On the Employment Effect of Carbon Taxes

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Abstract

Do the effects of tax policies on unemployment differ across educational groups? To answer this question, I exploit the introduction of a carbon tax in British Columbia, Canada, to identify its causal impact on unemployment. I find no employment effect on high-educated workers, but a substantial increase in the unemployment rate for medium- and low-educated workers, both of whom engage in the manufacturing industries that are more energy-intensive, relative to high-educated workers. Evidence indicates that some of the low-educated unemployed might have shifted to low energy-intensive manufacturing industries, leading to labor reallocation. Meanwhile, the policy lengthens the average unemployment duration of the low-educated by 3.9 weeks. Some of them might become discouraged workers, lowering their labor force participation rate by 3.0 percentage points in the long-term. Whereas the distributional effects of tax policies are largely neglected in the literature, this paper suggests that the heterogeneity in the effects across educational groups could be considerable.

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1 Introduction

An extensive literature examines the effects of environmental policies on manufacturing employment and overall employment level.[1] However, the distributional effects across demographic groups and the mechanisms through which these policies affect the labor market are in large part neglected in the literature. This article investigates the underlying question: *how and why do the impacts of revenue-neutral carbon taxes on labor market differ across educational groups?*[2] Answering this question provides additional sets of insights on the distributional effect of carbon taxes as a policy tool and the set of facts that enhance our understanding of the functioning of the labor market.

First, this paper investigates the impact of revenue-neutral carbon taxes on employment at its extensive margin and intensive margin. Often, tax policies, including carbon taxes, are opposed by the public and policymakers because of concern over job loss. Examining the effects will aid in understanding the effect of tax policies on job loss at both the extensive and intensive margin. In addition, this article examines whether such employment effect differs across educational groups. This question is important for policy because the public and policymakers are probably concerned whether policies were implemented at the expense of the poor (who are likely low-educated workers).[3] While Chan and Yip (2017) show that the distribution of educational attainment is a key determinant of the unemployment distribution across demographic groups, answering the questions on the heterogeneity in the effects across educational groups enhances our understanding of the mechanism through which tax policies affect the unemployment distribution.

Second, this paper explores who experiences labor reallocation and which industries unemployed workers reallocate to. Yamazaki (2017) finds that there is an adverse employment effect on manufacturing industries and an increase in the overall employment level as a result of a revenue-neutral carbon tax. This likely occurs as a result of labor reallocation: unaffected

[1] For example, manufacturing employment is found to decline substantially under the Clean Air Act (Greenstone, 2002; Walker, 2011) and a cap-and-trade program (Curtis, 2014), namely the NO_x Budget Trading Program. Meanwhile, Berman and Bui (2001) and Martin et al. (2014) find no support that the reduction in manufacturing employment is large under stringent environmental regulations in Los Angeles, the United States, or a higher carbon tax in the United Kingdom. While the literature tends to suggest that environmental policies depress manufacturing employment (Greenstone, 2002; Walker, 2011; Curtis, 2014; Yamazaki, 2017), the overall employment level is found to increase under revenue-neutral carbon taxes in Yamazaki (2017).

[2] Revenue-neutral carbon taxes recycle taxes on polluting activities to lower other taxes. This policy is one of the preferred policy choices in, for example, British Columbia, Canada, and Switzerland. Readers who are interested in the literature on "double dividend hypothesis" of revenue-neutral carbon taxes are referred to Parry and Bento (2000), Manresa and Sancho (2005), Chiroleu-Assouline and Fodha (2006), Bento and Jacobsen (2007), Dissou and Sun (2013), and Williams et al. (2015).

[3] An extensive literature expresses concerns on environmental injustice: any environmental regulation might shift pollution into regions with more poor and minority population (Banzhaf and Walsh, 2008; Kaswan, 2008; Gamper-Rabindran and Timmins, 2011).

industries might absorb some of the unemployed from the manufacturing industries. Since reallocation costs could be significant, as found in Walker (2013), this paper examines who bears the reallocation cost and which industries unemployed workers reallocate to.

Qualitative predictions from economic theory about the impact of revenue-neutral carbon taxes on employment might be ambiguous. On the one hand, a carbon tax increases firms' cost and lowers their profit, decreasing labor demand. On the other hand, a revenue-neutral policy, unlike a simple carbon tax, redistributes revenues: the increase in tax revenue from the carbon tax is offset by tax cuts to other taxes on individuals, families, and business. This tax cut may increase the firms' profit and wages, which in turn increases labor demand and supply, respectively. Since the theoretical prediction of a revenue-neutral carbon tax on employment is unclear, this leaves economists an empirical question on how revenue-neutral carbon taxes affect employment.[4]

I therefore exploit the introduction of a revenue-neutral carbon tax in British Columbia (BC), namely the Carbon Tax Shift (CTS) reform, to identify its causal impact on employment. In particular, I adopt a difference-in-differences (DID) approach to capture the causal effect of the reform. The reform is unique in North America because of the revenue neutrality of the tax and is suitable for a policy evaluation on employment because it provides numerous control groups that are probably unaffected by the reform. Unlike the nationwide program in Switzerland, the CTS reform was implemented only in one Canadian province. Other provinces could, therefore, serve as control groups that are likely to share a similar economic environment but are unaffected by the reform.[5]

Instead of using industry- or plant-level data as in the literature, this paper utilizes *monthly individual-level* data from the Canadian Labor Force Survey. The detailed information on individual background characteristics allows us to investigate the impact across educational groups. This exercise not only directly speaks to who are affected but could also be relevant to policymakers who decide how to redistribute the tax revenue in the future. Moreover, the individual microdata allows us to estimate the impact on both employment and other labor market outcomes, providing a complete picture of the channel through which the reform affects labor market activities.

Several interesting findings emerge from my analysis. First, this paper provides the first piece of evidence on the adverse effect of revenue-neutral carbon taxes on the overall unem-

[4]A more rigorous theoretical foundation on the employment effect of revenue-neutral carbon taxes can be found in Sun and Yip (2017).

[5]One might be concerned with the small number of control provinces in the inference. The standard errors are clustered at the level of age, marital status, and province, providing us with sufficient number of clusters. Furthermore, I estimate the standard errors using a bootstrap procedure proposed by Cameron et al. (2008) to resolve the problem. More details follow in Section 3.

ployment rate. Estimates suggest a strong, negative, and robust effect of the reform on the overall unemployment rate, amounting to 1.8 percentage points. Meanwhile, I find that the reform lengthens both the unemployment spell and job tenure and has no effect on the wage, suggesting that the increase in the overall unemployment rate is mainly driven by a decline in the hiring rate rather than the increase in the separation rate. This paper confirms that, under more stringent environmental policies, the overall unemployment rate (not just the rate in affected industries) increases and such increase is mainly attributed to the decline in the hiring rate.[6]

Second, the responses are heterogeneous across industries. This paper reveals that the adverse employment effects are homogeneous amongst low energy-intensive industries but more pronounced in high energy-intensive industries, both of which are consistent with the taxation structure. For example, in the first two years after the reform, workers in the utilities industry and high energy-intensive non-durable manufacturing industry, respectively, experience an increase of 5.4 and 4.9 percentage points in the unemployment rate, more than doubling the average adverse employment effect. Hence, the first two findings confirm that environmental policies like revenue-neutral carbon taxes increase the unemployment rate in both the manufacturing industry and the economy as a whole.

Third, this article is the first to document the heterogeneity in the employment effects of environmental policies across educational groups. This study finds no evidence of an effect on the high-educated workers; meanwhile, medium- and low-educated workers are found to experience a rise in the unemployment rate by 1.6 and 2.8 percentage points in the short-term. Moreover, I find that, relative to the high-educated, the medium- and the low-educated engage in manufacturing industries that are 26% and 33% more energy-intensive, partly explaining the heterogeneity in the employment effect. Also, evidence indicates that some of the low-educated may shift from high energy-intensive manufacturing industries to their low energy-intensive counterparts, leading to labor reallocation. While Walker (2013) shows that such reallocation costs can be significant, this paper adds to the literature by demonstrating that it is the low-educated that bear the reallocation costs.

Fourth, I find that the adverse employment effect is persistent. While the negative employment effect on the medium- and the low-educated persists at 1.6 and 2.8-2.9 percentage points in the first five years of the reform, the unemployment rates restore to the pre-reform level from the sixth year onwards. Moreover, this paper is the first to show that some of the low-educated

[6]This finding coheres with Brochu and Green (2013), in which the increases in the overall unemployment rate under higher minimum wage regimes are found to be attributed to the reduction in the hiring rate. Furthermore, this result provides support to the recent business cycle literature (Petrongolo and Pissarides, 2008; Elsby et al., 2009; Shimer, 2012), which finds that the variation in the unemployment rate is mainly attributed to the variation in the hiring rate, not the separation rate.

unemployed might become discouraged, lowering their labor force participation rate by 3.0 percentage points in the long-term (between the third and the fifth year of the policy). Hence, it is possible that the unemployment rate of low-educated workers would have increased by more than 2.9 percentage points if the discouraged workers had stayed in the labor market in the long-term.

Many studies document the benefit of the CTS reform. For example, per capita fuel consumption is found to decrease by 19 % relative to the rest of Canada during 2008-2012 (Elgie and McClay, 2013; Murray and Rivers, 2015). Furthermore, Elgie and McClay (2013) show that the GHG emissions per capita drop by 9% during the same period, providing evidence for the improvement of the environment. To the best of my knowledge, Yamazaki (2017) is the only work that attempts to estimate the causal employment effect of the reform. Using yearly industry-level data, he finds that, despite the rise in the unemployment rate in high energy-intensive industries, the overall unemployment rate decreases by two percentage points. However, my replication exercise uncovers that his result is at best biased.[7] In contrast, my results are obtained using the correct definition of the post-reform period (i.e., the period after July 1, 2008), consistent with the effects on other labor market outcomes (shown in Appendix A), and robust to the estimation window (shown in Appendix B), the control group (shown in Appendix C), the sample of examination (shown in Appendix D), and the model specification (shown in Section 4). Of paramount importance, I provide evidence on the validity of the underlying assumptions of the estimation method, which will be discussed in great detail in Appendix B.

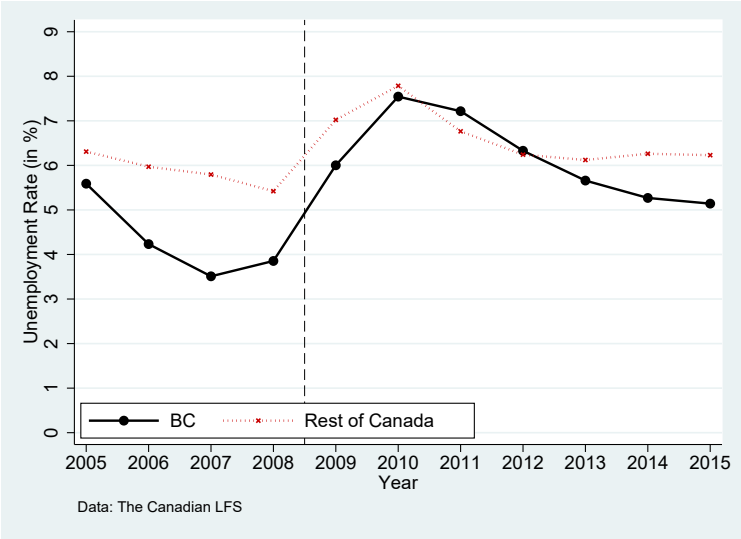
This paper proceeds as follows. Section 2 discusses the details of the CTS reform. Section 3 describes identification methods and summarizes the data. Section 4 presents empirical findings. Section 5 concludes the paper. Appendix A presents other impacts in the labor market and is followed by a series of robustness checks in Appendix B. Appendix C presents the replication results as in Yamazaki (2017). Appendix D presents similar analyses using the female data.

2 The Carbon Tax Shift Reform in British Columbia

The Ministry of Finance officially announced on February 19, 2008, that a carbon tax would be imposed on July 1, 2008, in BC, about 4.5 months after the announcement. Such a carbon tax is based on greenhouse gas emissions generated from burning fuels. The taxes were applied to the consumption of fossil fuels in BC (households and industries). Since the reform was announced a few months before its implementation, the anticipation effect is expected to be insignificant (which is shown in Appendix B). It allows our estimation to fully capture its causal effect in the

[7] I will explain in details why neither his choices of the post-reform period, the estimation window, nor the control group is defined properly in Appendix C.

Figure 1: The Unemployment Rate Dynamics in BC and the Rest of Canada 2004-2015



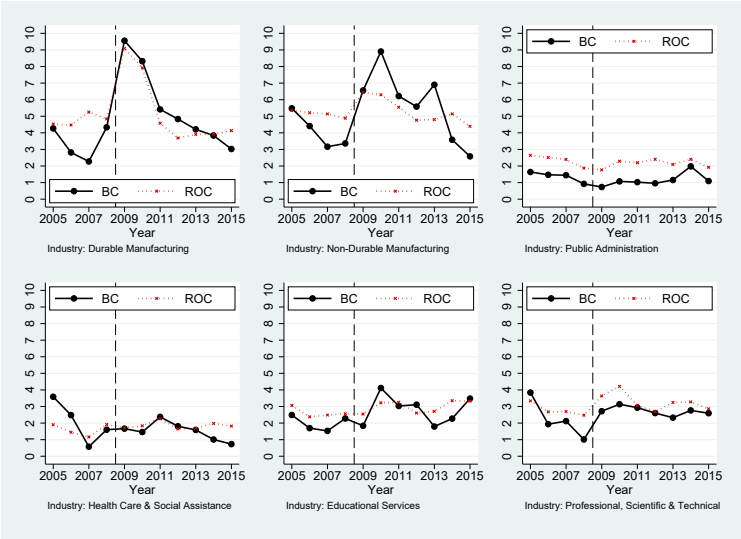
Notes: Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to male labor market participants aged 25-54. Each dot represents the unemployment rate in the corresponding year. For example, the first dot represents the unemployment rate during July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008).

labor market.

The policy is revenue-neutral so that all carbon tax revenues are returned to residents and firms by the reductions in corporate taxes, personal income taxes, and lump-sum transfers.[8] This feature distinguishes my study from other prior works on the impact of a simple carbon tax. The initial carbon tax rate of the reform was $10 per tonne of CO2-equivalent emissions in 2008, and the rate increased by $5 per tonne annually until reaching $30 per tonne of CO_2 on July 1, 2012. Notice that the initial carbon taxes were only $3.5, $0.04, and $5 per tonne of CO2-equivalent in Quebec in 2007, the San Francisco Bay Area, California, in 2008, and Maryland in 2010, respectively. Compared to these other rates in North America, the initial increase in the BC's carbon tax rate was sufficiently high to provide a signal to fossil fuel users so that industries could optimize with respect to the incentives created by the tax reform with little concern on the tax salience (Chetty et al., 2009). In fact, Rivers and Schaufele (2015a) find that the reform reduced carbon dioxide emissions from gasoline consumption by 2.4 million tonnes, suggesting

[8] Elgie and McClay (2013) give the details in the distribution of the tax revenues. For example, the carbon tax revenues were used to lower the corporate income tax rate and the two lowest personal income tax rates by 5%. Meanwhile, the government funds a low-income tax credit and a rebate of up to $200 for northern and rural homeowners. In fact, the government returned $318 million more in income tax cuts than it collected to date in 2012. Therefore, the documented adverse employment effect in this paper is expected to be larger if the refunds do meet the tax revenues in amount.

Figure 2: The Unemployment Rate Dynamics by Industry in BC and the Rest of Canada



Notes: Data are from the Canadian LFS July 2004-June 2015. I restrict the samples to male labor market participants aged 25-54. Each dot represents the unemployment rate in the corresponding industry in a given year. For example, the first dot represents the unemployment rate during July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008).

that industries did respond to the reform.

Figure 1 displays the overall unemployment rate in BC and the rest of Canada between July 2004 and June 2015. Each dot represents the unemployment rate in the corresponding year. For example, the first dot represents the overall unemployment rate between July 2004 and June 2005. The vertical line represents the first month in which the CTS reform was implemented. The figure demonstrates that the overall unemployment rate in BC rose from 3.85% in 2008 to 6.00% in 2009 and to 7.54% in 2010, adding up to an increase of 3.69 percentage points in the two years subsequent to the reform. In the rest of Canada, the rate increased from 5.42% in 2008 to 7.79% in 2010, amounting to an increase of 2.37 percentage points. Hence, the figure reflects an extraordinary increase in the BC's overall unemployment rate after the CTS reform.

Figure 2 demonstrates the annual unemployment rate by industry in BC and the rest of Canada. The figure indicates that the unemployment rates in BC's manufacturing industries grew much faster than the rates in the rest of Canada. In contrast, workers in public administration, health care and social assistance, educational services, and professional, scientific, and technical industries experienced similar trends in the unemployment rates in BC and the rest of Canada throughout the 10 years between 2004 and 2015. These sets of results seemingly suggest that the extraordinary rise in the overall unemployment rate in BC was in large part attributed to the increase in the tax rates in the manufacturing industry. If manufacturing in-

dustries emit carbon much more than other industries, the CTS reform is expected to increase the costs of businesses in the manufacturing industry more. Therefore, the evidence of the two figures coheres with the tax structure of the reform. Although the figures seem to suggest that the CTS reform increased the BC's overall unemployment rate, it is impetuous to conclude that the massive increase of the rate in BC relative to the rest of Canada was attributed to the CTS reform. The phenomenon could be driven partly or entirely by changes in demographic characteristics. Rigorous analyses are conducted to uncover the causal link between the CTS reform and the labor market activities in the rest of this paper.

3 Identification Methods and Data Description

This section discusses the identification methods and describes the data. To isolate the effects of changes in the demographic composition of the sample, a multivariate regression analysis is used to control for observable factors. To capture the causal effect of the reform, I adopt a DID approach. Since the reform was implemented in BC, I define the treatment group as workers in BC. Their counterparts in other provinces could serve as a control group because (i) the Canadian population inside and outside BC should experience similar country-wide economic shocks in the absence of the reform and (ii) the CTS reform should not influence the labor markets outside of BC (a detailed discussion follows in Appendix B). This strategy controls for the trends of labor market outcomes in Canada and thus impairs the common trend assumption. Using the DID approach, the causal effect of the reform can be estimated by a regression model as follows:

$$Y_{ijt} = \alpha + \beta_1(BC_j \times Post_t) + \beta_2(Year \times Month)_t + \beta_3 BC_j + X_{ijt}^T\gamma + \varepsilon_i \tag{1}$$

where i, j, and t denote individual, province, and time, respectively. BC_j equals one if a respondent lives in BC, and zero otherwise. $Post_t$ equals one if it is observed in July 2008 or later, and zero otherwise. Therefore, the term $BC_j \times Post_t$ takes the value of one if a respondent lives in BC in a post-reform period, and zero otherwise. The reference period (pre-reform period) is defined as July 2007-June 2008. Although the short-term effect of the reform is our main interest, this study will also present the long-term effect of the reform. Here, the short- and the long-term horizons are defined as July 2008-June 2010 and July 2010-June 2013 respectively. $(Year \times Month)_t$ is a dummy for a month in a given year.

X_{ijt}^T is a vector of individual characteristics, including dummies for five-year age group, the highest qualification attained, marital status, and industry. These regressors control for variations in the group composition. Y_i is an economic outcome of respondent i, which is *Un-*

employed, *Weekly Working Hours*, *Unemployment Duration*, *Job Tenure*, *Real Hourly Wage*, and *Labor Force Participation*. *Unemployed* equals one if a respondent is unemployed, and zero otherwise. *Unemployed* therefore is zero if a respondent is self-employed. *Labor Force Participation* equals one if a respondent participates in the labor market, and zero otherwise. Hence, *Labor Force Participation* is one for the employed and the unemployed, and zero otherwise. *Unemployment Duration* and *Job Tenure* are measured in weeks and months, respectively. *Real Hourly Wage* is measured in Canadian dollars.[9]

Equation (1) can be generalized to address various questions. First, to estimate the heterogeneous effects of the reform across industries, the following regression model is estimated:

$$\begin{aligned} Y_{ijkt} &= \alpha + \sum_{k=1} \beta_{1k}(BC_j \times Post_t \times Industry_k) + \sum_{k=1} \beta_{2k}(Post_t \times Industry_k) \\ &\quad + \sum_{k=1} \beta_{3k}(BC_j \times Industry_k) + \beta_4(BC_j \times Post_t) + \sum_{k=1} \beta_{5k} Industry_k \\ &\quad + \beta_6 Treat_j + \beta_7 Year_t + X_{ijt}^T \gamma + \varepsilon_i \end{aligned} \tag{2}$$

where *Industry*$_k$ is a industry dummy, which equals one if a respondent engages in industry k. I will discuss the choice of industries in detail in Section 4.4. To explore changes in Y_i in BC and each of the control provinces subsequent to July 2008, equation (1) is generalized by replacing $BC_j \times Post_t$ with a full set of province dummy times $Post_t$ interaction terms:

$$Y_{ijt} = \alpha + \sum_{j=1} \beta_{1j}(Prov_j \times Post_t) + \beta_2 Year_t + \sum_{j=1} \beta_{3j} Prov_j + X_{ijt}^T \gamma + \varepsilon_i \tag{3}$$

where $Prov_j$ equals one if it is observed in province j, and zero otherwise. Hence, if a respondent is from BC, $Prov_j = BC_j$. Alternatively, equation (1) can be generalized by replacing $BC_j \times Post_t$ with a full set of treatment$\times$ year interaction terms to explore the impact of the reform over time as follows:

$$Y_{ijt} = \alpha + \sum_{t} \beta_{1t}(BC_j \times d_t) + \sum_{t} \beta_{2,t} d_t + \beta_3 BC_j + X_{ijt}^T \gamma + \varepsilon_i \tag{4}$$

where d_t is a dummy if a respondent is observed between July in year t and June in year $t+1$, and zero otherwise.

The coefficient β_1 is the DID estimate of primary interest because it captures the variation in the value of an economic outcome subsequent to the reform specifically to BC. The DID estimation method requires five identifying assumptions so that β_1 can capture the average treatment effect on the treated (ATT) of the CTS reform. First, there is no anticipatory effect.

[9] Using the CPI, nominal wages are converted into the 2009 real wages throughout this article.

Second, a common trend assumption is valid. Third, a stable unit treatment value assumption (SUTVA) is satisfied. Fourth, the estimates are free from any sample selection biases. Fifth, no shock, policy, or law other than the CTS reform would affect the economic outcomes of our interest differently and specifically to BC residents within the period of examination. Each of the assumptions will be discussed in detail in Appendix B.

The present study utilizes the public-use files of the Canadian Labor Force Survey (LFS) between July 2004 and June 2015. The Canadian LFS is a monthly household survey, which includes approximately 100,000 individuals.[10] I restrict samples to the male population between the ages of 25 and 54. It is somewhat standard in the labor economics literature to study the male sample. One of the rationales is that the female population is more detached from the labor market. If the reform happens to alter the labor force participation rate, the estimated employment effect is likely subject to selection bias.[11] I examine this prime-age sample so that we can ignore the delayed entry of young workers into the labor force and changes in the retirement decisions of older workers. Notice that most respondents complete their studies by the age of 25. If the CTS reform happens to alter the optimal stopping time of education, the samples of the examination will be different before and after the reform. To minimize the risk of contaminating the estimates, individuals under 25 are excluded. Besides, variations in provincial minimum wages might lower the employment rate of the least-skilled workers such as young workers, female workers, and low-educated workers (Neumark and Wascher, 2006; Gorry, 2013). The exclusion of those below 25 and female observations could, therefore, avoid the estimation biases stemming from the variations in the provincial minimum wages.[12]

In the analysis on employment effect, only samples of labor market participants are included. The sample of employed workers is used to estimate the effect on *Weekly Working Hours*, *Job Tenure*, and *Real Hourly Wage*. Since unemployment duration can only be observed for the unemployed, samples are restricted to the unemployed in the analysis of the effect on

[10]The main purpose of the Canadian LFS is to generate data for official labor force statistics and is similar in nature to the United States Current Population Survey.

[11]The issue regarding sample selection bias will be discussed in detail in Appendix B.

[12]Besides the concerns on the minimum wage, various family policies, such as the Qubec Parental Insurance Plan, began in other provinces at the provincial level in the late 2000s. The literature suggests that these policies increase the fertility rate for female workers, encouraging them to stay away from work. These types of policies might enlarge the estimated employment effect for women. Of course, the CTS reform and the recession might create the other two mixed effects on female workers. First, due to a prolonged unemployment duration, female unemployed workers might become discouraged workers, leaving the labor force. Second, the added-worker effect might encourage the non-participants to work. While the first effect happens to both male and female workers, the latter effect is mainly found among female workers. To make the identification clear, I exclude female workers from the sample. I also examine the effect of the reform on female employment. Results are reported in Appendix D. The estimates suggest that the female unemployment rate increases by 1.2 and 1.5 percentage points in the short- and the long-term, respectively. The number of weekly working hours is also found to decline but the result is subject to the measure of the number of weekly hours.

Unemployment Duration. The entire male working population aged 25-54 is used to estimate the causal effect on the labor force participation rate.

In the Canadian LFS, the age of respondents can only be identified in a five-year category. Therefore, the control variable of age is a dummy variable for the age groups 25-29, 30-34, 35-39, 40-44, 45-49, and 50-54. The control variable of the highest education attained is a dummy variable for postgraduates, college graduates, diploma graduates, individuals with some post high-school education, high-school graduates, and high-school dropouts. The control variable of marital status equals one if a respondent is married, and zero otherwise. I also control dummies for 18 industries. I exclude all the observations with a missing value in any of the dummies for age, education, and marital status.

I first estimate β_1 with a short estimation window (July 2007-June 2010) to obtain a more precise employment effect of the CTS reform in the short-term. Also, I exclude observations beyond 2010 because BC's minimum wage increased in 2011 by 18.8%, which potentially influences labor market outcomes, especially the unemployment rate.[13] A longer series of the observations are used for the long-term analysis (July 2010-June 2013) and robustness checks, both of which will be discussed in Appendix B.

Observations from BC, Manitoba (MB), New Brunswick (NB), Nova Scotia (NS), Prince Edward Island (PE), Quebec (QC), and Saskatchewan (SA) are included in the sample. That is, I exclude observations from three provinces: Alberta (AB), Newfoundland and Labrador (NL), and Ontario (ON). First, specified gas emitters regulation began in AB during 2007-2008, around when the CTS reform was implemented. Large final emitters of six gases incurred higher flow cost, thereby potentially affecting AB's unemployment rate. Moreover, AB borders BC in the west; BC's labor demand and supply might easily shift to AB, violating SUTVA. Therefore, the observations from AB are excluded. Second, variations in the minimum wage likely affect the unemployment rate. I exclude provinces increases of CAN $1.5 or more in the minimum wage two years after the CTS reform (i.e., July 2008-June 2010). Since the minimum wages increased by CAD$1.5 in NL and ON, accounting for over 17%, the observations from NL and ON are excluded.[14]

The remaining issue relates to the estimation of standard errors. Bertrand et al. (2004) raise concerns about the correlation of the regressors within clusters in DID estimation. Accordingly, the cluster-robust standard errors are estimated to generalize the Huber-White sandwich estimates of OLS standard errors to the clustered setting to account for possible heteroscedas-

[13]The hourly minimum wage remained CAD$8.00 in BC during July 2004-April, 2011, and increased to CAD$8.75 in May 2011, and to CAD$9.5 in November, 2011.

[14]In NL, the hourly minimum wage was CAD$8.00 in July 2008. It increased to CAD$8.50 in January 2009, to CAD$9.00 in July 2009, and to CAD$9.50 in January 2010. In ON, the hourly minimum wage was CAD$8.75 in July 2008 and increased to CAD$9.50 in March 2009 and to CAD$10.25 in March 2010.

ticity and within non-treated group dependence of standard errors. According to Bertrand et al. (2004) and Angrist and Pischke (2008), standard errors should be clustered by 42 levels or more. Nevertheless, this analysis provides only seven clusters at the province level. Hence, the standard errors are clustered at the level of seven provinces, the six age groups, and the two marital statuses, providing us with 84 clusters. I also estimate the standard errors using a bootstrap procedure proposed by Cameron et al. (2008). Since most standard errors are identical up to three decimal places and the main conclusions of this paper do not alter using the two estimation methods, I only report the standard errors using the former method.

4 The CTS Reform in the Labor Market

This section presents the main results of this paper and is divided into six subsections. Subsection 4.1 presents the descriptive statistics about the unemployment rate by educational level. In Subsection 4.2, I show that the reform reduces the extensive margin of employment (i.e., the unemployment rate goes up) and has no impact on its intensive margin (i.e., the number of weekly working hours remains unchanged). Subsection 4.3 illustrates that the reform lengthens both the unemployment duration and the job tenure, suggesting that the adverse employment effect is primarily driven by the decline in the hiring rate rather than the rise in the separation rate. Subsection 4.4 demonstrates the heterogeneity in the employment effects across industries: while the adverse employment effects in the low energy-intensive industries are weak, workers in utilities industries and high energy-intensive industries suffer from larger adverse employment rate. Also, it provides evidence that the medium- and the low-educated tend to work in the affected industries, in part explaining the heterogeneity in the employment effect across educational groups. This subsection presents evidence that some of low-educated workers might shift from high energy-intensive manufacturing industries to low energy-intensive manufacturing ones, leading to labor reallocation. Subsection 4.5 gives evidence on the persistence of the adverse employment effect in the long-term (between the third and the fifth year of the reform). Meanwhile, I show that some of the low-educated unemployed might become discouraged and leave the labor force in the long-term. Subsection 4.6 discusses and summarizes the results.

4.1 Descriptive Statistics on the Unemployment Rate

Table 1 presents the unemployment rates in BC and the control provinces during the pre- and the post-reform period. The pre- and the post-reform periods are July 2007-June 2008 and July 2008-June 2010, respectively. Overall, the unemployment rate increases in BC and the control provinces, probably because of the recession. BC and the other provinces experience an in-

Table 1: Changes in the Unemployment Rate by Educational Attainment

Descriptive Statistics (In Percentage)

		(1)	(2)
	Period	**British Columbia**	**Control Provinces**
Aggregate	Pre-Reform	3.85	5.95
		(0.106)	(0.067)
	Post-Reform	6.77	7.10
		(0.981)	(0.051)
Differences	Post-Pre	2.91***	1.15***
		(0.157)	(0.087)
High-Educated Workers	Pre-Reform	3.26	2.90
		(0.208)	(0.112)
	Post-Reform	4.68	3.85
		(0.172)	(0.088)
Differences	Post-Pre	1.43***	0.945***
		(0.287)	(0.150)
Medium-Educated Workers	Pre-Reform	3.48	5.42
		(0.151)	(0.094)
	Post-Reform	6.32	6.71
		(0.143)	(0.073)
Differences	Post-Pre	2.84***	1.29***
		(0.228)	(0.124)
Low-Educated Workers	Pre-Reform	4.73	8.21
		(0.200)	(0.131)
	Post-Reform	8.82	9.44
		(0.193)	(0.099)
Differences	Post-Pre	4.09 ***	1.23 ***
		(0.303)	(0.168)

Notes: Data come from the Canadian LFS July 2007- June 2010. The pre-reform period and the post-reform period are July 2007-June 2008 and July 2008-June 2010 respectively. Samples are restricted to the males aged 25-54 who participates to the labor market. BC is the treatment province. Control provinces include MB, NB, NS, PE, QC, and SK. High-educated workers are respondents who are university graduates and postgraduates, low-educated workers are respondents who are high-school dropouts and graduates, and the rest are the medium-educated. Standard errors are reported in parentheses. Significance levels: ***=1%, **=5%, *=10%.

crease of 2.91 and 1.15 percentage points, respectively, in the unemployment rate. To conclude, subsequent to the CTS reform, BC suffers from an unemployment rate of 1.76 percentage points higher than the control provinces.

In addition, Table 1 reports the unemployment rate by educational attainment, namely high-educated, medium-educated, and low-educated workers. High-educated workers are defined as those who hold bachelor's or postgraduate degrees. Medium-educated workers are individuals with some post-secondary education and associate degree holders. Low-educated workers are high-school dropouts and graduates. Several points about the statistics deserve emphasis.

First, the increase in the unemployment rate is less pronounced among high-educated workers than medium- and low-educated workers both in BC and other provinces. The increases reach about 1.43 and 0.95 percentage points among high-educated workers in BC and the other provinces, 2.84 and 1.29 percentage points for medium-educated workers, and 4.09 and 1.23 percentage points for low-educated workers.

Second, the difference in the change in the unemployment rate across educational groups is larger in BC than in other provinces. The increase ranges from 1.43 to 4.09 percentage points in BC but only from 0.95 to 1.29 percentage points in the other provinces after the reform. The increase for the high-educated in BC is 1.51 times of that in the control provinces, 2.20 times for the medium-educated, and 3.33 for the low-educated.

In conclusion, Table 1 documents that (i) both BC and the control provinces experience a rise in the unemployment rate (2.91 and 1.15 percentage points, respectively), with an increase of 1.76 more percentage points in BC; (ii) the increases are close across educational groups in the control provinces (0.95-1.23 percentage points) but not in BC (1.43-4.09 percentage points); (iii) the rise in the unemployment rate for high-educated workers is also close between BC and the control provinces (1.43 and 0.95 percentage points, respectively); and (iv) medium- and low-educated workers experience a substantial increase in the unemployment rate in BC (2.84 and 4.09 percentage points, respectively) but not in the control provinces (1.29 and 1.23 percentage points). Although the statistics seem to suggest that the CTS reform increases BC's unemployment rate, particularly among the medium- and the low-educated, it is impetuous to conclude that the large increase in the unemployment rate in BC relative to the control provinces is attributed to the CTS reform. The differences in the trends of the unemployment rate could be driven partly or entirely by changes in demographic characteristics.

4.2 The Adverse Employment Effects of the CTS Reform

This section presents the causal effect of the CTS reform on the extensive and the intensive margin of employment. I first use a linear probability model to obtain the DID estimates in

equation (1) to estimate the causal effect on the unemployment rate (the extensive margin of employment). Regarding the intensive margin of employment, I estimate the impacts of the reform on the number of the actual and the usual weekly working hours on the main job and all jobs by estimating equation (1).

Table 2 and 3 report the main DID coefficients estimated in equation (1), presenting the probability of unemployment and the percentage change in the number of weekly working hours. While the dependent variable is *Unemployed* in Table 2, the dependent variables, described at the top of the table, are four different measures of weekly working hours in Table 3. In each model, the year×month dummy is controlled. While models (2)-(5) control the provincial fixed effect, models (3)-(5) include dummies for age, marital status, and educational level. Only models (4) and (5) include dummies for industry and its interaction term with year. When the employment effect by level of education is estimated, the dummy for the educational category is excluded. Model (3) is considered the preferred model for two reasons. First, unemployed workers are more unlikely to report their industry as compared to employed workers. Once the industry dummy is included, the missing value problem might bias the result. Second, while the choices of a worker's industry and his employment status could potentially be determined simultaneously, model (3) controls the most number of observed characteristics that is possibly orthogonal to the dependent variable. Models (4) and (5) therefore serve as robustness checks.

In Table 2, estimates show that BC's unemployment rate increases by 1.8 percentage points after July 2008, close to the statistics (1.76 percentage points) in Table 1. The estimates are statistically significant at one percent level, and the results are robust to several model specifications. Furthermore, I estimate the employment effect using three different samples: the high-, the medium-, and the low-educated. The estimates suggest that there is essentially no employment effect on the high-educated. Not only are the estimates small (less than one percentage point), but they are also statistically insignificant at any conventional level in the preferred model. However, the unemployment rate of the medium- and the low-educated increases by 1.6 and 2.8 percentage points, both of which are close to the descriptive statistics in Table 1. The results are statistically significant at one percent level and robust to choices of model specification.

According to Table 3, none of the main DID estimates are statistically significantly different from zero at any conventional level regardless of the measure of working hours and model specifications. Moreover, I find no effect on the number of weekly working hours using samples of various educational groups.[15] These results suggest that the reform only reduces employment from its extensive margin but not its intensive margin. This result is in line with empirical

[15]These results are robust to choices of model specification, which are shown in Table A.1-4 in Appendix A.

Table 2: The Negative Employment Effect of the CTS Reform During 2008-2010

Difference-in-Differences Analysis

Dependent Variable: Unemployed
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2010

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers (Pre-Reform Rate: 3.85%)					
BC×Post	0.018***	0.017***	0.018***	0.017***	0.016***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Adjusted R-squared	0.004	0.010	0.028	0.034	0.034
Sample: High-Educated Male Workers (Pre-Reform Rate: 3.26%)					
BC×Post	0.005	0.005	0.005	0.009*	0.009*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.001	0.003	0.006	0.012	0.013
Sample: Medium-Educated Male Workers (Pre-Reform Rate: 3.48%)					
BC×Post	0.016***	0.015***	0.016***	0.015***	0.014***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.004	0.009	0.018	0.027	0.027
Sample: Low-Educated Male Workers (Pre-Reform Rate: 4.73%)					
BC×Post	0.029***	0.028***	0.028***	0.024***	0.023***
	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)
Adjusted R-squared	0.007	0.019	0.031	0.041	0.042
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. In column (1)-(3), the numbers of observations are 92,853, 218,671, and 163,131 for high-educated, medium-educated, and low-educated workers, respectively. In column (4)-(5), the numbers of observations are 91,932, 216,321, and 160,114 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table 3: The Effect of the CTS Reform on Weekly Working Hours During 2008-2010

Difference-in-Differences Analysis				
Dependent Variable				
Column 1: ln(Usual Weekly Working Hours on the Main Job)				
Column 2: ln(Usual Weekly Working Hours on All Jobs)				
Column 3: ln(Actual Weekly Working Hours on the Main Job)				
Column 4 : ln(Actual Weekly Working Hours on All Jobs)				
Treatment Group: BC				
Control Group: MB, NB, NS, PE, QC, & SK				
Estimation Window: July 2007-June 2010				
Post-Reform Period: July 2008-June 2010				
	(1)	**(2)**	**(3)**	**(4)**
Sample: All Male Workers				
Pre-Reform Mean	41.40	42.13	41.55	42.24
BC×Post	-0.000	0.001	-0.002	-0.001
	(0.004)	(0.003)	(0.005)	(0.005)
Adjusted R-squared	0.024	0.024	0.023	0.024
Sample: High-Educated Male Workers				
Pre-Reform Mean	39.92	40.73	40.67	41.46
BC×Post	0.002	0.002	-0.004	-0.003
	(0.008)	(0.009)	(0.009)	(0.009)
Adjusted R-squared	0.017	0.017	0.025	0.025
Sample: Medium-Educated Male Workers				
Pre-Reform Mean	41.18	41.95	41.37	42.09
BC×Post	-0.001	0.001	-0.006	-0.004
	(0.005)	(0.005)	(0.008)	(0.008)
Adjusted R-squared	0.017	0.018	0.022	0.023
Sample: Low-Educated Male Workers				
Pre-Reform Mean	42.55	43.17	42.31	42.89
BC×Post	-0.002	-0.003	0.003	0.003
	(0.005)	(0.005)	(0.006)	(0.006)
Adjusted R-squared	0.022	0.026	0.024	0.026

Notes: Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. Control variables include dummies for year×month, province, age group, and marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

regularities: cyclical variations in the total working hours in a labor force arise from changes in the number of employed workers but not changes in the number of working hours per worker (Shimer, 2010). Moreover, the adverse employment effect is heterogeneous across educational groups; the lower the educational level, the larger is the impact.

4.3 The Decline in the Hiring Rate or the Rise in the Separation Rate?

The previous subsection shows what are affected and who are affected, providing evidence of the rise in the unemployment rate of the medium- and the low-educated following the reform. To paint an overall picture of the impact of the reform in the labor market, this subsection investigates the means as to how the reform increases the unemployment rate. Due to the lack of information on the transition rate from employment to unemployment or from unemployment to employment, I could not perform any direct statistical test on the causal link between the CTS reform and the transition rates. However, this subsection provides several pieces of suggestive evidence to illustrate that the adverse employment effects are primarily driven by the decline in the hiring rate rather than the rise in the separation rate.

According to the search and matching literature (Rogerson et al., 2005; Rogerson and Shimer, 2011), the unemployment rate decreases with the hiring rate and increases with the separation rate. If the adverse employment effect happens to be driven (primarily) by the decline in the hiring rate, the reform will lengthen both the unemployment duration and the job tenure. In contrast, if the adverse employment effect is attributed to the increase in the separation rate, I expect the unemployment duration and the job tenure to decline. I will also discuss other possibilities.

In this analysis, I estimate the impacts of the reform on the length of the unemployment duration (measured in weeks) and the job tenure (measured in months) by estimating equation (1). The dependent variables are the logarithm of the variables of interest so that the main DID estimates can be interpreted as the percentage change of the variable following the reform. I control all the covariates in the preferred model as in Table 2. The DID main estimates are reported in Table 4. The estimates suggest that the reform increases the unemployment spell and the job tenure by 12.6% and 5.9% at the five percent significance level, amounting to 2.2 weeks and 5.9 months, respectively. While the protracted spells of unemployment only appear among the low-educated (the average length of the unemployment spell increases by 22.9% [about 3.9 weeks]), the reform only lengthens the job tenure for the medium- and the low-educated.[16] These findings are consistent with the heterogeneity in the adverse employment effect across educational groups.

[16]These results are robust to choices of model specification, which are shown in Table A.5-6 in Appendix A.

Table 4: The Effect of the CTS Reform on Unemployment Duration, Job Tenure, and Real Wage During 2008-2010

Difference-in-Differences Analysis			
Dependent Variable			
Column 1: ln(Unemployment Duration)			
Column 2: ln(Job Tenure)			
Column 3: ln(Real Wage)			
Treatment Group: BC			
Control Group: MB, NB, NS, PE, QC, & SK			
Estimation Window: July 2007-June 2010			
Post-Reform Period: July 2008-June 2010			
	(1)	**(2)**	**(3)**
Sample: All Male Workers			
Pre-Reform Mean	17.19	99.57	20.10
BC×Post	0.126**	0.059**	0.002
	(0.057)	(0.024)	(0.005)
Observations	29,355	443,671	359,873
Adjusted R-squared	0.027	0.124	0.175
Sample: High-Educated Male Workers			
Pre-Reform Mean	20.08	92.94	25.53
BC×Post	0.133	0.050	0.010
	(0.104)	(0.034)	(0.015)
Observations	3,284	89,410	72,960
Adjusted R-squared	0.041	0.165	0.083
Sample: Medium-Educated Male Workers			
Pre-Reform Mean	16.55	97.47	19.94
BC×Post	0.049	0.055**	0.010
	(0.084)	(0.027)	(0.009)
Observations	12,683	205,321	169,929
Adjusted R-squared	0.022	0.116	0.090
Sample: Low-Educated Male Workers			
Pre-Reform Mean	17.14	106.15	17.18
BC×Post	0.229***	0.084**	-0.006
	(0.079)	(0.036)	(0.010)
Observations	13,388	148,940	116,984
Adjusted R-squared	0.032	0.111	0.108

Notes: Unemployment Duration and Job Tenure are measured in weeks and months respectively. Wages are CPI-adjusted and are measured in Canadian dollars. Control variables include dummies for year×month, province, age group, and marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of male workers. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the male aged 25-54 who are unemployed in the analysis of unemployment duration and employed in the analysis of job tenure and real wage. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Thus far, it has been documented that the length of the unemployment duration and the job tenure increases following the reform. In principle, the increase in the length of the job tenure could be driven by the decline in the separation rate and the hiring rate. Of course, this effect could also result from a selection process: workers with shorter job tenures are more likely to lose their jobs after the reform. However, if the protracted job tenure is attributed to this mechanism, I expect that the reform also increases the wages for the medium- and the low-educated because wages increase with seniority (Medoff and Abraham, 1980, 1981; Barbezat, 2004; Zwick, 2011). Therefore, I explore this possibility by further estimating the wage effect of the CTS reform using the real hourly wage measured in 2009 Canadian dollars. Estimates are reported in column 3 of Table 4, and the results suggest that no wage effect of the CTS reform is found at any conventional significance level regardless of educational level.[17] The result reveals that the selection process is unlikely to be the driving force for the protracted job tenure, leaving only the possibilities of the decline in the separation rate and the hiring rate. Analogously, the prolonged unemployment duration should be attributed to the decrease in the separation rate and the hiring rate.

Nevertheless, the drop in the hiring rate is the only possibility that reconciles the documented effects on the overall unemployment rate, unemployment duration, and job tenure. Suppose that the increase in the length of the unemployment spell and job tenure arises from the decrease in the separation rate. According to the job search theory, the overall unemployment rate is expected to decline. To cohere with the rise in the overall unemployment rate, I conclude that the CTS reform likely lowers the hiring rate, lengthening both the unemployment duration and the job tenure and thus increasing the unemployment rate. These results complement to the empirical study (Curtis, 2014), which provides evidence that the fall in the hiring rate, rather than the rise in the separation rate, causes the manufacturing employment level to decline under stringent environmental regulations in the United States. Moreover, these findings cohere with the recent business cycle literature (Petrongolo and Pissarides, 2008; Elsby et al., 2009; Shimer, 2012), which suggests that the variation in the unemployment rate is mainly attributed to the variation in the hiring rate, not the separation rate.

4.4 The Heterogeneous Effects across Industries

This subsection investigates heterogeneous employment effects across industries in the short-term (the first two years of the reform). The exercise is informative for three reasons. First, the heterogeneity in the employment effect across educational groups arises probably because more of the medium- and the low-educated engage in the affected industries. Which industries are

[17] These results are robust to choices of model specification, which are shown in Table A.7 in Appendix A.

most affected in practice? To answer this question, it is essential to estimate the heterogeneous effects across industries. Second, answering this question serves as an internal validity check on the results. If the adverse employment effects are homogeneous across all industries or happen mainly in low energy-intensive industries, it may be reasonable to argue that the estimated adverse effects result from an unobserved factor, irrelevant to the CTS reform. Third, this analysis also provides an opportunity to measure the effects of the CTS reform across industries for policymakers' future references.

In this analysis, three hypotheses are tested. First, industries, including (i) manufacturing, (ii) mining, oil, and gas, and (iii) utilities are expected to have a larger adverse employment effect following the reform. The utilities industry is expected to suffer from a more substantial negative employment effect because the production levels of its major downstream industries are expected to shrink. Regarding manufacturing industries, not all industries in this category are energy-intensive; emissions are relatively more abundant in metallic- and chemical-related manufacturing. Therefore, while the Canadian LFS divides manufacturing industries into durable manufacturing and non-durable manufacturing industries, I further divide them into high energy-intensive manufacturing and low energy-intensive manufacturing industries. Hence, durable manufacturing industry is divided into high energy-intensive durable manufacturing (HDM) and low energy-intensive durable manufacturing (LDM), and non-durable manufacturing industry is divided into high energy-intensive non-durable manufacturing (HNM) and low energy-intensive non-durable manufacturing (LNM).[18] If the rise in the unemployment rate results from the CTS reform, it is likely that the impact is more pronounced in HDM, HNM, utilities, and mining, oil, and gas, not in LDM or LNM.

The second hypothesis is that there is no difference in the employment effect between transportation and other low energy-intensive industries.[19] The carbon tax applies to any consumption of fossil fuels, not limited to industry. Although carbon emissions in transportation are relatively more abundant, private transportation is subject to the CTS reform in the same manner as public transportation. Hence, the demand for public transportation would not eas-

[18]HDM includes non-metallic mineral product manufacturing, primary metal manufacturing, fabricated metal product manufacturing, machinery manufacturing, computer and electronic product manufacturing, electrical equipment, appliance, and component, transportation equipment manufacturing, and furniture and related product manufacturing. The rest of the durable manufacturing is LDM. Regarding the non-durable manufacturing industry, I classify textile mills and textile product mills, clothing manufacturing, paper manufacturing, and printing and related support activities as LNM and the other industries as the non-durable manufacturing into HNM, including includes food, beverage and tobacco product manufacturing, petroleum and coal products manufacturing, chemical manufacturing, plastics and rubber products manufacturing, and miscellaneous manufacturing.

[19]The other low energy-intensive industries include agriculture, construction, wholesale trade, retail trade, warehousing and storage, finance, insurance, real estate, professional, scientific and technical support, management, administrative and other support, educational services, health care and social assistance, information, culture and recreation, accommodation and food services, other services, and public administration.

ily be substituted. Consequently, the impacts of the policy on transportation and the other low energy-intensive industries are virtually identical.

Lastly, the employment effects are identical within low energy-intensive industries because there exists no policy other than the CTS reform that leads to a decline in the employment in low energy-intensive industries. Hence, I hypothesize that the employment effects in the industries, including (i) agriculture, (ii) information, culture & recreation, (iii) health care and social assistance, (iv) management, administrative and other support, (v) other services, (vi) retail trade, and (vii) warehousing and storage, are no different from other low energy-intensive industries.

Using equation (2), coefficients are estimated using a linear probability model. Samples are restricted to the observations during July 2007-June 2010. The pre- and the post-reform periods are July 2007-June 2008 and July 2008-June 2010, respectively. Year$\times$Month and provincial fixed effect and the dummies for age, marital status, and educational level are controlled. Estimates are reported in Table 5. While the entire sample is used for column 1, samples of the high-, the medium-, and the low-educated are used in columns 2, 3, and 4. Each column shows 13 estimates $\hat{\beta}_{1j}$ and $\hat{\beta}_4$.

On the first page of Table 5, most the estimates are consistent with my expectation. First, $\hat{\beta}_4$ is 0.018 at one percent significance level. The estimates reveal that the unemployment rate increases by about 1.8 percentage points in the low energy-intensive industries. Second, workers in the utilities industry experience the adverse employment effect that is 3.6 percentage points larger than other low energy-intensive industries. This result is consistent with the taxation scheme. Third, the employment effects are heterogeneous within manufacturing industries. While the unemployment rate increases by 3.7 and 4.9 percentage points in HDM and HNM, the estimates suggest that the adverse unemployment effects are close to zero in LDM and LNM. Fourth, it is surprising that the adverse employment effects are identical in mining, oil, and gas and the other low energy-intensive industries.

Coefficients on the second page of Table 5 are in line with the taxation scheme of the CTS reform as well. In the seven low energy-intensive industries, each of the corresponding $\hat{\beta}_{1j}$ is not statistically significant. This result indicates that the employment effect on each of the industries is not different from the other low energy-intensive industries. In addition to the estimation of the parameters, I perform a post-estimation test on the equality of these effects. In particular, I test the hypothesis that the estimated effects $\hat{\beta}_{1j}$ for these industries are all equal to zero. Such a hypothesis cannot be rejected at any conventional significance level, suggesting that each of the effects across these low energy-intensive industries and the effect on the other low energy-intensive industries are statistically equal. In sum, while most the high energy-intensive industries experience a larger adverse employment effect, the employment effects are

Table 5: The Heterogeneous Employment Effects of the CTS Reform Across Industries During 2008-2010

Difference-in-Difference-in-Differences Analysis

Dependent Variable: Unemployed
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2010

	(1)	(2)	(3)	(4)
Sample	**All**	**Hi-Edu**	**Med-Edu**	**Low-Edu**
BC×Post	0.018***	0.007*	0.016**	0.032***
	(0.005)	(0.003)	(0.006)	(0.007)
BC×Post×Utilities	0.036***	0.014	0.047**	0.033
	(0.013)	(0.029)	(0.019)	(0.031)
BC×Post×Mining, Oil, & Gas	-0.010	-0.042	-0.018	-0.012
	(0.012)	(0.033)	(0.017)	(0.019)
Durable Manufacturing Industry				
BC×Post×High Energy-Intensive	0.019	0.021	0.034***	-0.006
	(0.012)	(0.058)	(0.012)	(0.020)
BC×Post×Low Energy-Intensive	-0.018	-0.009	0.005	-0.051*
	(0.018)	(0.045)	(0.024)	(0.026)
Non-Durable Manufacturing Industry				
BC×Post×High Energy-Intensive	0.031**	0.073**	-0.010	0.043*
	(0.015)	(0.030)	(0.021)	(0.024)
BC×Post×Low Energy-Intensive	-0.015	0.022	-0.012	-0.056**
	(0.015)	(0.034)	(0.023)	(0.022)
Adjusted R-squared	0.030	0.012	0.022	0.041
Year×Month Fixed Effect	Yes	Yes	Yes	Yes
Province Fixed Effect	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Marital Status	Yes	Yes	Yes	Yes
Education	Yes	No	No	No

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. The numbers of observations are 91,932, 216,321, and 160,114 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table 5: The Heterogeneous Effects of the CTS Reform on Employment Across Industries During 2008-2010 (Cont.)

Difference-in-Difference-in-Differences Analysis

Dependent Variable: Unemployed
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2010

	(1)	**(2)**	**(3)**	**(4)**
Sample	**All**	**Hi-Edu**	**Med-Edu**	**Low-Edu**
BC×Post×Agriculture	-0.014	-0.132***	-0.012	-0.004
	(0.019)	(0.049)	(0.015)	(0.029)
BC×Post×Information, Culture	-0.014	-0.002	-0.004	-0.040
& Recreation	(0.008)	(0.017)	(0.009)	(0.024)
BC×Post×Health Care	-0.016	-0.008	-0.015	-0.024
& Social Assistance	(0.010)	(0.012)	(0.014)	(0.014)
BC×Post×Management,	0.011	0.052	0.006	0.001
Administrative & Other Support	(0.014)	(0.044)	(0.017)	(0.022)
BC×Post×Other Services	-0.004	0.017	-0.011	-0.007
	(0.013)	(0.037)	(0.013)	(0.023)
BC×Post×Retail Trade	-0.011	0.005	-0.012	-0.024*
	(0.008)	(0.018)	(0.013)	(0.013)
BC×Post×Transportation	-0.005	0.013	-0.020**	-0.002
& Warehousing	(0.006)	(0.015)	(0.009)	(0.011)
Adjusted R-squared	0.030	0.012	0.022	0.041
Prob>F-Statistics of Equality to Zero	0.298	0.164	0.291	0.265
Year×Month Fixed Effect	Yes	Yes	Yes	Yes
Province Fixed Effect	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Marital Status	Yes	Yes	Yes	Yes
Education	Yes	No	No	No

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. The numbers of observations are 91,932, 216,321, and 160,114 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

statistically homogeneous in the low energy-intensive ones. These documented employment effects are thoroughly consistent with the taxation scheme; while the CTS reform increases the unemployment rate in each of the industries, such effect is especially large in the high energy-intensive industries (high energy- manufacturing durable and non-durable industries and utilities industries). In what follows, I discuss the effects of the reform on the unemployment rates in various industries by educational group.

The Employment Effects on High-Educated Workers across Industries. Column 2 demonstrates that $\hat{\beta}_4$ is marginally statistically significant at 10 percent level. Nevertheless, the estimate, an increase of about 0.7 percentage points, is materially small and in line with the results above. Meanwhile, the estimates indicate that there is no employment effect in all industries except HNM and the agriculture industry. Although it makes sense that the adverse effect appears in high energy-intensive industries, it is interesting that such effect only appears in the high energy-intensive non-durable manufacturing industries but not in their durable counterparts. Regarding the positive employment effect in the agriculture industry, the current information cannot provide any good explanation as to why employment expands in this industry. However, this result somehow coheres with Rivers and Schaufele (2015b), who find no support of any adverse effect of the reform on the volume of the international trade in BC's agricultural commodities.

The Employment Effects on Medium-Educated Workers across Industries. First, $\hat{\beta}_4$ in column 3 suggests that medium-educated workers experience a decrease in unemployment rate of 1.6 percentage points after the reform; this result is statistically significant at five percent level. Second, estimates indicate a causal effect of about 34 and 47 additional unemployment per thousand medium-educated workers in HDM and the utilities industries, respectively. In contrast to the high-educated, the reform only increases the unemployment rate in HDM for the medium-educated. Such negative employment effects do not appear in other manufacturing industries.

The Employment Effects on Low-Educated Workers across Industries. Column 4 shows that $\hat{\beta}_4$ is statistically significant at one percent level, suggesting that low-educated workers experience an increase in their unemployment rate of 3.2 percentage points. The adverse employment effect is substantial in HNM, amounting to 7.5 percentage points. The estimates show that the unemployment rate is exceptionally large in HNM, not other industries. Lastly, but most interestingly, the results suggest that the unemployment rate for the low-educated decreases by 5.1 and 5.6 percentage points in LDM and LNM, respectively, relative to the other low energy-intensive industries. The estimates are materially and statistically significant. The reduction in the unemployment rate in low energy-intensive manufacturing industries may be driven by labor reallocation within the manufacturing industry, as predicted by theoretical model described by

Table 6: Distribution of High-, the Medium-, and the Low-Educated Workers in Energy-Intensive Industries in BC

Simple Regression Analysis

Dependent Variable:	**ln(Energy Intensity)**		**Utility**	
	(1)	**(2)**	**(1)**	**(2)**
The Medium-Educated	0.265***	0.259***	0.005*	0.006*
	(0.057)	(0.053)	(0.003)	(0.003)
The Low-Educated	0.336***	0.326***	-0.001	-0.001
	(0.055)	(0.048)	(0.003)	(0.003)
Constant	0.364***	0.349***	0.007**	0.005
	(0.064)	(0.056)	(0.002)	(0.004)
Adjusted R-squared	0.027	0.040	0.001	0.003
Age	No	Yes	No	Yes
Marital Status	No	Yes	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2008. Samples are restricted to employed male workers aged 25-54 in BC. I merge the LFS data with three-digit industry energy intensity from NBER Productivity Database. Energy intensity is measured by the total industry energy expenditure divided by the total value of shipments for the industry in 2007. Its values range from 0.627 to 5.670, with the mean of 2.289 in BC during July 2007-June 2008. Utility equals one if a respondent works in utility industry, and zero otherwise. The numbers of observations are 3,984 and 31,976 in column (1)-(2) and (3)-(4), respectively. Robust standard errors in parentheses are clustered at five-year age group dummy and marital status. (12 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Hafstead and Williams III (2016). The CTS reform increases costs in the high energy-intensive manufacturing industries, lowering their demands for labour. The low energy-intensive manufacturing industries might absorb some of the low-educated unemployed because the required skills for low-educated workers are probably similar within the manufacturing industry, shifting the employment from the high energy-intensive manufacturing industries to their low energy-intensive counterparts. These results complement to empirical studies such as Walker (2013), which provides evidence of the labor reallocation under stringent environmental regulations in the United States. The fact that no such reallocation for high- and medium-educated workers is found somewhat coheres with the human capital theory: the high- and the medium-educated accumulate more industry- and plant-specific human capital than their low-educated counterparts, thereby increasing their cost to work in other industries and thus making them relatively less mobile across industries.

Next, I explore a potential mechanism through which the impact of the reform differs across educational levels. While this paper documents that the CTS reform increases the unemployment rate for the medium- and the low-educated only, this subsection documents a larger negative employment effect in high energy-intensive manufacturing industries and the utilities

industry. I hypothesize that more of the medium- and the low-educated engage in the affected industries. Therefore, I estimate the association between the energy intensity of the manufacturing industry and the educational level and the fraction of workers of various educational levels in the utilities industry. Such a statistical relationship is estimated using the following model:

$$Y_i = \alpha + \beta_1 Med_i + \beta_2 Low_i + X_i^T \gamma + \varepsilon_i$$

where Med$_i$ (Low$_i$) equals one if a respondent is a medium-educated worker (a low-educated worker), and zero otherwise. Control variables include dummies for age and marital status. Dependent variables, Y_i, are the logarithm of energy intensity of industry and the dummy for the utilities industry. To construct a variable of energy intensity, I match the Canadian LFS data with the three-digit industry energy intensity of 2007 from the NBER Productivity database. Following Curtis (2014), energy intensity is measured by the total energy expenditure in each industry divided by its total value of shipments. The information on the energy intensity is available for the manufacturing industry only. Therefore, when the logarithm of energy intensity is used as a dependent variable, the sample is restricted to BC's male employed workers in the manufacturing industry one year prior to the implementation of the CTS reform (i.e., July 2007-June 2008). The corresponding estimates are reported in columns 1 and 2 in Table 6. When the dummy for the utilities industry is used as a dependent variable, the full sample of BC's male employed workers during the same period is used. The estimates are reported in columns 3 and 4 in Table 6.

In columns 1 and 2, estimates suggest that the medium- and the low-educated workers, on average, engage in manufacturing industries that are 26% and 33% more energy-intensive than those in which high-educated workers engage. The results in columns 3 and 4 indicate that the medium-educated workers are 0.6% more likely to engage in the utilities industry than the high-educated, and the likelihood of working in utilities industry for the low- and the high-educated is statistically indifferent.

These estimates provide one of the channels through which the revenue-neutral carbon tax has a larger impact on medium- and low-educated workers. First, these workers are more likely to engage in manufacturing industries that are more energy-intensive. Second, more of the medium-educated engage in the utilities industry, which is largely affected by the reform. The first set of results explains why both medium- and low-educated workers suffer more from the increase in the unemployment rate than high-educated workers. The second set explains why only the medium-educated, not the high- or the low-educated, in the utilities industry experience a rise in unemployment rate. However, the magnitude of the coefficient of *BC*×*Post* remains larger for the low-educated (3.0 percentage points) than the medium-educated (1.6 percentage

points) even though dummies for manufacturing and utilities industries are controlled. In other words, the fact that more of the medium- and the low-educated engage in the affected industry accounts for a portion (but not all) of the heterogeneity in the negative employment effect. However, such heterogeneity is somewhat consistent with the job search theory: Chan and Yip (2017) show that if there exists a negative aggregate shock, the negative employment effect will be larger for the lower educational group, providing one of the possible explanations as to why the adverse employment effect differs across educational groups under the CTS reform.

4.5 The Long-Term Impacts of the CTS Reform

This subsection purposes to investigate the impact of the CTS reform on the unemployment rate in the long-term. As mentioned above, the long-term is defined as the period between the third and the fifth year of the reform.[20] Again, the heterogeneity in the effects by educational groups is also of interest. First, I present the evidence on the persistence of the adverse employment effect on its extensive margin but not its intensive margin. Second, I provide suggestive evidence that some of the low-educated unemployed might become discouraged workers, lowering their labor force participation rate. If the discouraged workers had stayed in the labor market, the documented adverse employment effect would have been larger.

I first estimate the employment effects (both the extensive and the intensive margin) using equation (1). In this analysis, I only include observations during July 2007-June 2008 (the pre-reform period) and July 2010-June 2013 (the post-reform period). Thus, observations in the first two years of the reform are excluded. Estimates are reported in Table 7 and Table 8. The results suggest that the overall unemployment rate increases by 1.9 percentage points. Furthermore, the unemployment rate of the high-educated remains unchanged, but the rate of the medium- and the low-educated increases by 1.6 and 2.9 percentage points, respectively. According to Table 8, no statistical evidence on the impact on weekly working hours is found regardless of educational level.[21] These estimated employment effects during July 2010-June 2013 are almost identical to those in the short-term (July 2008-June 2010).

Such an increase in the unemployment rate may arise from changes in BC's minimum wage. Nevertheless, it is well documented that the negative employment effect of the minimum wage largely concentrates on teenagers and women (Neumark and Wascher, 2006; Gorry, 2013). This paper analyzes the male sample aged 25 or above. Therefore, the extent to which the estimated employment effect is driven by the variations in BC's minimum wage is limited.

To understand the source of the employment effect, I further estimate the heterogeneous

[20] I will show that the adverse employment effect vanishes from the sixth year onwards in the next section.

[21] These results are robust to choices of model specification, which are shown in Table A.8-A.11 in Appendix A.

Table 7: The Negative Employment Effect of the CTS Reform During 2010-2013

Difference-in-Differences Analysis

Dependent Variable: Unemployed
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2008 & July 2010-June 2013
Post-Reform Period: July 2010-June 2013

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers (Pre-Reform Rate: 3.85%)					
BC×Post	0.019***	0.018***	0.019***	0.013***	0.012***
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
Adjusted R-squared	0.002	0.008	0.026	0.031	0.032
Sample: High-Educated Male Workers (Pre-Reform Rate: 3.26%)					
BC×Post	0.007	0.007	0.007	0.005	0.004
	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)
Adjusted R-squared	0.001	0.002	0.005	0.009	0.010
Sample: Medium-Educated Male Workers (Pre-Reform Rate: 3.48%)					
BC×Post	0.016***	0.015***	0.016***	0.011***	0.010***
	(0.005)	(0.005)	(0.005)	(0.003)	(0.004)
Adjusted R-squared	0.003	0.008	0.016	0.025	0.025
Sample: Low-Educated Male Workers (Pre-Reform Rate: 4.73%)					
BC×Post	0.031***	0.028***	0.029***	0.019***	0.019***
	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)
Adjusted R-squared	0.005	0.016	0.029	0.038	0.039
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2008 and July 2010-June 2013. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. In column (1)-(3), the numbers of observations are 128,282, 286,094, and 208,017 for high-educated, medium-educated, and low-educated workers, respectively. In column (4)-(5), the numbers of observations are 126,710, 282,399, and 203,582 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table 8: The Effect of the CTS Reform on Weekly Working Hours During 2010-2013

Difference-in-Differences Analysis				
Dependent Variable				
Column 1: ln(Usual Weekly Working Hours on the Main Job)				
Column 2: ln(Usual Weekly Working Hours on All Jobs)				
Column 3: ln(Actual Weekly Working Hours on the Main Job)				
Column 4 : ln(Actual Weekly Working Hours on All Jobs)				
Treatment Group: BC				
Control Group: MB, NB, NS, PE, QC, & SK				
Estimation Window: July 2007-June 2008 & July 2010-June 2013				
Post-Reform Period: July 2010-June 2013				
	(1)	**(2)**	**(3)**	**(4)**
Sample: All Male Workers				
Pre-Reform Mean	41.40	42.13	41.55	42.24
BC×Post	-0.003	-0.002	-0.005	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Adjusted R-squared	0.024	0.025	0.022	0.023
Sample: High-Educated Male Workers				
Pre-Reform Mean	39.92	40.73	40.67	41.46
BC×Post	-0.005	-0.008	-0.012	-0.015*
	(0.008)	(0.007)	(0.008)	(0.008)
Adjusted R-squared	0.020	0.019	0.024	0.024
Sample: Medium-Educated Male Workers				
Pre-Reform Mean	41.18	41.95	41.37	42.09
BC×Post	-0.005	-0.001	-0.009	-0.004
	(0.004)	(0.004)	(0.008)	(0.007)
Adjusted R-squared	0.017	0.018	0.019	0.020
Sample: Low-Educated Male Workers				
Pre-Reform Mean	42.55	43.17	42.31	42.89
BC×Post	-0.002	-0.004	0.001	0.001
	(0.006)	(0.006)	(0.009)	(0.009)
Adjusted R-squared	0.024	0.027	0.023	0.026

Notes: Data come from the Canadian LFS July 2007-June 2008 and July 2010-June 2013. Samples are restricted to the males aged 25-54 who participates to the labor market. Control variables include dummies for year×month, province, age group, and marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. The numbers of observations are 114,856, 248,816, and 175,698 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

employment effects across industries using equation (2). Again, only the observations during July 2007-June 2008 (Pre-Reform Period) and July 2010-June 2013 (Post-Reform Period) are included. Estimates are reported in Table A.12 in Appendix A. While the negative employment effect reaches 3.1 percentage points in the high energy-intensive durable manufacturing industries relative to the control industries in the control provinces, the other industries experience an identical adverse employment effect. The heterogeneity in the negative employment effect across industries coheres with the tax structure of the reform, suggesting that the persistence of the employment effect probably stems from the CTS reform.

Furthermore, I estimate the heterogeneous employment effects across industries using samples of various educational groups. Coefficients are obtained by estimating equation (2), and the results are reported in columns 2, 3, and 4. Estimates suggest that the unemployment rates in other low energy-intensive industries are 1.1 and 2.0 percentage points higher than the pre-reform level for the medium- and the low-educated. Besides the increase of 2.0 percentage points, there is an additional increase of 4.8 percentage points in the unemployment rate for the low-educated in the high energy-intensive durable manufacturing industries, more than doubling the average employment effect. While the adverse employment effect on the medium-educated is gone in both the manufacturing and the utilities industries, the adverse employment effect of the CTS reform seems to persist in the low-educated groups in the high energy-intensive durable manufacturing industries only. If the documented negative employment effect is driven by the changes in the minimum wage, I expect that the adverse employment effect on the low-educated does not concentrate only on the high energy-intensive durable manufacturing industry. Therefore, the estimates speak directly to the concern on the source of the employment effect in the long-term, concluding that the adverse employment effect of the CTS reform is persistent.

Next, I proceed to estimate both the short-term and the long-term impact of the reform on the labor force participation rate. This exercise is informative for two reasons. First, if the reform alters the decision on labor force participation, the estimated employment effect might suffer from sample selection bias. Second, this analysis also provides an opportunity to measure the consequences of the CTS reform on labor force participation for the reference of future policymakers. The short-term and the long-term effects on the labor force participation rate are estimated separately using a linear probability model, and the coefficient estimates are reported in columns 1 and 2 (the short-term) and columns 3 and 4 (the long-term) in Table 9. In columns 1 and 2, estimates are materially small and statistically insignificantly different from zero at any conventional level, suggesting that the CTS reform does not change the labor force participation rate in the short-term regardless of educational level. However, the results show that the labor force participation rate in the long-term is 1.2 percentage points lower than the

pre-reform level. When examining the samples of various educational groups, the results reveal that such a decline in the labor force participation rate, amounting to 3.0 percentage points, appears only in the low-educated.

One possible reason for this phenomenon is that unemployed low-educated workers become discouraged due to a long duration of unemployment. If the unemployed are more likely to leave the labor force than their employed counterparts, the estimates are biased: if the unemployed had stayed in the labor force, the estimated negative employment effect would have been larger. In this case, the estimated long-term employment effect for the low-educated could at best serve as a lower bound of the effect. In conclusion, the employment effect of the CTS reform is long-lasting (five years), suggesting that the reform creates not only a one-shot, but also a dynamic, job loss and labor reallocation. Seemingly, the estimated adverse employment effects are close in the short- and the long-term, such a negative employment effect would have been greater in the long-term if the discouraged low-educated had stayed in the labor force.

4.6 Discussion on the Impact of CTS on Labor Market Activities

Whereas Yamazaki (2017) documents a positive employment effect of the CTS policy, this section not only corrects our understanding of the impact of the CTS on the overall unemployment rate but also provides the overall picture of the reform in the labor market. The reform depresses labor demand, thereby lowering the hiring rate. As a consequence, the unemployment rate and the length of the unemployment spell and the job tenure increase. These estimates suggest that the unemployment rate increases by 1.8 percentage points in BC subsequent to July 2008; moreover, they provide evidence for the adverse employment effect on medium- and low-educated workers but not their high-educated counterpart. The results are robust to several model specifications. If the underlying assumptions of the identification method are valid, the estimated main DID parameters capture the causal employment effect of the CTS reform: the policy causes the overall male unemployment rate to rise by 1.8 percentage points while the unemployment rate of the medium- and the low-educated increase by 1.6 and 2.8 percentage points two years following the policy.

Since the high energy-intensive manufacturing industry and the utilities industry are affected more by the reform, the increase in the unemployment rates in these industries is seen more than the increase in the low energy-intensive industries. Probably because more of the medium- and low-educated, relative to the high-educated, work in manufacturing industries that are more energy-intensive, the increases in the unemployment rates are higher for the medium- and the low-educated. Due to the higher unemployment rate, some of the unemployed might shift from the high energy-intensive manufacturing industries to their low energy-intensive man-

Table 9: The Effect of the CTS Reform on the LFP Rate

Difference-in-Differences Analysis				
Dependent Variable: Labor Force Participation				
Treatment Group: BC				
Control Group: MB, NB, NS, PE, QC, & SK				
Pre-Reform Period:	**July 2007-June 2008**		**July 2007-June 2008**	
Post-Reform Period:	**July 2008-June 2010**		**July 2010-June 2013**	
	(1)	**(2)**	**(3)**	**(4)**
Sample: All Male Workers (Pre-Reform Rate: 91.08%)				
BC×Post	-0.003	-0.004	-0.013**	-0.012**
	(0.004)	(0.004)	(0.005)	(0.005)
Adjusted R-squared	0.004	0.056	0.004	0.052
Sample: High-Educated Male Workers (Pre-Reform Rate: 94.09%)				
BC×Post	0.004	0.003	0.003	0.003
	(0.006)	(0.006)	(0.007)	(0.007)
Adjusted R-squared	0.001	0.010	0.001	0.011
Sample: Medium-Educated Male Workers (Pre-Reform Rate: 93.13%)				
BC×Post	-0.002	-0.002	-0.007	-0.008
	(0.006)	(0.006)	(0.005)	(0.005)
Adjusted R-squared	0.004	0.018	0.003	0.018
Sample: Low-Educated Male Workers (Pre-Reform Rate: 87.06%)				
BC×Post	-0.009	-0.009	-0.031**	-0.030***
	(0.009)	(0.008)	(0.012)	(0.011)
Adjusted R-squared	0.011	0.044	0.011	0.042
Year×Month Fixed Effect	Yes	Yes	Yes	Yes
Province Fixed Effect	Yes	Yes	Yes	Yes
Age	No	Yes	No	Yes
Marital Status	No	Yes	No	Yes

Notes: LFP equals one if a respondent participates in the labor market, and zero otherwise. Data come from the Canadian LFS. Samples are restricted to the males aged 25-54. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. In column (1)-(2), the numbers of observations are 98,472, 235,561, and 188,874 for high-educated, medium-educated, and low-educated workers, respectively. In column (3)-(4), the numbers of observations are 136,455, 308,669, and 241,167 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

ufacturing counterparts, leading to labor reallocation. However, such reallocation is only seen among the low-educated. In the first two years of the reform, unemployed workers tend to stay in the labor force, leaving the labor force participation rate unchanged. However, some of the low-educated unemployed might become discouraged workers and leave the labor market between the third and the fifth year; otherwise, the increase in the unemployment rate of the low-educated would have been larger than the increase in the short-term.

In sum, the results reveal that a revenue-neutral carbon tax program is implemented at the cost of the traditionally disadvantaged groups, the low-educated. While the reform has no employment effect on high-educated workers, medium- and low-educated workers suffer from a higher unemployment rate. Moreover, only low-educated workers are found to bear the reallocation cost, which is shown to be significant (Curtis, 2014). Furthermore, the adverse employment effect is long-lasting so that some of the low-educated might become discouraged and leave the labor market in the long-term. While the high-educated are free from job loss or labor reallocation, the CTS reform is implemented at the cost of the medium- and the low-educated. Moreover, such cost of job loss and labor reallocation is not a one-shot but a dynamic effect. Although this paper does not aim to make a moral judgment on any policy, this result contributes to a broader literature that expresses concerns on environmental injustice: any environmental regulation might shift pollution into regions with more poor and minority populations (Banzhaf and Walsh, 2008; Kaswan, 2008; Gamper-Rabindran and Timmins, 2011). Here, we are uncertain whether traditionally disadvantaged groups are more likely to expose themselves to pollution under the CTS reform, but this paper does provide solid evidence that environmental policies, like the CTS reform, are implemented at the cost of the low-educated.

Furthermore, this paper provides empirical supports for economic modelings. In a loose sense, this result does reveal that pollution (carbon use) and workers (the medium- and the low-educated) are gross complements in production. Since this paper is the first to document the adverse effect of carbon taxes on the overall unemployment rate, the findings could serve as an empirical foundation for the treatment of pollution as an input (Pethig, 1976; Copeland and Taylor, 1994; Tombe and Winter, 2015). Next, the findings also indicate that heterogeneous employment effects should be considered in any computable general equilibrium (CGE) analyses. For example, Beck et al. (2015) show that the negative welfare effect of the CTS reform on households with below-median income is smaller than that on households with above-median income. Nevertheless, the model ignores the employment effect of a policy that favors the high-educated, not the low-educated. Also, the results of this paper suggest that conducting welfare analysis without considering the heterogeneous employment effects may underestimate the social cost of similar policies. As documented, the overall adverse employment effect is significant, which are sometimes considered in the class of CGE model with homogeneous

workers (Hafstead and Williams III, 2016). Nevertheless, the fact that such enormous employment effects only happen to the traditionally disadvantaged groups is often (if not completely) neglected in the CGE literature. Therefore, the results of this section call for the reexamination of a similar welfare analysis that incorporates the heterogeneous employment effects of the reform into the CGE model.

5 Conclusion

This paper exploits a unique design feature of the CTS reform to bring new evidence to bear on two important questions. First, how do policy shocks affect labor market outcomes? Second, how and why do the effects differ across educational groups?

This paper provides support that the CTS reform leads to an increase of 1.8-1.9 percentage points in the overall unemployment rate but has no effect on the number of weekly working hours over five years. The reform depresses labor demand, thereby lowering the hiring rate. As a consequence, the unemployment rate and the length of the unemployment spell and the job tenure increase. Moreover, this paper provides evidence for the adverse employment effect on medium- and low-educated workers but not their high-educated counterparts. The results are robust to several model specifications. Hence, this article concludes that revenue-neutral carbon taxes cause the overall male unemployment rate to rise by 1.8 percentage points and the unemployment rate of the medium- and the low-educated to increase by 1.6 and 2.8 percentage points two years following the policy.

Since the high energy-intensive manufacturing industry and the utilities industry are affected more by the reform, the increase in the unemployment rates in these industries is seen more than the increase in the low energy-intensive industries. Probably because, relative to the high-educated, more of the medium- and the low-educated engage in manufacturing industries that are more energy-intensive, the increases in the unemployment rates are higher for the medium- and the low-educated. Due to the higher unemployment rate, some of the unemployed might shift from the high energy-intensive manufacturing industries to their low energy-intensive manufacturing counterparts, leading to labor reallocation. However, such reallocation is only seen among the low-educated. In the first two years of the reform, unemployed workers tend to stay in the labor force, leaving the labor force participation rate unchanged. However, some of the low-educated unemployed might become discouraged workers and leave the labor market between the third and the fifth year; otherwise, the increase in the unemployment rate of the low-educated would have been more than 2.9 percentage points.

Most of the results are robust to alternative model specifications. Also, I find no anticipatory effect of the reform. Using the strategies of the multiple pseudo-treatment groups and the

multiple pseudo-intervention dates, the placebo tests provide solid support for the validity of the common trend assumption. Also, the suggestive evidence is provided to illustrate the low likelihood of the shift of the labor demand and supply out of BC in the short-term. Moreover, the estimation finds no evidence that the CTS reform alters the labor force participation rate in BC in the short-term, suggesting that these results are reassuring and not subject to any selection biases. Evidence suggests that the documented employment effect on the overall unemployment rate is the lower bound of the true effect in the long-term. That is, if the low-educated unemployed had stayed in BC's labor market, the documented adverse employment effect on them would have been larger than 2.9 percentage points in the long-term. In conclusion, the documented heterogeneity in the effects of policy shocks like revenue-neutral carbon taxes are robust and considerable in both the short- and the long-term.

6 Online Appendix A: Other Impacts in the Labor Market

Table A.1: The Effect of the CTS on Usual Weekly Working Hours on the Main Job

Difference-in-Differences Analysis

Dependent Variable: ln(Usual Weekly Working Hours on the Main Job)
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2010

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	0.000	0.000	-0.000	-0.001	0.002
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Adjusted R-squared	0.002	0.010	0.024	0.076	0.076
Sample: High-Educated Male Workers					
BC×Post	0.002	0.002	0.002	0.001	0.003
	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
Adjusted R-squared	0.001	0.007	0.017	0.057	0.058
Sample: Medium-Educated Male Workers					
BC×Post	-0.000	-0.000	-0.001	-0.002	-0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.001	0.008	0.017	0.072	0.073
Sample: Low-Educated Male Workers					
BC×Post	-0.001	-0.001	-0.002	-0.001	0.003
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Adjusted R-squared	0.005	0.013	0.022	0.083	0.084
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.2: The Effect of the CTS on Usual Weekly Working Hours on All Jobs

Difference-in-Differences Analysis

Dependent Variable: ln(Usual Weekly Working Hours on All Jobs)
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2010

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	0.001	0.001	0.001	0.000	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Adjusted R-squared	0.002	0.012	0.024	0.072	0.073
Sample: High-Educated Male Workers					
BC×Post	0.002	0.003	0.002	0.002	0.004
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)
Adjusted R-squared	0.001	0.007	0.017	0.053	0.053
Sample: Medium-Educated Male Workers					
BC×Post	0.002	0.002	0.001	0.000	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.001	0.010	0.018	0.068	0.069
Sample: Low-Educated Male Workers					
BC×Post	-0.002	-0.002	-0.003	-0.002	0.002
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Adjusted R-squared	0.005	0.016	0.026	0.082	0.083
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.3: The Effect of the CTS on Actual Weekly Working Hours on the Main Job

Difference-in-Differences Analysis

Dependent Variable: ln(Actual Weekly Working Hours on the Main Job)
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2010

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	-0.001	-0.001	-0.002	-0.003	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.014	0.016	0.023	0.050	0.050
Sample: High-Educated Male Workers					
BC×Post	-0.004	-0.003	-0.004	-0.005	-0.004
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Adjusted R-squared	0.016	0.018	0.025	0.040	0.040
Sample: Medium-Educated Male Workers					
BC×Post	-0.005	-0.005	-0.006	-0.008	-0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Adjusted R-squared	0.014	0.016	0.022	0.051	0.051
Sample: Low-Educated Male Workers					
BC×Post	0.004	0.004	0.003	0.004	0.007
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Adjusted R-squared	0.014	0.017	0.024	0.057	0.058
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.4: The Effect of the CTS on Actual Weekly Working Hours on All Jobs

Difference-in-Differences Analysis					
Dependent Variable: ln(Actual Weekly Working Hours on All Jobs)					
Treatment Group: BC					
Control Group: MB, NB, NS, PE, QC, & SK					
Estimation Window: July 2007-June 2010					
Post-Reform Period: July 2008-June 2010					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	-0.000	0.000	-0.001	-0.002	0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.014	0.017	0.024	0.050	0.050
Sample: High-Educated Male Workers					
BC×Post	-0.003	-0.003	-0.003	-0.004	-0.003
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
Adjusted R-squared	0.017	0.018	0.025	0.038	0.039
Sample: Medium-Educated Male Workers					
BC×Post	-0.003	-0.003	-0.004	-0.005	-0.003
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Adjusted R-squared	0.014	0.017	0.023	0.050	0.050
Sample: Low-Educated Male Workers					
BC×Post	0.004	0.004	0.003	0.003	0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Adjusted R-squared	0.015	0.019	0.026	0.058	0.058
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.5: The Effect of the CTS Reform on the Unemployment Duration During 2008-2010

Difference-in-Differences Analysis			
Dependent Variable: Log(Unemployment Duration)			
Treatment Group: BC			
Control Group: MB, NB, NS, PE, QC, & SK			
Estimation Window: July 2007-June 2010			
Post-Reform Period: July 2008-June 2010			
	(1)	**(2)**	**(3)**
Sample: All Male Workers			
BC×Post	0.137**	0.128**	0.135**
	(0.058)	(0.058)	(0.057)
Adjusted R-squared	0.003	0.012	0.021
Sample: High-Educated Male Workers			
BC×Post	0.148	0.148	0.136
	(0.111)	(0.110)	(0.111)
Adjusted R-squared	0.004	0.009	0.035
Sample: Medium-Educated Male Workers			
BC×Post	0.065	0.058	0.049
	(0.086)	(0.086)	(0.082)
Adjusted R-squared	0.002	0.008	0.017
Sample: Low-Educated Male Workers			
BC×Post	0.241***	0.228***	0.245***
	(0.081)	(0.080)	(0.080)
Adjusted R-squared	0.003	0.018	0.024
Year×Month Fixed Effect	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes
Age	No	No	Yes
Marital Status	No	No	Yes

Notes: Unemployment Duration is measured in weeks. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who are unemployed. The numbers of observations are 3,284, 12,683, and 13,388 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.6: The Effect of the CTS Reform on the Job Tenure During 2008-2010

Difference-in-Differences Analysis					
Dependent Variable: Log(Job Tenure)					
Treatment Group: BC					
Control Group: MB, NB, NS, PE, QC, & SK					
Estimation Window: July 2007-June 2010					
Post-Reform Period: July 2008-June 2010					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	0.072**	0.072**	0.059**	0.053**	0.047*
	(0.028)	(0.027)	(0.024)	(0.025)	(0.025)
Adjusted R-squared	0.001	0.002	0.124	0.155	0.155
Sample: High-Educated Male Workers					
BC×Post	0.038	0.038	0.049	0.044	0.037
	(0.042)	(0.042)	(0.034)	(0.035)	(0.036)
Adjusted R-squared	0.002	0.004	0.165	0.189	0.189
Sample: Medium-Educated Male Workers					
BC×Post	0.077**	0.077**	0.055**	0.047*	0.043
	(0.030)	(0.030)	(0.027)	(0.027)	(0.028)
Adjusted R-squared	0.001	0.002	0.116	0.150	0.150
Sample: Low-Educated Male Workers					
BC×Post	0.093**	0.094**	0.083**	0.077**	0.072*
	(0.037)	(0.037)	(0.035)	(0.036)	(0.037)
Adjusted R-squared	0.002	0.005	0.110	0.147	0.148
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Job Tenure is measured in months. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who are employed. The numbers of observations are 89,410, 205,321, and 148,940 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.7: The Wage Effect of the CTS Reform During 2008-2010

Difference-in-Differences Analysis

Dependent Variable: Log(Real Wage)
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2010

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	0.004	0.004	0.002	0.002	0.001
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Adjusted R-squared	0.015	0.033	0.174	0.273	0.273
Sample: High-Educated Male Workers					
BC×Post	0.006	0.005	0.009	0.005	0.010
	(0.015)	(0.015)	(0.015)	(0.013)	(0.013)
Adjusted R-squared	0.000	0.009	0.083	0.190	0.191
Sample: Medium-Educated Male Workers					
BC×Post	0.014	0.014	0.010	0.010	0.009
	(0.010)	(0.010)	(0.009)	(0.008)	(0.008)
Adjusted R-squared	0.019	0.041	0.089	0.215	0.215
Sample: Low-Educated Male Workers					
BC×Post	-0.006	-0.003	-0.006	-0.004	-0.006
	(0.011)	(0.010)	(0.010)	(0.011)	(0.011)
Adjusted R-squared	0.032	0.071	0.107	0.208	0.209
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Wages are CPI-adjusted, and are measured in Canadian dollar. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the male aged 25-54 who are employed. The numbers of observations are 72,960, 169,929, and 116,984 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.8: The Effect of the CTS on Usual Weekly Working Hours on the Main Job

Difference-in-Differences Analysis

Dependent Variable: ln(Usual Weekly Working Hours on the Main Job)
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2008 & July 2010-June 2013
Post-Reform Period: July 2010-June 2013

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	0.000	0.000	-0.000	-0.001	0.002
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Adjusted R-squared	0.002	0.010	0.024	0.076	0.076
Sample: High-Educated Male Workers					
BC×Post	0.002	0.002	0.002	0.001	0.003
	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
Adjusted R-squared	0.001	0.007	0.017	0.057	0.058
Sample: Medium-Educated Male Workers					
BC×Post	-0.000	-0.000	-0.001	-0.002	-0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.001	0.008	0.017	0.072	0.073
Sample: Low-Educated Male Workers					
BC×Post	-0.001	-0.001	-0.002	-0.001	0.003
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Adjusted R-squared	0.005	0.013	0.022	0.083	0.084
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2008 and July 2010-June 2013. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.9: The Effect of the CTS on Usual Weekly Working Hours on All Jobs

Difference-in-Differences Analysis

Dependent Variable: ln(Usual Weekly Working Hours on All Jobs)
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2008 & July 2010-June 2013
Post-Reform Period: July 2010-June 2013

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	0.001	0.001	0.001	0.000	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Adjusted R-squared	0.002	0.012	0.024	0.072	0.073
Sample: High-Educated Male Workers					
BC×Post	0.002	0.003	0.002	0.002	0.004
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)
Adjusted R-squared	0.001	0.007	0.017	0.053	0.053
Sample: Medium-Educated Male Workers					
BC×Post	0.002	0.002	0.001	0.000	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.001	0.010	0.018	0.068	0.069
Sample: Low-Educated Male Workers					
BC×Post	-0.002	-0.002	-0.003	-0.002	0.002
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Adjusted R-squared	0.005	0.016	0.026	0.082	0.083
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2008 and July 2010-June 2013. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.10: The Effect of the CTS on Actual Weekly Working Hours on the Main Job

Difference-in-Differences Analysis

Dependent Variable: ln(Actual Weekly Working Hours on the Main Job)
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2008 & July 2010-June 2013
Post-Reform Period: July 2010-June 2013

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	-0.001	-0.001	-0.002	-0.003	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.014	0.016	0.023	0.050	0.050
Sample: High-Educated Male Workers					
BC×Post	-0.004	-0.003	-0.004	-0.005	-0.004
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Adjusted R-squared	0.016	0.018	0.025	0.040	0.040
Sample: Medium-Educated Male Workers					
BC×Post	-0.005	-0.005	-0.006	-0.008	-0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Adjusted R-squared	0.014	0.016	0.022	0.051	0.051
Sample: Low-Educated Male Workers					
BC×Post	0.004	0.004	0.003	0.004	0.007
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Adjusted R-squared	0.014	0.017	0.024	0.057	0.058
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2008 and July 2010-June 2013. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.11: The Effect of the CTS on Actual Weekly Working Hours on All Jobs

Difference-in-Differences Analysis					
Dependent Variable: ln(Actual Weekly Working Hours on All Jobs)					
Treatment Group: BC					
Control Group: MB, NB, NS, PE, QC, & SK					
Estimation Window: July 2007-June 2008 & July 2010-June 2013					
Post-Reform Period: July 2010-June 2013					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Male Workers					
BC×Post	-0.000	0.000	-0.001	-0.002	0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.014	0.017	0.024	0.050	0.050
Sample: High-Educated Male Workers					
BC×Post	-0.003	-0.003	-0.003	-0.004	-0.003
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
Adjusted R-squared	0.017	0.018	0.025	0.038	0.039
Sample: Medium-Educated Male Workers					
BC×Post	-0.003	-0.003	-0.004	-0.005	-0.003
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Adjusted R-squared	0.014	0.017	0.023	0.050	0.050
Sample: Low-Educated Male Workers					
BC×Post	0.004	0.004	0.003	0.003	0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Adjusted R-squared	0.015	0.019	0.026	0.058	0.058
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Data come from the Canadian LFS July 2007-June 2008 and July 2010-June 2013. Samples are restricted to the males aged 25-54 who participates to the labor market. Education dummies for seven educational categories are controlled in models (3)-(5) in the analysis using the sample of all male workers. The numbers of observations are 82,759, 188,992, and 136,682 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.12: The Heterogeneous Effects of the CTS Reform on Employment Across Industries During 2010-2013

Difference-in-Difference-in-Differences Analysis

Dependent Variable: Unemployed
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2008 & 2010-June 2013
Post-Reform Period: July 2010-June 2013

	(1)	(2)	(3)	(4)
Sample	**All**	**Hi-Edu**	**Med-Edu**	**Low-Edu**
BC×Post	0.012***	0.002	0.011**	0.020***
	(0.004)	(0.005)	(0.005)	(0.006)
BC×Post×Utilities	0.011	0.012	0.009	0.005
	(0.009)	(0.008)	(0.015)	(0.027)
BC×Post×Mining, Oil, & Gas	-0.001	0.009	-0.001	-0.015
	(0.015)	(0.028)	(0.018)	(0.024)
Durable Manufacturing Industry				
BC×Post×High Energy-Intensive	-0.001	-0.036	0.012	-0.003
	(0.013)	(0.036)	(0.014)	(0.017)
BC×Post×Low Energy-Intensive	0.003	0.014	0.013	-0.016
	(0.016)	(0.040)	(0.024)	(0.020)
Non-Durable Manufacturing Industry				
BC×Post×High Energy-Intensive	0.031***	0.027	0.015	0.048*
	(0.010)	(0.029)	(0.016)	(0.026)
BC×Post×Low Energy-Intensive	-0.009	0.026	-0.028	-0.011
	(0.015)	(0.021)	(0.024)	(0.021)
Adjusted R-squared	0.027	0.008	0.020	0.041
Year×Month Fixed Effect	Yes	Yes	Yes	Yes
Province Fixed Effect	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Marital Status	Yes	Yes	Yes	Yes
Education	Yes	No	No	No

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2008 and 2010-June 2013. Samples are restricted to the males aged 25-54 who participates to the labor market. The numbers of observations are 126,710, 282,399, and 203,582 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table A.12: The Heterogeneous Effects of the CTS Reform on Employment Across Industries During 2010-2013 (Cont.)

Difference-in-Difference-in-Differences Analysis

Dependent Variable: Unemployed
Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2008 & 2010-June 2013
Post-Reform Period: July 2010-June 2013

	(1)	**(2)**	**(3)**	**(4)**
Sample	**All**	**Hi-Edu**	**Med-Edu**	**Low-Edu**
BC×Post×Agriculture	0.026	-0.058	0.015	0.041
	(0.016)	(0.040)	(0.023)	(0.030)
BC×Post×Information, Culture	0.002	0.009	0.010	-0.017
	(0.007)	(0.016)	(0.012)	(0.020)
BC×Post×Health Care	-0.013	-0.005	-0.004	-0.042***
	(0.009)	(0.012)	(0.013)	(0.015)
BC×Post×Management,	0.014	0.022	0.014	0.013
Administrative & Other Support	(0.012)	(0.041)	(0.018)	(0.019)
BC×Post×Other Services	-0.003	0.023	-0.016	0.007
	(0.010)	(0.024)	(0.013)	(0.017)
BC×Post×Retail Trade	-0.001	0.015	-0.013	0.001
	(0.006)	(0.016)	(0.012)	(0.009)
BC×Post×Transportation	-0.007	0.031**	-0.018**	-0.011
& Warehousing	(0.006)	(0.014)	(0.009)	(0.010)
Adjusted R-squared	0.027	0.008	0.020	0.041
Prob>F-Statistics of Equality to Zero	0.298	0.164	0.291	0.265
Year×Month Fixed Effect	Yes	Yes	Yes	Yes
Province Fixed Effect	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Marital Status	Yes	Yes	Yes	Yes
Education	Yes	No	No	No

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2008 and 2010-June 2013. Samples are restricted to the males aged 25-54 who participates to the labor market. The numbers of observations are 126,710, 282,399, and 203,582 for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

7 Online Appendix B: The Validity of the Underlying Assumptions

To interpret the estimates as the causal impact of the CTS reform, important assumptions must hold. First, there is no anticipatory effect. Second, a common trend assumption is valid. Third, a SUTVA is satisfied. That is, the CTS reform does not alter the unemployment rate in the control provinces. Fourth, the reform does not alter the sample selection process. More specifically, the labor participation and migration decisions are unaffected by the reform. Fifth, there exists no shock, policy, or law other than the CTS reform that affects specifically to BC's unemployment rate within the period of examination. Although most of these assumptions are not directly testable, a few steps are proposed to assess their plausibility.

7.1 Assessing the Anticipatory Effect

The analyses of this paper assume that there is no employment effect of the CTS reform prior to its implementation on July 1, 2008. Despite a short period between the announcement and the implementation of the reform (less than 4.5 months), I estimate the announcement effect on BC's unemployment rate using a triple difference (DDD) approach. The pre- and the post-announcement periods are defined as January and the months between March and June, respectively. The observations in February are excluded because half of the month indeed belongs to the pre-announcement period, and the other half of February belongs to the post-announcement period. Hence, the first difference arises from the difference in the likelihood of being unemployed between the pre- and the post-announcement period. However, such difference might not capture the causal effect of the announcement because of seasonality. That is, it is possible that the unemployment rate between March and June is different from that in January. Therefore, I control the seasonal employment effect between the two periods using the observations in 2005, 2006, and 2007. The seasonal employment effect between 2005 and 2007 serves as the second difference. Lastly, the employment effect in 2008 could be country- and year-specific because of the recession. Hence, I include the observations from the other six provinces to control the country-specific effect. Therefore, the other six provinces serve as the third difference. The DDD estimates can be obtained from the following regression model:

$$\begin{aligned} Y_{ijt} &= \alpha + \beta_1(Treat_j \times Y2008_t \times Post_p) + \beta_2(Treat_j \times Y2008_t) + \beta_3(Treat_j \times Post_p) \\ &\quad + \beta_4(Y2008_t \times Post_p) + \beta_5 Treat_j + \beta_6 Y2008_t + \beta_7 Post_p + X_{ijt}^T\gamma + \varepsilon_i \end{aligned}$$

$Y2008_t$ equals one if a respondent was observed in 2008, and zero otherwise. $Post_p$ equals one if a respondent is observed between March and June, and zero otherwise. Samples are restricted to the male respondents from BC and the other six control provinces in January, March, April, May, and June during 2005-2008. Control variables include dummies for the age, marital status, educational level, industry, and industry$\times$year interaction terms of respondents. Again, standard errors are clustered at the level of province, age groups, and marital status, providing us with 84 clusters.

$\hat{\beta}_1$ will capture the announcement effect on BC's overall unemployment rate. Estimates $\hat{\beta}_1$, $\hat{\beta}_2$, $\hat{\beta}_3$, and $\hat{\beta}_4$ are reported in Table B.1. The results indicate that the $\hat{\beta}_1$ range from -0.004 to 0.001; the estimates are materially and statistically insignificantly different from zero at any conventional level. Hence, the results reveal that BC's unemployment rate does not increase between the announcement and the implementation of the CTS reform, providing no statistical support of any anticipatory effect.[22]

Next, it is possible that the public could have anticipated and responded to the CTS reform before its announcement (February 19, 2008). In this case, I expect that the keyword "Carbon Tax" would frequently be searched on the Google search engine in BC before February 2008. I therefore plot the weekly Google trend of the keyword "Carbon Tax" in BC during 2004-2010 in Figure B.1. The y-axis measures the weekly frequency of the keyword "Carbon Tax" relative to the most frequent week, at which the frequency is normalized to 100. According to the figure, the sharp peak happens in the 27th week of 2008, the week before the implementation of the CTS reform. The second most frequent week takes place in the 8th week of 2008, the week following the official announcement of the policy. Obviously, the search frequencies in other weeks are low, reflecting that the public did not anticipate the reform prior to the announcement. In sum, this section finds no evidence on any anticipatory effect before the announcement date or the implementation date of the reform.

7.2 Assessing the Common Trend Assumption

Another assumption is that the employment trajectories of workers in the control provinces are representative of the trajectories that would have been observed from workers of similar characteristics had the CTS reform not been introduced in BC. I utilize (i) pseudo-treatment groups, (ii) pseudo-intervention dates, and (iii) the combination of (i) and (ii) to address the issue.

First, I redefine the treatment group as workers in BC, the five pseudo-treatment groups as workers in MB, NS, PE, QC, and SA, and the control group as workers in NB. If the common

[22] $\hat{\beta}_1$, $\hat{\beta}_2$, and $\hat{\beta}_3$ provide insight on the other assumption.

trend assumption is satisfied, the negative treatment effect is likely to be found in the treatment group but not in those five pseudo-treatment groups. If the main DID estimate for BC happens be negative, the common trend assumption between BC and NB is doubted. If most of the estimates are materially and statistically indifferent from zero, the trends of the overall unemployment rate in MB, NS, PE, QC, SA, and NB are not parallel. In this case, the estimated employment effect is likely biased because the assumption of the common trend between BC and some of the control provinces is probably unsatisfied. Equation (3) in Section 3 is estimated, where $Prov_j$ denotes BC and the five pseudo-treatment groups j. Coefficients are estimated using a linear probability model, and the estimates $\hat{\beta}_{1j}$ are reported in Table B.2. Each column shows estimates from a different model specification, which is described at the bottom of the table.

The estimates indicate that the reform causes BC's unemployment rate to decline by 1.7 percentage points at one percent significant level in the preferred model, and the estimates are robust to model specifications. Meanwhile, the estimates show that changes in the likelihood of being unemployed among workers of similar characteristics are small in magnitude and are statistically distinguishable from zero among the five of the six control provinces in Column 2. However, the estimate is small in magnitude, less than half of the estimates in BC. Moreover, the estimate is only marginally statistically significant at 10 percent level in one of the four model specifications but is insignificant in the other three specifications. Hence, I conclude that the probability of unemployment of observably similar workers follows similar paths among the six control provinces.

Second, I evaluate the credibility of the assumption using multiple pseudo-intervention dates: July 2004, July 2005, and July 2006. I extend the period of examination to July 2004-June 2015, where July 2007-June 2008 is the reference period. I estimate the pseudo-treatment effect using equation (4). Control variables include the age, marital status, and educational level of respondents. Figure B.2 displays the main DID estimates $\hat{\beta}_{1t}$ using *Unemployed* as a dependent variable. Each dot represents the main DID estimate using one control province in the corresponding period from July of year t to June of year $t+1$. For example, the first dot represents the main DID estimate of the period July 2004-June 2005, reflecting the employment effect relative to the reference period July 2007-June 2008. Since July 2007-June 2008 is a reference period, the corresponding DID estimate is normalized to zero. The vertical line represents July 2008, at which point the CTS reform begins.

In this exercise, attention should be paid to the yearly employment effects before July 2008. If there exists any employment effect in a pseudo-intervention date, the unemployment trends in BC and the control group are not parallel in the corresponding year. In this case, the credibility of the common trend assumption is doubtful. On the contrary, if the yearly estimates in the

pseudo-intervention dates are all close to zero, the trend in BC and the average trend in the control group are parallel during July 2004-June 2008 and are likely to be parallel immediately following July 2008, at which point the CTS reform is implemented. In addition to the analysis of the entire sample, I also conduct the analysis using samples of three various educational groups to verify the credibility of the assumption of the common trend between BC and the six control provinces prior to the reform.

According to Figure B.2, estimates, largely in line with the assumption, remain steady around zero in the pre-reform period (July 2004-June 2008). The results suggest that the employment effects are close to zero prior to the reform, confirming that the unemployment trends are similar in the four consecutive years before the implementation of the reform. Equally importantly, the estimates increase subsequent to July 2008. In fact, both the steady trends in the pre-reform period and the increase in the estimates following July 2008 sufficiently provide the support on the credibility of the common trend assumption. Additionally, the estimates continue to rise following July 2009, but such an increase is not as large as that after July 2008. This finding is in line with the annual increase in the carbon tax. The identical features are found in the figures using the entire sample, the medium-educated sample, and the low-educated sample.

Recall that the employment effect on the high-educated is found to be zero. In the figure using the high-educated sample, the yearly employment effects are all statistically indifferent from zero, revealing that the unemployment rate of the high-educated in BC and the other control provinces share an identical trend for 10 consecutive years. This evidence provides more solid support for the credibility of the common trend assumption in the other two educational groups.

Lastly, I repeat this analysis six times using a different control province as a pseudo-treatment group. The observations from BC are excluded. Using MB, NB, NS, PE, QC, and SK as a pseudo-treatment group in each of the analyses, the other five provinces serve as a control group. The corresponding main DID estimates are displayed in the Figure B.3-8. According to the figures, subsequent to July 2008, the unemployment rates for medium- and low-educated workers are higher in MB relative to the other control groups. Such increases are small relative to the ones in BC. Also, there is an extraordinary fall in MB's unemployment rates during July 2006-June 2007, which is used as a reference period in the estimation. This explains why the employment effects are all negative in the years other than July 2006-June 2007. In fact, MB's unemployment rates are quite steady except for during the reference period. For each of the other control provinces, the unemployment rates are materially and statistically stable throughout the entire period of examination, regardless of educational level. This exercise suggests that not only is the trend in BC parallel to the average trend in the control group, but also parallel to the trend in each of the six control groups. In sum, this subsection, though it does not directly

verify the validity of the common trend assumption, provides solid evidence for the common trend assumption upon which the DID method heavily relies on.

7.3 Assessing the Stability of Unit Treatment Value Assumption

This paper thus far has assumed that the CTS reform has no employment effect on the control group. Throughout the paper, our analyses involve two types of control groups: workers in the control provinces and workers in low energy-intensive industries (in the analysis of the heterogeneous employment effects across industries).

I first address the SUTVA on the control provinces from two dimensions: the labor demand side and the labor supply side. If the introduction of the reform shifts production (the labor demand) from BC to the other provinces, this would bias our DID estimates. The estimates will then capture both the increase in BC's unemployment rate and the fall in the unemployment rate in the control provinces, exaggerating the negative employment effect. On the contrary, if the reform encourages more of BC's unemployed workers to migrate to the other control provinces, our estimates at best serve as the lower bound of the adverse employment effect.

First, if the DID estimates capture the shift in labor demand from BC to the control provinces, the unemployment rate is expected to increase in BC before it increases in the control provinces. Recall that the public does not anticipate the CTS reform before its official announcement. If there is a shift in labor demand, BC's unemployment rate should start to rise between the date of announcement and the date of implementation. Nevertheless, the estimates shown in Table B.1 suggest that BC's unemployment rate is statistically no different from the rates in the control provinces during this period, providing no support for the shift in the labor demand.

Second, if the CTS reform shifts the labor demand to the control provinces subsequent to the implementation of the reform, I expect that the unemployment rates of the control provinces drop after July 2008. However, the unemployment rate might increase in all provinces due to the recession. If the unemployment rate in any control province happens to fall after July 2008, the SUTVA is doubtful. Figure B.9 displays yearly unemployment rate by control province. Each dot represents an unemployment rate six months before and after the corresponding year indicated on the x-axis. The vertical dash line represents the timing of the implementation of the reform. The figure illustrates that the unemployment rate in each of the six control provinces follows a decreasing trend before mid-2008 but increases afterward, providing no evidence of the fall in their unemployment rates. Therefore, our analyses find no signs of the shift in labor demand to support the violation of the SUTVA.

Next, I proceed to investigate the shift in the labor supply after the reform. Using the number of migration into and out of BC as two dependent variables, I estimate the migration

effects by estimating equation (1) in Section 3. The main DID estimates are plotted in Figure B.10. The vertical dash line represents the timing of the implementation of the CTS reform. The figure shows that, relative to the number in control provinces, the number of BC's in-migrations and out-migrations are small and less than 1000 people during July 2008-June 2011.[23] Although the estimate suggests that the BC's out-migration amounts to about 2000 people more during July 2012-June 2013, the number seems to be small as compared to BC's total population. It is, therefore, safe to conclude that the estimated short-term employment effects are unlikely biased due to the shift in labor supply between BC and the control provinces, but the long-term estimates might be slightly biased.[24]

Regarding the SUTVA on the low energy-intensive industries, Hafstead and Williams III (2016) argue that if workers are reallocated from "regulated" industries to "unregulated" industries, the DID estimates are likely exaggerated. First, there are no unregulated industries in the CTS reform; carbon taxes were applied to the consumption of fossil fuels in BC (households and industries). If there is any labor reallocation from the high energy-intensive industries to the low energy-intensive ones, the overall adverse employment effects (the decline of 1.8 percentage points) have already taken the reallocation into account. In other words, the overall employment effect will not be biased due to any labor reallocation among industries within BC.

Second, labor reallocation, if exists, probably shifts workers to industries that require similar skills. While workers likely move from the high energy-intensive manufacturing industries to their low energy-intensive counterparts, the likelihood that labor reallocation from manufacturing industries to other industries, such as education services, accommodation and food services, and health care and social assistance, etc., is indeed low. In the analysis of heterogeneous effects across industries, the utilities industry, high energy-intensive manufacturing industries, low energy-intensive manufacturing industries, and the other nine industries are considered as the treatment groups. While signs of labor reallocation to the low energy-intensive manufacturing industries for the low-educated workers are found, the evidence that there is reallocation to the other nine industries is at best weak. Therefore, reallocation from manufacturing industries to control industries, such as education services, accommodation and food services, and health care and social assistance etc., is unlikely to be the case.

In sum, this subsection provides suggestive evidence to support that the possibilities of the employment effect on the control groups, both the workers in the control provinces and the ones in the low energy-intensive industries, are small, reassuring the credibility of the SUTVA.

[23] According to Statistics Canada, the average population between July 2008-June 2011 is about 4,423,523.

[24] If the unemployed are more likely to seek jobs in the other provinces, the documented long-term employment effect could at least serve as the lower bound. That is, if the unemployed had stayed in BC, the documented unemployment rate would have been larger.

7.4 Assessing the Selection Issue

This subsection explores the possibility of biases arising from sample selection process. For example, if the CTS reform alters the decisions on labor force participation, it would potentially lead to selection biases. Suppose unemployed workers become discouraged and decide not to seek any job because of the reform. They would be classified as non-participants in the data, not unemployed workers. Consequently, the fraction of unemployed workers becomes smaller in the sample of examination, thereby lowering the estimated likelihood of unemployment. The estimated adverse employment effect (1.8 percentage points) could at best serve as a lower bound of the true effect. However, Table 9 in Section 4.5 indicates that the decisions on labor force participation are unaffected in the short-term regardless of educational level. Hence, the estimated employment effects are free from any selection bias arising from the sample selection process in the short-term. As highlighted in the previous section, the labor force participation rate of the low-educated does decline in the long-term, suggesting that the adverse employment effects (both the overall effect and the effect on the low-educated) could at best serve as a lower bound. That is, if the unemployed had stayed in the labor force, the estimated adverse employment effect would have been larger.

Furthermore, I investigate the change in the composition of the labor force subsequent to the reform. Equation (3) in Section 3 is estimated and the dependent variables are the indicator variables for *Age Group 25-34*, *Age Group 35-44*, *The High-Educated*, and *The Medium-Educated*. *Age Group 25-34* (*Age Group 35-44*) equals one if a respondent ages 25-34 (25-44), and zero otherwise. Table B.3 reports the $\hat{\beta}_1$ and the five $\hat{\theta}_j$. Results indicate that none of the estimates are statistically distinguishable from zero, providing no evidence for the changes in the demographic characteristics in BC or the six control provinces. Recall that the sample of examination includes male aged 25-54. If the proportion of workers aged 25-44 does not change, the fraction of workers aged 45-54 remains unchanged as well. Similarly, the results suggest that the proportion of high- and medium-educated workers remains constant over time; hence, the fraction of the low-educated remains stable. These analyses conclude that the composition of the labor force does not change in BC and the six control provinces.

7.5 Assessing the Causes for the Employment Effect Other than the CTS Reform

This paper documents that BC's unemployment rate increases subsequent to July 2008. This finding may indicate that the CTS reform causes BC's unemployment rate to rise by 1.8 percentage points, or it may reflect the existence of other negative employment shocks that specifically affect BC's labor market. Despite no implementation of any new environmental or labor market

policy other than the CTS reform in BC around July 2008, the estimated employment effect could be driven by the recession of 2007-2009.[25] It is true that the recession had an adverse employment effect to the Canadian economy as a whole, but if the negative effect was larger in BC's economy, such unparallel impact on BC could explain the negative DID estimates in the post-reform period. This subsection explores the possibility that the negative employment effect is attributed to the recession.

Table B.1 shows that the coefficients of Y2008×March-June are around 0.006, significant at five percent level, suggesting that the recession causes the unemployment rate in BC and the control provinces to rise by 0.6 percentage points during March-June in 2008. Meanwhile, the coefficients of BC×Y2008×March-June are materially and statistically indifferent from zero. The results indicate that the recession causes the unemployment rate to rise in BC and the control provinces, and such an increase is statistically no different between BC and the other control provinces prior to the implementation of the CTS policy. This evidence indicates that the recession does not lead to a larger adverse effect on BC's unemployment rate prior to the reform.

Suppose that the recession happens to exert a larger impact on the BC's economy only after July 2008. I expect that the impact is identical across industries and educational groups. Nevertheless, as discussed in Subsection 4.4, the documented negative employment effect is larger in high energy-intensive durable manufacturing, high energy-intensive non-durable manufacturing, and utilities industry but is identical across other industries, including low energy-intensive durable manufacturing and low energy-intensive nondurable manufacturing industries. The heterogeneity in the adverse employment effect fits the tax structure of the CTS reform. Also, while the adverse employment effects for the medium- and the low-educated are found, the unemployment rate of the high-educated are unaffected. This evidence casts doubt on the claim that the estimated negative employment effect stems from the recession.

Furthermore, Table B.2 documents that following July 2008 the unemployment rate is higher in BC relative to NB. Meanwhile, the estimates suggest that the employment effects are statistically identical across all the control provinces as in NB. Therefore, if the increase in the unemployment rate in BC is driven by the recession, then the negative shock of the recession is specifically large in BC but is identical in each of the six control provinces.

To be consistent with the empirical findings, the documented negative employment effect in BC could be attributed to the recession only if the employment effect of the recession satisfies all the following features. First, its employment effect happens to be identical in BC and the control provinces prior to July 2008. Second, the effect is specifically large in BC subsequent to July

[25] The NBER's Business Cycle Dating Committee dates the beginning of the recession to be December 2007 and the end to be June 2009.

2008 but identical across the control provinces. Third, its impact is larger in the high energy-intensive manufacturing and utility industries but identical across other industries. Fourth, the employment effect is more significant for BC's medium- and low-educated workers, who are likely to work in the industries that are more energy-intensive. Fifth, the effect on BC's high-educated workers is identical to the effect in the control provinces. While the likelihood that these five conditions hold simultaneously is low (if not close to zero), this paper concludes that the documented adverse employment effect (1.8 percentage points) is attributable to the CTS reform, not the recession.

Table B.1: Robustness Check—No Anticipatory Effect

Difference-in-Difference-in-Differences Analysis

Dependent Variable: Unemployed
Treatment Group: BC in 2008
Control Group 1: BC in 2005-2007
Control Group 2: MB, NB, NS, PE, QC, & SK in 2008
Estimation Window: January, March-June of 2005-2008
Treatment Period: March-June 2008

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
BC×Y2008×March-June	0.003	0.004	0.002	-0.001	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
BC×Y2008	0.004	0.003	0.005	0.005	0.006
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
BC×March-June	0.005	0.005	0.005	0.006	0.006
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Y2008×March-June	0.007**	0.006**	0.006**	0.007**	0.007**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Adjusted R-squared	0.003	0.010	0.031	0.040	0.041
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Education Level	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Unemployed equals one if a respondent is unemployed, and zero otherwise. Data come from the Canadian LFS. Samples are restricted to the males aged 25-54 who participates to the labor market. Yearly dummies are controlled in all models. The numbers of observations are 259,638 and 256,123 in column (1)-(3) and (4)-(5), respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table B.2: Common Trend Assumption

Difference-in-Differences Analysis

Dependent Variable: Employed
Treatment Group: BC
Five Pseudo-Treatment Groups: MB, NS, PE, QC, & SK
Control Group: NB
Estimation Window: July 2007-June 2013
Post-Reform Period: July 2008-June 2013

	(1)	**(2)**	**(3)**	**(4)**
BC×Post	0.015**	0.017***	0.015***	0.014***
	(0.006)	(0.005)	(0.004)	(0.004)
MB×Post	0.007	0.008*	0.006	0.006
	(0.005)	(0.005)	(0.004)	(0.004)
NS×Post	-0.003	-0.001	0.002	0.002
	(0.007)	(0.007)	(0.005)	(0.005)
PE×Post	0.005	0.006	0.007	0.007
	(0.011)	(0.010)	(0.009)	(0.009)
QC×Post	-0.006	-0.006	-0.002	-0.002
	(0.005)	(0.004)	(0.004)	(0.004)
SK×Post	-0.006	-0.005	-0.004	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Adjusted R-squared	0.008	0.026	0.032	0.033
Year×Month Fixed Effect	Yes	Yes	Yes	Yes
Age	No	Yes	Yes	Yes
Marital Status	No	Yes	Yes	Yes
Education Level	No	Yes	Yes	Yes
Industry	No	No	Yes	Yes
Industry×Year	No	No	No	Yes

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the males aged 25-54 who participates to the labor market. BC is the treatment province. Control provinces include MB, NB, NS, PE, QC, and SK. The numbers of observations are 940,405 and 926,393, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table B.3: Changes in Demographic Characteristics During 2008-2013

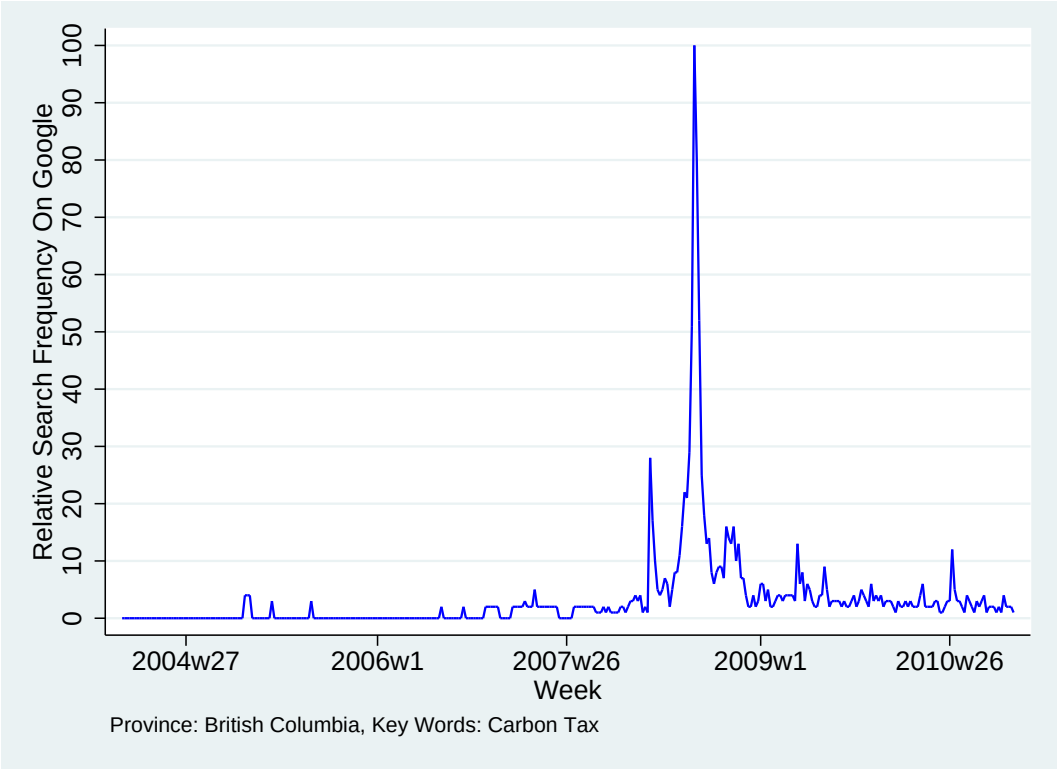
Difference-in-Differences Analysis

Treatment Group: BC
Five Pseudo-Treatment Groups: MB, NS, PE, QC, & SK
Control Group: NB
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2013

	(1)	**(2)**	**(3)**	**(4)**
Dependent Variable:	**Age Group**		**Education Group**	
	25-34	**35-44**	**High**	**Medium**
BC×Post	0.006	0.006	-0.000	0.011
	(0.014)	(0.015)	(0.010)	(0.010)
MB×Postp	0.016	-0.007	-0.006	0.012
	(0.019)	(0.020)	(0.010)	(0.012)
NS×Post	0.020	-0.000	0.018	0.015
	(0.016)	(0.019)	(0.014)	(0.012)
PE×Postp	-0.014	-0.008	0.017	0.015
	(0.022)	(0.020)	(0.014)	(0.015)
QC×Postp	0.004	0.015	-0.000	0.016
	(0.016)	(0.020)	(0.009)	(0.011)
SK×Postp	0.026	0.018	0.006	0.004
	(0.016)	(0.016)	(0.010)	(0.010)
Adjusted R-squared	0.002	0.001	0.003	0.005
Year×Month Fixed Effect	Yes	Yes	Yes	Yes
Province Fixed Effect	Yes	Yes	Yes	Yes

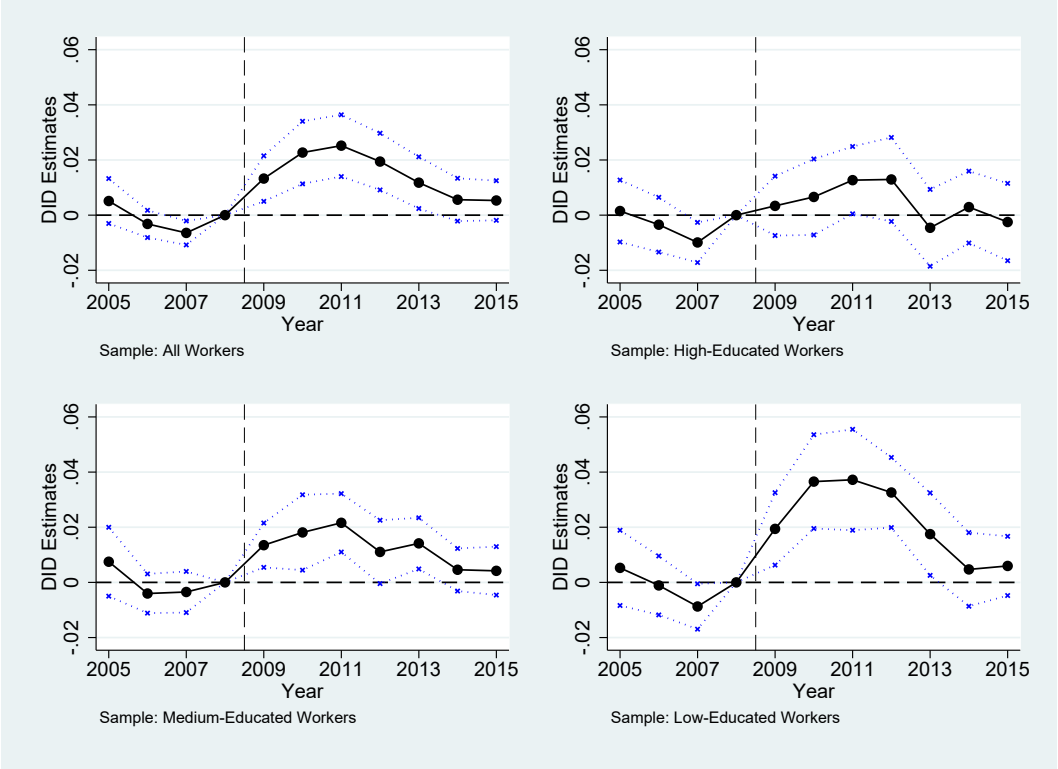
Age 25-34 (35-44) equals one if a respondent ages 25-34 (35-44), and zero otherwise. High-educated workers equal one if a respondent is a bachelor's degree holders and postgraduates, and zero otherwise. Medium-educated workers equal one if a respondent receives some college education and associate degree. Data come from the Canadian LFS July 2007- June 2013. Samples are restricted to males aged 25-54 who participate to the labor market. BC is the treatment province. Placebo interventions take place in provinces: MB, NS, PE, QC, and SK. NB is the control province. The numbers of observation are 1,037,214 in all columns. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Figure B.1: The Google Trends of Carbon Tax in BC During 2004-2010



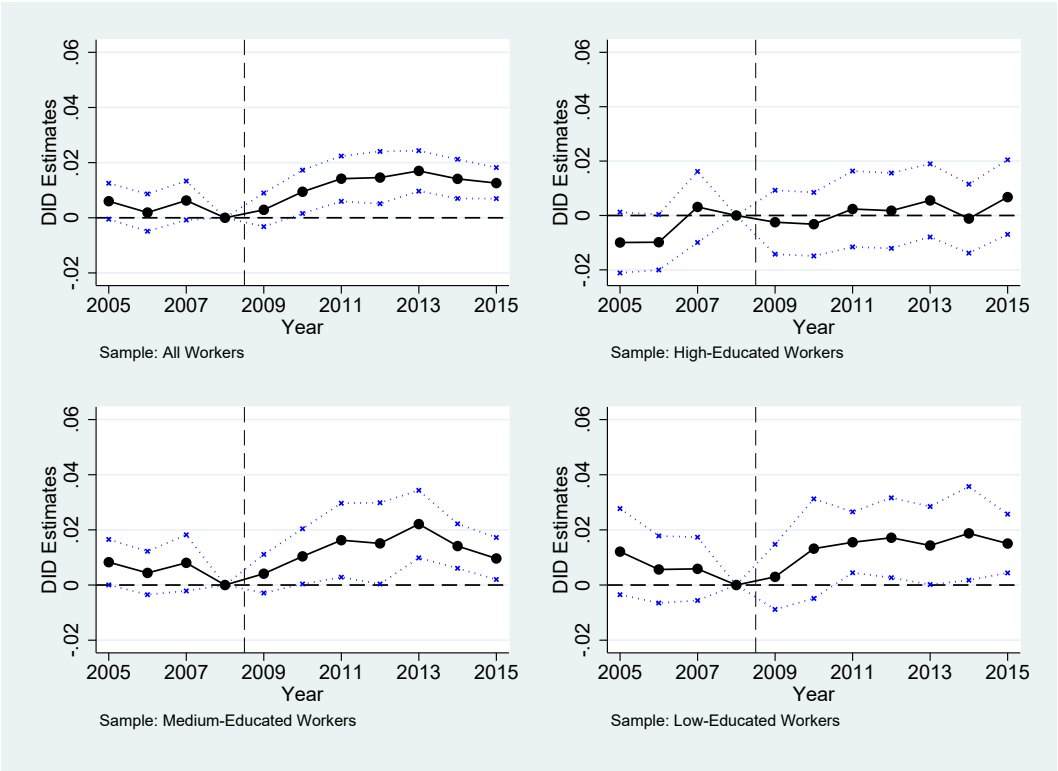
x-axis and y-axis indicate the timing and the relative frequency of the search of the words "Carbon Tax" on the Google search engine. The frequency in the most frequent week is normalized to 100. Hence, the value on the Y-axis measures the fraction of the search frequency relative to the most frequent week during 2004-2010.

Figure B.2: The Unemployment Effect of the CTS Reform



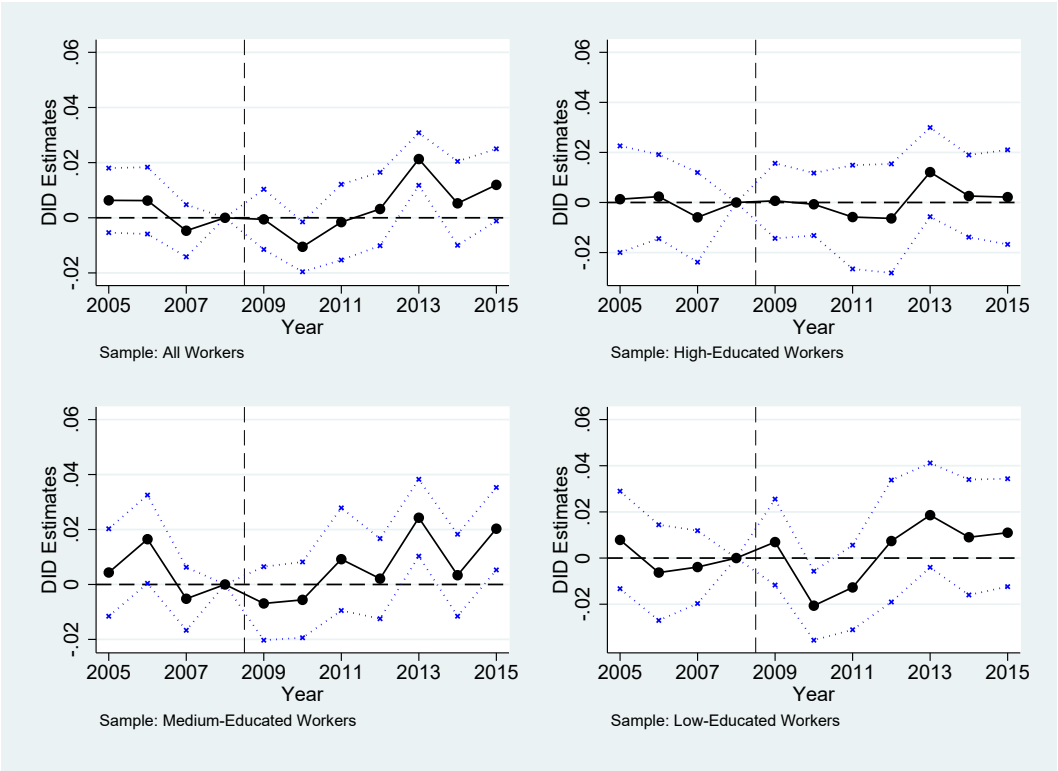
The dependent variable is Unemployed, which equals one if a respondent is unemployed, and zero otherwise. Control variables include dummies for year×month, province, age group, and marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to the male labor market participants aged 25-54. The reference period is July 2007-June 2008. Each dot represents the main DID estimate in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008). BC is the treatment province. Control provinces include MB, NB, NS, PE, QC,) and SK. The dashed line represents the 95 percent confidence interval.

Figure B.3: The Relative Unemployment Trend in Manitoba



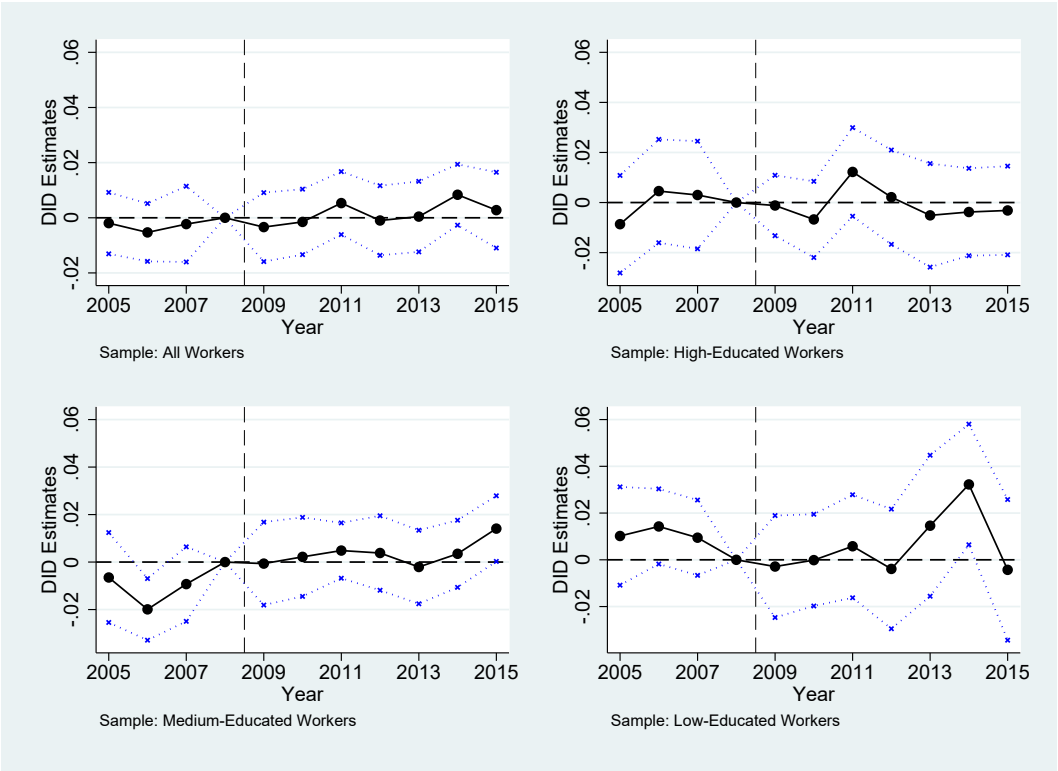
The dependent variable is Unemployed, which equals one if a respondent is unemployed, and zero otherwise. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to the male labor market participants aged 25-54. The reference period is July 2007-June 2008. Each dot represents the main DID estimate in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008). MB is the treatment province. Control provinces include NB, NS, PE, QC, and SK. The dash line represents the 95 percent confidence interval. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters)

Figure B.4: The Relative Unemployment Trend in New Brunswick



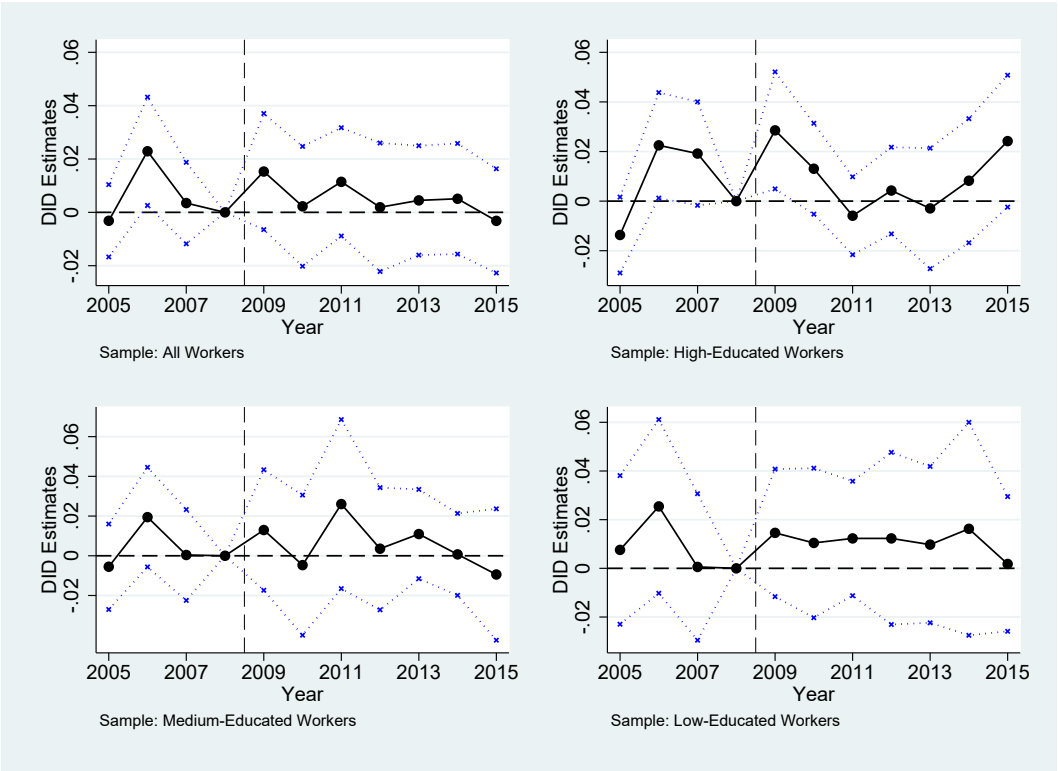
The dependent variable is Unemployed, which equals one if a respondent is unemployed, and zero otherwise. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to the male labor market participants aged 25-54. The reference period is July 2007-June 2008. Each dot represents the main DID estimate in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008). NB is the treatment province. Control provinces include MB, NS, PE, QC, and SK. The dash line represents the 95 percent confidence interval. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters)

Figure B.5: The Relative Unemployment Trend in Nova Scotia



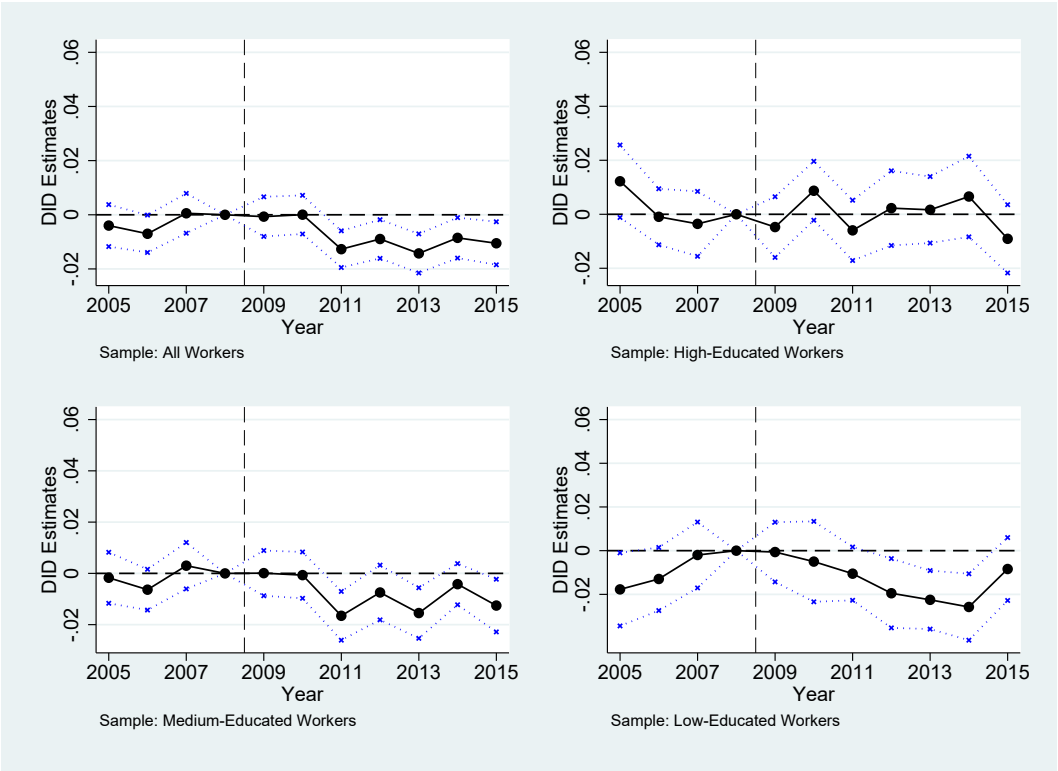
The dependent variable is Unemployed, which equals one if a respondent is unemployed, and zero otherwise. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to the male labor market participants aged 25-54. The reference period is July 2007-June 2008. Each dot represents the main DID estimate in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008). NS is the treatment province. Control provinces include MB, NB, PE, QC, and SK. The dash line represents the 95 percent confidence interval. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters)

Figure B.6: The Relative Unemployment Trend in Prince Edward Island



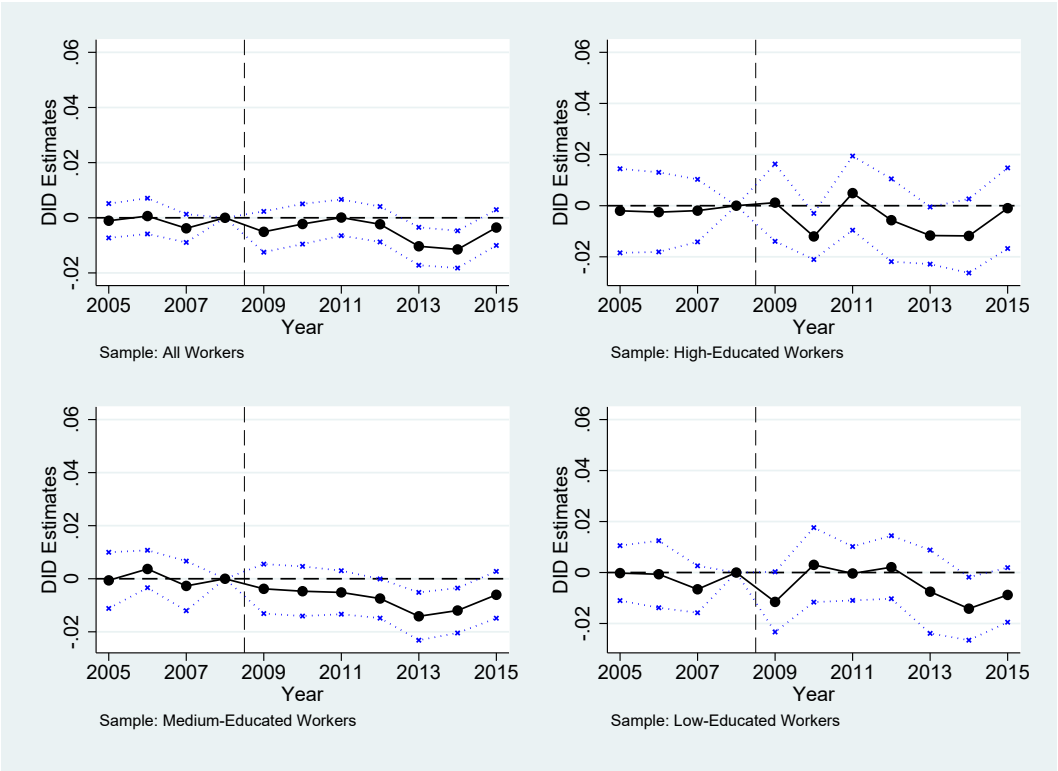
The dependent variable is Unemployed, which equals one if a respondent is unemployed, and zero otherwise. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to the male labor market participants aged 25-54. The reference period is July 2007-June 2008. Each dot represents the main DID estimate in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008). PE is the treatment province. Control provinces include MB, NB, NS, QC, and SK. The dash line represents the 95 percent confidence interval. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters)

Figure B.7: The Relative Unemployment Trend in Quebec



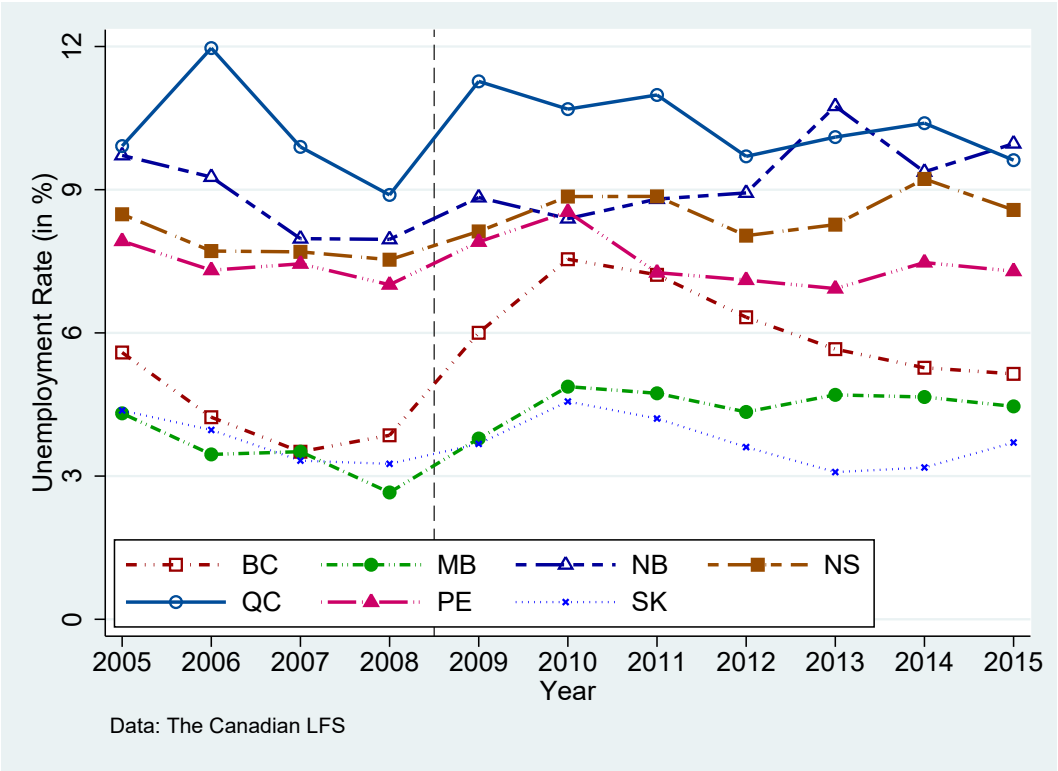
The dependent variable is Unemployed, which equals one if a respondent is unemployed, and zero otherwise. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to the male labor market participants aged 25-54. The reference period is July 2007-June 2008. Each dot represents the main DID estimate in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008). QC is the treatment province. Control provinces include MB, NB, NS, PE, and SK. The dash line represents the 95 percent confidence interval. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters)

Figure B.8: The Relative Unemployment Trend in Saskatchewan



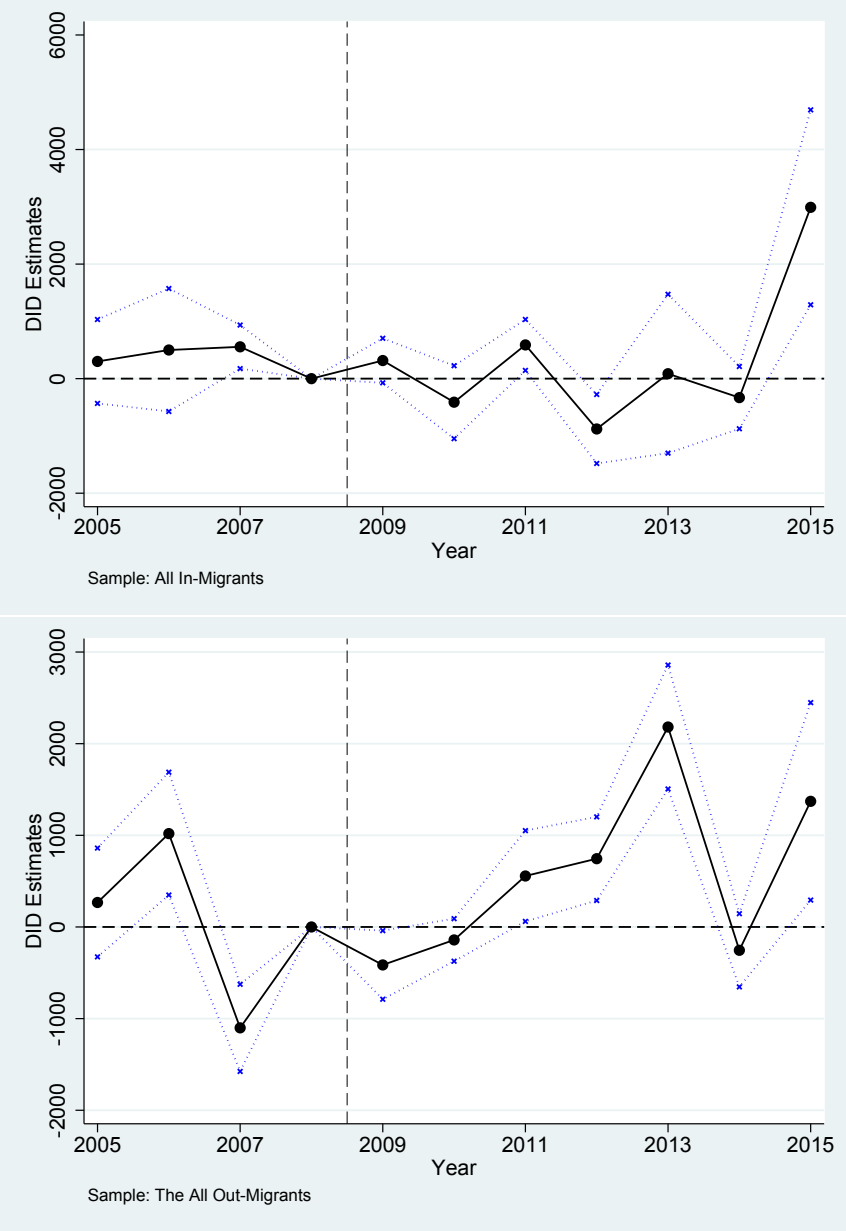
The dependent variable is Unemployed, which equals one if a respondent is unemployed, and zero otherwise. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all male workers. Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to the male labor market participants aged 25-54. The reference period is July 2007-June 2008. Each dot represents the main DID estimate in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008). SK is the treatment province. Control provinces include MB, NB, NS, PE, and QC. The dash line represents the 95 percent confidence interval. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters)

Figure B.9: The Unemployment Rate by Control Province



Data are from the Canadian LFS July 2004-June 2015. Samples are restricted to the male labor market participants who were 25-54 years old. Each dot represents the unemployment rate in the corresponding year. For example, the first dot represents the unemployment rate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008).

Figure B.10: The Relative Migration Trend in British Columbia



Data are from Statistics Canada. The reference period is July 2007-June 2008. Each dot represents the main DID estimate in the corresponding year. Bootstrapped standard errors are clustered at the level of province. The dot lines present the 95 percent confidence interval. For example, the first dot represents the main DID estimate of the period July 2004-June 2005. The vertical line represents the first month the CTS reform was implemented (July 2008). SK is the treatment province. Control provinces include MB, NB, NS, PE, and QC. The dependent variable is the total number of interprovincial in-migrant (The Upper Panel) and out-migrant (The Lower Panel) in each month.

8 Online Appendix C: A Comparison with Yamazaki (2017)

This section purposes to replicate the results in Yamazaki (2017) using the Canadian LFS data. In contrast to my study, Yamazaki (2017) utilizes industry-level data. I therefore utilize the samples of both males and females aged 25-54 in this replication exercise.

Besides the aggregation level of the data, the two studies are different in three dimensions: (i) the period of examination, (ii) the choice of control groups, and (iii) the definition of pre- and post-reform period. First, whereas Yamazaki (2017) restricted to his sample to the observations of 2001-2013, my main results examine the sample of July 2007-June 2010. Second, the two studies select two different sets of provinces as the control group. In Yamazaki (2017), AB, MB, NB, NL, NS, ON, PE, and SK serve as the control group. However, only MB, NB, NS, PE, SK, and QC are used as the control provinces in my study. Third, I define the period subsequent to July 2008 as the post-reform period because the CTS reform was implemented on July 1, 2008. Since Yamazaki (2017) utilizes yearly data, observations between 2008 and 2013 are defined as the post-reform period.

I show that the positive employment effect can be found only if the dimensions above are defined in the same way as the ones used in the research design of Yamazaki (2017). A slight variation in any one of the aspects would lead to the conclusion of a negative employment effect. In what follows, I obtain the coefficient estimates of the equation (1) in Section 3 using a linear probability model. The observations in BC are defined as the treatment group.

Variations in the Estimation Window. In this exercise, I estimate the employment effect of the reform using the same choice of control groups and the definition of the pre- and the post-reform period as in Yamazaki (2017). The employment effects are estimated using different estimation windows. The signs of the main DID estimates are reported in Table C.1. Each regression model controls for yearly and province fixed effects and dummies for gender, age, marital status, and educational level. The beginning and the end of the estimation window are indicated in the first column and the first row, respectively. According to Table C.1, the estimated employment effects are positive if the estimation window starts from 2001. The estimates are also positive with the estimation windows 2002-2010, 2002-2011, and 2003-2010. For the rest of the estimation windows, the estimates are all negative. For example, the employment effects are found to be negative if the estimation window starts with any year between 2004 and 2007. Recall that the common trend assumption is shown to be satisfied from 2004-2007, not including 2001. The inclusion of the observations of 2001 might require further evidence for the common trend assumption.

Variations in the Control Provinces. The employment effect is estimated using various sets of provinces as the control group. In this exercise, the examination period and the defi-

nitions of the pre- and the post-reform period are the same as in Yamazaki (2017). Estimates are reported in Table C.2. Each panel uses one set of provinces as a control group. Each column shows estimates from a different model specification, which is described at the bottom of the table. Panel A uses the same control group as in Yamazaki (2017). Unsurprisingly, the estimated employment effect is positive in the absence of controlled individual characteristics. In fact, the estimates turn negative once the dummies for industries are controlled. Yamazaki (2017) excludes observations in QC in his analysis. Once QC is considered as one of the control provinces, the estimates are all negative regardless of model specification (as shown in Panel B). I also estimate the employment effect using the same set of provinces as in my analysis to serve as the control group. Again, the coefficient estimates are all negative as shown in Panel C. Lastly, I conduct the same analysis using the provinces that are commonly used in both my study and Yamazaki (2017). Again, none of the estimates is positive.

The Correction of the Definition of the Post-Reform Period. As discussed, Yamazaki (2017) defines the period from 2008 onwards as the post-reform period. Nevertheless, the post-reform period began in July 2008. I estimate the employment effect using the correct post-reform period but stick to the same examination period and choice of control provinces as in Yamazaki (2017). According to Table C.3, the estimates are all negative regardless of the choices of model specification and control province.

These three simple exercises seem to conclude that the estimated positive employment effect is not robust to the choices of estimation window or control province. Moreover, once the definition of the post-reform period is corrected, the estimated employment effects are all negative.

Table C.1: Robustness to Estimation Windows

Difference-in-Differences Analysis				
Dependent Variable: Unemployed				
Treatment Group: BC				
	2010	2011	2012	2013
2001	-	-	-	-
2002	-	-	+	+
2003	-	+	+	+
2004	+	+	+	+
2005	+	+	+	+
2006	+	+	+	+
2007	+	+	+	+

Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. Control provinces include AB, MB, NB, NL, NS, ON, PE, and SK. Year$\times$Month and province fixed effects, and dummies for gender, age, marital status, and education are controlled.

Table C.2: Robustness to Control Provinces

Difference-in-Differences Analysis					
Dependent Variable: Unemployed					
Treatment Group: BC					
Estimation Window: January 2001-December 2013					
Post-Reform Period: January 2008-December 2013					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Panel A: Control Group: AB, MB, NB, NL, NS, ON, PE, & SK					
BC×Post	-0.0004	-0.0013	-0.0013	0.0007	0.0008
	(0.0021)	(0.0020)	(0.0017)	(0.0012)	(0.0011)
Adjusted R-squared	0.0013	0.0092	0.0225	0.0272	0.0281
Panel B: Control Group: AB, MB, NB, NL, NS, ON, PE, QC, & SK					
BC×Post	0.0018	0.0010	0.0009	0.0019	0.0021*
	(0.0021)	(0.0021)	(0.0018)	(0.0012)	(0.0012)
Adjusted R-squared	0.0013	0.0078	0.0215	0.0259	0.0267
Panel C: Control Group: MB, NB, NS, PE, QC, & SK					
BC×Post	0.0071***	0.0059***	0.0057***	0.0048***	0.0051***
	(0.0019)	(0.0020)	(0.0015)	(0.0010)	(0.0011)
Adjusted R-squared	0.0016	0.0058	0.0221	0.0282	0.0288
Panel D: Control Group: MB, NB, NS, PE, & SK					
BC×Post	0.0050***	0.0031*	0.0033**	0.0037***	0.0039***
	(0.0019)	(0.0018)	(0.0014)	(0.0011)	(0.0011)
Adjusted R-squared	0.0017	0.0078	0.0244	0.0314	0.0321
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Female	No	No	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Education Level	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	Yes	Yes

Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS. Samples are restricted to respondents aged 25-54 who participates to a labor market. In column (1)-(3), the numbers of observations are 5,870,870, 7,131,554, 3,992,506, and 2,731,822 for Panel A-D, respectively. In column (4)-(5), the numbers of observations are 5,777,945, 7,011,220, 3,925,582, and 2,692,307 for Panel A-D, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. Significance levels: ***=1%, **=5%, *=10%.

Table C.3: Robustness to the Definition of the Post-Reform Period

Difference-in-Differences Analysis					
Dependent Variable: Unemployed					
Treatment Group: BC					
Estimation Window: July 2001-June 2013					
Post-Reform Period: July 2008-June 2013					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Panel A: Control Group: AB, MB, NB, NL, NS, ON, PE, & SK					
BC×Post	0.0016	0.0007	0.0008	0.0026**	0.0026**
	(0.0022)	(0.0021)	(0.0018)	(0.0013)	(0.0012)
Adjusted R-squared	0.0014	0.0092	0.0223	0.0270	0.0278
Panel B: Control Group: AB, MB, NB, NL, NS, ON, PE, QC, & SK					
BC×Post	0.0038*	0.0031	0.0031*	0.0040***	0.0041***
	(0.0022)	(0.0022)	(0.0019)	(0.0013)	(0.0013)
Adjusted R-squared	0.0013	0.0078	0.0213	0.0257	0.0264
Panel C: Control Group: MB, NB, NS, PE, QC, & SK					
BC×Post	0.0095***	0.0082***	0.0083***	0.0072***	0.0073***
	(0.0020)	(0.0020)	(0.0016)	(0.0011)	(0.0011)
Adjusted R-squared	0.0016	0.0058	0.0218	0.0278	0.0283
Panel D: Control Group: MB, NB, NS, PE, & SK					
BC×Post	0.0072***	0.0054***	0.0057***	0.0058***	0.0058***
	(0.0020)	(0.0020)	(0.0016)	(0.0011)	(0.0011)
Adjusted R-squared	0.0017	0.0077	0.0240	0.0309	0.0315
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Female	No	No	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Education Level	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	Yes	Yes

Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS. Samples are restricted to the male respondent aged 25-54 who participates to the labor market. In column (1)-(3), the numbers of observations are 5,870,870, 6,584,637, 3,682,810, and 2,519,227 for Panel A-D, respectively. In column (4)-(5), the numbers of observations are 5,335,732, 6,474,322, 3,621,774, and 2,483,184 for Panel A-D, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. Significance levels: ***=1%, **=5%, *=10%.

9 Online Appendix D: The Employment Effect on Females

Table D.1: The Negative Employment Effect of the CTS Reform on Female During 2008-2010

Difference-in-Differences Analysis					
Dependent Variable: Unemployed					
Treatment Group: BC					
Control Group: MB, NB, NS, PE, QC, & SK					
Estimation Window: July 2007-June 2010					
Post-Reform Period: July 2008-June 2010					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Female Workers					
BC×Post	0.013***	0.012***	0.012***	0.013***	0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Adjusted R-squared	0.001	0.004	0.015	0.022	0.022
Sample: High-Educated Female Workers					
BC×Post	0.014***	0.014***	0.014***	0.012***	0.012***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Adjusted R-squared	0.005	0.005	0.006	0.013	0.014
Sample: Medium-Educated Female Workers					
BC×Post	0.009***	0.009***	0.009***	0.011***	0.010***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Adjusted R-squared	0.001	0.002	0.004	0.016	0.017
Sample: Low-Educated Female Workers					
BC×Post	0.017**	0.016**	0.017***	0.016***	0.017***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Adjusted R-squared	0.001	0.012	0.017	0.032	0.033
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the females aged 25-54 who participates to the labor market. In column (1)-(3), the numbers of observation are 113,616, 216,805, and 121,745 for high-educated, medium-educated, and low-educated, respectively. In column (4)-(5), the numbers of observation are 112,517, 213,998, and 118,909 for high-educated, medium-educated, and low-educated, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table D.2: The Effect of the CTS Reform on Female Weekly Working Hours During 2008-2010

Difference-in-Differences Analysis

Dependent Variable
Column 1: ln(Usual Weekly Working Hours on the Main Job)
Column 2: ln(Usual Weekly Working Hours on All Jobs)
Column 3: ln(Actual Weekly Working Hours on the Main Job)
Column 4 : ln(Actual Weekly Working Hours on All Jobs)

Treatment Group: BC
Control Group: MB, NB, NS, PE, QC, & SK
Estimation Window: July 2007-June 2010
Post-Reform Period: July 2008-June 2010

	(1)	**(2)**	**(3)**	**(4)**
Sample: All Female Workers				
BC×Post	-0.005	-0.006	-0.014*	-0.015**
	(0.007)	(0.007)	(0.007)	(0.007)
Adjusted R-squared	0.013	0.013	0.020	0.020
Sample: High-Educated Female Workers				
BC×Post	0.003	-0.002	-0.015	-0.021
	(0.012)	(0.012)	(0.014)	(0.014)
Adjusted R-squared	0.013	0.013	0.020	0.020
Sample: Medium-Educated Female Workers				
BC×Post	-0.025***	-0.022**	-0.029***	-0.026***
	(0.009)	(0.009)	(0.009)	(0.009)
Adjusted R-squared	0.012	0.012	0.018	0.017
Sample: Low-Educated Female Workers				
BC×Post	0.017	0.013	0.010	0.007
	(0.016)	(0.016)	(0.017)	(0.016)
Adjusted R-squared	0.010	0.009	0.014	0.013

Notes: Data come from the Canadian LFS July 2007-June 2010. Samples are restricted to the females aged 25-54 who participates to the labor market. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all female workers. The numbers of observation are 94,294, 182,597, and 101,667 for high-educated, medium-educated, and low-educated, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table D.3: The Negative Employment Effect of the CTS Reform on Female During 2010-2013

Difference-in-Differences Analysis					
Dependent Variable: Unemployed					
Treatment Group: BC					
Control Group: MB, NB, NS, PE, QC, & SK					
Estimation Window: July 2007-June 2010 & July 2010-June 2013					
Post-Reform Period: July 2010-June 2013					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Sample: All Female Workers					
BC×Post	0.016***	0.015***	0.015***	0.012***	0.011***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Adjusted R-squared	0.001	0.004	0.016	0.022	0.022
Sample: High-Educated Female Workers					
BC×Post	0.016***	0.016***	0.016***	0.010***	0.010***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Adjusted R-squared	0.004	0.004	0.005	0.011	0.013
Sample: Medium-Educated Female Workers					
BC×Post	0.015***	0.015***	0.015***	0.010***	0.010***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Adjusted R-squared	0.002	0.004	0.006	0.017	0.017
Sample: Low-Educated Female Workers					
BC×Post	0.015***	0.013**	0.014***	0.014***	0.013***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.002	0.010	0.017	0.031	0.032
Year×Month Fixed Effect	Yes	Yes	Yes	Yes	Yes
Province Fixed Effect	No	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes
Marital Status	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Industry×Year	No	No	No	No	Yes

Notes: Unemployed equals one if a labor market participant is unemployed, and zero otherwise. Data come from the Canadian LFS July 2007-June 2008 and July 2010-June 2013. Samples are restricted to the females aged 25-54 who participates to a labor market. In column (1)-(3), the numbers of observation are 160,736, 283,802, and 150,476, for high-educated, medium-educated, and low-educated workers, respectively. In column (4)-(5), the numbers of observation are 158,664, 279,239, and 146,224, for high-educated, medium-educated, and low-educated workers, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

Table D.4: The Effect of the CTS Reform on Female Weekly Working Hours During 2010-2013

Difference-in-Differences Analysis				
Dependent Variable				
Column 1: ln(Usual Weekly Working Hours on the Main Job)				
Column 2: ln(Usual Weekly Working Hours on All Jobs)				
Column 3: ln(Actual Weekly Working Hours on the Main Job)				
Column 4 : ln(Actual Weekly Working Hours on All Jobs)				
Treatment Group: BC				
Control Group: MB, NB, NS, PE, QC, & SK				
Estimation Window: July 2007-June 2008 & July 2010-June 2013				
Post-Reform Period: July 2010-June 2013				
	(1)	**(2)**	**(3)**	**(4)**
Sample: All Female Workers				
BC×Post	-0.007	-0.009	-0.015*	-0.017*
	(0.009)	(0.009)	(0.009)	(0.009)
Adjusted R-squared	0.012	0.012	0.018	0.018
Sample: High-Educated Female Workers				
BC×Post	-0.001	-0.008	-0.017	-0.024**
	(0.010)	(0.009)	(0.011)	(0.011)
Adjusted R-squared	0.012	0.012	0.018	0.019
Sample: Medium-Educated Female Workers				
BC×Post	-0.024**	-0.022**	-0.028***	-0.026**
	(0.010)	(0.010)	(0.010)	(0.010)
Adjusted R-squared	0.012	0.011	0.016	0.015
Sample: Low-Educated Female Workers				
BC×Post	0.015	0.011	0.009	0.004
	(0.017)	(0.016)	(0.018)	(0.018)
Adjusted R-squared	0.008	0.007	0.011	0.011

Notes: Data come from the Canadian LFS July 2007-June 2008 and July 2010-June 2013. Samples are restricted to the females aged 25-54 who participates to the labor market. Control variables include dummies for year×month, province, age group, marital status. Education dummies for seven educational categories are controlled in the analysis using the sample of all female workers. The numbers of observation are 133,431, 238,733, and 125,111 for high-educated, medium-educated, and low-educated, respectively. Robust standard errors in parentheses are clustered at the level of province, five-year age group dummy, and marital status. (84 Clusters) Significance levels: ***=1%, **=5%, *=10%.

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