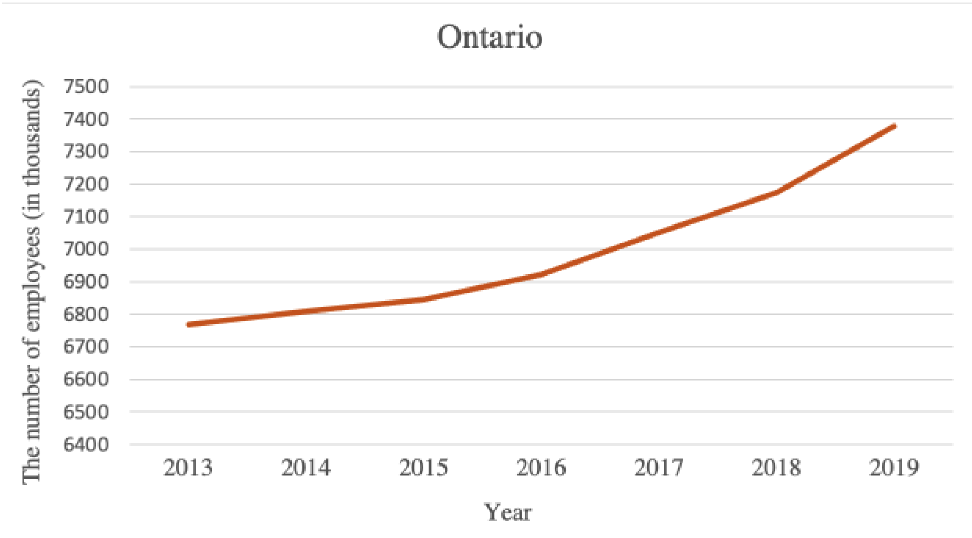
(a) Unemployment rates



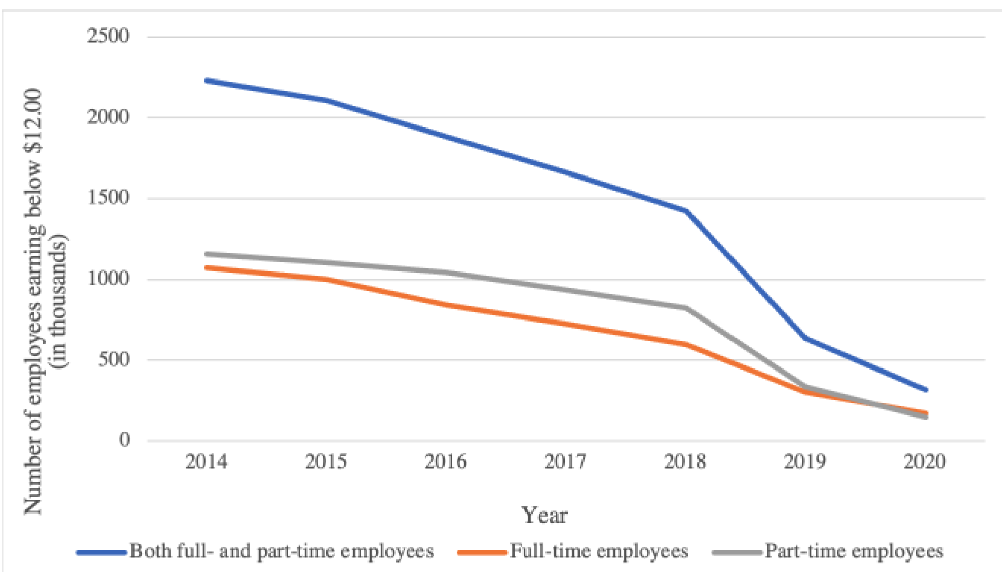
(b) Employment-to-Population ratio



(c) Employment



(d) Hourly wage distribution in Canada



Source: Statistics Canada, Labour Force Survey tabulations

Table I. Summary Statistics

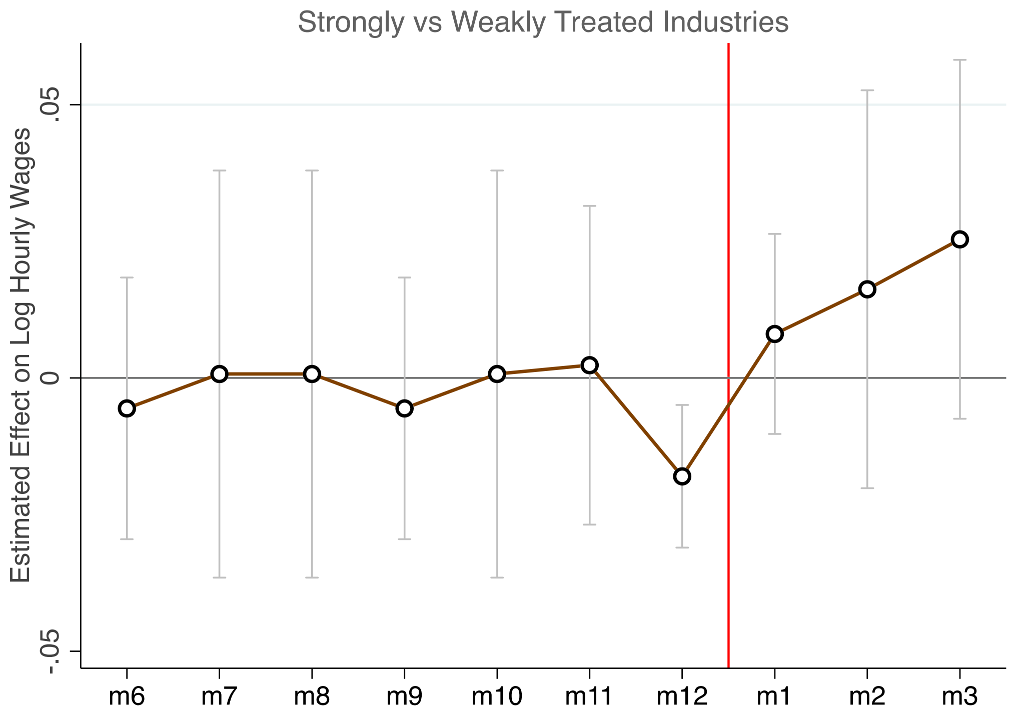
	Control Group	Treatment Group
Age	44.17	36.67
Proportion of teenagers (percentage)	7.41	34.29
Gender		
Male	0.61	0.46
Education		
Less than HS	0.026	0.20
HS Completed	0.12	0.29
College Completed	0.62	0.37
Above College	0.20	0.028
Marital Status		
Never Married	0.21	0.47
Married	0.60	0.36
Widowed	0.013	0.016
Separated	0.030	0.022
Divorced	0.050	0.034
Census Metropolitan Area		
Ottawa-Gatineau	0.10	0.044
Toronto	0.22	0.19
Hamilton	0.051	0.052
Other	0.63	0.71
Unemployment rate	4.5	2.3
If employed;		
Full-time/part-time status		
Full-time	0.87	0.60
Part-time	0.13	0.40
Union membership		
Covered by a union contract	0.57	0.11

Not covered	0.43	0.89
Firm size		
<100 employees	0.12	0.36
100<x<500 employees	0.12	0.10
x>500 employees	0.76	0.54
Average hourly earnings (in $2022)	$41.12	$18.21
Usual hours worked per week at the main job	35.2 hours	32.8 hours
Job tenure with the current employer (months)	120	70
Number of employees (full-time and part-time)	23,656	26,507
Job permanency		
Permanent	0.84	0.87
Temporary	0.16	0.13
GAP	0.0047	0.077
Observations	23,656	26,507

Source: LFS June 2017-March 2018

Notes: Sample is individuals over 17 years old, not self-employed or unpaid family worker. The data is averaged over 10 months. Column (1)-(2) display the mean values of each demographic variable and labor force characteristics for weakly treated (control) and strongly treated (treatment) industries. Average hourly earnings are in $2022, adjusted according to the inflation rate since 2017. The data on average hourly earnings are averaged over 7 months in 2017, to avoid the effect of the increase in 2018 on the reported hourly wages.

Figure II. Event-study for the wage effect



Notes: This plot is generated by estimating separate treatment effects for each month using the difference-in-difference equation (1). Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker. The regression uses a cross-industry design and controls for gender, age, years of schooling, working experience, union membership, permanency of the job, firm size, full-time/part-time status, number of weeks and hours worked, occupation, and marital status. Includes industry and time fixed effects. Standard errors are clustered at the industry level. Annual earnings are in $2022, deflated using the inflation rates available online.

Table II. Wage Effect: Main Result

Cross-Industry	(1)	(2)	(3)	(4)
Strongly Treated$_j$ x Post$_m$	0.0468***	0.0461	0.0639	0.0635*
	(0.00824)	(0.0169)	(0.0133)	(0.0134)
Controls	No	No	Yes	Yes
Time FE	No	Yes	Yes	Yes
Industry FE	No	No	No	Yes
Province/City FE	No	No	No	Yes
Industry-year FE	No	No	No	Yes
Observations	50,163	50,163	50,163	50,163
R-squared	0.410	0.411	0.631	0.633

Notes: Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker, with no missing industry and hourly wage data. The sample has 50,163 observations. The regression is a difference-in-difference design. The dependent variable is log hourly wages. The variable of interest denotes the treated industries in year 2018. Column (1) includes no covariates. Column (2) includes month dummies, Column (3) further includes controls for gender, age, immigration status, years of schooling, marital status and working experience. Column (4) additionally includes industry, metropolitan and industry-year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table II. The Wage Effect: Main Result with Time Trends

Cross-Industry	(5)	(6)	(7)
Strongly Treated$_j$ x Post$_m$	0.0678**	0.0735***	0.0507***
	(0.0147)	(0.00871)	(0.0379)
Controls	Yes	Yes	Yes
Time FE	No	No	No
Industry FE	Yes	Yes	Yes
Province/City FE	Yes	Yes	Yes
Linear time trends	Yes	No	No
Differing linear time trends	No	Yes	No
Industry-specific time trends	No	No	Yes
Observations	50,163	50,163	50,163
R-squared	0.633	0.633	0.633

Notes: Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker, with no missing industry and hourly wage data. The sample has 50,163 observations. The regression is a difference-in-difference design. The dependent variable is log hourly wages. The variable of interest denotes the treated industries in year 2018. Column (1) and (2) includes linear time trends, and differing time trends for each group without any time/month dummies. Column (3) replaces the linear and differing time trends with industry-specific time trends which are generated by the interaction term between time trend and each industry. All specifications account for industry and metropolitan fixed effects, and worker-level controls: gender, age, immigration status, years of schooling, marital status and working experience. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table III. The Wage Effect: Heterogeneity Analysis

	Teenagers	Low-Educated	Firm Size	Gender
Cross-Industry	(1)	(2)	(3)	(4)
Strongly Treated$_j$ x Post$_m$	0.0135	-0.0469**	0.0868**	0.0427*
	(0.0116)	(0.0164)	(0.0299)	(0.0211)
Strongly Treated$_j$ x Post$_m$ x Dummy	0.131***	0.196***	0.0987**	-0.0472
	(0.0337)	(0.0314)	(0.0298)	(0.0320)
Post$_m$ x Dummy	-0.055	-0.0846*	0.0209	0.0209
	(0.0313)	(0.0347)	(0.0110)	(0.0198)
Controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Observations	50,163	50,163	50,163	50,163
R-squared	0.340	0.353	0.343	0.345

Notes: The methodology used to assess the heterogeneity in wage effect is a triple difference-in-difference design. Conditional on being employed, I test whether the wage response of treated groups differs for a particular subsegment of low-wage workers relative to high-wage workers. Row 2 presents the variable of interest, which is an interaction term indicating the subsegment of workers in the treated industries in the post-reform year. Column (1) includes an age dummy which is 1 if the employee is below 20 years-old. Column (2) includes an education dummy which is 1 if the employee has an education level below post-secondary school. Column (3) includes a firm dummy which is 1 if the employee works at a firm with more than 500 employees in total. Column (4) includes a gender dummy which gets 1 if the employee is male. The sample has no missing data on industry, and hourly wages. The sample only includes individuals who are not self-employed or unpaid family worker. All specifications account for time, industry and metropolitan fixed effects, and worker-level controls excluding the characteristics contained in the dummy variable. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table IV. The Wage Effect: GAP measure

Cross-Industry	(1)	(2)	(3)
GAP_j x $Post_m$	0.545***	0.534***	1.999***
	(0.267)	(0.0581)	(0.399)
Industry FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Industry baseline characteristics interacted with linear time trend	No	No	Yes
Industry-specific time trends	No	Yes	No
Observations	140,357	140,357	140,357
R-squared	0.496	0.497	0.575

Notes: The sample includes all industries with their corresponding gap measures. The sample has 140,357 observations in total. The dependent variable remains log hourly wages for an employee. The gap variable measures the proportional increase in average hourly wages for an industry to meet the new minimum rate. It quantifies the extent of which an industry is exposed to the new minimum wage. All models include industry and time fixed effects. Column (2) further contains industry-specific time trends which are generated by interacting each industry dummy with the time trend. Column (3) extends the model by instead including interaction term between linear time trends and industry baseline characteristics. These industry-baseline characteristics are firm size, establishment size and occupation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table V. The Wage Effect: Robustness Checks

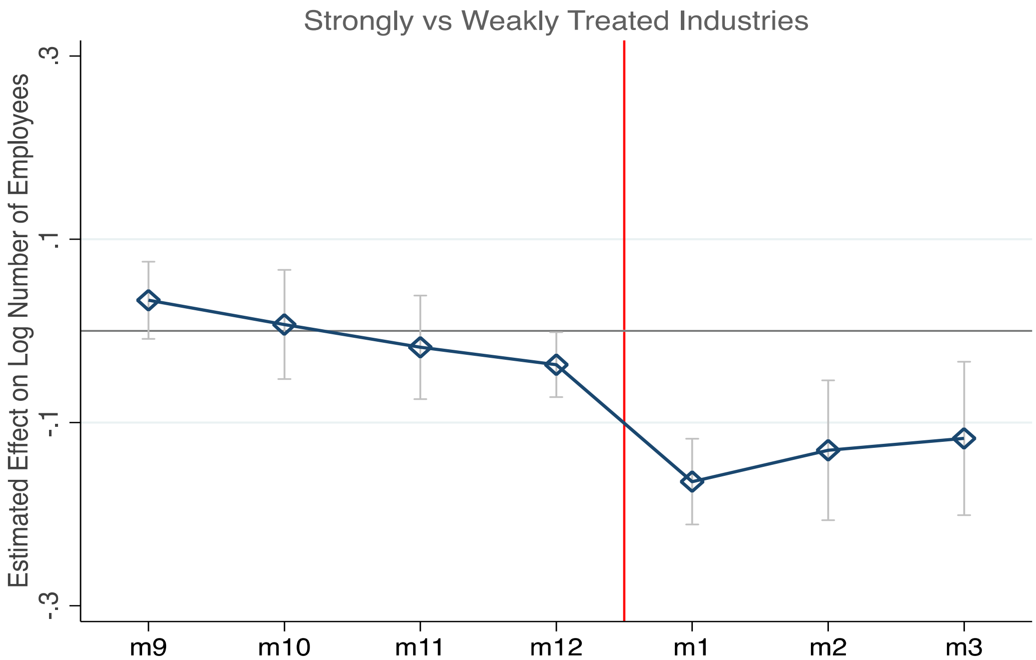
Cross-Industry	(1)	(2)	(3)	(4)
Strongly Treated$_j$ x Post$_m$	0.0665***	0.0855***	0.0638***	0.0638***
	(0.0153)	(0.0177)	(0.0029)	(0.0031)
Controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Province/City FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Part-time only	No	Yes	No	No
Full-time only	Yes	No	No	No
Two-way clusters	No	No	Yes	No
W/out agriculture	No	No	No	Yes
Observations	36,374	13,789	50,163	49,273
R-squared	0.478	0.528	0.552	0.640

Notes: Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker, with no missing industry and hourly wage data. The regression is a difference-in-difference design. The dependent variable is log hourly wages. The variable of interest in each panel denotes the change in the dependent variable for treated industries in year 2018. Column (1) and (2) restricts the sample to only full-time and part-time workers respectively. The model in Column (3) two-way clusters standard errors at the industry and individual level. Column (4) excludes data on workers in agriculture industry to eliminate any possible sectoral shocks. All specifications account for month, industry and metropolitan dummies, industry-year fixed effects and worker-level characteristics: gender, age, immigration status, years of schooling, marital status and working experience. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure III. Event-study for the employment effect on an intensive margin

Notes: This event-study plot is generated by estimating separate treatment effects for each month using the difference-in-difference equation (5). Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker with no missing industry. The dependent variable is log number of hours worked at the main job per week. The regression uses a cross-industry design and controls for age, gender, immigration, marital status, years of schooling, union membership, working experience, job permanency, firm size, occupation, public/private sector, hourly wages earned, full-time/part-time status, and occupation. Includes industry, time and metropolitan fixed effects. Standard errors are clustered at the industry level.

Figure IV. Event-study for the employment effect on an extensive margin



Notes: This event-study plot is generated by estimating separate treatment effects for each month using the difference-in-difference equation (5). Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker with no missing industry. The dependent variable is log number of full-time equivalent employees per the group of industries. The regression uses a cross-industry design and controls for age, gender, immigration, marital status, years of schooling, union membership, working experience, job permanency, firm size, occupation, public/private sector, hourly wages earned, and occupation. Includes industry, time and metropolitan fixed effects. Standard errors are clustered at the industry level.

Table VI. The Employment Effect: Main Result and Placebo Test

	June 2017 – March 2018			
Cross-Industry	(1)	(2)	(3)	(4)
Panel A: Actual hours worked at the main job				
Strongly Treated$_j$ x Post$_m$	-0.0606***	-0.0656*	-0.0342***	-0.0354**
	(0.0199)	(0.0300)	(0.0106)	(0.00889)
Observations	50,163	50,163	50,163	50,163
R-squared	0.0485	0.0535	0.554	0.552
Panel B: Extra hours worked				
Strongly Treated$_j$ x Post$_m$	-0.0887**	-0.0771*	-0.0574*	-0.0482
	(0.0239)	(0.0370)	(0.0282)	(0.0298)
Observations	50,163	50,163	50,163	50,163
R-squared	0.059	0.060	0.132	0.140
Panel C: Employment				
Strongly Treated$_j$ x Post$_m$	-0.117***	-0.149***	-0.157***	-0.157***
	(0.0165)	(0.0234)	(0.0212)	(0.0238)
Observations	50,163	50,163	50,163	50,163
R-squared	0.166	0.184	0.129	0.922
Controls	No	No	Yes	Yes
Time FE	No	Yes	Yes	Yes
Industry FE	No	No	No	Yes
Province/City FE	No	No	No	Yes
Industry-year FE	No	No	No	Yes

Notes: Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker, with no missing industry data. The sample has 50,163 observations. The regression is a difference-in-difference design. The variable of interest in each panel denotes the change in the dependent variable for treated industries in year 2018. The dependent variable in Panel A is the log number of weekly working hours per an individual in a treated or control industry. In Panel B, the dependent variable denotes the change in extra number of hours worked per a treated or control worker. The outcome variable becomes log number of full-time equivalent employees per a group of industries. The models in Column (1) do not include any covariates. Month dummies (time fixed effects) are then added to the models in each panel in Column (2) followed by worker-level controls in Column (3). These individual-level characteristics refer to a worker's age, gender, immigration, marital status, years of schooling, union membership, working experience, job permanency, firm size, occupation, public/private sector, hourly wages earned, and occupation. In the main specification in Column (4), additional covariates of metropolitan, industry and industry-year fixed effects are included. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table VII. The Employment Effect: Main Result with Time Trends

Cross-Industry	(5)	(6)	(7)	(8)
Panel A: Actual hours worked at the main job				Placebo
Strongly Treated$_j$ x Post$_m$	-0.0354**	-0.0956***	-0.0468***	0.0024
	(0.00887)	(0.0179)	(0.00311)	(0.0017)
Observations	50,163	50,163	50,163	50,163
R-squared	0.552	0.552	0.552	0.0027
Panel B: Extra hours worked				
Strongly Treated$_j$ x Post$_m$	-0.0913**	-0.0868*	-0.0923***	-0.0015
	(0.0251)	(0.0374)	(0.0107)	(0.00090)
Observations	50,163	50,163	50,163	50,163
R-squared	0.139	0.139	0.139	0.115
Panel C: Employment (log number of employed)				
Strongly Treated$_j$ x Post$_m$	-0.139**	-0.243***	-0.168***	0.00016
	(0.0423)	(0.0347)	(0.00311)	(0.0014)
Observations	50,163	50,163	50,163	50,163
R-squared	0.913	0.916	0.552	0.008
Controls	Yes	Yes	Yes	Yes
Time FE	No	No	No	Yes
Industry FE	Yes	Yes	Yes	Yes
Province/City FE	Yes	Yes	Yes	Yes
Linear time trends	Yes	No	No	No
Differing linear time trends	No	Yes	No	No
Industry-specific flexible time trends	No	No	Yes	No

Notes: Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker, with no missing industry data. The sample has 50,163 observations. The regression is a difference-in-difference design. The variable of interest in each panel denotes the change in the dependent variable for treated industries in year 2018. The dependent variable in Panel A is the log number of weekly working hours per an individual in a treated or control industry. In Panel B, the dependent variable denotes the change in extra number of hours worked per a treated or control worker. The outcome variable becomes log number of full-time equivalent employees per a group of industries. In Column (1), month dummies are replaced by linear time trends. In Column (2), linear time trends are replaced by differing linear time trends for each group. In Column (3), I instead include industry-specific non-linear time trends. These are generated from an interaction term between each industry dummy and squared time trends. All specifications include industry, metropolitan fixed effects and individual-level controls: age, gender, immigration, marital status, years of schooling, union membership, working experience, job permanency, firm size, occupation, public/private sector, hourly wages earned, and occupation. In Column (5), the model is the same as Column (4) except that the treatment period is defined as August 2017. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table VIII. The Employment Effect: GAP measure

	Log Number of FTE Employees			
	(1)	(2)	(3)	(4)
GAP_j x $Post_m$	-2.135***	-2.045***	-2.059***	-2.059***
	(0.607)	(0.568)	(0.537)	(0.583)
Observations	140,357	140,357	140,357	140,357
R-squared	0.993	0.994	0.993	0.994
Industry FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry baseline characteristics interacted with linear time trend	No	No	Yes	No
Industry-specific time trends	No	Yes	No	No
Industry baseline characteristics interacted with month fixed effects	No	No	No	Yes

Notes: The sample includes all industries with their corresponding gap measures. The sample has 140,357 observations in total. The dependent variable is log number of full-time equivalent employees per industry. The gap variable measures the proportional increase in average hourly wages for an industry to meet the new minimum rate. It quantifies the extent of which an industry is exposed to the new minimum wage. All models include industry and time fixed effects. Column (2) further contains industry-specific time trends which are generated by interacting each industry dummy with the time trend. Column (3) extends the model by instead including interaction term between linear time trends and industry baseline characteristics. Column (4) instead includes industry baseline characteristics interacted with month dummies. These industry-baseline characteristics are firm size, establishment size and occupation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table IX. The Employment Effect: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline Specification	Excluding agriculture	Excluding management occupations	Only industries in Toronto	A different control group	Only full-time	Only part-time
Strongly $Treated_j$ x $Post_m$	-0.157***	-0.147***	-0.156***	-0.156***	-0.262	-0.162***	-0.140***
	(0.0238)	(0.0189)	(0.0236)	(0.0160)	(0.0673)	(0.0286)	(0.0159)
Observations	50,163	49,273	46,503	10,267	95,940	36,374	13,789
R-squared	0.922	0.847	0.922	0.878	0.993	0.916	0.942

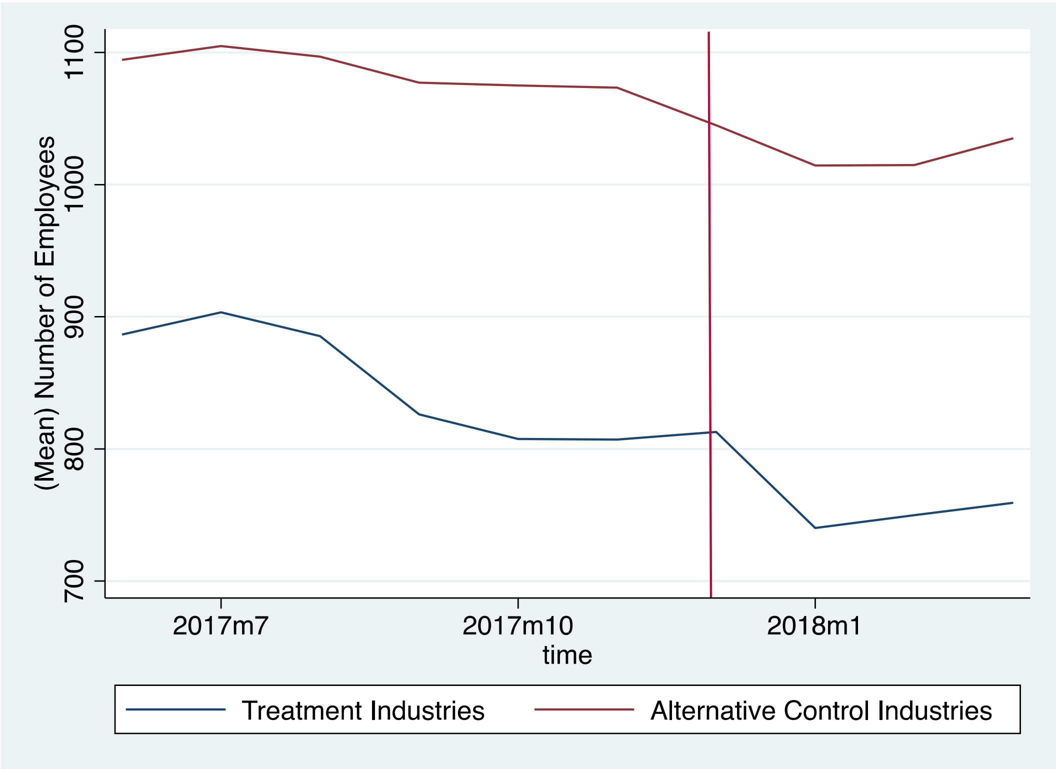
Notes: Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker, with no missing industry and hourly wage data. The regression is a difference-in-difference design. The dependent variable is log number of full-time equivalent employees per industry. The variable of interest in each panel denotes the change in the dependent variable for treated industries in year 2018. Column (1) presents the estimate from my benchmark specification. Column (2) and (3) exclude workers in agriculture industry and with management occupations from the sample respectively. Colum (4) includes industries that are located in Toronto, Ontario. Column (5) uses an alternative control group composed of industries with the second lowest gap measure after the main control industries. The sample is restricted to only full-time and part-time workers in Column (6) and (7) respectively. All specifications account for month, industry and metropolitan dummies, industry-year fixed effects and worker-level characteristics: gender, age, immigration status, years of schooling, marital status and working experience. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table X. The Employment Effect: Part-time and Full-time substitution

	(1)	(2)	(3)	(4)
	Log number of part-time employees	Log number of total employees	The proportion of part-time employees	The proportion of teenagers employed
Strongly Treated$_j$ x Post$_m$	0.0535	-0.0891**	0.037*	0.00098
	(0.120)	(0.0319)	(0.0625)	(0.00419)
Observations	50,163	50,163	50,163	49,596
R-squared	0.988	0.947	0.983	0.819

Notes: Conditional on being employed, the sample is individuals over 17 years old, not self-employed or unpaid family worker, with no missing industry and hourly wage data. The regression is a difference-in-difference design. In Column (1), the dependent variable is log number of part-time employees per industry. In Column (2), the dependent variable becomes log number of total employees including both full-and part-time workers. In Column (3), the dependent variable is the proportion of part-time employees relative to full-time employees. In Column (4), the dependent variable is the number of employees below 20 years old divided by total number of employees of all ages. The variable of interest measures the change in the dependent variables for the treatment group relative to the control group before and after the reform. All specifications account for month, industry and metropolitan dummies, industry-year fixed effects and worker-level characteristics: gender, age, immigration status, years of schooling, marital status and working experience. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure V. Average employment in the treatment and alternative control industries



Notes: The trends show the evolution of the average employment for two different group of industries. Employment is measured by the number of employees (both part-and full-time). The treatment industries consist of agriculture, retail trade and accommodation and food services. I construct an alternative control group from mining, utilities, construction, manufacturing. These groups are determined based on their minimum-to-median wages and the calculated gap measures. The treatment takes place in the first month of 2018, January. The sample has 95,940 observations in total.

Appendix

Table A.I. GAP values

Industry	GAP measure
Agriculture	0.0482
Forestry and logging and support activities for forestry	0.00421
Mining, quarrying, and oil and gas extraction	0.00173
Utilities	0.00146
Construction	0.00302
Manufacturing - durable goods	0.00355
Manufacturing - non-durable goods	0.00863
Wholesale trade	0.00879
Retail trade	0.0589
Transportation and warehousing	0.0117
Finance and insurance	0.00446
Real estate and rental and leasing	0.0211
Professional, scientific and technical services	0.00432
Business, building and other support services	0.0342
Educational services	0.00613
Health care and social assistance	0.00396
Information, culture and recreation	0.0269
Accommodation and food services	0.109
Other services (except public administration)	0.0213
Public administration	0.00274

Notes: The table displays the gap measure for each industry available in LFS data. The measures are averaged over 2017. A greater gap measure indicates that the industry must increase its average hourly wages to meet the new minimum wage rate relatively more.