

# ON THE LABOR MARKET ADJUSTMENTS OF ENVIRONMENTAL TAXES

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## Abstract

I exploit British Columbia's carbon tax to study labor market adjustments in downturns. Combining matching with a difference-in-differences approach, I find that environmental taxes could lead to recession-like labor markets, allowing the study of how labor markets adjust to negative shocks. Average incumbent wages are found completely rigid over years, longer than many economists expect. Wage adjustments operate mainly through labor turnovers: hiring wages drop substantially. The tax increases job-separation rates and decreases job-finding rates, both of which are essential in explaining the unemployment effect. Despite substantially increasing unemployment, the shock increases mainly the number of unemployment spells, not the duration of unemployment spells, questioning the need to substantially extend benefit periods in downturns. This paper provides new micro-evidence on labor market adjustments, derives implications on labor market policies, and calls for careful attention to the transitional labor market adjustments of environmental policies.

**JEL Classification:** E24, H23, J31, J63, Q52

**Keywords:** Environmental Taxes; Labor Market Adjustments; Wage Rigidity; Job Transition Rates; Long-Term Unemployment.

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\*Department of Economics, University of Calgary, 2500 University Dr. N.W., Calgary, AB, T2N 1N4 (email: cman.yip@gmail.com) I am indebted to Lucija Muehlenbachs, Stefan Staubli, Atsuko Tanaka, and Trevor Tombe for their patience, guidance, encouragement support, many insightful conversations, and deep generosity of time. I thank Werner Antweiler, Don Fullerton, Fabian Lange, Charles Mason, Robert Oxoby, Brandon Schaufele, Meng Sun, and seminar participants at the 2019 NAREA Conference, the 5th Canadian Phd & Early Career Workshop in Environmental & Resource Economics, the 2019 Chinese Economists Society, the 2019 CEA Conference, the Environmental Tax Workshop 2018 at the University of Maryland, the 2018 AERE@SEA Conference, the 2018 CREE Annual Conference, the University of Calgary, and the Sun Yat-sen University for their useful comments.

# 1 Introduction

Conventional wisdom holds that negative shocks create job losses. For instance, oil crises increase cost of production, leading to the 1973 and the 1979 recessions and the early 1990s recession in the United States (Blanchard and Gali, 2007). Other examples include sectoral shocks such as environmental policies (Greenstone, 2002; Greenstone et al., 2012; Walker, 2013; Curtis, 2017; Yip, 2018) and import competitions (Autor et al., 2013, 2016). Despite decades of research, we know relatively little of labor market adjustments beyond unemployment and perhaps wage effects.

For example, nominal wage rigidity is considered to be a major cause of cyclical unemployment (Keynes, 1936). With this rigidity, expansionary monetary policies reduce real wages and thus increase employment. A similar essence can be found in dynamic models with nominal wage contracts in Fischer (1977) and Taylor (1980). Wage rigidity, together with price rigidity (Calvo, 1983), coins the foundation of the New-Keynesian literature. While Christiano et al. (2005) shows that nominal wage rigidity plays a more important role in explaining monetary policy shocks than nominal price rigidity, whether nominal wages are downward rigid and how wages are adjusted are yet to know.

Our understanding of how unemployment fluctuates is also equivocal. The seminal paper of Darby et al. (1986) claims that cyclical unemployment is almost entirely driven by variations in job-separation rates. While this belief has been prevalent for many years, Hall (2005c) says that “*in the modern US economy, recessions do not begin with a burst of layoffs*”, Shimer (2012) concludes that job-separation rates are comparatively acyclic, and many recent theoretical studies refer to Hall (2005a,b,c), Shimer (2005, 2012), and Hagedorn and Manovskii (2008) to motivate their treatment of job-separation rates as acyclical. The opposition between the two thoughts called for other waves of influential studies (Elsby et al., 2009; Fujita and Ramey, 2009; Elsby et al., 2013; Coles and Moghaddasi Kelishomi, 2018).

Unsurprisingly, without a good understanding of how unemployment and wage effects operate, the implication of labor market policies is also controversial. Unemployment benefit periods are often extended substantially to help smooth the consumption of unemployed workers over a prolonged period of unemployment in economic downturns (Jung and Kuester, 2015; Mitman and Rabinovich, 2015; Landais et al., 2018a,b). If unemployment fluctuations happen to be driven mainly by the number of unemployment spells, not the duration of unemployment spells, the extension of unemployment benefits periods serves little purpose. Rather, it may worsen the unemployment rate. While an extensive literature studies the policy impacts of unemployment benefits on labor market outcomes (Card and Levine, 2000; Røed and Zhang, 2003; Lalive, 2008; Caliendo et al., 2013; Schmieder et al., 2016), relatively little is known about the effect of negative shocks on long-term unemployment.<sup>1</sup>

This paper revisits these topics and studies how labor markets adjust in downturns. I argue that the equivocal understanding of the labor market adjustments arises, in part, because a vast literature studies labor market behaviors via recessions. However, the identification of the causal effects of negative shocks from recessions could be challenging. A recession is defined by the decline in economic activities, not

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<sup>1</sup>Short-term unemployment is defined as unemployment with unemployment duration less than six months. The rest of unemployment is defined as long-term unemployment.

by the shocks themselves.<sup>2</sup> As a result, the arrival dates of shocks are indefinite: we may observe the arrival date of their impacts but not the arrival date of the shocks. Moreover, the severity of shocks from recessions is usually vaguely measured by the magnitude of the decline in economic activities such as unemployment rates—one of the labor market outcomes, again not by the shocks themselves. Upon arrival, recessions are pervasive: their impacts affect almost every local labor market. To estimate the causal effects of recessions, an instrumental variable approach is often employed to tackle potential endogeneity problems. However, the justifications of an instrumental variable rely heavily on verbal arguments. Last but not least, recessions often coexist with other policies such as the extension of unemployment benefit periods and expansionary monetary policies, threatening the identification of the causal effects of recessions.

To establish the causal relationship, I exploit a unique opportunity provided by the introduction of a revenue-neutral carbon tax policy in British Columbia (BC). In principle, this policy, similar to an oil crisis, increases cost of production and potentially creates a negative shock. It allows clean identifications for four reasons. First, the pre- and post-shock periods are definite. The policy was introduced on July 1, 2008. Since then, the shock has influenced BC's labor market. Second, this policy provides a novel source of exogenous variations in the stringency of the shock. The policy began with a carbon tax of CAD\$10 per tonne of carbon dioxide equivalent (CDE) emissions in 2008, and the carbon tax rate increased by CAD\$5 annually until it reached CAD\$30 in 2012. We directly observe the variations of the stringency through the shock, not through its labor market consequences. Third, this policy provides control labor markets from the rest of Canada (ROC) that are likely unaffected by the shock. Fourth, this policy was not followed by major policy reforms. Unlike recessions, there did not exist any substantial extension of unemployment benefit periods or expansionary monetary policies in BC, relative to the ROC.

Combining the Coarsened Exact Matching (CEM) method with the difference-in-differences (DID) approach, this paper finds that environmental taxes, such as BC's carbon tax, even though revenue-neutral, lead to recession-like labor markets, providing a platform to study how labor market adjusts to shocks in downturns.

## 1.1 Related Literature on Labor Economics

This paper contributes to the literature that documents empirical evidence on nominal wage rigidity (Bowlus et al., 2002; Goette et al., 2007; Carneiro et al., 2012; Martins et al., 2012; Haefke et al., 2013; Barattieri et al., 2014; Elsby et al., 2016; Elsby and Solon, 2018). This paper differs from the literature in at least two dimensions. First, this paper utilizes a different approach to measure wage rigidity. A large body of literature measures wage rigidity by comparing individual wage growths (i.e., either the magnitude or the likelihood of wage growth, or both) between two or multiple points of time (Goette et al., 2007; Martins et al., 2012; Barattieri et al., 2014; Elsby et al., 2016; Elsby and Solon, 2018).

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<sup>2</sup>According to NBER, a recession does not define in terms of one measure; “rather, a recession is a significant decline in economic activity spread across the economy, lasting more than a few months, normally visible in real GDP, real income, employment, industrial production, and wholesale-retail sales”.

This approach utilizes lagged wages as reference levels to measure wage rigidity. When a majority of employees experience zero wage growth in downturns (i.e., the current wage is no different from the reference level), economists tend to consider it as evidence of nominal wage rigidity. Consider an extreme situation in which employees should have experienced positive wage growths in the absence of negative shocks. Because of the shocks, firms re-optimize their profits and find it optimal to leave wages unchanged. In the end, complete wage flexibility of an economy may allow firms to set the wages unchanged. If lagged wage levels are used as references to define wage rigidity, we will find another evidence of complete wage rigidity in the case of complete wage flexibility.

This paper, instead of using lagged wages, utilizes the wage levels employees should have received in the absence of shocks as references. If the actual wage level is statistically lower than the wage levels employees should have received otherwise, we conclude that wages are not completely rigid. Our approach requires a counterfactual wage level in which employees should have received; nevertheless, such counterfactual wage levels may not be provided by longitudinal data sets. This difficulty is also highlighted by [Elsby et al. \(2016\)](#): “*inferring convincingly clear-cut counterfactual (wage) distributions from observational data turns out to be beyond the reach of even the most skillful researchers*”. Applying a right method (i.e., the combination of the CEM and the DID approaches) to an appropriate policy scenario (i.e., BC’s carbon tax), this paper solves the problem: it compares the actual wage level with the one employees should have received in the absence of negative shocks (i.e., BC’s carbon tax).

Using this approach, my paper reconciles the sticky wage theory with recent documentations of wage procyclicality in the literature ([Bowlus et al., 2002](#); [Carneiro et al., 2012](#); [Martins et al., 2012](#); [Haefke et al., 2013](#); [Elsby et al., 2016](#)). I find that the causal effect of the shock on the nominal incumbent wage is negligible over years, reinforcing our understanding of nominal wage rigidity among incumbent employees. While a recent literature documents wage procyclicality among new hires, [Gertler et al. \(2016\)](#) argue that hiring wages are found to be procyclical partly because of wage gains through job-to-job transitions in expansions. By exploring the labor market adjustments under a negative shock, I contribute to this fierce debate by providing the causal estimate of the adverse wage effect among new hires. These findings provide support on (i) the sticky wage theory among incumbent employees and (ii) hiring wage procyclicality.

Another departure from this literature arises from the natures of shocks. While an extensive literature focuses on temporary shocks in recessions, the short periods of these shocks may hinder economists from exploring wage adjustments before recoveries. In contrast, the permanent shock created by BC’s carbon tax provides us with an opportunity to study wage adjustments during a prolonged post-shock period.

By exploiting this opportunity, this paper fine-tunes the conventional wisdom about the mechanism of wage rigidity. One of the “*textbook*” explanations of the sticky wage theory is that wages are not easily adjusted under labor contracts and they are adjusted through renegotiation of contracts ([Barro, 1977](#); [Fischer, 1977](#); [Thomas and Worrall, 1988](#)). Nevertheless, this paper finds that wages are completely rigid among incumbent employees in the first five years of the shock, over which incumbent employees are expected to renegotiate contracts at least once. This finding (i) suggests that downward nominal wage rigidity lasts longer than many economists expect (i.e., it last over five years), (ii) casts doubt on labor

contracts explaining rigidity and on wage adjustments operating through renegotiation during contract renewals, and (iii) provides evidence that average wages are slowly (and mainly) adjusted through new contracts during hiring processes.

Furthermore, this paper contributes to the board literature on unemployment fluctuations (Hall, 2005a,b,c; Davis et al., 2006; Fujita and Ramey, 2009; Elsby et al., 2009; Shimer, 2012; Elsby et al., 2013). It finds that over 40 percent of the unemployment effect is attributable to increased job-separation rates, and over half of the effect arises from decreased job-finding rates. These findings challenge the widespread view that job-separation rates are acyclical (Hall, 2005a,b,c; Shimer, 2012). In contrast to correlations documented in this literature, this paper provides causal estimates supporting that (i) job-separation rates increase in response to negative shocks, (ii) the variations in both job-separation and job-finding rates are essential in explaining unemployment fluctuations, and (iii) the impacts on job-finding rates last longer than job-separation rates.

This paper also speaks to the policy implications derived from this literature. Unemployment benefit periods are lengthened to help unemployed workers smooth consumption over the prolonged period of unemployment. These programs include the Emergency Unemployment Compensation and the Extended Benefits in the United States.<sup>3</sup> I find that about 79 percent of the unemployment effect of the shock is attributable to short-term unemployment. In other words, the shock mainly increases the number of unemployment spells, not the duration of unemployment spells. This finding raises important questions that contribute to a broad literature on financial support over the prolonged period of unemployment (Røed and Zhang, 2003; Lalive, 2008; Caliendo et al., 2013; Jung and Kuester, 2015; Mitman and Rabinovich, 2015; Kolsrud et al., 2018; Landais et al., 2018a,b): (i) if the extent to which negative shocks lengthen unemployment spells is limited, why are the entitlement periods of unemployment benefits extended substantially in economic downturns, and (ii) are the prolonged unemployment spells mainly attributed to the shocks or the extensions of benefit periods?

## 1.2 Related Literature on Environmental Economics

This paper adds to the literature that investigates the effects of environmental policies on labor market outcomes (Greenstone, 2002; Walker, 2011; Kahn and Mansur, 2013; Curtis, 2017; Yip, 2018). Environmental policies are often labelled as “job-killing” policies, fostering political forces undermining environmental law making and enforcement. An earlier empirical literature on environmental policies focuses on manufacturing employment. Yet, these policies may shift employment from manufacturing to other sectors, making the overall employment effect ambiguous.

This paper, complementary to the computable general equilibrium (CGE) literature (Hafstead and Williams, 2018; Castellanos and Heutel, 2019), reveals the overall impacts of environmental taxes in the labor market. While Hafstead and Williams (2018) shows that the unemployment effects of revenue-neutral carbon taxes are, at best, weak in a CGE model, Castellanos and Heutel (2019) concludes that

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<sup>3</sup>For example, the unemployment benefit periods extended from 26 weeks to 99 weeks in most of the states in the 2008 recession.

*“models omitting labor mobility frictions may greatly under-predict sectoral unemployment effects”*. Using microeconomic techniques, this paper speaks to the causal effects of revenue-neutral environmental taxes: they could increase the unemployment rate as high as two percentage points.<sup>4</sup>

The exploration of the policy impacts on the two job transition rates is novel. First, it finds that the increased job-separation rate and the decreased job-finding rate are essential in explaining the significant unemployment effect of environmental taxes. In other words, environmental taxes could increase the unemployment risk of the entire labor force either through the increased risk of being laid off or the decreased likelihood of being hired.

Second, this paper finds it essential to understand the policy impacts on job transition rates so as to fully understand the dynamics of the unemployment effects of environmental taxes. In the earlier stage of BC’s carbon tax, its significant unemployment effect is contributed by both job transition rates. Since the policy impact on the job-separation rate is found temporary, the unemployment effect shrinks in the later stage of the policy. It explains why the significant unemployment effect is short-lived even though the policy is permanent. However, the small unemployment effect persists because the policy impact on the job-finding rate lasts long. The diminishing unemployment effect simply reflects the functioning of the labor market and is likely unrelated to sectoral reallocation (Hafstead and Williams, 2018; Castellanos and Heutel, 2019) or the creation of green jobs (Wagner, 2005; Vona et al., 2018), both of which are prevalent arguments disclaiming the job-killing feature of environmental policies.

This paper, perhaps surprisingly, is the first to document the impact of environmental policies on returns to labor hours using micro-data. While prior studies document the adverse effects of environmental policies on earnings (Walker, 2013; Curtis, 2017), it is unclear whether the earnings losses arise from the reduction in returns to labor, measured by hourly wages, or the reduction in labor hours, the intensive margin of employment. This paper adds to the literature by documenting the adverse effect of the environmental tax on returns to labor hours among new hires, not among incumbent employees.<sup>5</sup>

This paper is closely linked to the literature on environmental justice (Banzhaf and Walsh, 2008; Gamper-Rabindran and Timmins, 2011; Banzhaf et al., 2019) and the regressivity of environmental policies (Metcalf, 1999; Fullerton and Heutel, 2007; Fullerton et al., 2011; Araar et al., 2011). While the adverse effects of BC’s carbon tax on unemployment and wages are larger for less-educated workers (i.e., traditionally disadvantaged groups), this paper, complementary to Yip (2018), raises concerns on the labor market analog of environmental justice and reveals the regressivity of environmental policies from the source side of labor income.

This paper advances our understanding in the labor market adjustments of environmental policies.

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<sup>4</sup>Yip (2018) uses a simple DID approach to estimate the unemployment effects of BC’s carbon tax. This paper differs from his in at least four dimensions. First, this paper examines the wage effects and the corresponding distributional effects, along with the unemployment effects, whereas his paper is silent on wage outcomes. Second, while his paper estimates the unemployment effect, this paper further decomposes the unemployment effect into the short-term and the long-term unemployment effects. Third, this paper investigates the processes by which carbon tax increases unemployment, which is absent in his paper. Fourth, this paper handles the problem with a more sophisticated approach (i.e., the CEM and the DID approach as opposed to a simple DID one in his), providing robust estimates.

<sup>5</sup>This finding coheres with Walker (2013) and Curtis (2017) in that environmental policies affect earnings through new hires.

By exploring the labor market adjustments, this paper sheds light on labor market policies along with environmental policies and uncovers the distributive costs in the labor market that are often neglected in the literature on the costs associated with environmental policies (Hazilla and Kopp, 1990; Bovenberg et al., 2005; Ryan, 2012; Fullerton and Monti, 2013; Williams et al., 2015; Rausch and Schwarz, 2016). Before moving to the next subsection, it is worth highlighting that this paper has no intention to support or object to any environmental policies. Rather, the implications of this paper resonate with Fried et al. (2018) in that “*policymakers must pay careful attention to not only the long-run outcomes, but also to the transitional welfare effects*” and the transitional labor market adjustments of environmental policies.

The paper is structured as follows. Section 2 describes identification strategies. Section 3 presents the labor market consequences of BC’s carbon tax. Section 4 paints the overall picture of the labor market adjustments and summarizes four lessons on labor economics and ten lessons on environmental economics from BC’s carbon tax.

## 2 Identification Strategies

### 2.1 Revenue-Neutral Carbon Tax in British Columbia

A carbon tax policy was introduced in BC in 2008. This tax is based on greenhouse gas emissions generated from burning fuels and is applied to the consumption of fossil fuels in BC including households and industries.<sup>6</sup> Carbon tax increases production cost, creating a negative shock. There are several reasons that BC’s carbon tax is a suitable medium to study the labor market adjustments.

First, the arrival date of shocks arising from the carbon tax is definite. While BC’s carbon tax policy was implemented on July 1, 2008, its labor market consequence follows after its implementation, providing the exact arrival date to define pre- and post-shock periods. Second, this policy provides a novel source of exogenous variations in the stringency of the shock. The severity of the shock from BC’s carbon tax increases with the carbon tax rate. The rate was initially CAD\$10 per tonne of CDE emissions in 2008, increased by CAD\$5 per tonne annually until reaching CAD\$30 per tonne of CDE on July 1, 2012, and stayed at CAD\$30 from that time onwards. If wages and unemployment happen to react to the shock, the sizes of the effects should increase with the tax rate and thus grow in the first few years. Third, this policy provides numerous control provinces that are likely unaffected by the shock, providing us numerous control labor markets that are potentially independent of the shock.

Fourth, the initial increase in 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.<sup>7</sup> If the initial increase

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<sup>6</sup>This policy is revenue-neutral so that the carbon tax revenues are returned to residents and firms through the reductions in corporate taxes, personal income taxes, and lump-sum transfers. Elgie and McClay (2013) give the details of the distribution of the tax revenues. The carbon tax revenues were used to lower the corporate income tax rate and the two lowest personal income tax rates by five percent. Meanwhile, the government funds a low-income tax credit and a rebate of up to \$200 for northern and rural homeowners.

<sup>7</sup>The initial carbon taxes were only CAD\$3.50, USD\$0.04, and USD\$5 per tonne of CDE in Quebec in 2007, the San Francisco Bay Area, California, in 2008, and Maryland in 2010, respectively.

was not high, we would be uncertain whether the labor market generally did not respond to shocks or the shock was too small. For example, a statistically insignificant wage effect can be attributable to wage stickiness, but the insignificant wage effect may simply reflect that the shock is weak. Indeed, [Rivers and Schaufele \(2015\)](#) find that BC's carbon tax policy decreases carbon dioxide emissions from gasoline consumption by 2.4 million tonnes during 2008-2012, suggesting that industries did respond to the policy.<sup>8</sup>

Nevertheless, this policy is by no means perfect. The public may react to this policy prior to their implementations. Estimates may, therefore, be contaminated by these anticipation effects: they end up capturing only the effects of the shocks in the post-policy period, not the entire effects of the shock. Notice that BC's carbon tax policy was announced on February 19 and was implemented on July 1 of the same year. The short period between the dates of announcement and implementation mitigates the potential anticipation effects of the shock, allowing us to capture the entire effects of the shock on the labor market adjustments. I will show in Subsection 3.7 that no statistical evidence on the anticipation effects on BC's carbon tax can be found.

## 2.2 Data Descriptions

The present study utilizes the public-use files of the Canadian Labour Force Survey (LFS) (July 2005-June 2015). The Canadian LFS is a monthly household survey, which includes approximately 100,000 individuals. 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. The public-use files are used so that the results of this paper can be easily replicated without accessing the data in Statistics Canada in Ottawa.

In addition to demographic information, such as age, gender, the highest education attained, marital status, etc., this survey includes detailed labor market variables, including months of tenure, industry etc.<sup>9</sup> I exclude workers aged 60 or above from my sample to avoid potential biases from retirement decisions. Key variables of interest are nominal wages and unemployment. Table 1 reports summary statistics on the hourly wage rates and the unemployment rates in BC and the ROC during the pre- and post-policy periods, which are July 2005-June 2008 and July 2008-June 2015, respectively.

Three points emerge from the table. First, hourly wage rates increase regardless of the province and educational category. Since there may exist a country-wide shock on the wage rates in Canada, a simple difference in the respective hourly wage rates between BC's pre- and post-policy periods may not be able to capture the wage effects of the carbon tax. Second, unemployment rates also increase for all educational levels in both BC and the ROC. For the same reason, a simple difference in the unemployment rates between the pre- and the post-policy periods fails to identify the unemployment effect. Third, the difference-in-differences in the hourly wage rates seem to suggest that the carbon tax

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<sup>8</sup>This finding is consistent with other studies. For example, per capita fuel consumption is found to decrease by 19 percent relative to the ROC ([Elgie and McClay, 2013](#); [Murray and Rivers, 2015](#)). [Elgie and McClay \(2013\)](#) show that the GHG emissions per capita drop by nine percent during the same period.

<sup>9</sup>I exclude observations with a missing value in any of the dummies for age, education, marital status, and the nature of job.



Table 1: Summary Statistics on Hourly Wage Rates and Unemployment Rates

	Average Hourly Wages			Unemployment Rates		
	BC	Rest of Canada	Difference- in-Differences	BC	Rest of Canada	Difference- in-Differences
<b>A. Sample: High-Educated Workers</b>						
July 2005-June 2008	23.30	24.27		3.38	3.68	
July 2008-June 2015	23.97	25.14		4.65	4.35	
Difference	0.67	0.87	-0.20	1.27	0.67	0.60
<b>B. Sample: Medium-Educated Workers</b>						
July 2005-June 2008	18.48	17.44		4.47	5.79	
July 2008-June 2015	19.41	18.66		6.36	6.54	
Difference	0.93	1.22	-0.29	1.89	0.75	1.14
<b>C. Sample: Low-Educated Workers</b>						
July 2005-June 2008	14.90	13.90		7.13	9.72	
July 2008-June 2015	15.93	14.96		10.00	11.04	
Difference	1.03	1.06	-0.03	2.87	1.32	1.55

Notes: Data come from the Canadian LFS July 2005-June 2015.

policy reduces the wage rates and increases the unemployment rates in BC regardless of educational category. However, the reduction could be driven partly or entirely by changes in the demographic composition of the sample. In order to identify the causal effects of BC's carbon tax, I will discuss the identification methods in the following subsection.

## 2.3 Identification Methods

I combine the CEM method with the DID approach to capture the causal effect of the carbon tax policy. Since the policy was implemented in BC, I define the treatment group as survey respondents in BC. Their counterparts in the ROC serve as a control group. The CEM is applied to balance treatment and control covariates. This method generates cells by dividing continuous variables into discrete intervals or categorical variables into fewer coarsened categories. If there does not exist either sample in BC or samples in the control group in a particular cell, all the observations in this cell are trimmed. The CEM algorithm returns a weight of zero to this cell. The CEM algorithm returns weights  $n_t^j/n_c^j \times N_c/N_t$  to each of the remaining cells  $j$ , where  $n_t^j$  and  $n_c^j$  are the sample sizes of BC and the control group in cell  $j$ .  $N_t$  and  $N_c$  are the total numbers of observations in the matched samples of BC and the control group, respectively. These weights are used to reweight observations in the matched control sample so as to balance the empirical distributions of the matching covariates between the two samples in the DID method. The set of matching variables includes dummies for gender, age group, the highest qualification attained, marital status, and industries.

Using the DID approach, the causal effect of the policy can be estimated by a weighted regression

model as follows:

$$Y_{ijt} = \alpha + \beta_1(BC_j \times Post_t) + X_{ijt}^T \gamma + \eta_j + \delta_t + \varepsilon_{ijt}, \quad (1)$$

where  $Y_{ijt}$  is an economic outcome of respondent  $i$  in province  $j$  at time  $t$ .  $X_{ijt}^T$  is a vector of individual characteristics, including dummies for gender, age group, the highest qualification attained, marital status, and industries. These regressors control for variations in the sample composition.

$BC_j$  equals one if a respondent lives in BC, and zero otherwise.  $Post_t$  equals one in July 2008 or later, and zero otherwise. Therefore, the term  $BC_j \times Post_t$  equals one if a respondent lives in BC after the policy, and zero otherwise. I also estimate the model by replacing  $BC_j \times Post_t$  with  $BC_j \times Tax_t$ , where  $Tax_t$  equals 0, 0.1, 0.15, 0.2, 0.25, and 0.3 if it is observed during July 2005-June 2008, July 2008-June 2009, July 2009-June 2010, July 2010-June 2011, July 2011-June 2012, and July 2012-June 2015, respectively. This estimation allows me to examine the effect of each Canadian dollar of the carbon tax. While  $\eta_j$  captures the province fixed effect,  $\delta_t$  captures the *Year*  $\times$  *Month* fixed effects such as exchange rate shocks.

The coefficient  $\beta_1$  is the DID estimate of our primary interest because it captures the average treatment effect on the treated. This estimation method requires a common trend assumption (i.e., the trends of a dependent variable in BC and the control provinces are parallel from the pre-policy to the post-policy period.). [Yip \(2018\)](#) provides evidence that there was a shock to BC's labor market (relative to the labor market in the ROC) in 2004 and suggests that observations prior to 2005 should be excluded from the pre-policy period. He shows that the unemployment trends are parallel between BC and the ROC during the three years prior to the policy (i.e., July 2005-June 2008), and the unemployment trends are parallel among provinces in the ROC during the entire period of examination (i.e., July 2005-June 2015). Therefore, I define the pre- and the post-policy period as July 2005-June 2008 and July 2008-June 2015, respectively. I will also provide evidence in Subsection 3.6 that wage trends are parallel between the treatment and control groups during July 2005-June 2008 regardless of educational category. And if a demographic group is less likely affected by BC's carbon tax, the wage trends of this group are parallel between BC and the ROC during the entire period of examination. Since this estimation method relies heavily on the common trend assumption, I will discuss the validity of this assumption in detail in Subsection 3.7.

The remaining issue relates to the estimation of standard errors. [Bertrand et al. \(2004\)](#) raises concerns about the correlation of the regressors within clusters in the DID estimation. Accordingly, the cluster-robust standard errors are estimated to generalize the Huber-White sandwich estimates of ordinary least squares standard errors to the clustered setting to account for possible heteroscedasticity 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. In this paper, standard errors are clustered at the level of ten provinces, two genders, nine age groups, and two marital statuses, providing us with 360 clusters. I also estimate the standard errors using a bootstrap procedure proposed by [Cameron et al. \(2008\)](#). Since the main conclusions of this paper do not change using the two estimation methods,

I only report the standard errors from the former method.

### 3 The Labor Market Consequences of Carbon Tax

This section comprises six subsections. Subsection 3.1 confirms that BC experiences wage and job losses subsequent to BC's carbon tax. Subsection 3.2 examines the heterogeneous effects across industries and across worker, supporting that the wage and the unemployment effects arise from BC's carbon tax. Subsection 3.3 answers whether wages are sticky among incumbent workers and new hires. Subsection 3.4 investigates whether the unemployment effect arises from job-finding rates or job-separation rates. Subsection 3.5 explores whether the unemployment effect is mainly attributed to the number of unemployment spells or the duration of unemployment spells. Subsection 3.6 examines the dynamics of the wage and the unemployment effects. To close this section, Subsection 3.7 provides evidence that the required identifying assumptions of the analyzes in this section are likely satisfied.

#### 3.1 The Wage and the Unemployment Effects of Carbon Tax

The examinations of the policy impact on wages and unemployment are informative for two reasons. First, it provides us with an opportunity to estimate the extent to which carbon tax affects the two labor market outcomes, which is relevant to policymakers. Second, the prediction of economic theories on the impact of environmental policies on labor demand is ambiguous. Carbon tax raises production cost, depressing labor demand (Bovenberg and van der Ploeg, 1998; Wagner, 2005; Hafstead and Williams, 2018; Sun and Yip, 2019). Meanwhile, the weak Porter hypothesis implies that carbon tax could induce innovation and thus labor demand. These two compelling forces make the economic prediction ambiguous. The examinations on the wage and the unemployment effects hence provide evidence on whether carbon tax creates positive or negative labor demand shocks to BC.

Table 2 presents the wage and the unemployment effects from the estimation of equation (1). In Panel A, the dependent variables are the logarithm of a hourly wage rate. The average hourly wage rate of BC's employees is CAD\$18.12 in the pre-policy period. Samples are restricted to all employees in this analysis. All specifications include the province and the  $Year \times Month$  fixed effects. The specification in column (2) also includes dummies for gender, age, educational level, marital status, and industry.

The results in columns (1) and (2) suggest that BC's average wage level drops in the post-policy period. The estimates indicate BC's average wage declines by 2.7 percent. The estimates are statistically significant at one percent level. I repeat the analyses by replacing  $BC_j \times Post_t$  with  $BC_j \times Tax_t$  and report the estimates in columns (3) and (4). The results suggest that each Canadian dollar of carbon tax decreases wages by 0.120-0.122 percent.

Next, I estimate the unemployment effect. Instead of using wages as the dependent variables, I conduct analogous analyses using a dummy variable for unemployment as the dependent variable. The sample of examination includes all labor force participants (LFP). All specifications include the province and the  $Year \times Month$  fixed effects. The specification in column (2) also includes dummies for gender,

Table 2: The Wage and the Unemployment Effects of Carbon Tax

<b>Difference-in-Differences Analysis</b>				
	(1)	(2)	(3)	(4)
<b>Panel A. Wage Effect</b>				
<i>BC</i> × <i>Post</i>	-0.027*** (0.008)	-0.027*** (0.004)		
<i>BC</i> × <i>Tax</i>			-0.122*** (0.038)	-0.120*** (0.015)
Adjusted $R^2$	0.026	0.467	0.026	0.467
<b>Panel B. Unemployment Effect</b>				
<i>BC</i> × <i>Post</i>	0.014*** (0.003)	0.014*** (0.003)		
<i>BC</i> × <i>Tax</i>			0.047*** (0.009)	0.047*** (0.011)
Adjusted $R^2$	0.006	0.031	0.006	0.031

Notes: Dependent variables are  $\ln(\text{wage})$  and a dummy variable for unemployment in Panel A and B, respectively. All specifications include the province and the  $\text{Year} \times \text{Month}$  fixed effects. Specifications also include the dummies for gender, age, educational level, and marital status in columns (2) and (4). In Panel A, a dummy variable for industry is also included in columns (2) and (4). Samples are restricted to all employees in Panel A and all LFP in Panel B. BC is the treatment group. The ROC is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations are 5,227,156 and 7,412,988 in Panel A and B. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

age, educational level, and marital status. BC's unemployment rate is 5.23 percentage points in the pre-policy period. Two points deserve highlights. First, the results suggest that carbon tax creates job losses. The estimates in columns (1) and (2) indicate that the unemployment rate increases by 1.4 percentage points following the policy. These estimates are statistically significant at one percent level. Second, the estimates in columns (5) and (6) reveal that each Canadian dollar increases the unemployment rate by 0.047 percentage points.

To complete the picture of labor market behaviors, I also estimate the change in weekly working hours subsequent to July 2008. Here, weekly working hours are measured in four ways: the usual weekly working hour in the main job, the usual weekly working hour in all jobs, the actual weekly working hour in the main job, and the actual weekly working hour in all jobs. The coefficient estimates from the estimation of equation (1) are reported in Table A1 in the Appendix. No statistical evidence is found to support changes in weekly working hours subsequent to July 2008 regardless of measures of working hours. The result suggests that employment only decreases from its extensive margin (i.e., unemployment), not from its intensive margin (i.e., working hour). This finding is largely consistent with empirical regularities: fluctuations in the total working hours in a labor force stem mainly from variations in the number of employees [Shimer \(2010\)](#). In the rest of this paper, I will focus on the wage and the unemployment effects for space consideration.

This subsection confirms that BC experiences wage and job losses subsequent to July 2008. One may cast doubt that these effects are attributed to the carbon tax policy. In fact, as mentioned above,

both the wage effect and the unemployment effect increase with each Canadian dollar of BC's carbon tax, providing the first support in this paper that these effects are related to the carbon tax policy. The next subsection will provide another evidence on the linkage between the wage and the unemployment effects and the carbon tax policy.

### 3.2 The Heterogeneous Effects

This subsection investigates the heterogeneous effects across workers and industries. Yip (2018) shows that less-educated workers tend to be engaged in industries that are more energy-intensive in the pre-policy period. If the documented wage and unemployment effects arise from the carbon tax policy, these effects will be concentrated on less-educated workers. Also, I will investigate the heterogeneous effects across industries. Carbon tax raises production cost; this impact is supposed to be more pronounced among industries that are more energy-intensive. If BC's carbon tax is the source of these effects, industries that are more energy-intensive should be affected more. Altogether, the adverse wage effects should be larger in industries with higher energy intensities, especially among less-educated workers.

Now, I proceed to investigate the heterogeneous effects across educational categories. This analysis is informative for two reasons. First, it serves as an internal validity check on the results above. If BC's wages happen to increase for some demographic groups and decrease for others, it may be reasonable to assume that the adverse wage effects arise from other unobserved factors that are likely unrelated to the carbon tax policy. A similar logic can be applied to the analysis of the unemployment effect. If the policy effects happen to be uniform across educational categories or more pronounced among more-educated workers, one should also cast doubt on the claim that the documented effects are attributable to the carbon tax policy. Second, the investigation of the heterogeneous effects provides us with an opportunity to understand the distributions of the costs associated with the carbon tax policy across workers, which is relevant to policymakers redistributing tax revenues accordingly.

Table 3 presents the wage effects on high-, medium-, and low-educated employed workers in columns (1) through (3) and the unemployment effects on the respective LFP in columns (4) through (6). Their average wages are CAD\$23.30, CAD\$18.48, and CAD\$14.90 and their unemployment rates are 3.38, 4.47, and 7.13 percentage points in the pre-policy period. All specifications include dummies for gender, age, educational level, and marital status and the province and the *Year*×*Month* fixed effects. Specifications in columns (1) through (3) also control for industry dummies.

Estimation results uncover the heterogeneity in the policy impacts. The estimates in columns (1) through (3) indicate that the average wages of high-, medium-, and low-educated workers drop by 0.5, 2.4, and 3.0 percent. While the estimate for the high-educated is statistically insignificant at any conventional level, the other two estimates are significant at one percent level. The estimates in (4) through (6) indicate that less-educated workers suffer more the brunt of the unemployment effects: an increase of 0.7, 1.3, and 1.8 percentage points for high-, medium-, and low-educated workers. These estimates are precise: they are all statistically significant at one percent level. The results suggest that the policy effects on wages and unemployment differ substantially by educational category.

Two more points merit comments. First, these results confirm that BC experiences wage and job losses in the post-policy period. The adverse wage effects and the unemployment effects appear regardless of educational group, suggesting that there is possibly a fall in BC's labor demand. Second, these results provide another support that the documented wage and unemployment effects are induced by the carbon tax policy. While less-educated workers are expected to be affected more by the carbon tax policy (Yip, 2018), both the wage and the unemployment effects are found more pronounced at lower educational levels. The distributions of the effects across educational categories cohere with the tax structure of the policy, suggesting that it is the carbon tax policy that creates the negative shock in BC.

Next, I investigate how the wage effects differ by the energy-intensity of industries. This investigation requires the information on energy intensity. Nevertheless, the Canadian LFS has no such information. In response to carbon tax, firms in energy-intensive industries may reduce their energy intensity in the post-policy period. Therefore, I construct an index that measures an industry-specific energy intensity in 2007, the year prior to the policy. In particular, I follow Curtis (2017) to construct an energy intensity index as follows.

After obtaining the Canadian LFS data, I merge three-digit-industry energy intensity data from the 2007 NBER Productivity Database. I divide total industry energy expenditure by the total value of shipments for the industry to construct an energy intensity index for 19 different manufacturing industries. As such, the index of any particular industry in any year is identical to its 2007 level; the energy intensity of an industry is unaffected by the policy impacts on its energy intensity in the post-policy period. Since the NBER Productivity Database only has the information on the energy expenditure for manufacturing industries, I restrict the sample to manufacturing employment in this analysis. To serve as a robustness check, I construct two more measures of energy intensity: the cost of energy expenditure per unit of value-added and per unit of payroll.

To examine how the wage effect differs by the energy intensity of industries, I adopt a triple differences approach:

$$\begin{aligned} \ln(Wage)_{ijkt} = & \alpha + \beta_1(\ln(EI)_k \times BC_j \times Post_t) + \beta_2(\ln(EI)_k \times Post_t) + \beta_3(\ln(EI)_k \times BC_j) \\ & + \beta_4(BC_j \times Post_t) + X_{ijkt}^T \gamma + \psi_k + \eta_j + \delta_t + \varepsilon_{ijkt}, \end{aligned} \quad (2)$$

where  $Wage$  measures the nominal hourly wage rate of respondent  $i$  in industry  $k$  in province  $j$  at time  $t$ .  $X_{ijkt}^T$  is a vector of individual characteristics, including dummies for gender, age group, the highest qualification attained, and marital status. These regressors control for variations in sample composition.  $EI_k$  measures the energy intensity of industry  $k$  in 2007, and  $\psi_k$  captures an industry fixed effect.

The estimate of our primary interest  $\hat{\beta}_1$  captures the policy effect on the wage elasticity with respect to energy intensity. If the adverse wage effect arises from the carbon tax, the adverse wage effect should be concentrated on industries that are more energy-intensive.  $\hat{\beta}_1$  is expected to be negative.

I use the three measures of energy intensity to estimate  $\hat{\beta}_1$  from equation (2) and report the estimates in Table 4. The dependent variables are the logarithm of a hourly wage rate. Energy intensity is measured by energy expenditure per unit value of shipments, per unit of value-added, and per unit of payroll in

Panels A, B, and C, respectively. Samples are restricted to all employees in manufacturing industries in columns (1) and (2). The average hourly wage of BC's employees in manufacturing industries is CAD\$19.25 in the pre-policy period. These two specifications include the province, the *Year*×*Month*, and industry fixed effects. The specification in column (2) also includes dummies for gender, age, educational level, and marital status.

The results suggest that the wage effects differ by industry: the reduction in wages is larger in industries with higher energy intensity subsequent to the policy. The estimates in columns (1) and (2) indicate that the wage elasticity with respect to the energy intensity at an industry level drops by about 1.8-3.8 percent. These estimates are statistically significant at one to five percent level. In other words, the policy reduces the average wage by 1.8-3.8 percent in response to each percent increase in the energy intensity of an industry after the implementation of the policy.

I repeat the analyses with samples of different educational categories. Columns (3) through (5) in Table 4 present the estimates  $\hat{\beta}_1$  from equation (2) with the sample of high-, medium-, and low-educated employees, respectively. A number of points emerge from the table. First, the carbon tax reduces the wage elasticity regardless of educational category. All estimates in columns (3) through (5) are negative, suggesting that the adverse wage effects of the policy are more pronounced in industries with higher energy intensity regardless of educational levels. Second, the adverse effects are more pronounced among less-educated employees.

Nevertheless, the estimates in columns (3) and (4) are all statistically insignificant at any conventional level in all the panels with the exception of the estimate in column (4) in Panel A that would be considered significant at the 10 percent level but not by stricter criteria. These estimates reveal that the policy impact on the wage elasticity is statistically negligible for the high- and medium-educated manufacturing employees. In contrast, the estimates in column (5) are larger in magnitude and all statistically significant at one to five percent level, suggesting that the interaction effect between the carbon tax and energy intensity is concentrated on the low-educated. The estimates suggest that the carbon tax reduces the average wage of the low-educated by 2.1-3.5 percent in response to each percent increase in energy intensity of the industry.

In sum, this subsection uncovers the heterogeneity in the impacts across workers and industries. First, the adverse wage effect and the unemployment effects are more pronounced at less-educated levels. Second, the policy impacts on wages are concentrated on more energy-intensive industries, especially among less-educated workers. These heterogeneities cohere with the tax incentive of the carbon tax and the distributions of human capital across industries in the pre-policy period, re-confirming that the negative wage effect and the unemployment effect are attributed to the carbon tax policy. I therefore conclude that BC's carbon tax creates the shock, leading to the adverse wage effect and the unemployment effect.<sup>10</sup> Furthermore, the heterogeneity also provides economists and policymakers with information on the distribution of cost associated with carbon tax, serving as guidance redistributing tax revenues accordingly.

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<sup>10</sup>Subsection 3.6 will provide further evidence that these effects result from the policy when the dynamic effects are explored.

Table 3: The Heterogeneous Effects Across Workers

<b>Difference-in-Differences Analysis</b>						
	(1)	(2)	(3)	(4)	(5)	(6)
$BC \times Post$	-0.005 (0.006)	-0.024*** (0.005)	-0.030*** (0.006)	0.007*** (0.002)	0.013*** (0.003)	0.018*** (0.005)
Adjusted $R^2$	0.273	0.391	0.480	0.005	0.015	0.043

Notes: Dependent variables are the logarithm of a hourly wage rate in columns (1)-(3) and are the dummy variables for unemployment in columns (4)-(6). All specifications include the dummies for gender, age, educational level, and marital status and the province and the  $Year \times Month$  fixed effects. Specifications in columns (1)-(3) also include the dummies for industry. Samples are restricted to all employees in columns (1)-(3) and all LFPs in columns (4)-(6). BC is the treatment group. The ROC is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations are 1,168,245, 2,575,287, 1,872,112, 1,533,481, 3,301,827, and 2,593,276 from column (1) to column (6). Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

Table 4: The Policy Effects on the Wage Elasticity w.r.t. Energy Intensity

<b>Difference-in-Differences Analysis</b>					
<b>Dependent Variable: <math>\ln(\text{Wage})</math></b>					
	(1)	(2)	(3)	(4)	(5)
<b>Panel A</b>					
$\ln(EI) \times BC \times Post$	-0.038*** (0.013)	-0.033*** (0.012)	-0.025 (0.030)	-0.028* (0.016)	-0.035** (0.015)
Adjusted $R^2$	0.171	0.369	0.196	0.324	0.382
<b>Panel B</b>					
$\ln(EI) \times BC \times Post$	-0.030** (0.012)	-0.028*** (0.010)	-0.021 (0.028)	-0.022 (0.015)	-0.032*** (0.012)
Adjusted $R^2$	0.171	0.370	0.196	0.324	0.382
<b>Panel C</b>					
$\ln(EI) \times BC \times Post$	-0.022** (0.010)	-0.018** (0.007)	-0.003 (0.023)	-0.016 (0.012)	-0.021** (0.009)
Adjusted $R^2$	0.171	0.369	0.196	0.324	0.381

Notes: Energy intensity is measured by energy expenditure per unit value of shipments, per unit of value added, and per unit of payroll in Panels A, B, and C, respectively. All specifications include the province, the industry, and the  $Year \times Month$  fixed effects. Specifications also include the dummies for gender, age, educational level, and marital status in columns (2) through (5). Samples are restricted to all manufacturing employees in columns (1) and (2) and are restricted to high-, medium-, and low-educated manufacturing employees in columns (3) through (5), respectively. BC is the treatment group. The ROC, excluding MB, is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations for high-, medium-, and low-educated employees are 81,650, 304,831, and 281,979, respectively. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.



### 3.3 Are Wages Rigid?

The previous subsections have documented the adverse wage effect of the carbon tax. I demonstrate in Appendix B that the sticky wage hypothesis can be rejected by the adverse wage effect in response to any negative shocks. The documentation of the adverse wage effects seems to suggest that wages are not sticky. Nevertheless, the question of whether wages are sticky is unsettled. Whose wages are sticky, and whose are not? Economic theory posits that stickiness in wages arises because wages are not easily renegotiated under contracts. For this reason, this subsection investigates the wage effects on incumbent workers and new hires. This investigation not only answers the question regarding wage stickiness but also uncovers the channels through which the carbon tax, as a cost shock, reduces average wages.

Table 5 presents the wage effects on incumbent workers and new hires from the estimation of equation (1). The dependent variables are the logarithm of a hourly wage rate. Samples are restricted to all employees in columns (1) and (2) and are restricted to high-, medium-, and low-educated workers in columns (3), (4), and (5). I define incumbent workers as those with tenure over seven years and new hires as workers with tenure below one year.<sup>11</sup> Only those with tenure over seven years are examined so that the entire sample of examinations are hired before the policy was implemented. In the analysis of the impacts on new hires, I exclude all observations between July 2008 and June 2009 to ensure that all observations in the post-policy period are hired after BC's carbon tax was introduced. All specifications include the province and the *Year*×*Month* fixed effects. Specifications in columns (2) through (5) also include dummies for gender, age, educational level, marital status, and industries.

The results suggest that the wage effects on incumbent workers are at best weak. The estimates in columns (1) and (2) indicate that BC's average wage of incumbent workers drop by 1.5 percent. These negative wage effects are attributed mainly from the medium-educated incumbent workers. The estimates in columns (3) and (5) suggest that the wages of high-educated and low-educated incumbent workers drop by about 1.1 and 0.6 percent. These estimates, however, are statistically insignificant at any conventional level. The estimate in column (5) suggests that the wage effect on medium-educated incumbent workers is statistical significant; nevertheless, this effect, amounting to a 1.5 percent decrease in their average hourly wage rate, is economically negligible. In fact, I will show that these small wage effects are unlikely driven by the carbon tax in subsection 3.6 because the wage differentials between BC and ROC are found identical between the three years prior to the policy and the first five years of the post-policy period regardless of educational category. In other words, this slight decline in the wage level first appears in BC after the fifth year of the policy. Hence, I will conclude that no statistical evidence on the negative wage effect can be found among incumbent employees.

Meanwhile, wages are not sticky among new hires. The average hourly wage rate of BC's new hires is CAD\$13.87 in the pre-policy period. The estimates in columns (1) and (2) indicate that BC's carbon tax reduces average wages of new hires by 4.8-4.9 percent. These estimates are statistically significant at one percent level. The wage effects of new hires are heterogeneous across educational categories. On average, high-, medium-, and low-educated new hires receive CAD\$18.68, CAD\$14.62, and CAD\$11.52

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<sup>11</sup>This definition of new hires is also used in [Carneiro et al. \(2012\)](#).

an hour. The estimates in columns (3) through (5) reveal that the adverse wage effects appear regardless of educational levels but are concentrated on medium- and low-educated new hires. The policy reduces the average wage of high-educated new hires by 1.1 percent. Not only is the estimate materially small but also statistically insignificant at any conventional level. The impacts on the medium- and low-educated new hires are remarkable: their wages, on average, drop by 4.9 and 6.0 percent. These estimates are considered statistically significant at one percent level.

This subsection discovers two important findings. First, the results of this subsection fine-tune our traditional thought about wage stickiness. Are wages rigid? Yes and No. Wages are found sticky among incumbent employees as to why carbon tax, as a cost shock or a negative shock, creates job losses. This finding supports the traditional thought that stickiness in wage creates cyclical unemployment (Keynes, 1936; Blanchard and Fischer, 1989). Meanwhile, the wages of new hires are found to drop substantially, explaining why average wages are found to be procyclical in the literature and revealing that the stickiness in hiring wages may not be the solution to the unemployment volatility puzzle (Shimer, 2005; Hall, 2005b; Costain and Reiter, 2008). This subsection uncovers the channel through which negative shocks reduce wages: wage cuts operate through hiring processes.

Second, this finding provides guidance as to how social benefit should be distributed accordingly. This subsection concludes that the policy impact on incumbent employees is negligible. While no statistical evidence on the wage effect of high-educated new hires, the wage losses are concentrated on medium- and low-educated new hires. These findings document the regressivity of carbon taxes from the source side of labor income: while Yip (2018) documents that the less-educated are more likely affected by the unemployment effect of BC's carbon tax, this paper finds that the adverse wage effects are also concentrated on the less-educated. The evidence of these two papers raises concerns on the labor market analogs of environmental justice.

### 3.4 Are Job-Separation Rates Acyclical?

This subsection examines the impacts on job transition rates. This examination uncovers the channels through which carbon tax increases unemployment and answers an unsettled question: how much do unemployment fluctuations attributed to variations in job-finding rates and job-separation rates?

This examination requires information on job transition rates. Unfortunately, the Canadian LFS data does not provide any information on these two rates. I therefore estimate the monthly job-finding rates and the monthly job-separation rates by group (i.e., BC and the ROC) from July 2005 to May 2015 rates using Shimer (2012) approach. Let  $f_{jt}$  and  $x_{jt}$  be the monthly job-finding rate and job-separation rate in group  $j$  in time  $t$ .<sup>12</sup>

Next, I obtain the policy effects on these two transition rates from the estimation of equation (1). Dependent variables are a job-finding rate and a job-separation rate. While BC is a treatment group,

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<sup>12</sup>I estimate a job-finding rate  $f_{jt}$  in province  $j$  in month  $t$  by solving  $e^{f_{jt}} = (u_{jt+1} - u_{jt+1}^s)/u_{jt}$  (i.e., equation (4) in Shimer (2012)), where  $u_{jt}$  is unemployment in group  $j$  in month  $t$  and  $u_{jt+1}^s$  is unemployment with spell within a month in month  $t + 1$ . Similarly, I solve  $u_{jt+1} = x_{jt}l_{jt}(1 - e^{-f_{jt}-x_{jt}})/(f_{jt} + x_{jt}) + e^{-f_{jt}-x_{jt}}u_{jt}$  (i.e., equation (5) in Shimer (2012)) to obtain a job-separation rate  $x_{jt}$ , where  $l_{jt}$  is the measure of the LFP.

Table 5: Impacts on Incumbent Workers and New Hires

<b>Difference-in-Differences Analysis</b>					
<b>Dependent Variable: ln(Wage)</b>					
	(1)	(2)	(3)	(4)	(5)
<b>Sample: Incumbent Workers</b>					
<i>BC</i> × <i>Post</i>	-0.015*	-0.015***	-0.011	-0.015***	-0.006
	(0.008)	(0.005)	(0.007)	(0.005)	(0.008)
Observations	1,648,169	1,648,169	436,609	892,349	542,950
Adjusted R <sup>2</sup>	0.034	0.330	0.156	0.233	0.300
<b>Sample: New Hires</b>					
<i>BC</i> × <i>Post</i>	-0.048***	-0.049***	-0.002	-0.049***	-0.060***
	(0.014)	(0.006)	(0.015)	(0.009)	(0.007)
Observations	878,241	878,241	148,779	432,212	427,621
Adjusted R <sup>2</sup>	0.043	0.469	0.274	0.401	0.489

Notes: All specifications include the province and the *Year* × *Month* fixed effects. Specifications also include the dummies for gender, age, educational level, marital status, and industry in columns (2) through (5). Samples are restricted to all employees in columns (1) and (2) and are restricted to high-, medium-, and low-educated employees in columns (3) through (5), respectively. BC is the treatment group. The ROC is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. Observations during July 2008-June 2009 are excluded if the sample of new hires is analyzed. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

the ROC serves as a control group.  $\beta_1$  is of our primary interest; it is the average treatment effect on the treated; it captures the causal effect of carbon tax on the two rates. Using  $f_{jt}$  and  $x_{jt}$  as dependent variables, I find that  $\hat{\beta}_1$  are -0.093 and 0.004, respectively. Both the estimates are statistically significant at one percent level. These findings suggest that BC's carbon tax, a cost shock, decreases a job-finding rate by 0.093 and increases a job-separation rate by 0.004.

To better interpret the policy impacts of the two rates, I measure these impacts in terms of unemployment. According to the job search theory, a steady-state unemployment rate is given by  $u_t = x_t / (f_t + x_t)$ . Suppose the policy has no impact on a job-finding rate. That is, the policy affects unemployment only through a job-separation rate. The policy impact on the job-separation rate will change the unemployment rate by  $\Delta u_x \equiv \frac{(x_t + \tau_x)}{f_t + (x_t + \tau_x)} - \frac{x_t}{f_t + x_t}$ , where  $\tau_x$  is the causal effect of the policy on a job-separation rate, equal to 0.004. In our case,  $f_t$  and  $x_t$  are the average job-finding rate and the average job-separation rate in BC in the pre-policy period, which equal 0.642 and 0.031. Hence,  $\Delta u_x = 0.5636$ . In other words, carbon tax increases an unemployment rate by 0.5636 percentage point through the increased job-separation rate.

Analogously, the policy impact on the job-finding rate will change the unemployment rate by  $\Delta u_f \equiv \frac{x_t}{(f_t + \tau_f) + x_t} - \frac{x_t}{f_t + x_t}$ , where  $\tau_f$  is the causal effect of the policy on a job-finding rate. In our case,  $\tau_f$  equals -0.093. Hence,  $\Delta u_f = 0.7386$ : carbon tax increases an unemployment rate by 0.7386 percentage point through the decreased job-separation rate. While the unemployment effect amounts 1.4 percentage points, we can conclude that about over 40 percent of the unemployment effects of BC's carbon tax policy is attributed to the increased job-separation rate, and over half of the effect arises from the decreased job-

finding rate.

The results reveal two pieces of important information. First, this subsection adds to this literature by discovering the processes by which the carbon tax policy increases unemployment. While an extensive literature documents that environmental policies create job losses (Greenstone, 2002; Walker, 2011; Kahn and Mansur, 2013; Curtis, 2017; Yip, 2018), none explores policy impacts on these job transition rates using microdata. Environmental policies make the unemployed harder to get a job and makes the employed more likely to get laid off. Second, this finding challenges the widespread view that job-separation rates are acyclical (Hall, 2005a,b; Rudanko, 2009; Shimer, 2012). The findings of this subsection provide support that both job-separation rates and job-finding rates react to negative shocks, and the variations in both rates explain a substantial amount of unemployment fluctuations.

### **3.5 Do Carbon Taxes Increase The Number or the Duration of Unemployment Spells?**

The previous subsection indicates that the decreased job-finding rate explains the unemployment effect slightly more than the increased job-separation rate. It is natural to expect that the unemployment effect mainly arises from the prolonged period of unemployment, not the increased number of unemployment. To understand the policy effect on the structure of unemployment, this subsection answers whether the unemployment effect is mainly driven by short- or long-term unemployment.

Answering this question provides important information to policymakers on the design on social benefit accordingly. If the shock happens to lengthen unemployment spells, the duration of unemployment benefit should be extended accordingly along with environmental policies and other cost shocks. However, if the extent to which the shock lengthens an unemployment spell is limited, the extension of benefit periods could not help unemployed workers smooth consumption over the prolonged period of unemployment. Instead, the extension likely worsens the unemployment rate.

I estimate the policy effects on short- and long-term unemployment from the estimation of equation (1). Instead of using a dummy for unemployment as a dependent variable, I conduct analogous analyses using dummies for short- and long-term unemployment as dependent variables. Long-term unemployment is defined as the unemployment with an unemployment spell over six months. The rest of the unemployment is defined as short-term unemployment.

Table 6 presents the unemployment effects from the estimation of equation (1). Samples are restricted to all LFP in columns (1) and (2) and are restricted to high-, medium-, and low-educated LFP in columns (3), (4), and (5). All specifications include the province and the *Year*×*Month* fixed effects. Specifications in columns (2) through (5) also include dummies for gender, age, educational level, and marital status.

Three points deserve highlights. First, the results suggest that the carbon tax policy increases both short- and long-term unemployment regardless of educational group. The estimates are all statistically significant at one percent level with the exception of the estimate in column (3) that would be considered significant at the 10 percent level. Second, the impacts are more pronounced on short-term unemployment. The estimates in columns (1) and (2) indicate the short- and the long-term unemployment increase

by 1.1 and 0.3 percentage points subsequent to the policy. According to subsection 3.1, the carbon tax increases the unemployment rate by 1.4 percentage points. Hence, about 79 and 21 percent of the unemployment effect are driven by the short- and the long-term unemployment. In other words, the carbon tax slightly extends the duration of unemployment; rather, it increases unemployment mainly through the increased number of unemployment.

The implications of these findings are rich. It decomposes the unemployment effect into the short- and long-term unemployment effects, providing guidance on the design of social benefit and helping economists understand the functioning of the labor market. While the previous subsection suggests that the decreased job-finding rate explains more of the unemployment effect than the increased separation rate, one may fathom that the shock will lead to a prolonged spell of unemployment. Surprisingly, this subsection shows that it is not the case: the carbon tax increases mainly the number of unemployment, not the duration of unemployment.

This finding challenges the traditional practice of the extension of the unemployment benefit period. Benefit periods are usually extended in economic downturns. One of the rationales is that negative shocks lengthen unemployment spells, and the extension of unemployment benefit helps unemployed workers smooth their consumption over the prolonged period of unemployment. However, the estimation of the impacts of negative shocks on long-term unemployment is empirically challenging. It is unclear whether the prolonged period of unemployment is attributed to negative or the extensions of benefit periods. The findings in this subsection, therefore, contribute to the literature by identifying the impacts of negative shocks on long-term unemployment, free from the effects of the extension of the unemployment benefits. This subsection reveals that the impact on the duration of unemployment is at best weak. If the extent to which negative shocks lengthens the duration of unemployment is limited, the extension of the benefit period may lose its function to help unemployed workers smooth consumption over the prolonged period of unemployment. Instead, extended benefit periods potentially worsen unemployment, deteriorating economic welfare.

### **3.6 The Labor Market Dynamics**

This subsection explores the dynamic effects of the policy on wages and unemployment. The purpose of this exercise is twofold. First, it provides us with an opportunity to estimate the extent to which the wage and the unemployment effects of the carbon tax over time. It serves as guidance on the design of social benefits and the direction to redistribute tax revenues over time. Second, this exercise serves as an internal validity check on the results above. If trends in these variables are parallel between BC and the control provinces prior to the implementation of the policy, these trends are likely parallel immediately following the policy. In other words, this analysis provides important information on the validity of the common trend assumption.

To do so, equation (1) is estimated by replacing  $BC_j \times Post_t$  with a full set of  $BC_j \times d_t$  interaction

Table 6: Impacts on Short- and Long-Term Unemployment

<b>Difference-in-Differences Analysis</b>					
	(1)	(2)	(3)	(4)	(5)
<b>Dependent Variable: Dummy Variable for Short-Term Unemployment</b>					
<i>BC</i> × <i>Post</i>	0.011***	0.011***	0.005***	0.010***	0.015***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.005)
Adjusted R <sup>2</sup>	0.004	0.031	0.005	0.014	0.041
<b>Dependent Variable: Dummy Variable for Long-Term Unemployment</b>					
<i>BC</i> × <i>Post</i>	0.003***	0.003***	0.001*	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Adjusted R <sup>2</sup>	0.002	0.005	0.002	0.004	0.007

Notes: Long-term unemployment is defined as the unemployment with unemployment duration longer than six months. The rest of unemployment is defined as short-term unemployment. All specifications include the province and the *Year* × *Month* fixed effects. Specifications also include the dummies for gender, age, educational level, and marital status in columns (2) through (5). Samples are restricted to all employees in columns (1) and (2) and are restricted to high-, medium-, and low-educated employees in columns (3) through (5), respectively. BC is the treatment group. The ROC is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations for high-, medium-, and low-educated workers are 1,529,284, 3,291,323, and 2,580,880, respectively. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

terms to explore the dynamic of the wage effects as follows:

$$Y_{ijt} = \alpha + \sum_t \beta_t(BC_j \times d_t) + X_{ijt}^T \gamma + \eta_j + \delta_t + \varepsilon_{ijt}, \quad (3)$$

where  $d_t$  equals one between July in year  $t$  and June in year  $t+1$  for all years between 2005 and 2014 except 2007, and zero otherwise. July 2007-June 2008 is excluded because it serves as a reference year.<sup>13</sup> Hence, the estimates corresponding to July 2007-June 2008 are normalized to zero. I investigate the dynamics of the aggregate effects and the heterogeneous effects by educational level. All specifications include the province and the *Year* × *Month* fixed effects and the dummies for age, gender, educational level, marital status, and industry. Similar to the analyses above, the estimates  $\hat{\beta}_t$  are of our interest. It measures the difference in an average hourly wage rate between BC and the control provinces during July in year  $t$  through June in year  $t + 1$ , relative to the reference year (e.g. June 2007-July 2008).

Given the wage and the unemployment effects documented in the previous subsections, several features of the estimates are expected. For example, all the  $\hat{\beta}_t$  prior to the reference year should not be statistically different from zero. The wage trends and the unemployment trends are expected to be parallel in BC and the ROC in the pre-shock period. That is, the differences in the average hourly wage rate and the unemployment rate between BC and the ROC prior to the reference year should be close to the differences in the reference year. Otherwise, one should cast doubt on the common trend assumption.

To interpret the estimates in Subsections 3.3 through 3.5 as causal effects, it also requires the trends in

<sup>13</sup>Samples prior to June 2005 are excluded because Yip (2018) shows in the appendix that there was a shock to employment in BC during June 2004-July 2005. Yip (2018) suggests that samples during July 2005-June 2008 are used as the pre-policy period.

the following lists of variables between BC and the ROC to be parallel: (i) the average wage of incumbent employees, (ii) the average wage of new hires, (iii) the job-separation rate, (iv) the job-finding rate, (v) the short-term unemployment rate, and (vi) the long-term unemployment rate.

In addition to the common trends of these variables in the pre-shock period, if the documented effects are driven by BC's carbon tax, these effects are expected to appear following the implementation of the policy. First, if wages are sticky among incumbent employees, the corresponding  $\hat{\beta}_t$  should be statistically indifferent from zero immediately following the policy. Second, if wage stickiness creates unemployment, BC's unemployment rate should go up following the policy; the corresponding  $\hat{\beta}_t$  should be positive. Third, if the unemployment effect is driven by the policy, the two transition rates should start to respond subsequent to the policy. Fourth, if carbon tax reduces average wages of new hires, the corresponding  $\hat{\beta}_t$  are expected to be negative. Fifth, since subsection 3.3 concludes that the wage effect on high-educated new hires is negligible, the corresponding  $\hat{\beta}_t$  should all be statistically zero.

Next in line is the labor market dynamics. As discussed in Section 2, the carbon tax rate increases by CAD\$5 annually until 2012. If the documented effects are driven by the carbon tax, the estimates  $\hat{\beta}_t$  are expected to gradually depart from zero in the first few years. To be specific, (i) the adverse hiring wage effect is expected to grow, (ii) the job-finding rate should continue to decrease, (iii) the job-separation rate should continue to increase, and (vi) the unemployment rate should continue to increase.

It is ambitious to study the whole picture of labor market adjustments within one episode because to have the above three sets of expectations satisfied in a particular labor market within a given period of time is empirically challenging. To meet this extraordinarily long list of expectations requires the extent to which the treatment and the control labor markets are affected by shocks other than carbon tax is similar during the entire period of examination. However, if the labor market dynamics happen to fulfill this long list of expectations, the conclusion of this paper is robust.

We now proceed to the results. Figures 1-3 display the yearly estimates. The dependent variables are the logarithm of a hourly wage rate in Figures 1 and 2. Samples are restricted to incumbent employees and new hires, respectively. In Figure 3, the dependent variable is the dummy variable for unemployment and the samples are restricted to LFP. There are four graphs in each figure, representing four different samples: the entire sample, the high-educated, the medium-educated, and the low-educated from top left to bottom right. In each graph, the vertical dashed line represents the first month of the carbon tax policy (i.e., July 2008). Since July 2007-June 2008 is the reference year, the corresponding dot is normalized to zero. The first dots represent the estimates  $\hat{\beta}_t$  corresponding to July 2005-June 2006. For example, the first dots in Figure 1 capture the average differences in hourly wage rates of incumbent workers between BC and the control provinces relative to the difference in the reference year. The dashed lines represent the 95 percent confidence interval.

A number of points emerge from these figures. First, the parallel trends in the hourly wage and the unemployment rate can be found during the pre-policy period. According to Figures 1-3, all these estimates prior to the policy are statistically indifferent from zero, with the exception of the high-educated incumbent employees.

These estimates suggest that the wage trends of the new hires and the unemployment trends between

BC and the ROC are parallel during the three years prior to the policy regardless of educational level. These trends likely remain parallel immediately following the implementation of the policy. Regarding incumbent employees, the parallel wage trends prior to the shock can be found among medium- and low-educated samples. In fact, most of the estimates lie above zero in the sample of high-educated employees, meaning that their relative wages in BC are one of the lowest in the reference year during the period of examination. As discussed later, the wage trend of high-educated incumbent employees between BC and the ROC is indeed quite steady over the entire examination period.

Second, no statistical evidence on the wage effect can be found among incumbent employees. According to Figure 1, the estimates for high-educated employees are all statistically insignificant in the first seven years of the policy, providing no statistical evidence on the policy effect on their wage. Subsection 3.1 pinpoints that the wage effect on medium-educated incumbent employees is statistically significant. The examination of this dynamic effect provides a clear picture that the negative wage effect is likely driven by another shock around 2014, not BC's carbon tax in 2008. If wages are adjusted because of the carbon tax policy, their wages should go down in the first few years, not in the sixth year of the policy. A similar argument can be applied to the sample of low-educated incumbent employees. The conservative stance of this exercise is that no statistical evidence on the wage effect can be found among incumbent employees over the first five years of the policy regardless of educational level.

This finding contributes to the understanding of the labor market in two dimensions. Obviously, the negligible wage effect in the first few years of the policy period supports the sticky wage theory. Interestingly, this stickiness holds over (at least) five years, longer than many economists thought of. The literature suggests that wages are not easily adjusted under labor contracts and they are adjusted once contracts are renegotiated (Barro, 1977; Fischer, 1977; Taylor, 1980; Thomas and Worrall, 1988). As noted by Elsbey et al. (2016), "*many workers, including ourselves, have their nominal wages rest only once a year*". If most of the contracts are renegotiated at least once during the first five years of the policy, the prolonged period of wage stickiness reveals that wage contracts may not be the reasons for wage stickiness, and wage adjustments do not operate through contract renewals.

Third, the carbon tax reduces the average wages of new hires. Figure 2 shows that all the estimates are negative in the post-policy period in the top-left figure. Obviously, hiring wages begin to fall right after the implementation of the policy, suggesting that the carbon tax reduces their wages and that their wages are not sticky. These adverse wage effects appear only among the medium- and the low-educated new hires. The estimates approach -0.05 after 2012, suggesting that the CAD\$30 dollar of the carbon tax rate reduces the hourly wage by five percent and this adverse wage effect seems permanent. This "seemingly permanent" wage effect of the new hires may reflect the permanent wage adjustment after the re-optimization of firms' profit functions under the permanent shock of the carbon tax. In contrast, the estimates are all statistically insignificant among the high-educated new hires over the entire period of examination. Not only does this result reveal the negligible wage effect on the high-educated new hires, but it also conveys an important message: if a demographic group is unaffected by the policy, the wage trend in BC is parallel to the one in the ROC during entire period of examination.

Fourth, BC's unemployment rate goes up after the carbon tax. Figure 3 shows that all the estimates



are positive in the post-policy period regardless of educational level, suggesting that wage stickiness does create unemployment. Since the carbon tax rate grows in the first few years, the unemployment effects enlarge regardless of educational level. This enlargement supports that the severity of the negative shock is useful: Since the dynamic of the unemployment effects largely coheres with the tax incentive of the carbon tax, we can conclude that the effect is likely driven by the carbon tax. The unemployment rates peak at about one, two, and three percentage points among the high-, the medium-, and the low-educated in the third year of the policy. In other words, every hundred low-educated LFP, three of them becomes unemployed because of the carbon tax. Another important highlight is that the unemployment effect shrinks after 2012 regardless of educational level. Since average wages are slowly adjusted through the hiring processes, this slow process of wage adjustment “breaks down” the stickiness and undermines the unemployment effect.

Next, I explore the dynamics of short- and long-term unemployment in Figures 4 and 5. Several points deserve mention. First, the pre-policy trends in short- and long-term unemployment are parallel in BC and the ROC. Prior to the reference year, the estimates  $\hat{\beta}_t$  are statistically and materially negligible. Since the trends in the short-term and the long-term unemployment are parallel, it is unsurprising that BC and the ROC share a parallel trend in the unemployment rate during the same period.

Second, the unemployment effect of the carbon tax is largely driven by the short-term unemployment effect. A direct comparison between Figures 3 and 4 can conclude that the dynamics of the short- and the long-term unemployment effects follow a similar trend in the entire period of examination. In contrast, the long-term unemployment effect is at best weak and is mostly concentrated on the low-educated.

Third, the negligible long-term unemployment effect on the high-educated provides additional support on the common trend assumption. According to Figure 5, the trends in the long-term unemployment are parallel in BC and the ROC over 10 years. Hence, if the medium- and the low-educated were not affected by the policy, the labor market outcomes in BC and the ROC would have followed the same trend like the parallel trends in the long-term unemployment of the high-educated.

Lastly, I estimate the dynamic effects on the transitions rates and plot the estimates  $\hat{\beta}_t$  in Figure 6. Obviously, the estimates are all close to zero in the pre-policy period. According to the search theory, the steady-state unemployment rate is determined by both the job separation and the job-finding rates. While the pre-policy trends in the two rates are parallel in BC and the ROC, it is unsurprising that they share an identical unemployment trend during the pre-policy period. Moreover, the sharp increase in the job-separation rate and the sharp decrease in the job-finding rate immediately following the policy confirm that it is the carbon tax that induces the changes in the two transition rates, and thus the unemployment.

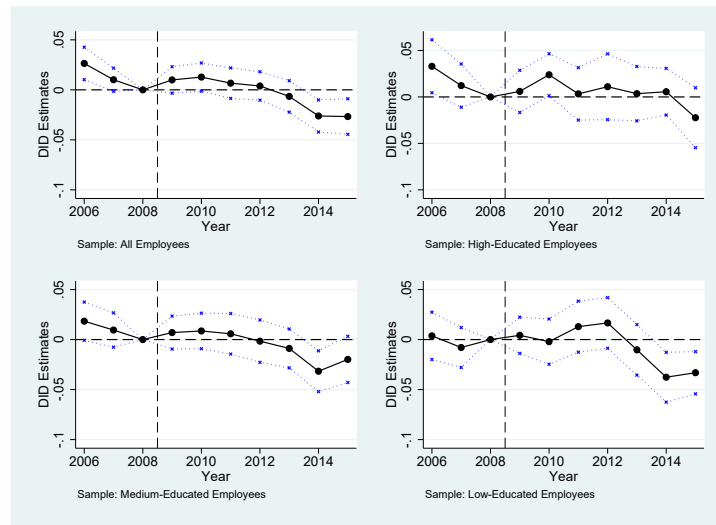
Interestingly, the recovery of the economy is initiated by the job-separation rate. BC’s job-finding rate remains low and steady between 2012 and 2015, during which the job-separation rate gradually returns to its pre-policy level, thereby diminishing the unemployment effect. According to the search theory, vacancies and unemployed workers match and draw a match-specific productivity level. The match forms a production unit as long as the match-specific productivity level is so high that the flow profit is nonnegative. The carbon tax increases the cost of production; hence, it requires a higher match-specific productivity level to form a production unit. As a result, the job-finding rate declines. Since the

policy shock is permanent, the impact on BC’s job-finding rate is permanent. Meanwhile, the carbon tax increases the job-separation rate because of the wage rigidity among incumbent employees. Since the carbon tax rate rises annually until 2012, the job-separation rate goes up until then. While wages are slowly adjusted, BC’s job-separation rate began to fall in 2012 until this rate reaches its pre-policy level, and thus BC’s unemployment rate began to fall in 2012.

To better understand the unemployment adjustment, I decompose the overall unemployment effect into the unemployment effects of the increased job-separation rate and the decreased job-finding rate in Figure 7. In the earlier stage of the shock, the unemployment effect is (roughly) equally attributed to the variation in each of the transition rates. While the unemployment effect of the decreased job-finding rate remains 0.6 percentage points in the later stage of the shock, the unemployment effect of the increased job-separation rate has started diminishing since BC’s carbon tax ceases to increase. This exercise reveals that the variations of the two job transition rates are essential in explaining the unemployment effect in the earlier stage of the shock, consistent with recent findings (Elsby et al., 2009; Fujita and Ramey, 2009; Elsby et al., 2013; Coles and Moghaddasi Kelishomi, 2018). If the shock lasts long enough, the unemployment effect is entirely driven by the job-finding rate, in line with the spirits of the earlier literature (Hall, 2005a,b,c; Shimer, 2005; Hagedorn and Manovskii, 2008; Shimer, 2012).

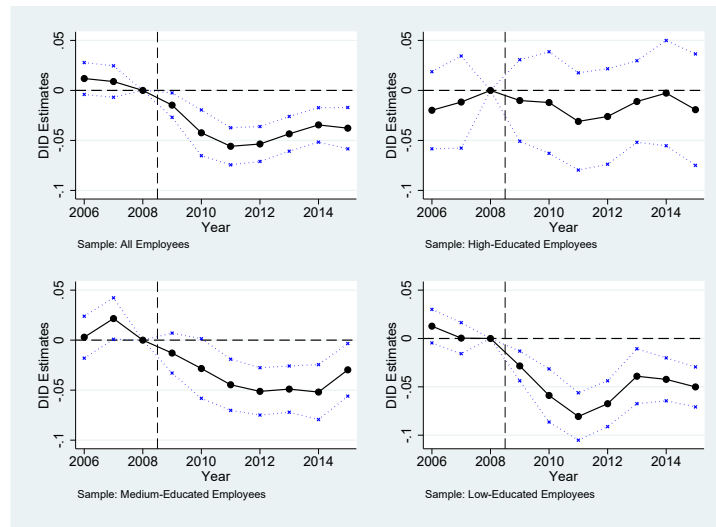
This subsection uncovers the dynamic effects of the carbon tax in the labor market and provides further support on the identifying assumptions. To avoid repetition, I discuss the identifying assumptions in the next subsection and leave the summary of the findings to Section 4.

Figure 1: The Dynamic Wage Effects on Incumbent Workers



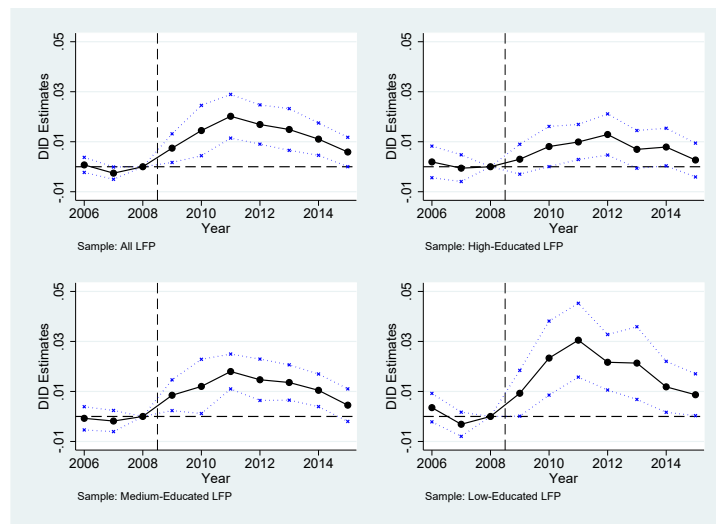
Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure 2: The Dynamic Wage Effects on New Hires



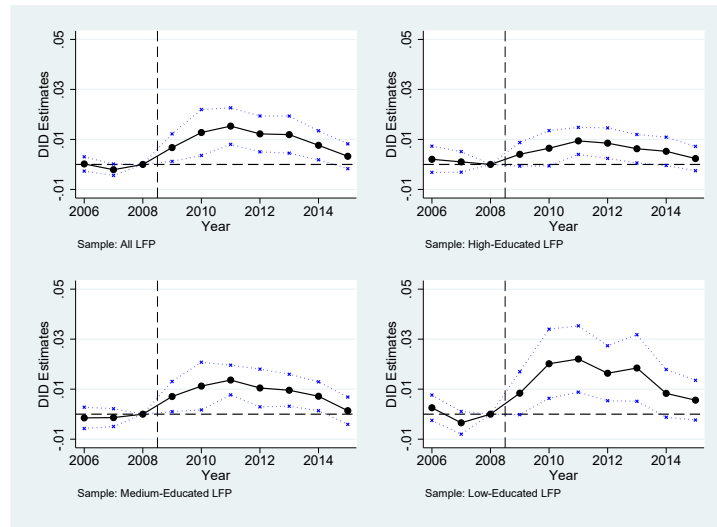
Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure 3: The Dynamic Unemployment Effects of the Carbon Tax Policy



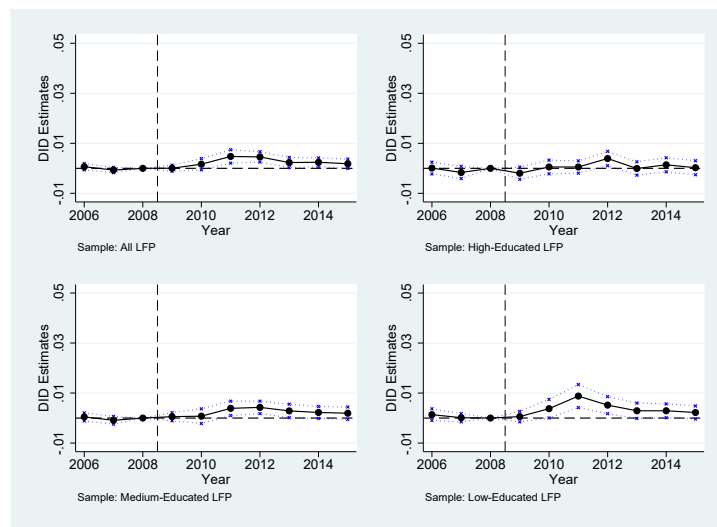
Notes: The dependent variables are dummy variables for unemployment. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure 4: The Dynamic Effects of the Carbon Tax Policy on Short-Term Unemployment



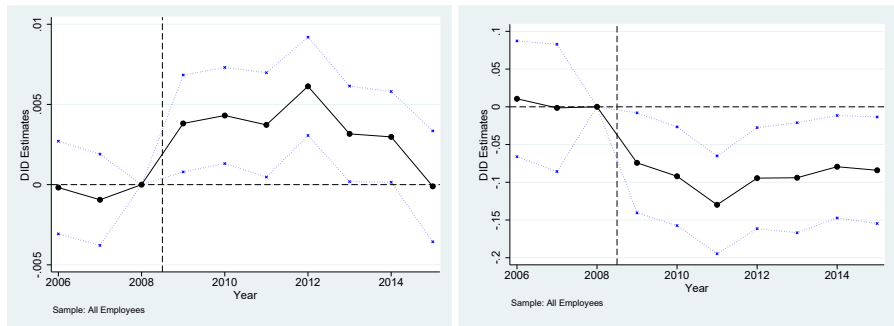
Notes: The dependent variables are dummy variables for short-term unemployment. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure 5: The Dynamic Effects of the Carbon Tax Policy on Long-Term Unemployment



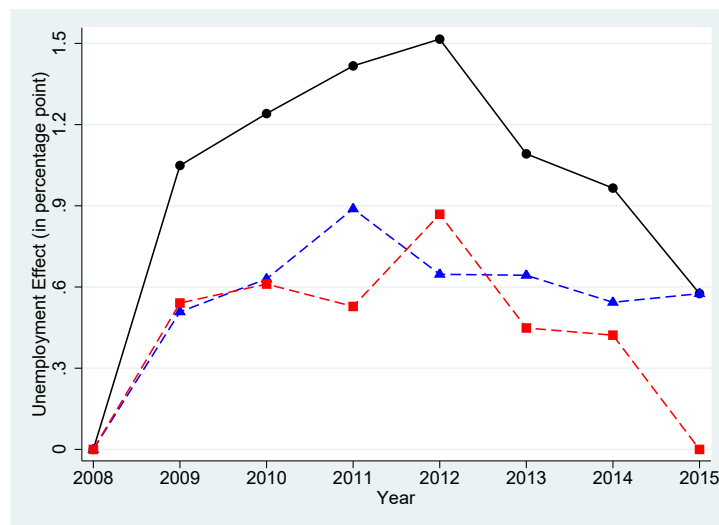
Notes: The dependent variables are dummy variables for long-term unemployment. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure 6: The Dynamic Effects of the Carbon Tax Policy on Job Transition Probabilities



Notes: The dependent variables are job-separation rates and job-finding rates in the Left and the Right panel, respectively. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure 7: The Dynamic Decomposition of the Unemployment Effect



Notes: The solid line is the unemployment effect in the steady state that is derived from the policy impacts on two job transition rates. The two dash lines represents the unemployment effects shutting down the policy impact on one of the job transition rates. The triangle (square) marker represents the unemployment effect from the job-finding (job-separation) rate.

### 3.7 Are the Results Credible?

To interpret the documented effects as the causal effects of the carbon tax, identifying assumptions are required. I will discuss the validity of these assumptions in the rest of this section.

**Assessing the Anticipatory Effect.** The analyses of this paper assume no anticipatory effects. If the public anticipates and reacts to BC's carbon tax policy, the estimates will end up capturing only the effects of the shocks in the post-policy period, not the entire effects of the shock. One should be aware that the period between the announcement and the implementation of the policy is less than 4.5 months. This short period of time limits the extent to which firms and workers respond to the policy. To ease the doubt on this claim, I estimate the anticipatory effect on BC's hourly wage rate and unemployment rate.

Here is the procedure to assess the anticipatory effect. The policy was announced on February 19, 2008, and was implemented on July 1 of the same year. Hence, I define the pre- and the post-announcement periods as January and the months between March and June in 2008, respectively. The observations in February are excluded because half of the month indeed belongs to the pre-announcement period, and the other half belongs to the post-announcement period. In this way, the first difference arises from the gaps in wage and unemployment rates between the pre- and the post-announcement periods. However, this difference may not capture the causal effect of the announcement because of seasonality. That is, the wage and the unemployment rates between March and June could be different from that in January. I control this seasonality using the observations in 2007, serving as the second difference. Lastly, BC's wage and unemployment effects in 2008 could be country- and year-specific. Hence, I include the observations from the other provinces to control the country-specific effect. Therefore, the difference between BC and the other provinces serves as the third difference. The estimates can be obtained from the following regression model:

$$Y_{ijt} = \alpha + \beta_1(BC_j \times 2008_t \times \text{March-June}_t) + \beta_2(BC_j \times 2008_t) + \beta_3(BC_j \times \text{March-June}_t) + \beta_4(2008_t \times \text{March-June}_t) + X_{ijt}^T \gamma + \eta_j + \delta_t + \varepsilon_{ijt}.$$

$2008_t$  equals one if a respondent is observed in 2008, and zero otherwise.  $\text{March-June}_t$  equals one if a respondent is observed between March and June, and zero otherwise. Samples are restricted to workers in BC and the other control provinces in January, March, April, May, and June during 2007-2008. All specifications include province and year fixed effects and the dummy variables for age, gender, educational level, and marital status. Again, standard errors are clustered at the level of province, age, gender, and marital status, providing us with 350 clusters.

Table 7 reports the estimates of our interest from the estimation of equation (4). Two points emerge from the table. First, the result suggests no anticipatory effects. The estimates  $\hat{\beta}_1$  are close to zero and are statistically insignificant at any conventional level, suggesting that BC's wage and unemployment rates remain unchanged after the announcement. It is therefore safe to conclude that the wage and unemployment trends between BC and the control provinces are parallel in the first half of 2008. While Section 3 provides evidence that the annual wage and unemployment trends are parallel between BC and the control provinces during the three years prior to the policy, this subsection provides support for the

Table 7: The Anticipatory Effects of BC's Carbon Tax

<b>Difference-in-Differences Analysis</b>		
	(1)	(2)
Dependent Variable	$\ln(Wage)$	<i>Unemployed</i>
<i>BC</i> × 2008 × <i>Mar-Jun</i>	0.001 (0.009)	0.000 (0.003)
<i>BC</i> × 2008	-0.007 (0.008)	0.004 (0.003)
<i>BC</i> × <i>Mar-Jun</i>	-0.006 (0.006)	0.002 (0.003)
2008 × <i>Mar-Jun</i>	-0.004 (0.004)	0.004*** (0.002)
Observations	443,385	624,806
Adjusted $R^2$	0.484	0.033

Notes: *Unemployed* equals one if a respondent is unemployed, and zero otherwise. All specifications include province and year fixed effects, and the dummy variables for age, gender, educational level, and marital status. Samples are restricted to all employees and LFP in column (1) and (2), respectively. The treatment group is BC's respondents between March and June in 2008. There are two control groups: the ROC between March and June in 2008 and BC's respondents between March and June in 2007. Data come from the Canadian LFS January, March, April, May, and June 2007-2008. Robust standard errors in parentheses are clustered at the level of province, age, gender, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

parallel trends in the wage and unemployment rates during the six months prior to the policy.

Second, the estimates  $\hat{\beta}_4$  provides additional support on the validity of other identifying assumptions. These estimates suggest that workers in Canada (both BC and the control provinces) experience a negligible wage effect and a small increase in the unemployment rate in the first half of 2008. These increases are likely attributed to the recession.<sup>14</sup> While the recession does increase the unemployment rate in Canada and the corresponding unemployment effect is not specifically larger in BC's labor market. In sum, this subsection finds no statistical evidence on any anticipatory effect between the announcement and the implementation dates of the policy.

**Common Trend Assumption.** This assumption requires the parallel trends in the wage and unemployment rates between BC and the control province during the period of examination (i.e., July 2005-June 2015). Despite no direct test on this common trend assumption, I summarize the suggestive evidence for the validity of this assumption found in this section.

The trends in wages and unemployment rates are parallel between BC and the control province prior to the policy. Subsection 3.6 provides evidence that the wage trends are parallel for both incumbent workers and new hires during July 2005-June 2008. The trends in their unemployment rates are also parallel during the same period. Moreover, when the unemployment effect is decomposed into the short- and the long-term unemployment effects, it is obvious that the trends in both the short- and the long-term unemployment rates are parallel between BC and the control province. As discussed above, the steady-state unemployment rate depends on both the job finding and the job-separation rates, according to the

<sup>14</sup>The NBER's Business Cycle Dating Committee dates the beginning of the recession to be December 2007.

job search theory. Since Subsection 3.6 documents that the trends in the two rates are parallel prior to the policy, the trends in the unemployment rates are parallel. In fact, this subsection finds no anticipatory effects, implying that the changes in the wages and the unemployment rates are statistically identical in BC and the ROC in the first half of 2008, which is the six months prior to the policy. While Subsection 3.6 provides evidence on the parallel yearly trends in the wages and the unemployment rates between BC and the ROC during the three years prior to the policy, this subsection provides support that their monthly trends are also parallel during the six months prior to the policy. With a long period of common trends in the wage and the unemployment rates prior to the policy, it is likely that these common trends follow right after the policy.

Evidence suggests that these trends are likely parallel over 10 years during July 2005-June 2015. While the impacts of BC's carbon tax on high-educated workers are at best weak, the wage and the unemployment trends are parallel for the high-educated. For example, the wage trends between BC and the ROC are parallel for high-educated incumbent workers and new hires during July 2005-June 2015. The long-term unemployment trend is also parallel during the same period. These findings suggest that if BC had not introduced the carbon tax in 2008, the trends in the wages and the unemployment rates between BC and the ROC would have been parallel for medium- and low-educated workers during the same period. Despite no direct test on this identifying assumption, this section does provide strong evidence on the validity of the common trend assumption.

One may cast doubt that the negative shock is driven by the 2008 recession, not BC's carbon tax. Again, there exists no direct test to verify the claim that the shock is driven by the carbon tax. However, the documented labor market consequences cohere with the taxation structure of carbon tax in three dimensions. First, the impacts in the labor market are more pronounced at higher carbon tax rates. Subsection 3.1 shows that both the wage and the unemployment effects enlarge as the carbon tax rate increases, consistent with the incentive created by carbon tax

Second, the heterogeneities in the wage and the unemployment effects are consistent with the distributions of energy intensity and human capital. Carbon tax increases production cost through emission; hence, it is expected that the labor market consequences are concentrated on industries with higher energy intensity levels. While Yip (2018) documents that less-educated workers tend to work in industries with higher energy intensity level in the pre-policy period, the wage and the unemployment effects are expected to be more pronounced for the less-educated. These two predictions are supported by the empirical findings in Subsection 3.2, providing another piece of evidence on the source of the negative shock.

Third, the effects of the negative shock fit well with the timing of the carbon tax policy. As shown in Subsection 3.6, the documented wage and unemployment effects appear immediately following the implementation of the policy, July 2008. While the carbon tax rate increased by CAD\$5 each year until 2012, the wage and the unemployment effects (if exist) are expected to grow in the first few years. All the figures in Subsection 3.6 support this prediction. Indeed, the dynamics of the wage and the unemployment effects strongly support that the documented effects arise from BC's carbon tax.

In fact, the documented effects are inconsistent with the timing of the recession. The NBER's Busi-



ness Cycle Dating Committee dates the beginning of the recession to be December 2007. It is expected that the corresponding wage and the unemployment effects may show up in the first half of 2008. This subsection shows that in the first half of 2008, the unemployment rate increases by 0.4 percent in Canada, suggesting that the recession does lead to labor market consequences. Probably because of the inflexibility of the labor market, no statistical evidence on the wage effect is found. Meanwhile, the trends in the wage and the unemployment rate for BC and the ROC are parallel in the first half of 2008. These results cast doubt on the claim that the labor market consequences of the 2008 recession are more severe in BC than in the ROC and the claim that the documented labor market consequences are driven from the recession.

**Other Concerns.** Some may cast doubt that the documented labor market consequences are driven by trade shocks. Unfortunately, there does not exist any direct test to disprove the claim. To control for variations in the exchange rate of Canadian dollar to United States dollar could, to a certain degree, ease the doubt. The *Year*×*Month* fixed effect in all regression models of this paper takes the variations in the exchange rate into account. However, a few may worry that the impacts of trade shocks are heterogenous across provinces: the documented effects may be driven by these heterogeneous effects. It is worthy highlighting that the *Year*×*Month* fixed effect does not remove these heterogenous effects. Hence, I re-conduct all the analyses of this paper by controlling *BC*×*Monthly Exchange Rate* and report the tables and figures in Appendix C. These tables and figures suggest that the conclusions of this paper are robust to exchange rate fluctuations.

Lastly, one may worry that the documented results are driven by any shocks in control labor markets, not BC's labor market. To address the issue, I re-conduct all the analyses by excluding each of the provinces and report the figures in Appendix D. These figures suggest that the conclusions of this paper are robust to choices of control labor markets.

## 4 Discussion

This section summarizes the processes by which BC's carbon tax policy shapes the labor market and discusses what we learn from this shock.

### 4.1 How Does the Carbon Tax Shape the Labor Market?

The carbon tax increases flow cost and thus depresses labor demand. Incumbent wages are completely rigid over at least five years; in plain English, incumbent wages do not respond to the policy. With this rigidity, many incumbent employees are laid off; otherwise, firms may suffer losses. The wage rigidity, in line with the traditional thought, creates (cyclical) unemployment through the increased job-separation rate.

Nevertheless, the sticky wage theory alone does not give a complete picture. With a higher flow cost, it requires a higher match-specific productivity level to formulate a production unit during a job search process. This tightened requirement reduces the job-finding rate, further worsening the (cycli-

cal) unemployment. The unemployment rate rises by 1.4 percentage points. About 79 percent of this unemployment effect is driven by short-term unemployment, and the rest is attributed to long-term unemployment. In other words, the shock significantly increases the number of unemployment spells and slightly lengthens the duration of unemployment spells.

Hiring wages are adjusted through hiring processes. Firms re-optimize their profits; wages are adjusted accordingly: the average hourly wage rates of new hires drop by 5.1 percent. As a result, the average wage of the economy drops by 2.7 percent (and is thus procyclical). The slow process of wage adjustments “breaks down” the wage rigidity in the employment. Hence, the job-separation rate decreases to its original level, and the unemployment effect diminishes. However, the unemployment rate approaches but never reaches its pre-shock level. Since the shock of the carbon tax is permanent, the impact on the job-finding rate lasts long. While the job-finding rate remains low, the unemployment rate is higher than its pre-shock level. Over this cycle, the variation in the job-separation rate explains at least 40 percent of the unemployment effect, and over half the unemployment effect is attributable to the decreased job-finding rate.

## 4.2 What Do We Learn from the Carbon Tax?

This paper gives four lessons on labor economics and ten lessons on environmental economics.

**Four Lessons on Labor Economics.** First, this paper reconciles the sticky wage theory with recent documentations of the procyclicality of the average hourly wage rate in the literature. Whereas traditional thought holds that wage rigidity is the major cause of cyclical unemployment (Keynes, 1936; Blanchard and Fischer, 1989), an extensive literature documents the procyclicality of average wages (Bowlus et al., 2002; Carneiro et al., 2012; Martins et al., 2012; Haefke et al., 2013; Elsby et al., 2016). Gertler et al. (2016) points out the problem of correlations: some of new hires experience wage gains through job-to-job transitions in expansions as to why the average hiring wage is found to be procyclical. This paper finds that the average incumbent wage is completely rigid; hence, when the shock hits the economy, the job-separation rate and thus the unemployment rate increase. Meanwhile, the average hiring wage is found to drop substantially, explaining why the average hourly wage rate is found procyclical in the literature.

The harmonization between the sticky wage theory and the wage procyclicality also speaks directly to the literature on the unemployment volatility puzzle (Shimer, 2005; Costain and Reiter, 2008): the Diamond-Mortensen-Pissarides model fails to generate the volatility of unemployment. Hall (2005b) argues that wage rigidity is the solution to this puzzle. Since then, a voluminous literature has rendered wage stickiness to improve their model performance (Blanchard and Gali, 2007; Hall and Milgrom, 2008; Gertler and Trigari, 2009; Kennan, 2010; Shimer, 2010). This paper, complementary to Martins et al. (2012) and Haefke et al. (2013), reveals that wage rigidity, especially among new hires, may not be the solution to this puzzle, providing robust empirical support to the view of Pissarides (2009).

Second, the conventional wisdom about the mechanism of wage rigidity is fine-tuned. The sticky wage theory posits that wages are rigid because they are not easily adjusted under labor contracts and

they are adjusted once contracts are renegotiated (Barro, 1977; Fischer, 1977; Thomas and Worrall, 1988). While labor contracts, in general, do not last longer than five years, incumbent employees are expected to renegotiate contracts at least once during the first few years of the shock. Nevertheless, this paper finds that wages are completely rigid among incumbent employees in the first five years of the shock, casting doubt on labor contracts explaining wage rigidity and wage adjustments operating through contract renewals. Instead, I provide evidence that wages are slowly adjusted through new contracts during hiring processes, modifying the sticky wage theory.

Third, the findings of this paper uncover the processes through which negative shocks increase unemployment (Hall, 2005a,c; Davis et al., 2006; Fujita and Ramey, 2009; Elsby et al., 2009; Shimer, 2012; Elsby et al., 2013). The sticky wage theory predicts that wage rigidity creates unemployment through increased job-separation rates. This theory provides an incomplete story. Using the search theory, the increased flow cost reduces the job-finding rate. Both the increased job-separation rate and the decreased job-finding rate jointly increase unemployment.

This paper further reveals the dynamics of the transition rates. While the shock is permanent, the impact on the job-finding rate lasts long and is seemingly permanent. However, as discussed in the second point, the hiring wages are procyclical; the re-employment of new hires will not increase the job-separation rate. Hence, the impact on job-separation rate is short-lived: this rate falls to its pre-shock level, in line with the calibrated model in Coles and Moghaddasi Kelishomi (2018). While the impact on job-separation rates is temporary, the impact on job-finding rates is seemingly permanent. As a result, the unemployment rate will approach but never reach its pre-shock level. While the literature focuses on the contemporary effects on the two transition rates, this paper uncovers the entire dynamics of the two rates. During the entire process, over 40 percent of the unemployment effect is attributable to the increased job-separation rate, and over half the effect arises from the decreased job-finding rate. These findings challenge the widespread view that job-separation rates are acyclical (Hall, 2005a,c; Shimer, 2012). This paper provides causal estimates supporting that (i) job-separation rates are countercyclical, and (ii) the variations in both job-separation rates and job-finding rates are essential in explaining unemployment fluctuations.

Fourth, short-term unemployment is the major component of cyclical unemployment. I find that about 79 percent of the unemployment effect of the shock is attributable to the short-term unemployment, revealing that these shocks mainly increase the number of unemployment spells, not the duration of unemployment spells. This finding raises questions about the entitlement period of unemployment benefits (Røed and Zhang, 2003; Lalive, 2008; Caliendo et al., 2013; Jung and Kuester, 2015; Mitman and Rabinovich, 2015; Kolsrud et al., 2018; Landais et al., 2018a,b). Extending the entitlement periods of unemployment benefits purposes to smooth the consumption of unemployed workers over the prolonged period of unemployment. For example, the unemployment benefit periods extended from 26 weeks to 99 weeks in most of the states in the 2008 recession. If the extent to which shocks lengthen unemployment spells is limited, why are the entitlement periods of unemployment benefits extended substantially in economic downturns? Moreover, while a voluminous literature documents causal linkages from the extended periods of unemployment benefits to unemployment durations (Lalive, 2008; Caliendo et al.,

2013), are the prolonged unemployment spells in slumps mainly attributed to these shocks or the extensions of benefit periods? While only a small part of the unemployment effect is attributed to long-term unemployment, this paper suggests benefit periods should be indexed to the long-term unemployment rate, as opposed to the common practice—the unemployment rate.

**Ten Lessons on Environmental Economics.** First, this paper reveals that environmental policies could lead to recession-like labor markets. While an earlier empirical literature on environmental policies focuses mainly on manufacturing employment (Greenstone, 2002), these policies may shift employment from manufacturing to other sectors (Walker, 2013). As noted by Hafstead and Williams (2018), *“these studies often employ a difference-in-differences approach, using firms in unregulated industries as controls ... such studies will not only miss the effects on unregulated firms, but also yield biased estimates of the effects on regulated firms”*. Using the whole economy in the ROC as controls, this paper concludes that environmental taxes create job and wage losses.

Second, this paper uncovers the mechanisms through which environmental policies create job losses. There is a paucity of literatures that investigate the impacts of environmental policies on the two job flow rates (i.e., a job-separation rate and a job-finding rate) partly because it requires the estimation of a dynamic search model using detailed monthly individual-level information, such as the length of unemployment spells, to obtain the two flow rates (See Shimer (2012)). Using quarterly industry-level data, Curtis (2017) measures the two transition rates using changes in unemployment stocks and finds that a cap-and-trade program increases unemployment via a lower job-finding rate, not a higher job-separation rate. Hence, a cap-and-trade program hurts unemployed workers through a lower likelihood of being hired and does no harm to employees in terms of unemployment risks.

In contrast, this paper follows the job-search literature to estimate the two flow rates. Ones may hold a superstitious belief that it is natural to increase job-separation rates and decrease job-finding rates to increase unemployment. Brochu and Green (2013) and Dube et al. (2016) provide two counterexamples, both of which show that minimum wage policies, even though increasing unemployment, reduce the two transition rates in Canada and the United States. I find that carbon taxes increase unemployment through an increased job-separation rate and a decreased job-finding rate. Hence, carbon taxes expose the entire labor force into a higher risk of unemployment either through the increased risk of being laid off or through the decreased likelihood of being hired.

Third, this paper is the first to decompose the overall unemployment effect of environmental policies into the short-term and the long-term effects of unemployment. While the documentation of the overall unemployment effect is rare in this literature, it is unsurprising that none conducts the decomposition analysis. This paper finds that the unemployment effect is mostly attributable to the short-term unemployment. In other words, environmental policies mainly increase the number of unemployment spells, not the duration of unemployment spells.

Fourth, this paper, perhaps surprisingly, is the first paper to document the impact of environmental policies on returns to labor using micro-data. While an earlier empirical literature on environmental policies focuses mainly on manufacturing, Walker (2013) and Curtis (2017) document that an intensity standard and a cap-and-trade program create earnings losses. While these policies also reduce employ-

ment, it is unclear whether the earnings losses arise from the reduction in returns to labor, measured by a hourly wage rate, or the reduction in labor hours, the intensive margin of employment. This paper adds to the literature by documenting the adverse effect of environmental tax on returns to labor: each Canadian dollar of carbon tax decreases hourly wage rates by 0.120-0.122 percent.

Fifth, the paper reveals the process by which environmental policies depress wages. Carbon taxes do not reduce the average wages of incumbent employees; instead, they reduce wages through labor turnovers. In essence, this process is similar to the documentation in [Walker \(2013\)](#) and [Curtis \(2017\)](#). One of the explanations of this wage-cutting process is that workers lose industry- and firm-specific human capitals after reemployed in other sectors, in line with the significant earnings losses due to sectoral reallocation documented in [Walker \(2013\)](#). Meanwhile, this paper reveals that it takes longer than our expectation to have wages fully adjusted: the wages of incumbent workers remain rigid after the usual length of contracts. This finding provides information to researchers that carefully calibrate the staggered-wage model to examine the unemployment effects of environmental regulation (e.g. [Hafstead and Williams \(2018\)](#) and [Castellanos and Heutel \(2019\)](#)). This prolonged period of wage rigidity, in part, explains why the unemployment effects of environmental taxes are consistently much larger in empirical studies than in calibrated models.

Sixth, the weak Porter hypothesis is too “weak”. Environmental policies raise production cost, depressing labor demand ([Bovenberg and van der Ploeg, 1998](#); [Wagner, 2005](#); [Hafstead and Williams, 2018](#); [Sun and Yip, 2019](#)). Meanwhile, the weak Porter hypothesis implies that these policies could induce innovation and thus labor demand. These two compelling forces make the economic prediction ambiguous. This paper shows that environmental policies such as BC’s carbon tax induce negative shocks. The results of this paper provide no evidence for or against the weak Porter hypothesis. Nevertheless, this paper does imply that if the weak Porter hypothesis is valid, it is so weak that the induced innovation effect is dominated by the direct cost effect of these policies.

Seventh, this paper gives a lesson on revenue-neutral environmental taxes. The literature on the unemployment effect of revenue-neutral environmental taxes suggests that the unemployment rate could go down ([Bovenberg and van der Ploeg, 1998](#); [Wagner, 2005](#); [Sun and Yip, 2019](#); [Castellanos and Heutel, 2019](#)) and if the unemployment effect exists, it is small ([Hafstead and Williams, 2018](#)). The intuition is that the environmental tax revenue is redistributed to the public by reducing payroll tax rates or in a lump-sum manner. Either way reduces firms’ cost, providing financial incentives to create vacancies and thus depressing unemployment. In contrast to these theoretical search models, this paper provides a causal estimate of revenue-neutral carbon taxes on the wage and the unemployment effects. The findings of this paper suggest that environmental taxes, even though revenue-neutral, decreases wages and increases unemployment.

A simple prediction on the overall unemployment effect from this literature may hide important messages of these policies. This paper reveals that the impacts could be large among low-educated workers: their unemployment rate could increase by three percentage points, their wages could decrease over seven percent, and the effects last long, at least seven years.

Eighth, this paper sheds light on the distribution of costs associated with environmental policies in the

labor market. Incumbent employees basically bear no associated cost in the labor market. If the policy is revenue-neutral so that payroll tax rates are reduced, incumbent employees benefit from more after-tax income. Unemployed workers bear the cost through the wage effect: once re-employed, they receive lower hourly wage rates. The unemployment induced by these policies bear the brunt of the costs: these workers lose jobs and, even if re-employed, receive lower hourly wage rates. This paper enhances our understanding of how the associated costs are distributed through the wage and the unemployment effects in the labor market. These distributive costs in the labor market are often neglected in the literature on the costs associated with environmental policies (Hazilla and Kopp, 1990; Bovenberg et al., 2005; Ryan, 2012; Fullerton and Monti, 2013; Williams et al., 2015; Rausch and Schwarz, 2016).

Ninth, this paper uncovers the issue regarding environmental justice in the labor market. The literature on environmental justice expresses concerns about whether environmental policies shift pollution into regions with more traditionally disadvantaged groups (Banzhaf and Walsh, 2008; Kaswan, 2008; Gamper-Rabindran and Timmins, 2011). Meanwhile, I find that the adverse effects of BC's carbon tax on wages and unemployment are more pronounced for the less-educated (i.e., traditionally disadvantaged groups), raising concerns on the labor market analog of environmental justice.

Tenth, it reveals the regressivity of environmental policies from the source side of income. Environmental policies are known to be regressive on the use side of income because the poorer tend to spend a higher fraction of their income on energy-intensive goods (Metcalf, 1999; Fullerton et al., 2011). Since these policies reduce returns to capitals that are disproportionately owned by the rich, they dampen the regressivity from the source side of capital income (Fullerton and Heutel, 2007; Araar et al., 2011). The findings of this paper highlight the regressivity from another source side of income: these policies cut wages and take away job opportunities, and these adverse effects are more pronounced at lower educational levels.

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## Appendix A: The Policy Impact on Weekly Working Hours

Table A1: The Impact of Carbon Tax on Weekly Working Hours

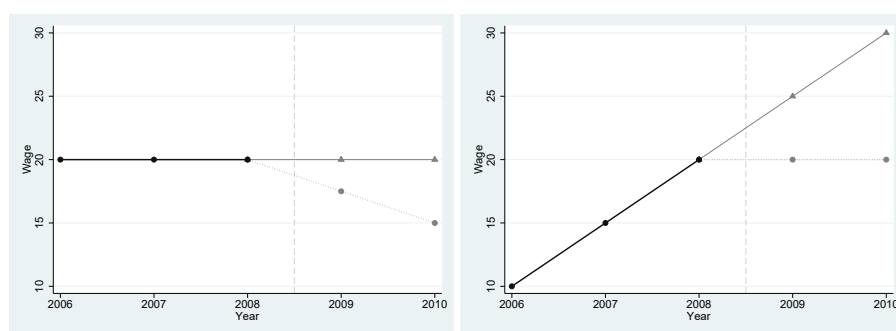
<b>Difference-in-Differences Analysis</b>				
	(1)	(2)	(3)	(4)
<i>BC</i> × <i>Post</i>	-0.004 (0.007)	-0.005 (0.007)	-0.007 (0.006)	-0.008 (0.006)
Adjusted R <sup>2</sup>	0.268	0.259	0.225	0.219

Notes: Dependent variables are the numbers of the usual weekly working hour in the main job, the usual weekly working hour in all jobs, the actual weekly working hour in the main job, and the actual weekly working hour in all jobs in Columns (1) through (4), respectively. Specifications include the dummies for gender, age, educational level, marital status, and industry, and the province and the *Year* × *Month* fixed effects. Samples are restricted to all employees. BC is the treatment group. ROC is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations ranges from 4,778,040 to 5,224,772. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

## Appendix B: The Definition of Wage Rigidity

This section redefines rigidity and discusses related identification problems. The discussion begins with a practical example. Suppose a researcher observes the hourly wage rates of two individuals in each year from 2006 to 2010. Figure B1 presents the hourly wage rates of the first (left figure) and the second (right figure) individuals. The first individual receives \$20 an hour during 2006-2008, as shown in the left figure. In the absence of any demand shock, such as BC's carbon tax, the wage would be \$20 in 2009 and 2010, represented by the triangles. Because of the shock, firms re-optimize their profit functions and find it optimal to offer this individual a lower wage level, represented by the dots in 2009 and 2010.

Figure B1: The Demonstration of Wage Rigidity



Notes: The left (right) figure demonstrates the measure of wage rigidity without (with) wage growth. Each dot represents the wage level of an individual in the corresponding year. The vertical line represents the first month of the shock (July 2008). Triangles represent the wage level in the absence of the shocks, and the dots in the corresponding year represents the optimal wage level offered by a firm.

This researcher measures wage rigidity using a common practice in the literature (Goette et al., 2007; Martins et al., 2012; Barattieri et al., 2014; Elsby et al., 2016). First, the researcher creates a dummy variable, which equals one if a wage level in one year is identical to the level in the previous year, and zero otherwise. We call this variable *Rigidity*.<sup>15</sup> Hence, if the wage level equals \$20 in 2009 and 2010, *Rigidity* will all be one in these years. The observation with *Rigidity* equal to one indicates that wages are completely rigid. Traditional thought holds that cyclical unemployment arises from this rigidity (Keynes, 1936; Blanchard and Fischer, 1989). In contrast, if the wage level equals the amount, presented by the dots, in 2009 and 2010, wages are said to be completely flexible. If wages lie between triangles and dots, wages are said to be partially rigid, and unemployment may increase.

This approach does not work well with the second observation. The wage of the second individual grows over time. In the absence of any shock, the individual would have received \$25 an hour. Because of the shock, it is optimal for a firm to offer this individual \$20 per hour. Suppose wages are completely flexible so that the firm can keep the individual with \$20 an hour. If the researcher defines and measures wage rigidity in the same way as the first observation, *Rigidity* equals one in 2009 and 2010 because

<sup>15</sup>The literature measures the wage growth and the hazard of a wage change to understand wage adjustment/stickiness. These measures share the same essence of the wage differential of an individual across periods.

the wage levels are all identical during 2008-2010. Nevertheless, this “rigidity” reflects complete wage flexibility and creates no unemployment.

These two observations reveal the problem of the common practice to define and measure wage rigidity. When *Rigidity* equals one, the rigidity could reflect the complete wage rigidity as in the first observation and the complete wage flexibility as in the second observation. This definition of “rigidity” fails to capture the wage rigidity that causes cyclical unemployment according to the sticky wage theory.

What is wrong with the measure? Wages are said to be rigid when they are rigid to a reference wage level. According to the common practice as in above, the wage level in the previous year serves as the reference level. This reference level has its own empirical advantages: it is observable and easily accessible. Using a longitudinal data, it is easier to obtain the wage level right before the shock than to obtain the one 10 years ago. Nevertheless, this reference level may not capture rigidity properly.

The wage level, at which individuals would otherwise receive in the absence of shocks (represented by triangles), could serve as the reference level to properly define rigidity. If wages are sticky to this reference level, wages are completely rigid in the cases of the first and the second observations. Since the wage level in the previous year (i.e., \$20) happens to be identical to this reference level in the first observation, *Rigidity* can identify wage rigidity using the one-year lagged wage as a reference level. If this reference level (i.e., \$25) and the lagged wage (i.e., \$20) are different as in the second observation, *Rigidity* fails to do its job. Hence, to define and to measure rigidity, we are required to identify the reference level that would otherwise be realized in the absence of shocks. This definition and measure of rigidity can generally be applied to the rigidity in wage, price, and others.

The identification of this reference level is, however, empirically challenging. The proposed reference level is counterfactual: no one is certain whether observed levels in the presence of shocks are the ones which would otherwise be realized in the absence of the shocks. The identification of this reference level, hence, shares the same essence as in the literature on policy evaluations: once a policy shock arrives, the outcomes in the absence of the shock are counterfactual (Angrist and Pischke, 2008).

The presence of a control group, rather than a longitudinal data, is a possible solution to this identification problem. Longitudinal data may provide rich information on the wage history of individuals but may not help researchers identify the proposed reference level at hand. In some circumstances, longitudinal data may not provide any relevant wage history. For example, they provide no information on the one-period lagged (or any lagged) wage of new hires and labor market entrants. In contrast, a control worker with a similar wage trend could help identify the counterfactual wage levels incumbent workers, new hires, and labor market entrants would otherwise receive in the absence of shocks.

The sticky wage theory can, therefore, be tested with the proposed reference level. If wages are sticky, the realized wages of the affected workers in the presence of a negative shock should be equal to the corresponding counterfactual reference wage levels. In other words, the sticky wage theory can be tested just as evaluating the wage consequence of a negative shock. If the sticky wage theory holds, ones should not be able to find any statistical evidence on the adverse wage effect. One can test the null hypothesis that the realized wage of the affected workers, on average, equals the realized wage of the workers in the control group in the absence of any other shock. If the null hypothesis is rejected, one can

conclude that the sticky wage theory is invalid.

As mentioned in the introduction, it is difficult to identify the causal effects of negative shocks during recessions on labor market consequences. One of the reasons is that it is challenging to find the control labor markets that are likely unaffected by the shocks. For the same reason, it is difficult to obtain the counterfactual reference wage levels so as to test the hypothesis. This difficulty is also highlighted by [Elsby et al. \(2016\)](#): “*inferring convincingly clear-cut counterfactual (wage) distributions from observational data turns out to be beyond the reach of even the most skillful researchers*”. To resolve this problem, this paper exploits the introduction of BC’s carbon tax to identify the causal wage effect so as to speak to the sticky wage hypothesis. The details of BC’s carbon tax policy and a potential control group are discussed in [Section 2](#).



## Appendix C: Robustness to Exchange Rate Fluctuations

Table C1: The Wage and the Unemployment Effects of Carbon Tax

<b>Difference-in-Differences Analysis</b>				
	(1)	(2)	(3)	(4)
<b>Panel A. Wage Effect</b>				
<i>BC</i> × <i>Post</i>	-0.026*** (0.007)	-0.026*** (0.004)		
<i>BC</i> × <i>Tax</i>			-0.118*** (0.47)	-0.117*** (0.023)
Adjusted $R^2$	0.026	0.467	0.026	0.467
<b>Panel B. Unemployment Effect</b>				
<i>BC</i> × <i>Post</i>	0.012*** (0.003)	0.012*** (0.003)		
<i>BC</i> × <i>Tax</i>			0.041*** (0.009)	0.040*** (0.011)
Adjusted $R^2$	0.006	0.031	0.006	0.031

Notes: Dependent variables are  $\ln(\text{wage})$  and a dummy variable for unemployment in Panel A and B, respectively. All specifications include  $BC \times \text{Exchange Rate}$  and the province and the  $\text{Year} \times \text{Month}$  fixed effects. Specifications also include the dummies for gender, age, educational level, and marital status in columns (2) and (4). In Panel A, a dummy variable for industry is also included in columns (2) and (4). Samples are restricted to all employees in Panel A and all LFP in Panel B. BC is the treatment group. The ROC is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations are 5,227,156 and 7,412,988 in Panel A and B. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

Table C2: The Heterogeneous Effects Across Workers

<b>Difference-in-Differences Analysis</b>						
	(1)	(2)	(3)	(4)	(5)	(6)
<i>BC</i> × <i>Post</i>	-0.004 (0.006)	-0.022*** (0.005)	-0.029*** (0.006)	0.006*** (0.002)	0.011*** (0.003)	0.016*** (0.005)
Adjusted $R^2$	0.273	0.391	0.480	0.005	0.015	0.043

Notes: Dependent variables are the logarithm of a hourly wage rate in columns (1)-(3) and are the dummy variables for unemployment in columns (4)-(6). All specifications include  $BC \times \text{Exchange Rate}$ , the dummies for gender, age, educational level, and marital status, and the province and the  $\text{Year} \times \text{Month}$  fixed effects. Specifications in columns (1)-(3) also include the dummies for industry. Samples are restricted to all employees in columns (1)-(3) and all LFP in columns (4)-(6). BC is the treatment group. The ROC is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations are 1,168,245, 2,575,287, 1,872,112, 1,533,481, 3,301,827, and 2,593,276 from column (1) to column (6). Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

Table C3: The Policy Effects on the Wage Elasticity w.r.t. Energy Intensity

<b>Difference-in-Differences Analysis</b>					
<b>Dependent Variable: ln(Wage)</b>					
	(1)	(2)	(3)	(4)	(5)
<b>Panel A</b>					
$\ln(EI) \times BC \times Post$	-0.038*** (0.014)	-0.033*** (0.012)	-0.026 (0.030)	-0.028* (0.016)	-0.035** (0.015)
Adjusted R <sup>2</sup>	0.171	0.369	0.196	0.324	0.382
<b>Panel B</b>					
$\ln(EI) \times BC \times Post$	-0.030** (0.012)	-0.028*** (0.010)	-0.021 (0.028)	-0.022 (0.015)	-0.032*** (0.012)
Adjusted R <sup>2</sup>	0.171	0.370	0.196	0.324	0.382
<b>Panel C</b>					
$\ln(EI) \times BC \times Post$	-0.021** (0.010)	-0.018** (0.007)	-0.003 (0.023)	-0.015 (0.012)	-0.021** (0.009)
Adjusted R <sup>2</sup>	0.171	0.369	0.196	0.324	0.381

Notes: Energy intensity is measured by energy expenditure per unit value of shipments, per unit of value added, and per unit of payroll in Panels A, B, and C, respectively. All specifications include  $BC \times Exchange Rate$ , and the province, the industry, and the  $Year \times Month$  fixed effects. Specifications also include the dummies for gender, age, educational level, and marital status in columns (2) through (5). Samples are restricted to all manufacturing employees in columns (1) and (2) and are restricted to high-, medium-, and low-educated manufacturing employees in columns (3) through (5), respectively. BC is the treatment group. The ROC, excluding MB, is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations for high-, medium-, and low-educated employees are 81,650, 304,831, and 281,979, respectively. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

Table C4: Impacts on Incumbent Workers and New Hires

<b>Difference-in-Differences Analysis</b>					
<b>Dependent Variable: ln(Wage)</b>					
	(1)	(2)	(3)	(4)	(5)
<b>Sample: Incumbent Workers</b>					
<i>BC</i> × <i>Post</i>	-0.015*	-0.015***	-0.010	-0.015***	-0.008
	(0.008)	(0.005)	(0.007)	(0.006)	(0.008)
Observations	1,648,169	1,648,169	436,609	892,349	542,950
Adjusted R <sup>2</sup>	0.034	0.330	0.156	0.233	0.300
<b>Sample: New Hires</b>					
<i>BC</i> × <i>Post</i>	-0.048***	-0.046***	-0.002	-0.045***	-0.057***
	(0.014)	(0.007)	(0.016)	(0.009)	(0.007)
Observations	878,241	878,241	148,779	432,212	427,621
Adjusted R <sup>2</sup>	0.043	0.469	0.274	0.401	0.489

Notes: All specifications include *BC*×*Exchange Rate* and the province and the *Year*×*Month* fixed effects. Specifications also include the dummies for gender, age, educational level, marital status, and industry in columns (2) through (5). Samples are restricted to all employees in columns (1) and (2) and are restricted to high-, medium-, and low-educated employees in columns (3) through (5), respectively. *BC* is the treatment group. *ROC* is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. Observations during July 2008-June 2009 are excluded if the sample of new hires is analyzed. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

Table C5: Impacts on Short- and Long-Term Unemployment

<b>Difference-in-Differences Analysis</b>					
	(1)	(2)	(3)	(4)	(5)
<b>Dependent Variable: Dummy Variable for Short-Term Unemployment</b>					
<i>BC</i> × <i>Post</i>	0.010***	0.010***	0.005***	0.009***	0.013***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)
Adjusted R <sup>2</sup>	0.004	0.031	0.005	0.014	0.041
<b>Dependent Variable: Dummy Variable for Long-Term Unemployment</b>					
<i>BC</i> × <i>Post</i>	0.002***	0.002***	0.001	0.002***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Adjusted R <sup>2</sup>	0.002	0.005	0.002	0.004	0.007

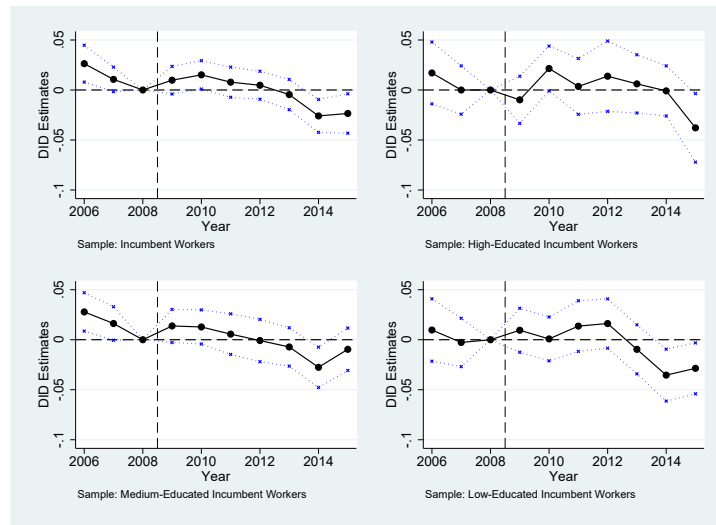
Notes: Long-term unemployment is defined as the unemployment with unemployment duration longer than six months. The rest of unemployment is defined as short-term unemployment. All specifications include *BC*×*Exchange Rate* and the province and the *Year*×*Month* fixed effects. Specifications also include the dummies for gender, age, educational level, and marital status in columns (2) through (5). Samples are restricted to all employees in columns (1) and (2) and are restricted to high-, medium-, and low-educated employees in columns (3) through (5), respectively. *BC* is the treatment group. The *ROC* is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations for high-, medium-, and low-educated workers are 1,529,284, 3,291,323, and 2,580,880, respectively. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

Table C6: The Anticipatory Effects of BC's Carbon Tax

<b>Difference-in-Differences Analysis</b>		
	(1)	(2)
Dependent Variable	$\ln(Wage)$	<i>Unemployed</i>
<i>BC</i> × 2008 × <i>Mar-Jun</i>	0.003 (0.010)	-0.002 (0.004)
<i>BC</i> × 2008	-0.012 (0.011)	0.011** (0.005)
<i>BC</i> × <i>Mar-Jun</i>	-0.008 (0.007)	0.004 (0.003)
2008 × <i>Mar-Jun</i>	-0.004 (0.004)	0.004*** (0.002)
Observations	443,385	624,806
Adjusted $R^2$	0.484	0.033

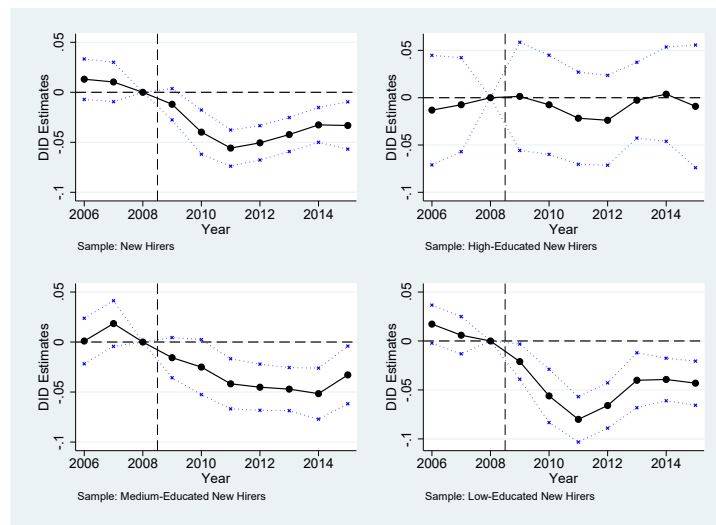
Notes: *Unemployed* equals one if a respondent is unemployed, and zero otherwise. All specifications include province and year fixed effects, and the dummy variables for age, gender, educational level, and marital status. Samples are restricted to all employees and LFP in column (1) and (2), respectively. The treatment group is BC's respondents between March and June in 2008. There are two control groups: the ROC between March and June in 2008 and BC's respondents between March and June in 2007. Data come from the Canadian LFS January, March, April, May, and June 2007-2008. Robust standard errors in parentheses are clustered at the level of province, age, gender, and marital status. Significance levels: \*\*\*=1%, \*\*=5%, \*=10%.

Figure C1: The Dynamic Wage Effects on Incumbent Workers



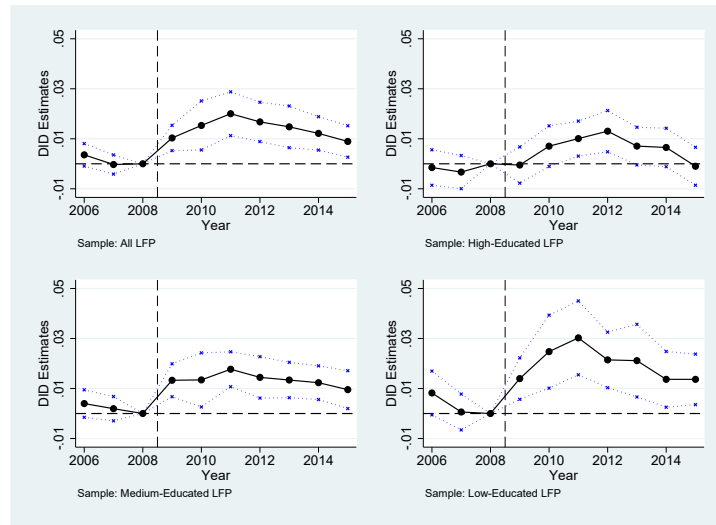
Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure C2: The Dynamic Wage Effects on New Hires



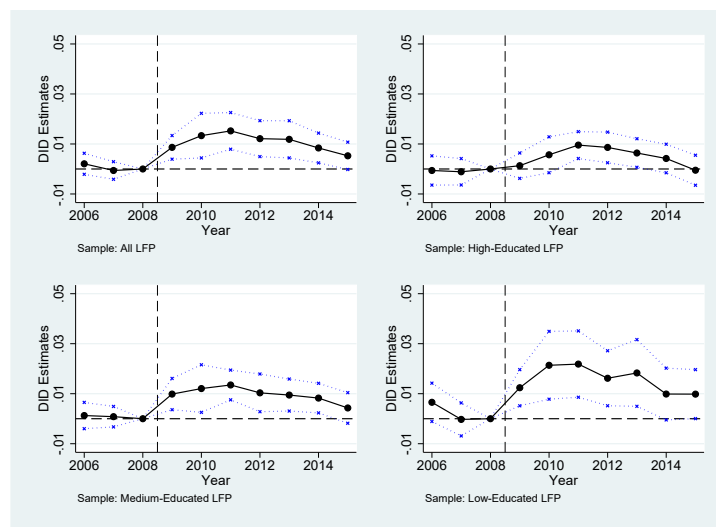
Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure C3: The Dynamic Unemployment Effects of the Carbon Tax Policy



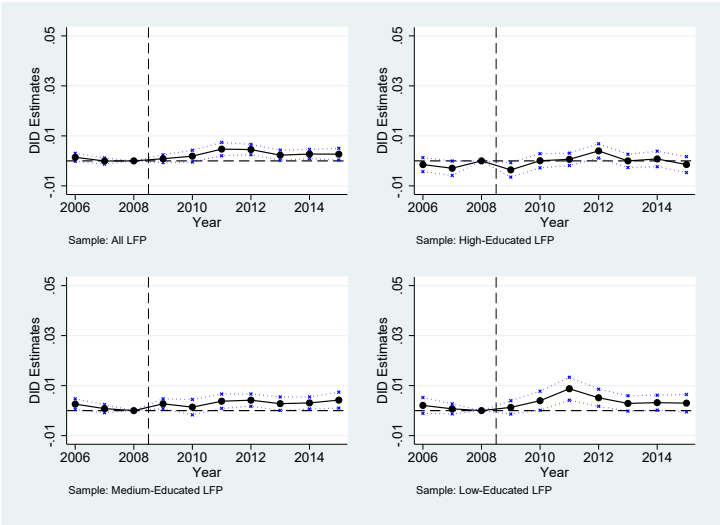
Notes: The dependent variables are dummy variables for unemployment. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure C4: The Dynamic Effects of the Carbon Tax Policy on Short-Term Unemployment



Notes: The dependent variables are dummy variables for short-term unemployment. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

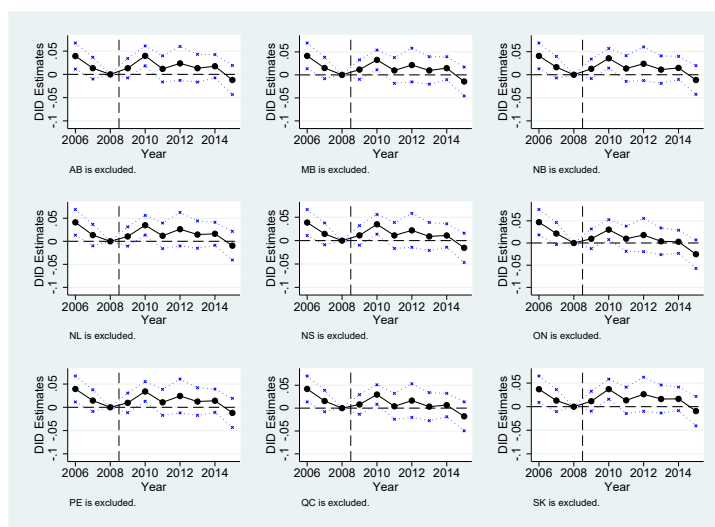
Figure C5: The Dynamic Effects of the Carbon Tax Policy on Long-Term Unemployment



Notes: The dependent variables are dummy variables for long-term unemployment. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

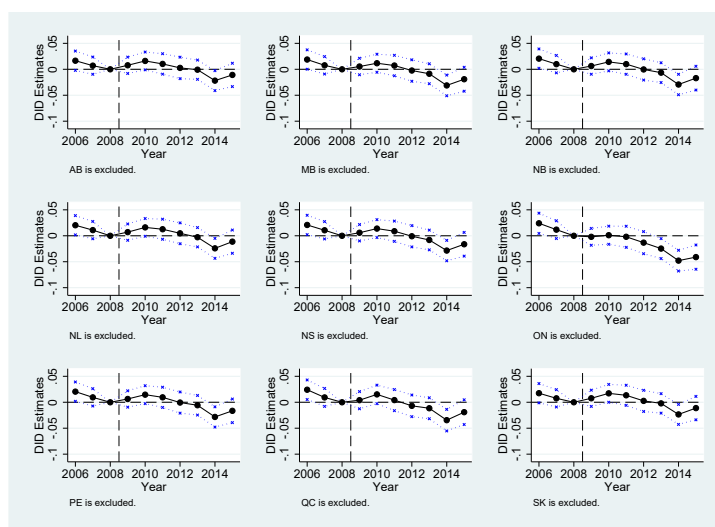
# Appendix D: Robustness to Choices of Control Provinces

Figure D1: The Dynamic Wage Effects on High-Educated Incumbent Workers



Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

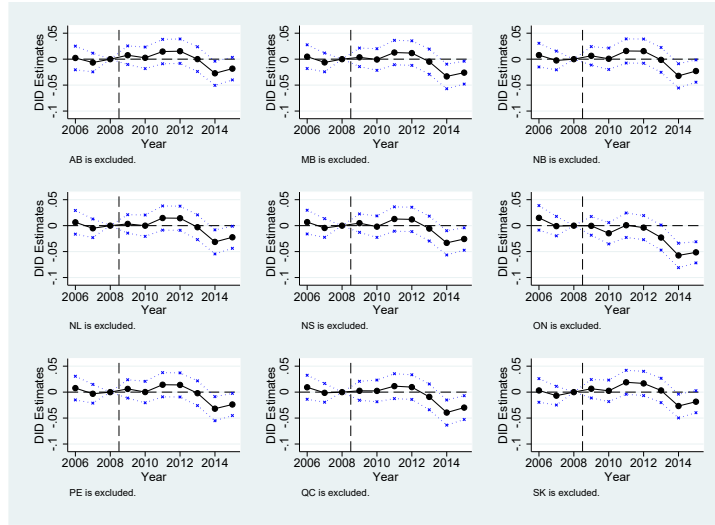
Figure D2: The Dynamic Wage Effects on Medium-Educated Incumbent Workers



Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

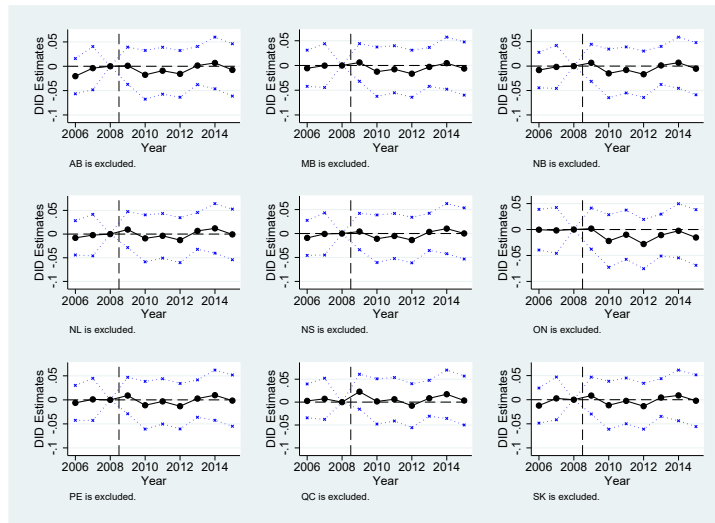


Figure D3: The Dynamic Wage Effects on Low-Educated Incumbent Workers



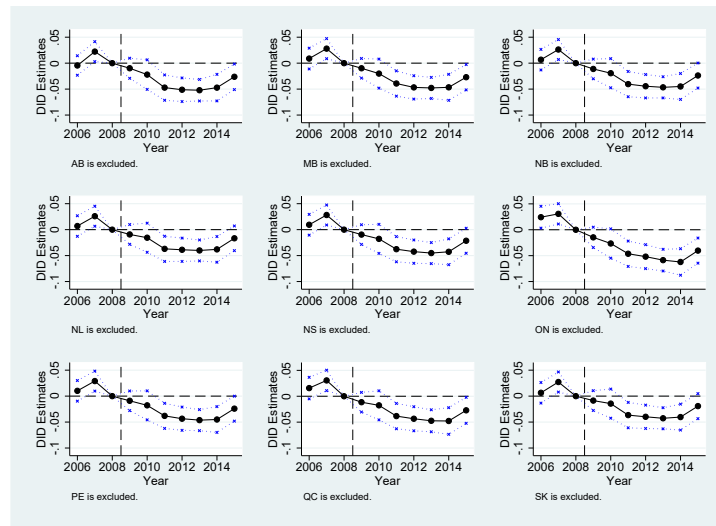
Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D4: The Dynamic Wage Effects on High-Educated New Hires



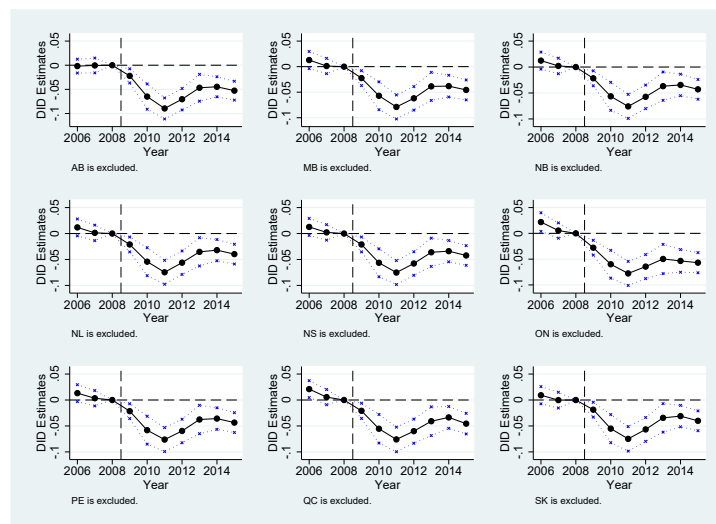
Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D5: The Dynamic Wage Effects on Medium-Educated New Hires



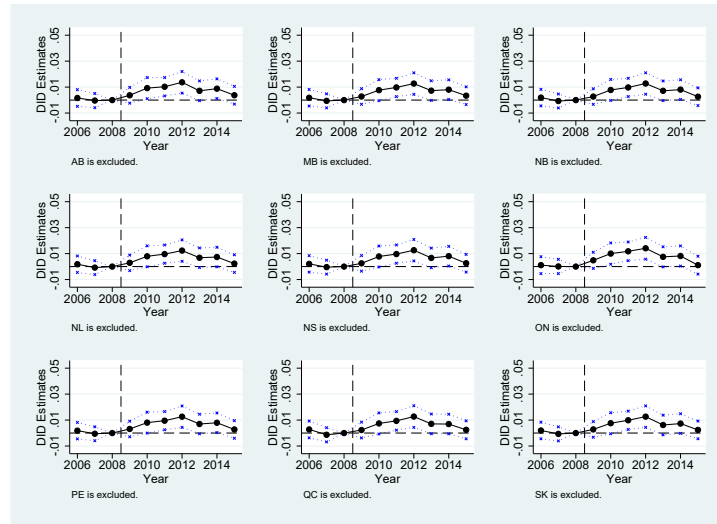
Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D6: The Dynamic Wage Effects on Low-Educated New Hires



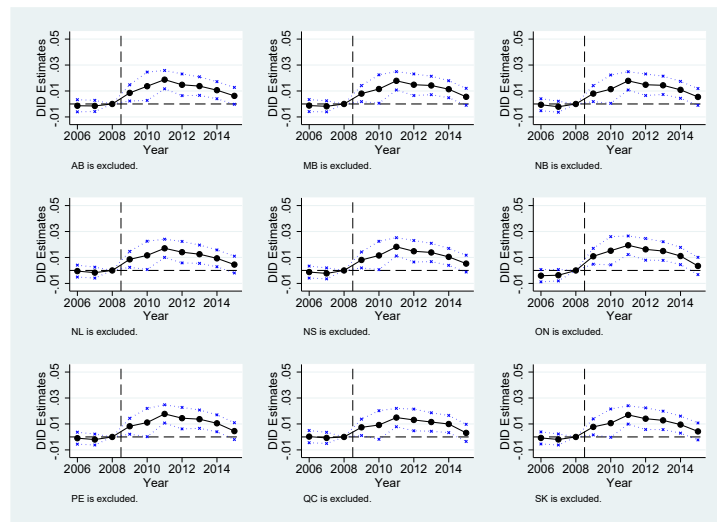
Notes: The dependent variables are  $\ln(\text{wage})$ . Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D7: The Dynamic Unemployment Effects on High-Educated Workers



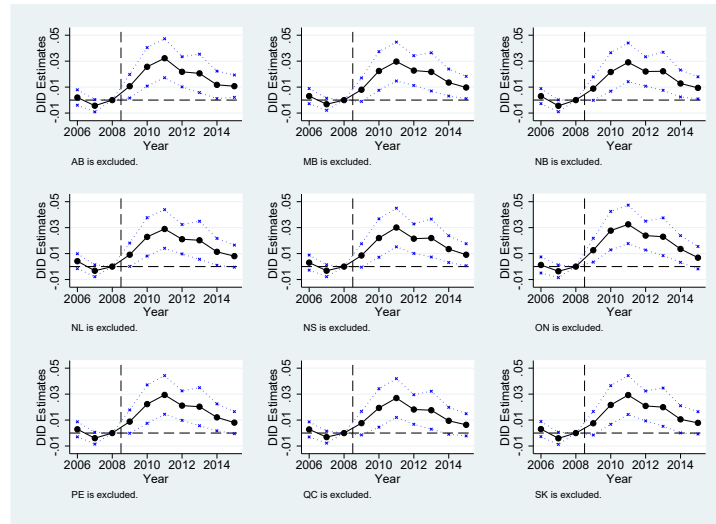
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D8: The Dynamic Unemployment Effects on Medium-Educated Workers



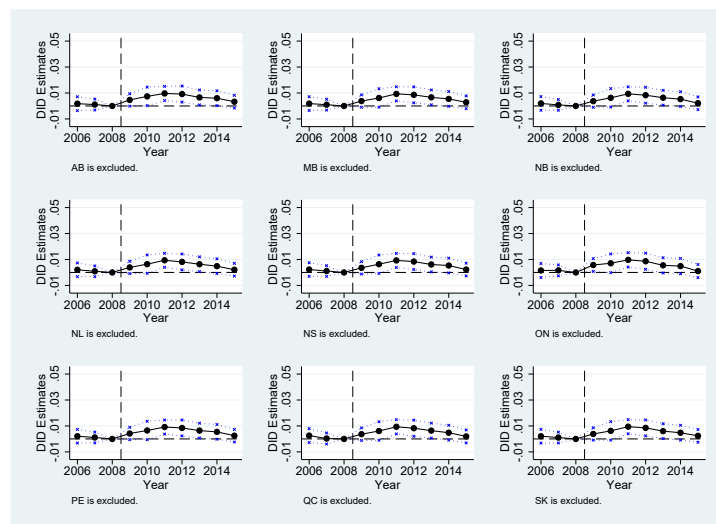
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D9: The Dynamic Unemployment Effects on Low-Educated Workers



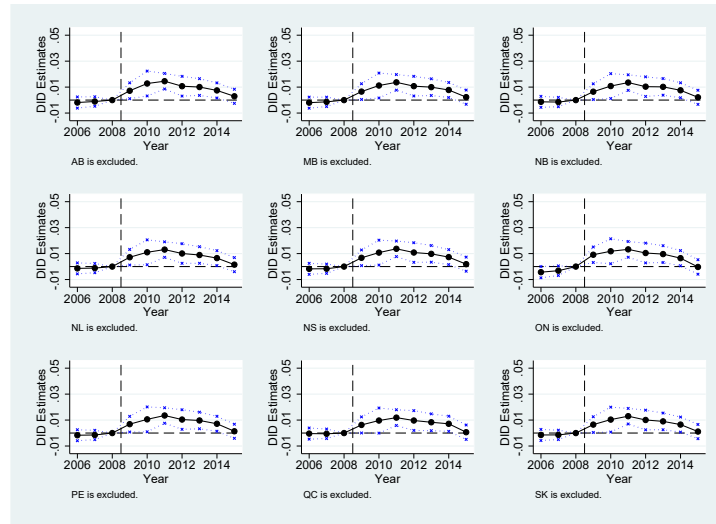
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D10: The Dynamic Short-Term Unemployment Effects on High-Educated Workers



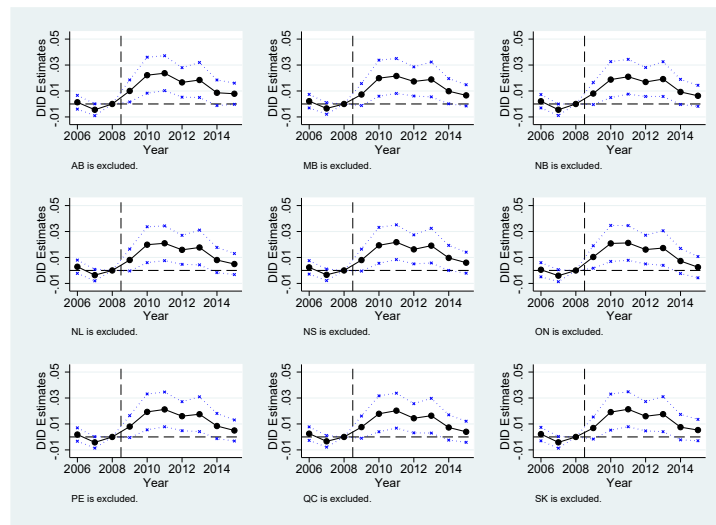
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D11: The Dynamic Short-Term Unemployment Effects on Medium-Educated Workers



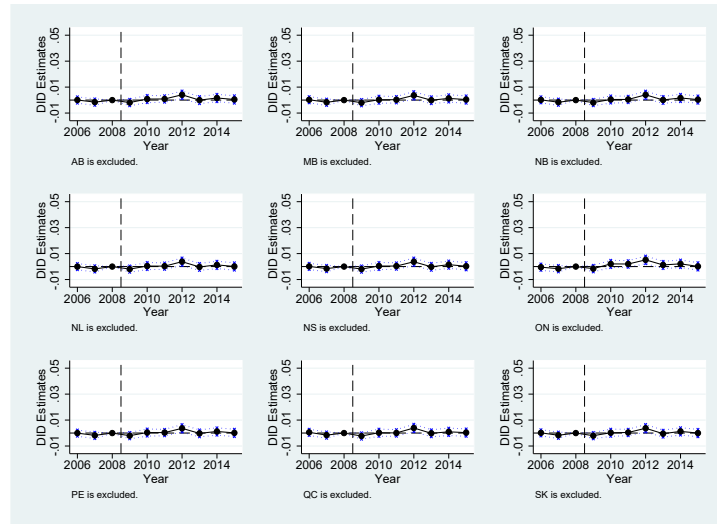
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D12: The Dynamic Short-Term Unemployment Effects on Low-Educated Workers



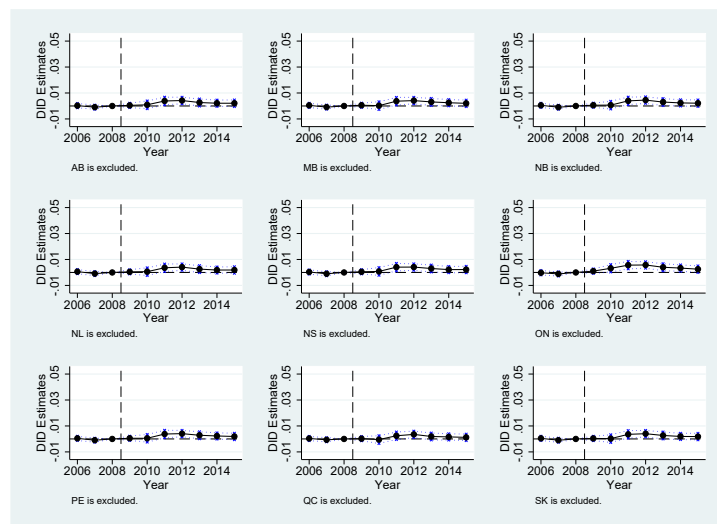
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D13: The Dynamic Long-Term Unemployment Effects on High-Educated Workers



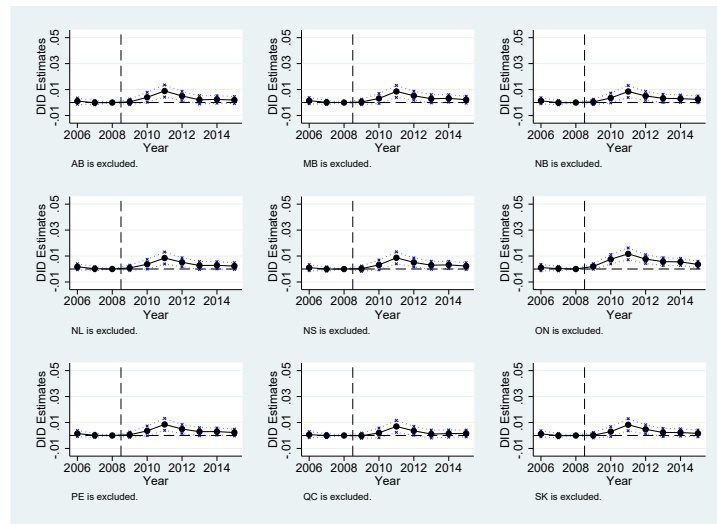
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D14: The Dynamic Long-Term Unemployment Effects on Medium-Educated Workers



Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure D15: The Dynamic Long-Term Unemployment Effects on Low-Educated Workers



Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.