LABOR MARKET DYNAMICS IN SLUMPS: WHAT, SO WHAT, AND NOW WHAT?

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Abstract

How does the labor market behave in slumps? I exploit the introduction of carbon tax in British Columbia to answer this question. Combining matching with a difference-in-differences method, I find that the carbon tax creates a negative labor demand shock. Average real wages are found completely rigid among incumbent employees, not among new hires. While the rigidity, in line with traditional thought, increases unemployment, the substantial decline in the average real wage of new hires explains why the real wage is found procyclical in the literature. This paper challenges the widespread view that job separation rates are acyclical: about 40 percent of the unemployment effect is attributable to increased job separation rates. The shock increases mainly the number of unemployment spells, not their duration, suggesting that substantially extending unemployment benefit periods in slumps is unnecessary.

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1 Introduction

At least since Keynes (1936), economists have been debating on the process by which the labor market behaves in slumps. For example, the sticky wage theory posits that wages are sticky because they 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). Traditional thought holds that this stickiness is the major cause of cyclical unemployment (Keynes, 1936; Blanchard and Fischer, 1989).[1] However, are wages really sticky? Numerous studies document that wages are procyclical (Bowlus et al., 2002; Carneiro et al., 2012; Martins et al., 2012; Haefke et al., 2013; Elsby et al., 2016) as to why Haefke et al. (2013) highlights that wage rigidity is unlikely the sole reason for unemployment volatility. So, what else creates unemployment volatility?

While these questions remain unanswered, the debate on the process by which unemployment fluctuates also rages on. Influential papers, like Hall (2005a) and Shimer (2012), are heavily cited as evidence on the acyclicality of job separation rates. In other words, unemployment fluctuates because workers find it harder to get a job in slumps, not because they are more likely laid off. As noted by Elsby et al. (2009), many theoretical works refer to this conventional wisdom to motivate their treatment of job separation rates as acyclical, and numerous studies attempt to explain this acyclicality (Hall, 2005a,b; Rudanko, 2009). Nevertheless, job separation rates are also found countercyclical using various decomposition methods (Elsby et al., 2009; Fujita and Ramey, 2009) and using a calibrated model (Coles and Moghaddasi Kelishomi, 2018).

These debates are fierce partly owing to a paucity of conclusive empirical evidence on the causal effects of negative labor demand shocks. A vast literature studies the relationship between recessions and their labor market consequences. Nevertheless, the identification of the causal effects of negative shocks from recessions could be challenging. Often, there exist no definite arrival dates of recessions. Moreover, the severity of shocks from recessions are usually vaguely measured by the magnitude of the decline in economic activities such as unemployment rates—one of the labor market outcomes, not by the severity of the shocks. Once recessions arrive, they are pervasive throughout the country: their impacts affect almost every single local labor market. To estimate the causal effects of recessions, an instrumental variable approach could be a right candidate to tackling potential endogeneity problems. However, the justifications of an instrumental variable rely heavily on verbal arguments. Hence, this literature focuses mainly on the correlations between negative shocks from recessions and their labor market outcomes, not their causal relationship.

I estimate the causal effects of negative labor demand shocks to answer following questions. In addition to the estimation of the wage and the unemployment effects, this paper uncovers the processes by which these effects operate: Whose wages are sticky, and whose are not? Can labor contracts explain

[1] While Shimer (2005) and Costain and Reiter (2008) document the unemployment volatility puzzle: the Diamond-Mortensen-Pissarides (DMP) model fails to generate the volatility of unemployment, Hall (2005b), in line with the traditional thought, argues that wage stickiness is the solution to this puzzle. Since then, a voluminous literature has rendered wage stickiness to improve their model performance (Blanchard and Galí, 2007; Hall and Milgrom, 2008; Gertler and Trigari, 2009; Kennan, 2010; Shimer, 2010).

the stickiness? Do wage adjustments operate through the renegotiation during contract renewals or new contracts during hiring processes? Does unemployment fluctuate because of variations in job separation rates or job finding rates? Are unemployment fluctuations mainly driven by the increase in the number of unemployment spells or the duration of unemployment spells?

I utilize an environmental policy as a medium to lend insights as to how negative labor demand shocks shape the labor market. These shocks could arise from oil crises that lead to the 1973 recession, the 1980 recession, and the early 1990s recession in the United States but could also be driven by policy shocks such as carbon tax and other environmental regulations, climate change shocks, and other cost shocks. This paper exploits a unique opportunity provided by the introduction of a revenue-neutral carbon tax policy in British Columbia (BC). Unlike recessions, the arrival date and the stringency of the shock are definite. A carbon tax of $10 per tonne of carbon dioxide equivalent (CDE) emissions was introduced on July 1, 2008, and the tax rate increased by $5 each year until 2012. Meanwhile, the policy shock in one labor market does not imply they will affect others, providing control labor markets that are likely unaffected by the policy. BC's carbon tax is suitable for a policy evaluation because it provides a novel source of exogenous variation in the stringency of the shock and provides numerous control provinces that are likely unaffected by the shock.

I apply the Coarsened Exact Matching (CEM) to the data to make the samples in BC and control provinces (i.e., the rest of Canada) observably similar, followed by a DID approach to capture the causal effects of the policy on labor market outcomes. A matching method is used for two reasons. First, it makes inferences less model dependent, reducing statistical bias (Ho et al., 2007). Second, the identifications of the policy effects on labor market outcomes such as wages are usually prone to the classical selection bias. Only employed workers receive wages and are thus included in the sample of examination. While the characteristics between employed and unemployed workers are different, the exclusion of unemployed workers from the sample creates the classical selection bias. To deal with this problem, it is shown empirically that a matching method yields accurate estimates (Dehejia and Wahba, 1999, 2002), especially under the DID framework (Smith and Todd, 2005). I adopt the CEM method because it dominates commonly used matching methods, including propensity score and Mahalanobis matching, by reducing imbalance, model dependence, estimation error, bias, variance, etc. (Iacus et al., 2011, 2012). This paper shows that this method does significantly increase the estimated wage effect but not the estimated unemployment effect.

This paper provides five primary contributions: First, this paper adds to the literature that investigates the impacts of environmental policies on employment (Greenstone, 2002; Walker, 2011; Kahn and Mansur, 2013; Curtis, 2017; Yip, 2018). While an earlier empirical literature on environmental policies focuses mainly on manufacturing employment, these policies may shift employment from manufacturing to other sectors. Yet, we know little of their overall effects in the labor market. 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*". My work adds to this literature by providing the estimates of the overall wage and unemployment effects in the labor market: BC's carbon tax

creates a negative labor demand shock, decreasing the average real wage by 2.7 percent and increasing the unemployment rate by 1.4 percentage points.[2] These findings suggest that carbon tax, even though revenue-neutral, depresses labor demand, providing an opportunity to uncover the processes by which negative labor demand shocks operate.

Second, this paper reconciles the sticky wage theory with recent documentations of the procyclicality of wages 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 negative labor demand shock on the real wage of incumbent workers is negligible over at least five years, reinforcing our understanding that wages are sticky among incumbent workers. This stickiness, in line with the traditional thought (Keynes, 1936; Blanchard and Fischer, 1989), creates cyclical unemployment. Meanwhile, I also provide the causal estimate of the negative wage effects on new hires: their average real wage drops by 5.1 percent, challenging the traditional thought about the stickiness of wages through hiring processes. Because of this substantial decrease, it is unsurprised that average real wages are found procyclical in the recent literature.

Third, this paper fine-tunes the conventional wisdom about the mechanism of wage stickiness. One of the explanations of the sticky wage theory is that wages are not easily adjusted under labor contract and they are adjusted through the renegotiation during contract renewals (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.[3] This finding casts doubt that labor contracts could explain the stickiness and that wage adjustments operate through contract renewals. Instead, this paper provides evidence that average wages are slowly adjusted through new contracts during hiring processes.

Fourth, this paper contributes to the board literature on unemployment fluctuations (Hall, 2005a,c; Davis et al., 2006; Fujita and Ramey, 2009; Elsby et al., 2009; Shimer, 2012; Elsby et al., 2013). In contrast to the correlation documented in the literature, this paper provides evidence on the causal effects of negative labor demand shocks on job transition rates: 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. The implications of these findings go beyond the revelation of the process by which carbon taxes increase unemployment. These findings challenge the widespread view that job separation rates are acyclical (Hall, 2005a,c; Shimer, 2012). In contrast to the commonly used decomposition approaches in this literature, 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

[2] 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 more robust estimates.

[3] As noted by Elsby et al. (2016), "*many workers, including ourselves, have their nominal wages rest only once a year*".

in explaining unemployment fluctuations.

Fifth, it questions conventional wisdom on the extension of the entitlement periods of unemployment benefits in slumps. For long, these benefit periods are lengthened to help unemployed workers smooth consumption over the prolonged period of unemployment in slumps.[4] However, I find that about 79 percent of the unemployment effect of the labor demand shock is attributable to short-term unemployment.[5] In other words, labor demand shocks mainly increase the number of unemployment spells, not the duration of unemployment spells. This finding raises important questions that contribute to a broad literature on 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): If the extent to which labor demand shocks lengthen unemployment spells is limited, why are the entitlement periods of unemployment benefits extended substantially in slumps? Are the prolonged unemployment spells in slumps mainly attributed to labor demand shocks or the extensions of benefit periods? Should the benefit periods be indexed to the unemployment rate or the long-term unemployment rate?

The paper is structured as follows. Section 2 describes identification strategies. Section 3 presents the labor market consequences of carbon tax as a negative labor demand shock. Section 4 paints the overall picture of the shock in the labor market and summarizes the lessons we learnt from 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.[6] According to economic theory, carbon tax increases production cost, thereby creating a negative labor demand shock. This shock provides a suitable medium to study how negative labor demand shocks shape the labor market for several reasons.

First, the arrival date of shocks arising from carbon tax is definite. Often, ones realize recessions through their labor market consequences such as high unemployment rates. No one knows the exact arrival date of recessions. While BC's carbon tax policy was implemented on July 1, 2008, one should expect its labor market consequence follows after its implementation. The exact arrival date provides us definite pre- and post-shock periods.

Second, the stringency of the carbon tax policy is also definite. The severity of recessions is deter-

[4]These programs include the Emergency Unemployment Compensation and the Extended Benefits in the United States. For example, the unemployment benefit periods extended from 26 weeks to 99 weeks in most of the states in the 2008 recession.

[5]Short-term unemployment is defined as unemployment with unemployment duration less than six months.

[6]This policy is revenue-neutral so that the carbon tax revenues are supposed to be 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.

mined in part by the magnitude of the decline in economic activities such as unemployment rates and output levels. Unlike recessions, the severity of the negative labor demand shock from BC's carbon tax increases with a carbon tax rate. The rate was initially $10 per tonne of carbon dioxide equivalent (CDE) emissions in 2008, increased by $5 per tonne annually until reaching $30 per tonne of CDE on July 1, 2012, and stayed at $30 from that time onwards. The variations in the carbon tax rate provide us the source of exogenous variations in the stringency of the shock to estimate the causal effects of the negative labor demand shock. 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, 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.[7] If the initial increase was not high, we would be uncertain whether the labor market did not respond to negative labor demand shocks or the shock was too small. For example, a statistically insignificant wage effect can be attributable to the 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.[8]

Fourth, the carbon tax provides numerous control provinces that are likely unaffected by the shock. Once recessions arrive, they are pervasive throughout the country: their impacts affect almost every single local labor market. In contrast, the introduction of BC's carbon tax does not necessarily imply that this policy will affect labor markets in other provinces, providing us numerous control labor markets that are likely independent of the shock.

Nevertheless, a carbon tax policy by no means dominates a recession as a medium to understand the labor market consequences of negative labor demand shocks. While the arrival and the stringency of recessions are not easily anticipated, the extent to which the public reacts to recessions is limited. However, the public may react to carbon tax policies 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 shocks. 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 potential anticipation effects of the shock, allowing us to capture the entire effects of the shock in the labor market. I will show in Subsection 3.7 that no statistical evidence on the anticipation effects on BC's carbon tax can be found.

[7] The initial carbon taxes were only $3.50, $0.04, and $5 per tonne of CDE in Quebec in 2007, the San Francisco Bay Area, California, in 2008, and Maryland in 2010, respectively.

[8] This finding is consistent with other studies. For example, per capita fuel consumption is found to decrease by 19 percent relative to the rest of Canada (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.

2.2 Data Descriptions

The present study utilizes the public-use files of the Canadian 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 easily be 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, employment mode (i.e., part-time versus full-time), industry etc.[9] I exclude workers aged 60 or above from my sample to avoid potential biases from retirement decisions. Key variables of interest are real wages and unemployment. The real wage is defined as the hourly wage rate on the main job throughout this paper, measured in 2002 Canadian dollar. Table 1 reports summary statistics on the real hourly wage rates and the unemployment rates in BC and the rest of Canada (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, real hourly wage rates increase regardless of 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 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 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 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

[9] I exclude observations with a missing value in any of the dummies for age, education, marital status, employment mode, and the nature of job.

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

	Average Real Hourly Wages			Unemployment Rates		
	BC	Rest of Canada	Difference-in-Differences	BC	Rest of Canada	Difference-in-Differences
A. Sample: High-Educated Workers						
July 2005-June 2008	23.31	24.27		3.38	3.68	
July 2008-June 2015	23.33	25.14		4.65	4.35	
Difference	0.02	0.87	-0.85	1.27	0.67	0.60
B. Sample: Medium-Educated Workers						
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
C. Sample: Low-Educated Workers						
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: These wage rates are measured in 2002 Canadian dollar. Data come from the Canadian LFS July 2005-June 2015.

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:

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

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 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. η_j and δ_t capture the province and the $Year \times Month$ fixed effects, respectively. According to Yip (2018), employment trends in BC and the rest of Canada are parallel during the three years prior to the policy (i.e., July 2005-June 2008). I will also provide evidence in Section 3 that wage trends are parallel

between the treatment and control groups during July 2005-June 2008 regardless of educational category. Therefore, I define the pre- and the post-policy period as July 2005-June 2008 and July 2008-June 2015, respectively.

The coefficient β_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.). Since this estimation method relies heavily on this assumption, I will discuss the validity of this assumption in Section 4.

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 nine provinces, two genders, nine age groups, and two marital statuses, providing us with 324 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 estimates the wage and the unemployment effects of the policy. This subsection confirms that BC's carbon tax induces a negative labor demand shock, lowering wages and increasing unemployment.

Subsection 3.2 examines the heterogeneous effects across industries and across workers. The heterogeneous effects across industries provide evidence that the policy impacts are concentrated on industries with higher energy intensity levels, reassuring that the negative labor demand shock arises from BC's carbon tax. While Yip (2018) shows that less-educated workers are more likely engaged in industries with higher energy intensity levels, the investigation of the heterogeneous effects across workers provide further support on the source of the labor demand shock.

Subsection 3.3 explores the policy impacts on incumbent workers and new hires, and Subsection 3.4 estimates the policy impacts on job finding and job separation rates. These two sections answer whether wages are sticky among incumbent workers and new hires and whether unemployment fluctuations arise 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 discusses required identifying assumptions of the analyses in this section.

3.1 The Wage and the Unemployment Effects of Carbon Tax

This subsection examines the policy impact on wages and unemployment. These examinations are informative for two reasons. First, 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, 2018). 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. Second, 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.

Table 2 presents the wage effects from the estimation of equation (1). The dependent variables are the logarithm of real wage. The average hourly wage rate of BC's employees is 18.12 Canadian dollars in the pre-policy period. Samples are restricted to all employees in this analysis.

To see the role of the CEM method and to serve as robustness checks, I estimate the wage effects with and without the matching method. While the estimate in column (1) is obtained using a simple DID approach, the estimates in columns (2) and (3) are obtained using the combination of the CEM method and the DID method. All specifications include the province and the $Year \times Month$ fixed effects. The specification in column (3) also includes dummies for gender, age, educational level, marital status, and industry.

The results in columns (1) through (3) suggest that BC's average wage level drops in the post-policy period. Using a simple DID method, the estimate in column (1) indicates that BC's wage level, on average, declines by 1.9 percent subsequent to the policy. The combination of the CEM method and the DID method enlarges the estimates. In columns (2) and (3), the estimates indicate that BC's average wage level declines by 2.7 percent. The estimates are statistically significant at one percent level. These results not only reveal the negative wage effects of carbon tax but also highlight the importance of the CEM method. The size of the estimated wage effect increases by 42 percent if I allow the matching method to make the treatment and control groups observably comparable.

I repeat the analyses by replacing $BC_j \times Post_t$ with $BC_j \times Tax_t$ and report the estimates in columns (4) through (6). The results suggest that each Canadian dollar of carbon tax decreases wages by 0.120-0.122 percent, and the CEM method again enlarges the size of the estimated wage effect associated with each dollar of carbon tax.

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. BC's unemployment rate is 5.23 percentage points in the pre-policy period.

Table 3 presents the unemployment effects from the estimation of equation (1). Three points deserve highlights. First, the results suggest that carbon tax creates job losses. The estimates in columns (1) through (3) indicate that the unemployment rate increases by 1.3-1.4 percentage points following the policy. These estimates are statistically significant at one percent level. Second, the estimates in columns

Table 2: The Wage Effects of Carbon Tax

Difference-in-Differences Analysis

Dependent Variable: ln(**Real Wage**)						
	(1)	(2)	(3)	(4)	(5)	(6)
BC×*Post*	-0.019***	-0.027***	-0.027***			
	(0.007)	(0.008)	(0.004)			
BC×*Tax*				-0.088**	-0.122***	-0.120***
				(0.036)	(0.038)	(0.015)
Adjusted R^2	0.026	0.026	0.467	0.026	0.026	0.467

Notes: Estimates in columns (1) and (4) are obtained using a simple DID method. Estimates in other columns are obtained using the CEM method and the DID method. 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 (3) and (6). 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 are 5,945,232 in columns (1) and (4), and are 5,227,156 in other columns. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: ***=1%, **=5%, *=10%.

(5) and (6) reveal that each Canadian dollar increases the unemployment rate by 0.047 percentage points. Third, the CEM method again enlarges the size of the estimates in this analysis. Allowing the CEM method to create observably similar treatment and control groups increases the size of the estimated unemployment effect by 0.1 percentage point.

It is unsurprised that the CEM method plays a more important role in correcting the selection bias in estimating the wage effect than the unemployment effect. Yip (2018) shows that BC's carbon tax has negligible effect on the labor force participation rate; hence, the policy may not change the composition of the labor force participants. In other words, the selection bias in estimating the unemployment effect is so small that the CEM method plays a rather little role in the analysis. If employers tend to lay off workers with less productivity levels and thus lower wage levels because of carbon tax, workers with lower wages will be excluded from the sample of the wage analysis. In other words, a simple DID method will underestimate the negative wage effects. Since the unemployment effect is economically and statistically significant, the selection bias could be large in the wage analysis. These findings explain why the CEM method enlarges the size of the estimated wage effects and this method is more important in correcting the bias in the estimation of the wage effect than the unemployment effect.

This subsection confirms that there was a negative labor demand shock to BC's economy subsequent to July 2008. One may cast doubt that this negative labor demand shock is attributed to the carbon tax policy. In fact, as mentioned above, the sizes of both the wage effect and the unemployment effect increase with each Canadian dollar of BC's carbon tax, providing the first evidence in this paper that this labor demand shock is related to the carbon tax policy. The next subsection will provide another evidence on the linkage between this negative labor demand shock and the carbon tax policy.

Table 3: The Unemployment Effect of Carbon Tax

Difference-in-Differences Analysis

Dependent Variable: Dummy Variable for Unemployment						
	(1)	(2)	(3)	(4)	(5)	(6)
BC×*Post*	0.013***	0.014***	0.014***			
	(0.003)	(0.003)	(0.003)			
BC×*Tax*				0.044***	0.047***	0.047***
				(0.009)	(0.009)	(0.011)
Adjusted R^2	0.006	0.006	0.031	0.006	0.006	0.031

Notes: Estimates in columns (1) and (4) are obtained using a simple DID method. Estimates in other columns are obtained using the CEM method and the DID method. 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 (3) and (6). 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 are 7,429,999 in columns (1) and (4), and are 7,412,988 in other columns. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: ***=1%, **=5%, *=10%.

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 into industries that are more energy-intensive in the pre-policy period. If the negative labor demand shock arises from the carbon tax, the wage and the unemployment 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 the carbon tax is the source of the labor demand shock, industries that are more energy-intensive should be affected more. Altogether, the negative 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 wage level happens 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. Also, if the policy effects happen to be uniform across educational categories or more pronounced among more-educated workers, one should cast doubt on the claim that the negative labor demand shock is attributable to the carbon tax policy. Second, the investigation of the heterogeneous effects provides us an opportunity to understand the distributions of the costs associated with the carbon tax policy across workers, which is revelent to policymakers redistributing tax revenues accordingly.

Table 4 presents the wage effects on high-, medium-, and low-educated employed workers in columns (1) through (3) and the unemployment effects on the respective labor force participants in columns (4) through (6). Their average wages are 23.30, 18.48, and 14.90 Canadian dollars 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.

The results uncover the heterogeneity in the policy impacts. The estimates in columns (1) through (3) indicate that the average wage of high-, medium-, and low-educated workers drops 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 a negative labor demand shock in the post-policy period. Both the negative wage effects and the unemployment effects appear regardless of educational group, suggesting that there is a fall in BC's labor demand. Second, these results provide another support that the carbon tax policy creates this negative labor demand shock. 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. Hence, 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 labor demand 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. 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 the 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 subsequent to the policy. Since the NBER Productivity Database has the information on the energy expenditure for manufacturing industries only, I restrict the sample to manufacturing employment in this analysis. To serve as a robustness check, I construct two other 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 as follows:

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

where *Real Wage* measures the real 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 ψ_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 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 5. The dependent variables are the logarithm of real hourly wages. 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 19.25 Canadian dollars in the pre-policy period. These two specifications include the province and the *Year*×*Month* fixed effects. The specification in column (2) also include 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. 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.

I repeat the analyses with the sample of each educational category. Columns (3) through (5) in Table 5 present the estimates $\hat{\beta}_1$ from equation (2). A number of points emerge from the table. First, 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 for all educational levels. Moreover, because of the policy, the wage elasticity with respect to energy intensity is larger in magnitude 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. 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 carbon tax and energy intensity is concentrated on the low-educated. The estimates suggest that 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, the heterogeneity in the wage effects across industries suggest that the policy impacts are concentrated on more energy-intensive industries, especially among less-educated workers. This heterogeneity coheres with the tax incentive of carbon tax and the distributions of human capital across industries in the pre-policy period.

This subsection uncovers the heterogeneity in the impacts across workers and industries. This het-

Table 4: The Heterogeneous Effects Across Workers

Difference-in-Differences Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
BC×*Post*	-0.005	-0.024***	-0.030***	0.007***	0.013***	0.018***
	(0.006)	(0.005)	(0.006)	(0.002)	(0.003)	(0.005)
Adjusted R^2	0.273	0.391	0.480	0.005	0.015	0.043

Notes: Dependent variables are the logarithm of real wage 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*×*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 labor force participants in columns (4)-(6). 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 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 5: The Policy Effects on the Wage Elasticity w.r.t. Energy Intensity

Difference-in-Differences Analysis

Dependent Variable: ln(Real Wage)					
	(1)	(2)	(3)	(4)	(5)
		Panel A			
ln(EI)×*BC*×*Post*	-0.038***	-0.033***	-0.025	-0.028*	-0.035**
	(0.013)	(0.012)	(0.030)	(0.016)	(0.015)
Adjusted R^2	0.171	0.369	0.196	0.324	0.382
		Panel B			
ln(EI)×*BC*×*Post*	-0.030**	-0.028***	-0.021	-0.022	-0.032***
	(0.012)	(0.010)	(0.028)	(0.015)	(0.012)
Adjusted R^2	0.171	0.370	0.196	0.324	0.382
		Panel C			
ln(EI)×*BC*×*Post*	-0.022**	-0.018**	-0.003	-0.016	-0.021**
	(0.010)	(0.007)	(0.023)	(0.012)	(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, 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 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 rest of Canada, 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%.

erogeneity provides economists and policymakers with information on the distribution of cost associated with carbon tax, serving as guidance redistributing tax revenues accordingly. Moreover, this heterogeneity re-confirms that the negative wage effect and the unemployment effect are attributed to the carbon tax policy.[10] Now, I proceed to investigate several research questions of this paper. Are wage sticky? Why does the literature document the procyclicality of real wage? If wages are not sticky, what creates cyclical unemployment? While the carbon tax increases the unemployment rate, how is unemployment created? Do unemployment fluctuations arise from increased job separation rates or decreased job finding rates? The rest of this section will answer these questions.

3.3 Are Wages Sticky?

Thus far, this section has documented that average wages decline in response to the labor demand shock. Apparently, this finding suggests that wages are not sticky.[11] 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 negative labor demand shocks, such as carbon tax, reduce average wages.

Table 6 presents the wage effects on incumbent workers and new hires from the estimation of equation (1). The dependent variables are the logarithm of real wage. 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.[12] Only those with tenure over seven years are examined for the impact on incumbent workers so that the entire sample of examinations are hired before the policy was implemented. In the analysis on 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 $Year \times Month$ 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

[10] Subsection 3.6 will provide further evidence that these effects result from the policy when the dynamic effects are explored.

[11] In the Appendix, I demonstrate that the hypothesis of a sticky wage can be rejected by the negative wage effect of negative labor demand shocks.

[12] This definition of new hires is also used in Carneiro et al. (2012).

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. Hence, I will conclude that no evidence on the negative wage effect can be found among incumbent workers.

Wages are not sticky among new hires. The average hourly wage rate of BC's new hires is 13.87 Canadian dollars 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 also heterogeneous across educational categories. On average, high-, medium-, and low-educated new hires receive 18.68, 14.62, and 11.52 Canadian dollars 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. On average, the medium- and low-educated new hires receive 4.9 and 6.0 lower wage per hour. These estimates are considered statistically significant at one percent level.

This subsection discovers two important findings. First, it uncovers the channel through carbon tax reduces wages: wage cuts operate through a hiring process. Who bears the cost of carbon tax in the labor market? Not incumbent workers. Do all new hires suffer from wage cuts? Not high-educated new hires. This finding is important because it provides guidance as to how social benefit should be distributed accordingly.

Second, the results of this subsection tine-tune our traditional thought about the stickiness of wages. Are wages sticky? Yes and No. Wages are found sticky among incumbent workers as to why carbon tax, as a cost shock or a negative labor demand shock, creates job losses. This finding supports the traditional thought that stickiness in wage creates cyclical unemployment. However, wages are not sticky among new hires, explaining why average wages are found procyclical in the literature (Bowlus et al., 2002; Carneiro et al., 2012; Martins et al., 2012; Elsby et al., 2016). If the wages of new hires were also sticky, the unemployment effect would have been larger. This is the reason as to why prior literature renders stickiness in real hiring wages to modify the DMP model so as to generate larger unemployment fluctuations and thus to resolve the unemployment volatility puzzle (Hall, 2005b; Hall and Milgrom, 2008; Gertler and Trigari, 2009; Kennan, 2010; Shimer, 2010). Nevertheless, this subsection provides the causal effect of a negative labor demand shock on hiring wages, and this wage effect is substantial. These causal estimates, complementary to Martins et al. (2012) and in line with the view of Pissarides (2009), reveal that hiring wages are not sticky and hence the stickiness in hiring wages could not be the solution to this puzzle.

The results in columns (3)-(5) indicate that the adverse wage effect appears regardless of educational category. The wage effect on high-educated workers is at best weak, amounting to a 0.8 percent decrease. Moreover, this estimate (Column (3) in Panel A) is statistically insignificant at any conventional level. The adverse wage effects on medium- and low-educated are more pronounced, reaching 2.4 and 2.8 percent, respectively. The estimates in columns (4) and (5) are significant at one percent level. An

Table 6: Impacts on Incumbent Workers and New Hires

Difference-in-Differences Analysis

Dependent Variable: ln(Real Wage)					
	(1)	(2)	(3)	(4)	(5)
Sample: Incumbent Workers					
BC×*Post*	-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^2	0.034	0.330	0.156	0.233	0.300
Sample: New Hires					
BC×*Post*	-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^2	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. 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%.

increase in each Canadian dollar of the carbon tax reduces the average wage levels of the high-, medium-, and low-educated employees by 0.047, 0.114, and 0.121 percent, respectively. They are 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 but not by stricter criteria.

Second, the adverse effects are more pronounced for less-educated workers. Both papers indicate that the adverse effects are negligible for the high-educated workers. Meanwhile, our results reflect that these effects are more remarkable among workers with less-educational level. Both reveal 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 concentrated on the less-educated. The evidence of these two papers raises concerns on the labor market analogues of environmental justice.

3.4 Impacts on Job Transition Rates

This subsection examines the impacts of carbon tax on job transition rates. This examination is informative for two reasons. First, it uncovers the channels through which carbon tax increases unemployment. Second, it answers an old yet unsettled question: do unemployment fluctuations arise from variations in job finding rates or job separation rates?

This examination requires information on job transition rates. Nevertheless, the Canadian LFS data is not longitudinal and does not provide any information on these two rates. I therefore estimate the

monthly job finding rates and the monthly job separation rates by province 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 province j in time t.[13]

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, the ROC serves as a control group. β_1 again is of our primary interest; 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. Both the estimates are statistically significant at one percent level. These findings suggest that labor demand shocks, like the carbon tax policy, decrease a job finding rate by 0.093 and increases a job separation rate by 0.004.

To understand the policy impacts of the two rates, I measure these impacts in terms of unemployment. According to 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 $\triangle u_x \equiv \frac{(x_t+\tau_x)}{f_t+(x_t+\tau_x)} - \frac{x_t}{f_t+x_t}$, where τ_x is the causal effect of the policy on a job separation rate and equals 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, $\triangle u_x = 0.5636$. 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 $\triangle u_f \equiv \frac{x_t}{(f_t+\tau_f)+x_t} - \frac{x_t}{f_t+x_t}$, where τ_f is the causal effect of the policy on a job finding rate. In our case, τ_f equals -0.093. Hence, $\triangle u_f = 0.7386$: carbon tax increases an unemployment rate by 0.7386 percentage point through the decreased job separation rate. We can therefore conclude that about over 40 percent of the unemployment effects of BC's carbon tax policy is attributed to the increased separation rate, and over half of the effect arises from the decreased job finding rate.

The results reveal two pieces of important information. First, it uncovers the channel through which environmental policies increase 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 job transition rates. This subsection adds to this literature by discovering the processes by which carbon tax increases unemployment: it makes 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 labor demand shocks, and the variations in both rates explain a substantial amount of unemployment fluctuations.

[13] I estimate a job finding rate f_{jt} in province j in month t by solving $e^{f_{jt}} = (u_{jt+1} - u^s_{jt+1})/u_{jt}$ (i.e., equation (4) in Shimer (2012)), where u_{jt} is unemployment in province j in month t and u^s_{jt+1} is unemployment with spell within a month in month $t+1$. Similarly, I solve $u_{jt+1} = x_{jt}l_{jt}(1 - e^{-f_{jt}-x_{jt}})/(f_{jt}+x_{jt}) + e^{-f_{jt}-x_{jt}}u_{ft}$ (i.e., equation (5) in Shimer (2012)) to obtain a job separation rate x_{jt}, where l_{jt} is the measure of labor force participants.

3.5 The Number or the Duration of Unemployment Spells?

This subsection answers whether the unemployment effect is mainly driven by the short- or the long-term unemployment. Answering this question helps us understand how carbon tax shapes the structure of unemployment. Moreover, it provides important information to policymakers on the design on social benefit accordingly. If carbon tax happens to lengthen unemployment spells, the duration of unemployment benefit could be extended accordingly. However, if the extent to which carbon tax 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 the short- and the 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 7 presents the unemployment effects from the estimation of equation (1). Samples are restricted to all labor force participants in columns (1) and (2) and are restricted to high-, medium-, and low-educated labor force participants 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 carbon tax 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 unemployments increase by 1.1 and 0.3 percentage points subsequent to the policy. According to subsection 3.1, carbon tax increase an 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, 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 short- and long-term unemployment, providing guidance on the design of social benefit and helping economists understand the functioning of the labor market. While the previous section suggests that the decreased job finding rate explains more of the unemployment effect than the increased separation rate, one may fathom that carbon tax will lead to a prolonged spell of unemployment. Surprisingly, this subsection shows that carbon tax increases mainly the number of unemployment, not the duration of unemployment.

This finding challenges the traditional practice of the extension of unemployment benefit period. In recessions, benefit periods are usually extended. One of the intuitions is that recessions 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

Table 7: Impacts on Short- and Long-Term Unemployment

Difference-in-Differences Analysis

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Dummy Variable for Short-Term Unemployment					
BC×*Post*	0.011***	0.011***	0.005***	0.010***	0.015***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.005)
Adjusted R^2	0.004	0.031	0.005	0.014	0.041
Dependent Variable: Dummy Variable for Long-Term Unemployment					
BC×*Post*	0.003***	0.003***	0.001*	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Adjusted R^2	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. 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%.

of negative labor demand shocks on long-term unemployment is empirically challenging. It is unclear whether the prolonged period of unemployment is attributed to negative labor demand shocks or the extensions of benefit periods. The findings in this subsection therefore contribute to the literature by identifying the impacts of negative labor demand shocks on long-term unemployment, free from the effects of the extension of the unemployment benefits. This subsection reveals that the impact of labor demand shocks on the duration of unemployment is weak. If the extent to which negative labor 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 Dynamics of the Effects of Carbon Tax

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 carbon tax affects average real wages and unemployment rates 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 parallel trends are likely to follow 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

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

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

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.[14] 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 effects documented in subsections 3.1 through 3.3, several features of the estimates are expected. First, if the common trend assumption is satisfied, the trends in wage rates between BC and the ROC are expected to be parallel in the pre-policy period. Therefore, one should expect that the difference in the average hourly wage rate between BC and the control provinces prior to the reference year is close to the difference in the reference year. In other words, all the $\hat{\beta}_t$ prior to the reference year should not be statistically different from zero.

Second, if the wages are sticky among incumbent workers, it is expected that the corresponding $\hat{\beta}_t$ are statistically indifferent from zero immediately following the policy. Third, if carbon tax reduces wages of new hires, the corresponding $\hat{\beta}_t$ are expected to be negative subsequent to the policy. Fourth, since the carbon tax rate increases by CAD$5 annually until 2012, one should expect that the sizes of the wage effect on new hires increase gradually in the first few years. In other words, the estimates $\hat{\beta}_t$ are expected to drop gradually in the first few years.

Figures 1 and 2 display yearly estimates $\hat{\beta}_t$, where the samples are restricted to incumbent employees and new hires, respectively. The first dots represent the estimates $\hat{\beta}_t$ of the period July 2005-June 2006, which capture the average differences in hourly wage rates between BC and the control provinces relative to the differences in the reference year. The vertical line represents the first month of the carbon tax policy (i.e., July 2008).

A number of points emerge from these figures. First, parallel wage trends can be found in Figures 1 and 2. Most of the estimates prior to the policy are statistically insignificant.[15] The trends shown in Figures 1 and 2 suggest that the common wage trend can be found among new hires of each educational group and incumbent employees for most educational categories.

Second, Figure 1 suggests that no evidence on the wage effect among incumbent employees can be

[14] 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 2007 are used as the pre-policy period.

[15] The only exception is the sample of the high-educated incumbent employees in Figure 1. In fact, most the estimates lie above zero in this figure, meaning that their average level in the reference year (relative to the control provinces) is one of the lowest during those ten years. In most of other years, the wage trend between BC and the ROC is steady over the examination period.

found. The estimates for high-educated employees are all statistically insignificant in the first seven years of the policy, providing no evidence on the policy effect on their wage. In subsection 3.1, the estimate suggests 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. 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 among low-educated incumbent workers. Hence, we can conclude that no wage effect among incumbent employees can be found regardless of educational level.

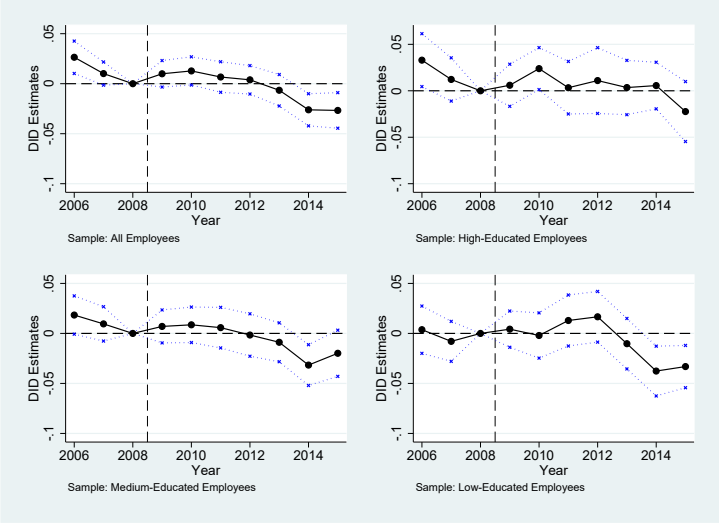
Third, the magnitudes of the adverse wage effect weakly increase in the first few years among the medium- and the low-educated new hires. Figure 2 suggest that the reduction in the average wage rate arises from the medium- and the low-educated, not the high-educated workers. Furthermore, the absolute values of these estimates grow gradually in the first few years, confirming that the adverse wage effects are more pronounced in the years of higher carbon tax rates. Together with Figure 1, one can conclude that such a reduction takes place through a hiring process, not through incumbent employees.

Fourth, the trends in the average wage of incumbent workers are similar over the entire period of examination. In Figure 1, most estimates are close to zero, revealing that the wage trends are parallel between BC and the control group when the samples are unaffected much by the policy. This similar pattern can also be seen from the sample of high-educated new hires in Figure 2. These findings provide strong evidence that if the medium- and the low-educated new hires were unaffected by BC's carbon tax, their wage trends would have also been parallel to the ones in the ROC.

These figures shed lights on the process by which labor demand shocks reduce wages. The literature (Barro, 1977; Fischer, 1977; Thomas and Worrall, 1988) suggests that wages are not easily adjusted under labor contract and that wages are adjusted once these contracts are renegotiated, explaining the sticky wage theory. Nevertheless, Figure 1 suggests that wages are completely rigid among incumbent employees at least in the first five years of the policy regardless of educational level. Unless most contracts last over five years, most of the incumbent employees should experience at least once contract renewals during the first five years of BC's carbon tax policy. If labor contract does explain the stickiness, one should expect that their average wage falls after contract renewals. Nevertheless, their average wages remain unchanged over several years. These findings cast doubt that labor contract explains the stickiness and that wage adjustment operates through the renegotiation of contract renewals. Instead, the findings support that an average real wage reduces through new contracts during hiring processes.

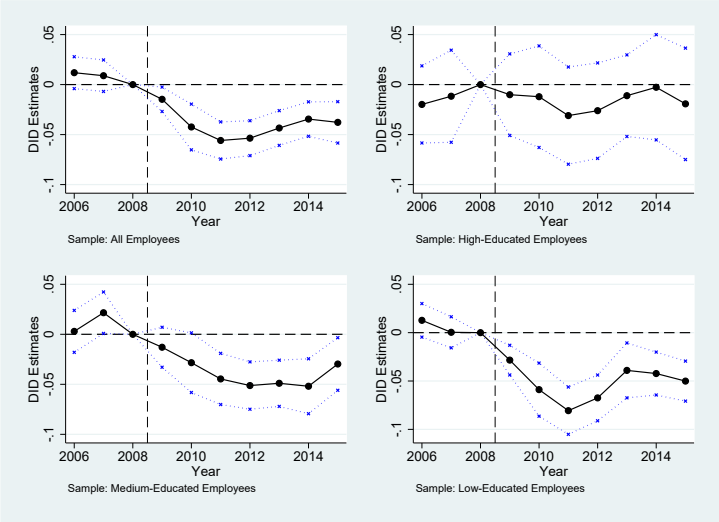
Next, Figures 3, 4, and 5 demonstrate the dynamics of the overall unemployment effect, the short-term unemployment effect, and the long-term unemployment effect. The dynamics of these effects are similar regardless of educational level. These three figures show that the estimates are all close to zero in the first three years, suggesting that the unemployment trends between BC and the ROC are parallel for all educational groups. The estimates subsequent to the policy first rise and then decline gradually. As discussed in subsection 3.1, the unemployment effect increases with the carbon tax rate. As the rate increased gradually until July 1, 2012 and stayed at the same level afterward, the sizes of these unemployment effects increased in the first few years. Moreover, these effects are more pronounced

Figure 1: The Dynamic Wage Effects on Incumbent Workers



Notes: The dependent variables are ln(real 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.

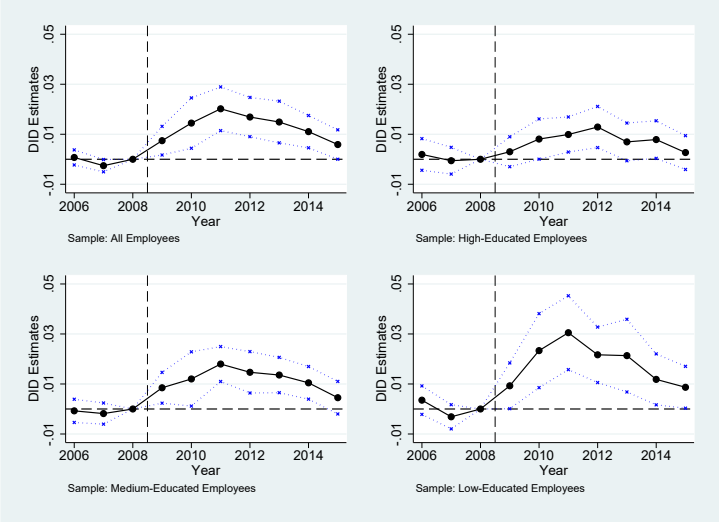
Figure 2: The Dynamic Wage Effects on New Hires



Notes: The dependent variables are ln(real 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.

among less-educated workers throughout the entire period of examination. At their peaks, the effects for the medium- and the low-educated double and triple the effect of the high-educated, respectively. This finding is consistent with the results above: the policy taxes away jobs disproportionately, more likely from the less-educated. The analyses on these dynamic unemployment effects reveal that the heterogeneity in the unemployment effect appears each year.

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.

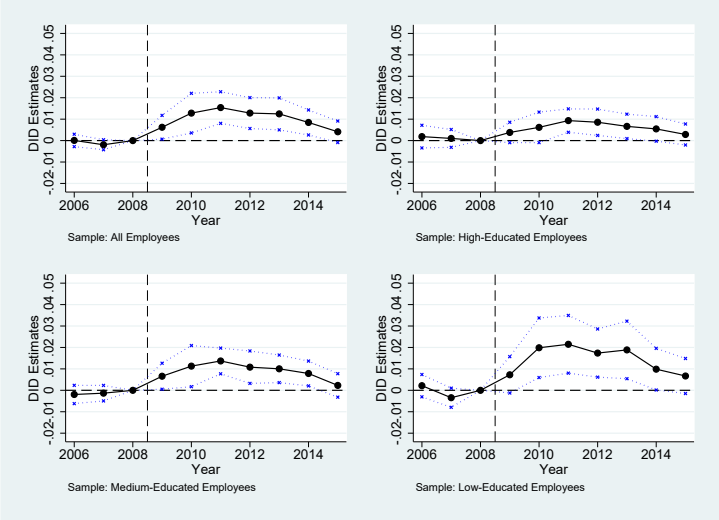
Furthermore, I estimate the dynamic effects on the transitions rates and plot the estimates in Figure 6. Obviously, the estimates are all close to zero in the pre-policy period. These estimates reveal the reasons behind the parallel unemployment trend between BC and the ROC. Since BC and the ROC share identical trends in both the job finding rate and the job separation rate in the pre-policy period, it is unsurprised that their unemployment trends are parallel. Also, the sharp increases in the job separation rate and the sharp decrease in the job finding rate immediately following the implementation of the carbon tax policy confirm that it is the carbon tax that induces the changes in these transition rates by which this policy increases unemployment.

3.7 Identifying Assumptions

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

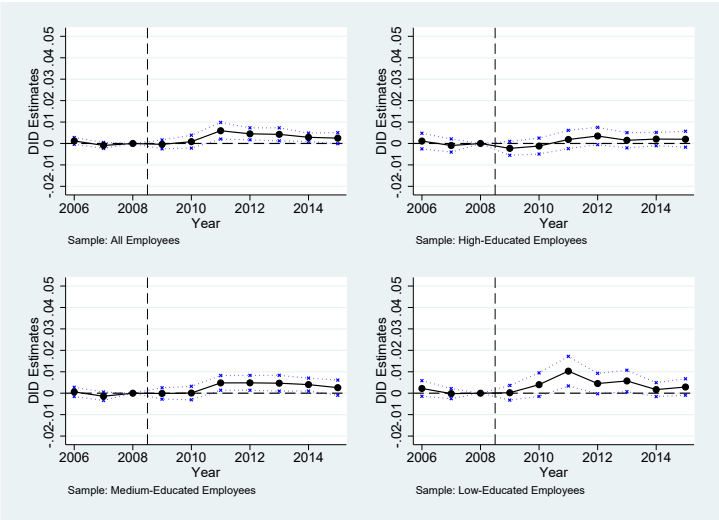
Assessing the Anticipatory Effect. The analyses of this paper assume no anticipatory effects. If the public anticipates and reacts to BC's carbon tax policy, the estimates will end up capturing only the

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



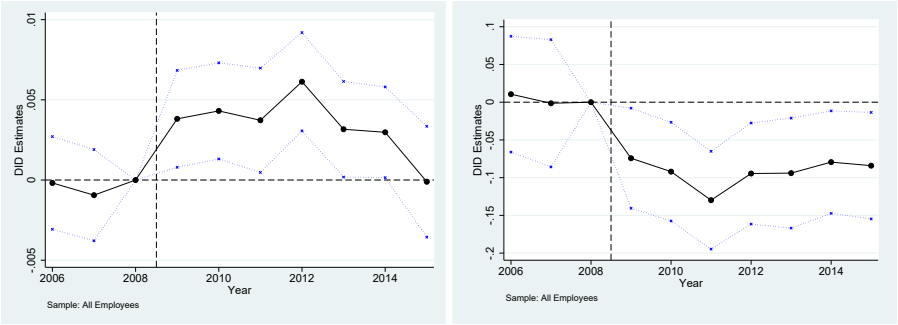
Notes: The dependent variables are dummy variables for short-term unemployment. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents 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.

effects of the shocks in the post-policy period, not the entire effects of the shocks. 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 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 levels 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 levels 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 because of the recession. 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:

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

2008_t equals one if a respondent is observed in 2008, and zero otherwise. $\textit{March-June}_t$ 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-

Table 8: The Anticipatory Effects of BC's Carbon Tax

Difference-in-Differences Analysis

	(1)	(2)
Dependent Variable	ln*(Real Wage)*	*Unemployed*
BC×*2008*×*Mar-Jun*	0.001	0.000
	(0.009)	(0.003)
BC×*2008*	-0.007	0.004
	(0.008)	(0.003)
BC×*Mar-Jun*	-0.006	0.002
	(0.006)	(0.003)
2008×*Mar-Jun*	-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 labor force participants 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 rest of Canada 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%.

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 8 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 rate 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 parallel trends in the wage and unemployment rate 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 no wage effect and a small increase in the unemployment rate in the first half of 2008. These increases are likely attributed to the recession.[16] 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 evidence on any anticipatory effect between the announcement and the implementation dates of the policy.

[16]The NBER's Business Cycle Dating Committee dates the beginning of the recession to be December 2007.

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 job search theory. While Subsection 3.6 documents that the trends in the two rates are parallel prior to the policy, it is unsurprised that 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 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 labor demand 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 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 labor demand 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 $5 each year until 2012, the magnitudes of the wage and the unemployment effects (if exist) are expected to increase 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 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 Business 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 average real hourly wage rates decrease by 0.4 percent, and the unemployment rate increases by 0.4 percent in Canada, suggesting that the recession does cause labor market consequences. Nevertheless, 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 is more severe in BC than in the ROC and that the documented labor market consequences are driven from the recession but not the carbon tax policy.

4 Discussion

This section discusses what happens in the labor market because of BC's carbon tax and what we learn from the carbon tax.

4.1 What Happens in the Labor Market?

I begin with the overall picture of carbon tax, as a cost shock and thus a negative labor demand shock, in the labor market. Carbon tax increases cost and thus depresses labor demand. Wages are found completely rigid among incumbent workers over at least five years. In other words, the real wages of incumbent employees are not adjusted in response to the negative labor demand shock. Consequently, the shock increases the job separation rate; more employees are laid off.

The shock creates unemployment. The increased cost provides less incentive for firms to hire new workers, thereby reducing the job finding rate. Both the increased job separation rate and the decreased job finding rate increase the stock of unemployment and thus the unemployment rate by 1.4 percentage points. About 79 percent of this unemployment effect is driven by the short-term unemployment, and the rest is attributed to the long-term unemployment. In other words, the shock slightly lengthens the duration of unemployment spells and significantly increases the number of unemployment spells. Because of the shock, the average real wages of new contracts drop substantially, amounting to a 5.1

percent decrease. Because of this decrease, the real wage, on average, drops by 2.7 percent and is thus procyclical.

The impacts are heterogenous across industries and workers. It is expected that the increase in cost is more pronounced among industries of higher energy intensity levels. Hence, the wage and the unemployment effects are concentrated on manufacturing industries of higher energy intensity levels, especially among less-educated workers. The shock increases the unemployment rates of the high-, the medium-, and the low-educated by 0.7, 1.3, and 1.8 percentage points, respectively. The heterogeneity across workers arises partly because Yip (2018) shows that less-educated workers tend to work in manufacturing industries of higher energy intensity levels.

The impacts are dynamic. Both the wage and the unemployment effects appear immediately following the shock. Since the carbon tax rate increases annually, the cost is expected to rise in the first few years. As a consequence, the wage effect on new hires grows over time while the average real wage of incumbent employees remains completely rigid. Meanwhile, the unemployment effect also rises gradually in the first few years. The two effects last seven years, the entire post-policy period of examination in my analyzes, and are expected to stay in the labor market after the seventh year of the shock.

4.2 What Do We Learn from Carbon Tax?

This paper gives five lessons in Labor Economics and ten lessons in Environmental Economics.

Five Lessons in Labor Economics. First, this paper is the first to utilize a policy shock of carbon tax to understand the functioning of the labor market in slumps. Often, the arrival date of recession is not definite. Moreover, recessions are pervasive throughout the country: almost every single local labor market is affected. Furthermore, recessions usually arrive with the extension of unemployment benefit periods, which is shown to causally affect wages (Caliendo et al., 2013; Schmieder et al., 2016; Nekoei and Weber, 2017), unemployment rates (Elsby et al., 2010), and unemployment durations (Lalive, 2008; Caliendo et al., 2013). These factors make the identification of the causal effect of negative labor demand shock empirically challenging.

I utilize carbon tax to solve this identification problem. As mentioned above, carbon tax, being a cost shock, depresses labor demand, creating a negative labor demand shock. The arrival date and the stringency of this shock is definite: the shock arrives at the implementation date of the policy and becomes more severe in the first five years. Carbon tax in one local labor market does not imply that the shock will affect others, providing control labor markets that are likely unaffected by the shock. The proposed solution of this paper opens up potential directions for future research. A similar method can be employed to identify the causal effects of negative labor demand shocks on firm decisions (Fort et al., 2013; Halling et al., 2016) and household decisions on labor supply (e.g. the added worker effect (Lundberg, 1985; Stephens, 2002; Blundell et al., 2016)) in slumps.

Second, this paper reconciles the sticky wage theory with recent documentations of the procyclicality of the average real wage in the literature. Traditional thought holds that wage stickiness is the major cause of cyclical unemployment (Keynes, 1936; Blanchard and Fischer, 1989). An extensive lit-

erature documents the procyclicality of the average real wage (Bowlus et al., 2002; Carneiro et al., 2012; Martins et al., 2012; Haefke et al., 2013; Elsby et al., 2016). While most of the existing works estimate correlations, this paper documents the casual effect of a negative labor demand shock on real wages. Apparently, the documentation casts doubt on the traditional thought: if wages are not sticky, what creates cyclical unemployment? I find that the average real wage of incumbent workers is completely rigid as to why the job separation rate increases when the negative labor demand shock arrives. Unemployment fluctuations arise partly because of this increased job separation rate. Meanwhile, the average real wage of new hires is found to drop substantially, explaining why the average real wage is found procyclical in the literature.

These findings are important in understanding the unemployment volatility puzzle. Shimer (2005) and Costain and Reiter (2008) find that the DMP model generates unemployment fluctuations that are significantly less than what we observe from the data, known as the unemployment volatility puzzle. To resolve this unemployment volatility puzzle, theorists (Hall, 2005b; Hall and Milgrom, 2008; Gertler and Trigari, 2009; Kennan, 2010; Shimer, 2010) suggest stickiness in real hiring wages to modify the DMP model so as to generate larger unemployment fluctuations. However, the causal estimates of this paper, complementary to Martins et al. (2012) and Haefke et al. (2013) and in line with the view of Pissarides (2009), reveal that the stickiness in hiring wages could not be the solution to this puzzle.

Third, the conventional wisdom about the mechanism of wage stickiness is fine-tuned. The sticky wage theory posits that wages are sticky 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, casting doubt that labor contracts explain the wage stickiness and that wage adjustments operate through contract renewals. Instead, I provide evidence that wages are adjusted through new contracts during hiring processes, modifying the sticky wage theory.

Fourth, the findings of this paper uncover the processes through which negative labor demand shocks increase unemployment (Hall, 2005a,c; Davis et al., 2006; Fujita and Ramey, 2009; Elsby et al., 2009; Shimer, 2012; Elsby et al., 2013). These shocks increase the job separation rate and decrease the job finding rate so as to increase unemployment. Over 40 percent of the unemployment effect is attributable to the increased job separation rate, and over half of 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.

Fifth, short-term unemployment is the major composition of cyclical unemployment. I find that about 79 percent of the unemployment effect of the labor demand shock is attributable to the short-term unemployment, revealing that labor demand 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 labor demand shocks lengthen unemployment spells is limited, why are the entitlement periods of unemployment benefits extended substantially in slumps? 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 labor demand shocks or the extensions of benefit periods? This paper suggests the benefit periods should be indexed to the long-term unemployment rate, as opposed to the common practice—the unemployment rate.

Ten Lessons in Environmental Economics. First, this paper adds to the literature that investigates the impacts of environmental policies on employment (Greenstone, 2002; Walker, 2011; Kahn and Mansur, 2013; Curtis, 2017; Yip, 2018). While an earlier empirical literature on environmental policies focuses mainly on manufacturing employment, these policies may shift employment from manufacturing to other sectors (Walker, 2013). As noted by Hafstead and Williams (2018), we know little of their overall effects in the labor market, especially among empirical studies. My work provides the estimates of the unemployment effect in the labor market: each Canadian dollar of carbon tax increases the unemployment rate by 0.047 percentage points, revealing that environmental policies create job losses even though sectoral reallocation is taken into account.

Second, this paper uncovers the mechanisms through which environmental policies create job losses. It is the first paper to document the impacts of environmental policies on two job transition rates. Carbon tax increases unemployment through the increased job separation rate and the decreased job finding rate. Many may believe that the impacts on these two rates are unsurprised: it is natural to increase job separate rates and decrease job finding rate to increase unemployment. Nevertheless, Brochu and Green (2013) and Dube et al. (2016) estimate the causal effects of minimum wages on the two rates in Canada and the United States. They find that minimum wages reduce the two rates even though minimum wages could increase unemployment. My findings hence call for the investigations of the relationship between environmental policies and the two job transition rates, beyond unemployment, so as to fully understand the impacts and the costs associated with environmental policies.

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 unsurprised that none conducts the decomposition analysis. This paper finds that the unemployment effect is mostly attributable to the short-term unemployment, suggesting that 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. 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 employment, it is unclear whether the earnings losses arise from the reduction in returns to labor, measured by real 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 environmental tax on returns to labor: each Canadian dollar of carbon tax decreases wages by 0.120-0.122 percent.

Fifth, the paper reveals the process by which environmental policies depress wages. Carbon tax does not directly reduce the average wages of incumbent employees; instead, this policy reduces wages through hiring processes. In essence, this process is similar to the documentation in Walker (2013) and Curtis (2017). One of the explanation of this wage-cutting process is that workers lose industry- and firm-specific human capital 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 sticky after the usual length of contracts. This finding provides information to researches that carefully calibrate the staggered-wage model to examine the unemployment effects of environmental regulation (e.g. Hafstead and Williams (2018)).

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, 2018). 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 labor demand shocks. The results of this paper provide no evidence for or against the weak Porter hypothesis. Nevertheless, this paper does tell 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 tax. The literature on the unemployment effect of revenue-neutral environmental tax suggests that the unemployment rate could go down (Bovenberg and van der Ploeg, 1998; Wagner, 2005; Sun and Yip, 2018) 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 tax on the wage and the unemployment effects. The findings of this paper suggest that environmental tax, even though revenue-neutral, decreases real wages and increases unemployment.

A simple prediction on the overall unemployment effect from this literature may hide important messages of these policies. My findings support Hafstead and Williams (2018) that overall, the unemployment effect, about a 1.4 percentage point increase, is small. Meanwhile, this paper reveals that the effect could be large among low-educated workers: their unemployment rate could increase by three percentage points, their real wage 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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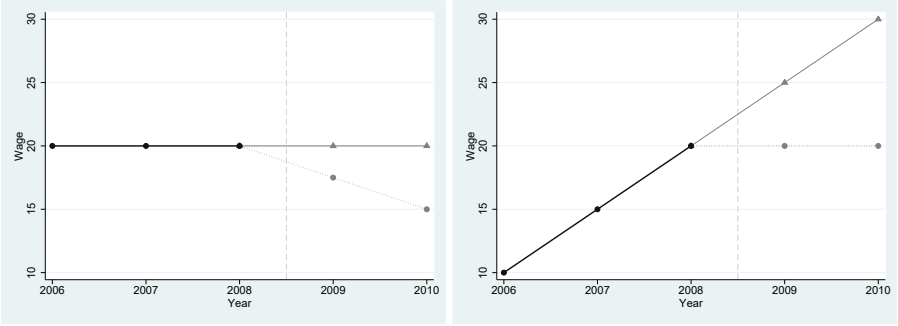
5 Appendix

5.1 The Definition of Stickiness

This section redefines stickiness 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 7 presents the hourly wage rates of the first (left figure) and the second (right figure) observations. The first individual receives $20 an hour during 2006-2008, as shown in the left figure. In the absence of any labor demand shock, such as 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 7: The Demonstration of Wage Stickiness



Notes: The left (right) figure demonstrates the measure of wage stickiness 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.

The researcher measures wage stickiness using the 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 *Stickiness*.[17] Hence, if the wage level equals $20 in 2009

[17]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.

and 2010, *Stickiness* will all be one in these years. The observation with *Stickiness* equal to one indicates that wages are completely rigid. Traditional thought holds that cyclical unemployment arises from this stickiness (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. In this case, the shock creates no unemployment. If wages lie between triangles and dots, wages are said to be partially rigid, and unemployment increases.

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 pays the individual $20. If the researcher defines and measures wage stickiness in the same way as before, *Stickiness* equals one in 2009 and 2010 because the wage levels are all identical during 2008-2010. Nevertheless, this "stickiness" reflects complete wage flexibility and creates no unemployment.

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

What is wrong with this definition? Wages are said to be sticky when they are sticky to a reference wage level. According to the common practice as in above, the wage level in the previous year serves as this 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 fails to define stickiness properly and thus fails to help *Stickiness* measure wage stickiness.

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 stickiness. 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, *Stickiness* can identify wage stickiness 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, *Stickiness* fails to do its job. Hence, to define and to measure stickiness, we are required to identify the reference level that would otherwise be realized in the absence of shocks. This definition and measure of stickiness can generally be applied to the stickiness in wage, price, and others.

The identification of this reference level is, however, empirically challenging. This 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 iden-

tification problem. Longitudinal data may provide rich information on the wage history of individuals but never helps identify the 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.

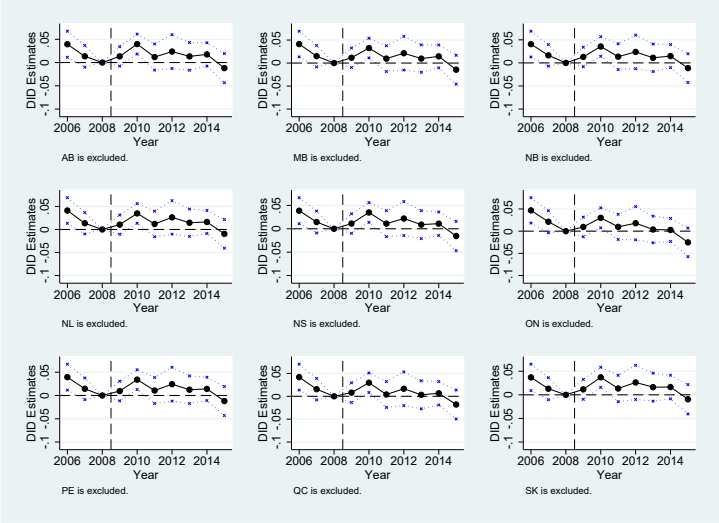
The sticky wage theory can therefore be tested as follows. If wages are sticky, the realized wages of the affected workers in the presence of a negative labor demand shock should be equal to the corresponding counterfactual reference wage levels. In other words, the sticky wage theory can be tested just as evaluating the wage consequence of a negative labor demand shock. If the sticky wage theory holds, there will be no wage effect of a negative labor demand shock. 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.

Testing the sticky wage theory is not straightforward. First, one is required to ensure the existence of a negative labor demand shock not through the wage effect. The rejection of the null hypothesis concludes nothing about the validity of the sticky wage theory in the absence of a negative labor demand shock. If the negative wage effect is the only way to confirm the existence of the shock, the null hypothesis to test the sticky wage theory will always be rejected. Hence, the sticky wage theory can only tested along with the confirmation of the existence of negative labor demand shocks.

Second, as mentioned in the introduction, it is difficult to identify the causal effects of negative labor demand shocks during recessions on labor market consequences. One of the reasons is that it is challenging to find the labor market and thus the control workers that are unlikely affected 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 revenue-neutral carbon tax in British Columbia to test the sticky wage theory and to identify the causal effects of negative labor demand shocks on other labor market consequences. The details of BC's carbon tax policy and a potential control group are discussed in Section 2.

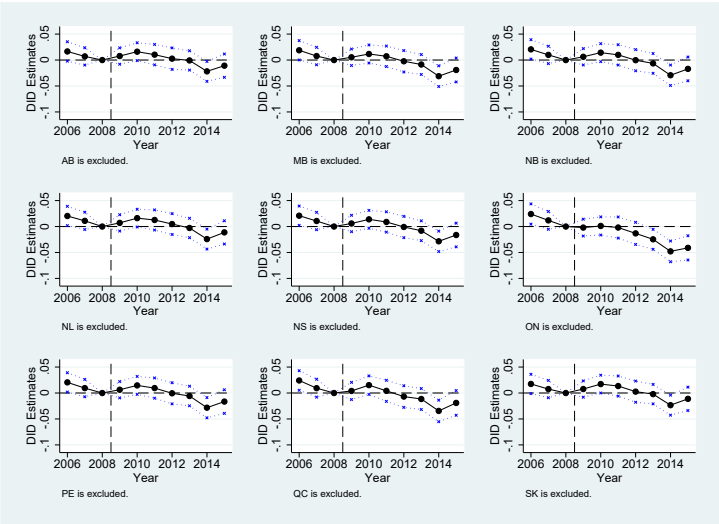
5.2 Other Figures

Figure 8: The Dynamic Wage Effects on High-Educated Incumbent Workers



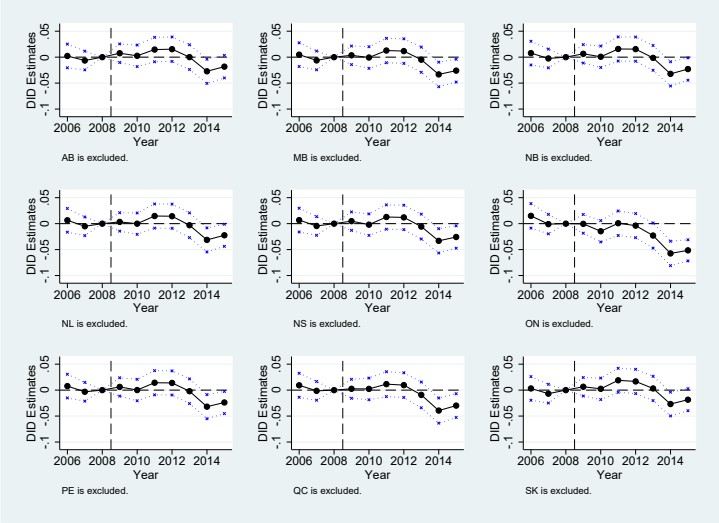
Notes: The dependent variables are ln(real wage). Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the 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 9: The Dynamic Wage Effects on Medium-Educated Incumbent Workers



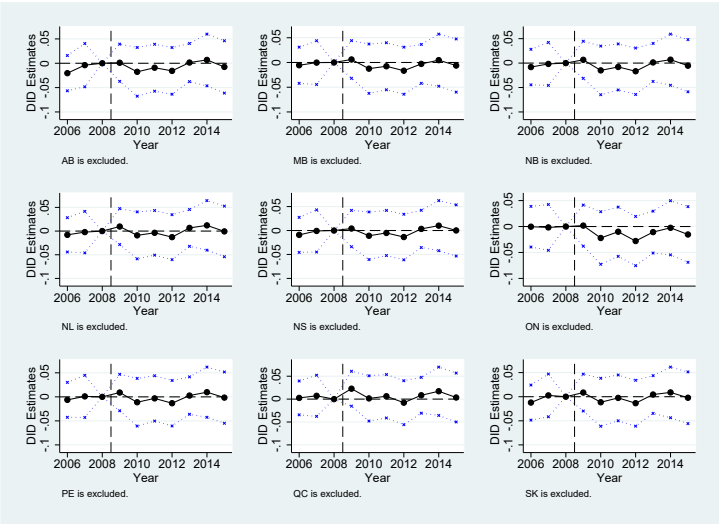
Notes: The dependent variables are ln(real wage). Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the 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 10: The Dynamic Wage Effects on Low-Educated Incumbent Workers



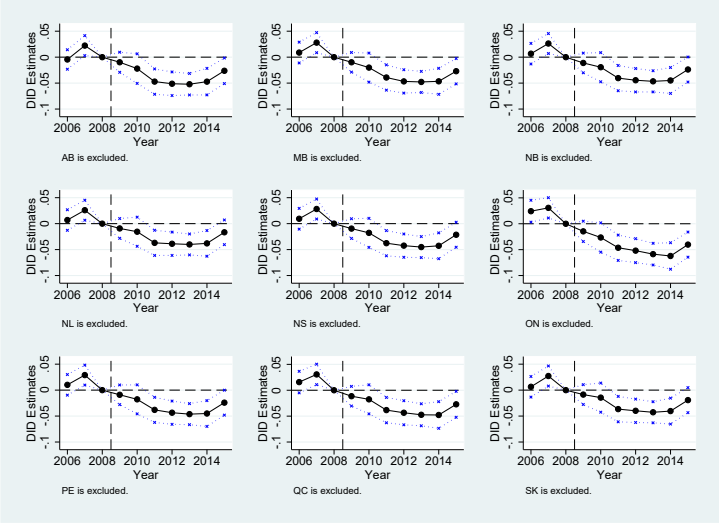
Notes: The dependent variables are ln(real wage). Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the 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 11: The Dynamic Wage Effects on High-Educated New Hires



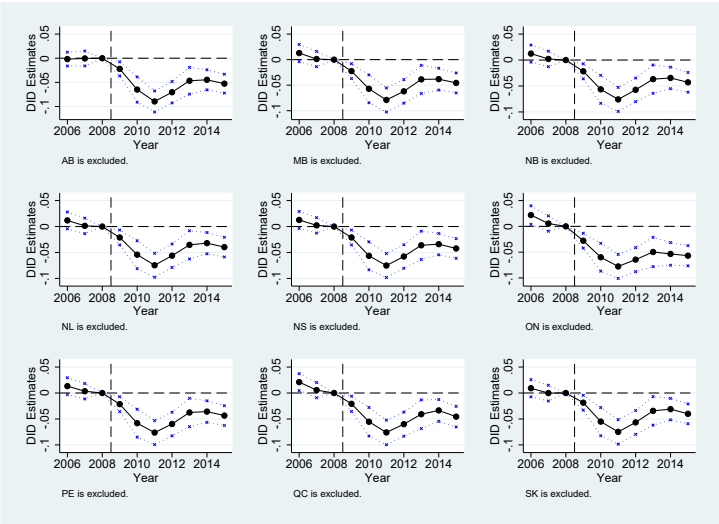
Notes: The dependent variables are ln(real wage). Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the 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 12: The Dynamic Wage Effects on Medium-Educated New Hires



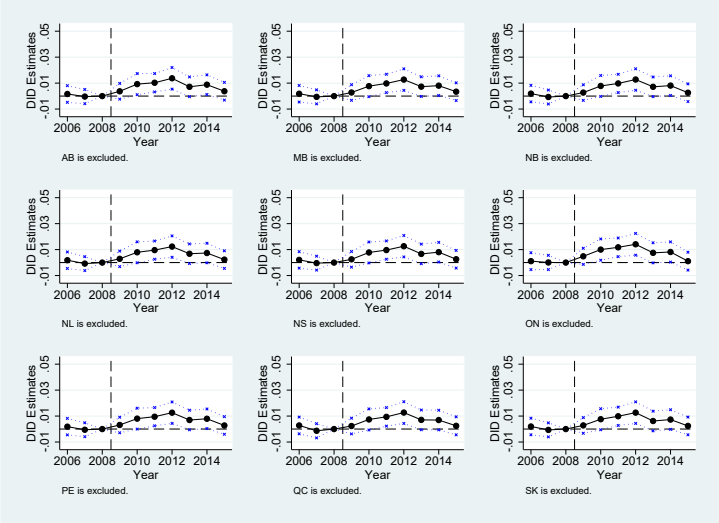
Notes: The dependent variables are ln(real wage). Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the 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 13: The Dynamic Wage Effects on Low-Educated New Hires



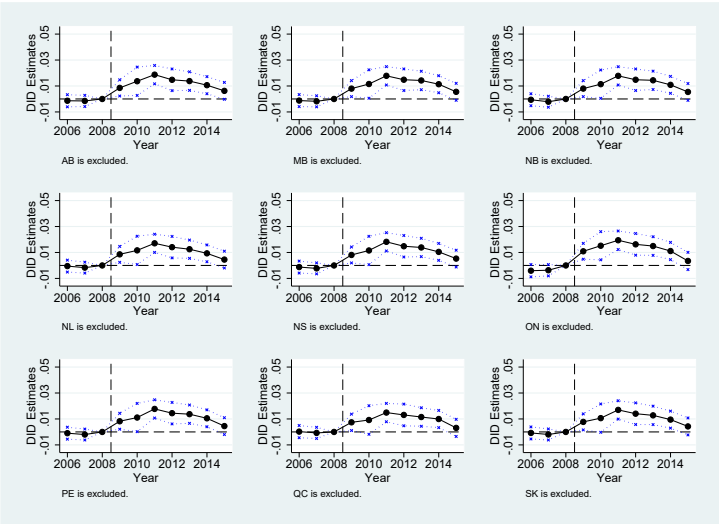
Notes: The dependent variables are ln(real wage). Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the 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 14: The Dynamic Unemployment Effects on High-Educated Workers



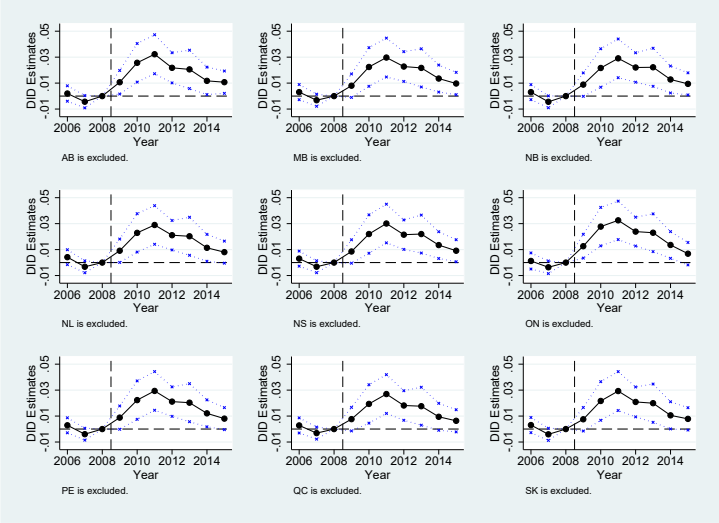
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period 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 15: The Dynamic Unemployment Effects on Medium-Educated Workers



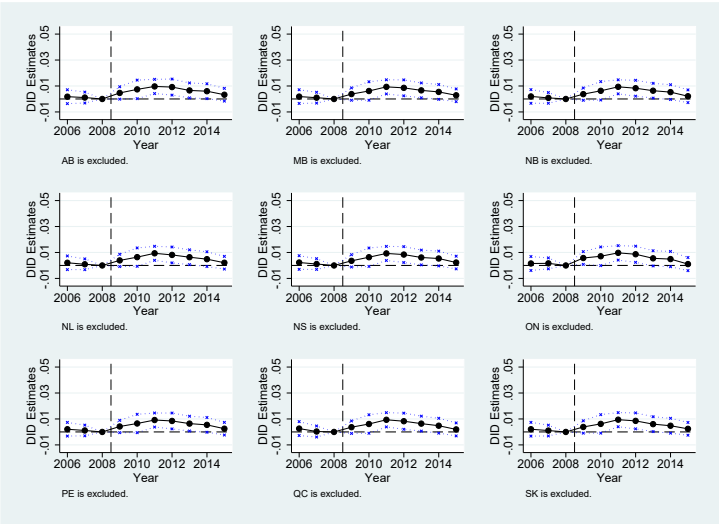
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period 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 16: The Dynamic Unemployment Effects on Low-Educated Workers



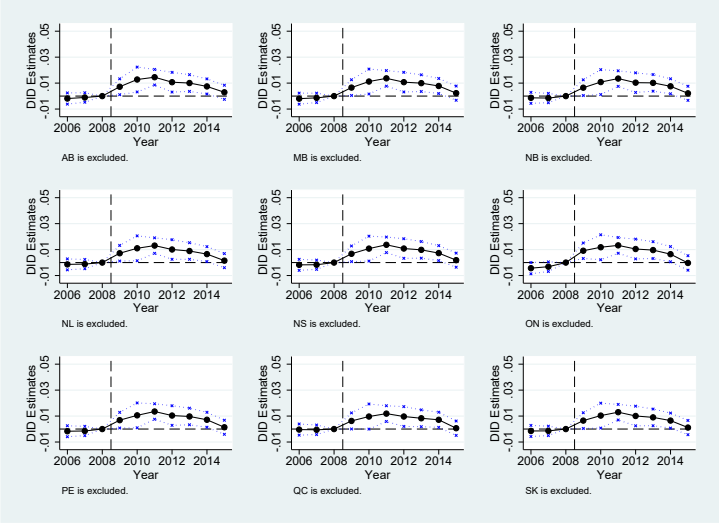
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period 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 17: The Dynamic Short-Term Unemployment Effects on High-Educated Workers



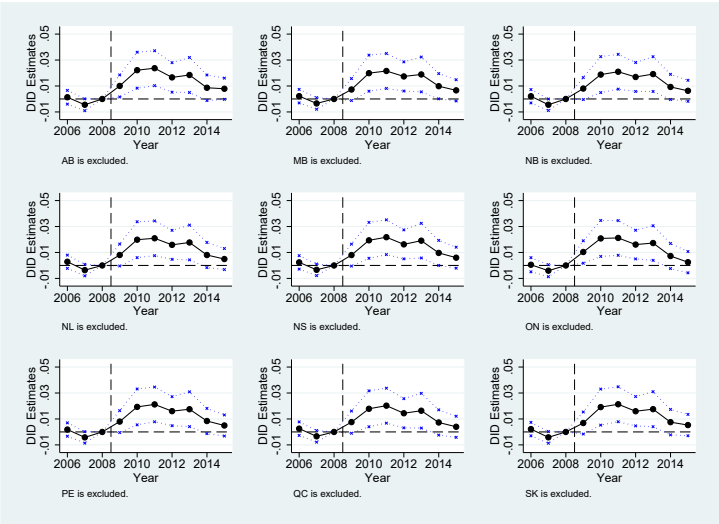
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period 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 18: The Dynamic Short-Term Unemployment Effects on Medium-Educated Workers



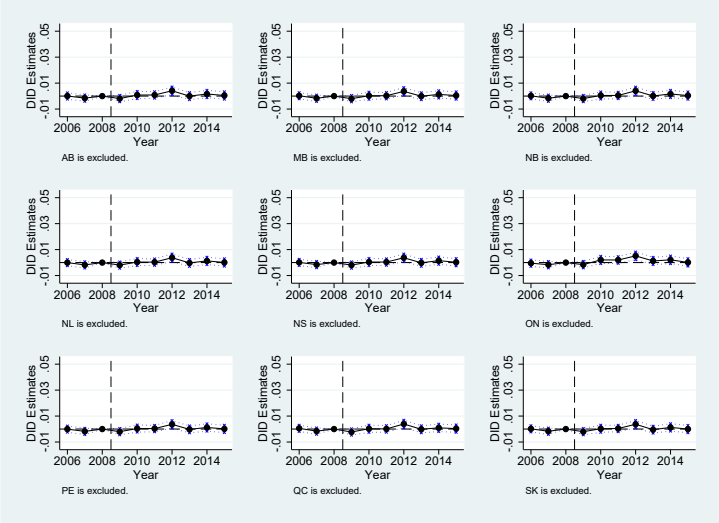
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period 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 19: The Dynamic Short-Term Unemployment Effects on Low-Educated Workers



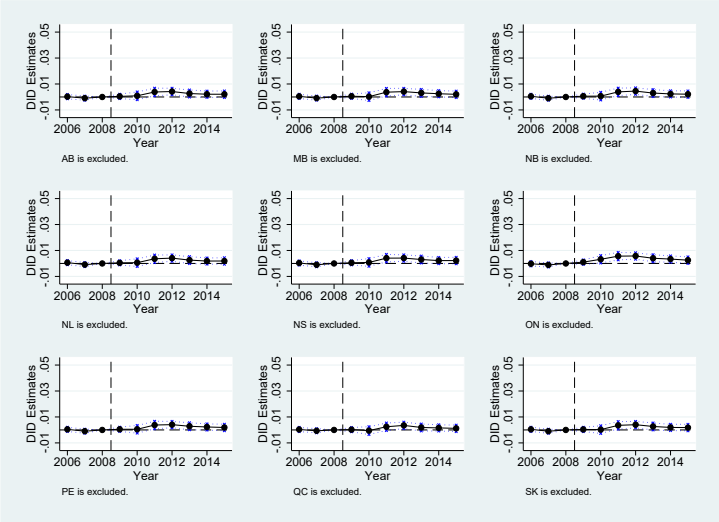
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period 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 20: The Dynamic Long-Term Unemployment Effects on High-Educated Workers



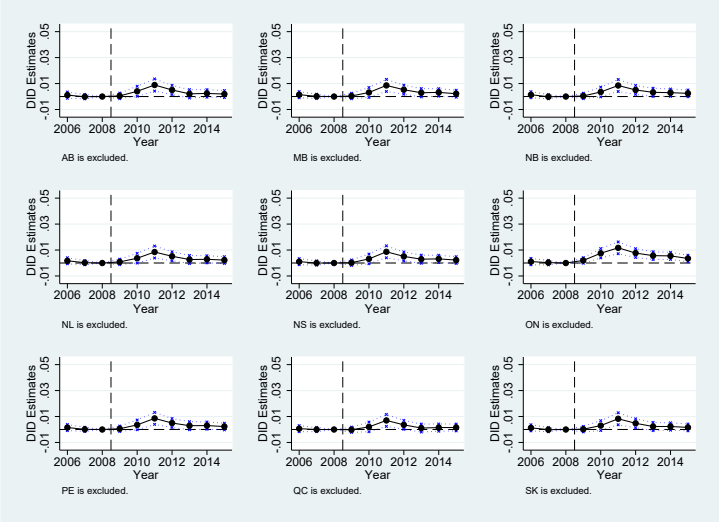
Notes: The dependent variables are *Unemployed*. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (3) in the corresponding year. For example, the first dot represents the main DID estimate of the period 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 21: 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 22: 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.