The Ins and Outs of Employment: Labor Market Adjustments to Carbon Taxes

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Abstract

Job cuts are tough; wage cuts aren't easy, either. I exploit British Columbia's carbon tax to study how jobs and wages are cut. Combining matching with a difference-in-differences approach, I uncover the dynamics of unemployment and wage effects. While the unemployment effect arrives without lags but decays quickly, the wage effect comes with lags but grows gradually. I provide strong evidence to conclude that a complete understanding of unemployment and wage effects requires an explanation of employment flows—the inflow and outflow of employment. This paper bridges between the literature and the public—it explains why prior studies may find the labor market effects of environmental policies weak and explains why the public is so concerned with potential job and wage losses created by environmental policies. This paper provides new micro-evidence on labor market adjustments and calls for attention to the transitional labor market adjustments to environmental policies.

JEL Codes: E24, H23, J31, J63, Q52

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1 Introduction

Environmental policies have swept the world for decades. These policies have received increasing attention from the public partly because of potential job and wage losses. Are environmental policies and adverse labor market consequences connected?

This question has been argued at length, and empirical evidence has been mixed. While many studies document the significant labor market effects of environmental policies in regulated firms (Greenstone, 2002; Walker, 2011; Kahn and Mansur, 2013; Curtis, 2017), some find the effects weak (Berman and Bui, 2001; Martin et al., 2014). Recent literature argues that these policies may shift employment from affected sectors to others, making the aggregate effects weak (Hafstead and Williams, 2018; Castellanos and Heutel, 2019; Azevedo et al., 2020). If these effects are weak, why is the public so concerned with environmental lawmaking? If these policies create significant job and wage losses, what hinders the literature from identifying the significant unemployment and wage effects? What is missing between the public and prior literature?

To answer these questions, this paper provides an in-depth study of how an environmental policy shapes a labor market. In addition to unemployment and wage effects, this paper studies the dynamics and the mechanisms through which these effects operate. For example, in response to environmental policies, does unemployment increase because employed workers become more likely laid off (i.e., a job-loss effect) or because unemployed workers find it harder to get hired (i.e., a job-finding effect)? Do wage adjustments operate through incumbent wages or hiring wages? These questions are important because they address the public concern on labor market responses to environmental policies. They help economists understand the functioning of labor markets by answering how jobs and wages are cut. Undoubtedly, a better understanding of the functioning of labor markets helps policymakers with the formulation of appropriate labor market policies along with environmental policies.

To establish a causal relationship, I exploit a unique opportunity provided by the introduction of a revenue-neutral carbon tax policy in British Columbia (BC). This policy provides an ideal platform to study how an environmental policy shapes a labor market for four reasons. First, the tax was introduced in BC only; hence, it provides numerous control labor markets from the rest of Canada (ROC). Second, it was implemented in July 2008; therefore, the pre- and post-policy periods are clearly defined. Third, the initial carbon tax was so high that the public is aware. This awareness is important to ensure industries respond to the policy, allowing for the study on labor market adjustments. Fourth, the policy provides a novel source of an exogenous variation in the stringency of the shock because the tax rate increased annually in the first five years. These four appealing features allow me to identify the causal effects of BC's carbon tax on labor market outcomes with the coarsened exact matching (CEM) method and the difference-in-differences (DID) approach.

This paper reveals the mechanisms explaining the differences in the dynamics between unemployment and wage effects. I find that the unemployment effect arrives without lags and decays quickly. The unemployment rate rose significantly following the policy because of the job-loss and job-finding effects. In other words, the tax made job losses increasingly common and made job-hunting harder. The unemployment effect quickly decayed because the job-loss effect was short-lived. Intuitively, the carbon tax increased the marginal cost of production, depressing labor demands. When firms decided to lay off workers, those workers were laid off without substantial lags. This mechanism coheres with Wolfers (2006) in that "*bad matches may be dissolved earlier*", explaining why the job-loss effect happens only in the early stage of the policy and why the significant unemployment effect decays quickly.

In contrast, the adverse wage effect comes with lags and grows gradually. The incumbent wage effect was negligible for years, reinforcing our understanding of nominal wage rigidity among incumbent workers. Since incumbent workers were the majority in employment, the initial average wage effect was negligible. Meanwhile, the average hiring wages plunged, and this effect lasted long. Average wages continued to decrease with a gradual increase in the proportion of new hires in employment. Wage adjustments operated through this slow process of labor turnovers, explaining why the average wage effect grew gradually.

These findings enhance our understanding of labor market adjustments. While it is a commonplace to say that wage cuts are alternatives to layoffs, I find the opposite—layoffs mediate wage cuts. Moreover, these findings reveal that a complete understanding of the dynamics of unemployment and wage effects requires an explanation of employment flows—the inflow and outflow of employment.

Furthermore, the findings bridge between the public and prior literature. This paper finds the unemployment and wage effects significant, explaining why the public is so concerned with potential job and wage losses created by environmental policies. Meanwhile, it explains why prior studies may fail to capture the significant unemployment and wage effects of environmental policies. This paper reveals that any studies, if estimating the unemployment effects of any environmental policies in the long run or averaging their effects over prolonged post-policy periods, may conclude with weak unemployment effects because the significant unemployment effect is short-lived. By comparing the wage rate between short pre- and post-policy periods, the standard DID approach may conclude with weak wage effects of environmental policies because the average wage effect comes with substantial lags. The finding of this paper partly explains why the adverse wage effect of environmental policies is rarely documented in the literature.

The results are highly policy-relevant. This paper suggests no urge to accommodate environmental policies with substantial extensions of unemployment benefit periods. The entitlement period of unemployment benefits is often extended substantially in downturns to smooth the consumption of unemployed workers over prolonged unemployment periods. Although environmental policies like BC's carbon tax may substantially increase the unemployment rate, I find that a majority of unemployed workers are able to find a job within half a year. In other words, environmental policies increase unemployment mainly through the increase in the number of unemployment spells, not the duration of unemployment spells. Substantial extensions of unemployment benefit periods along with environmental policies are unnecessary.

Moreover, carbon tax revenues can be wisely redistributed in labor markets. Since incumbent workers keep their jobs and have their wages unaffected by carbon taxes, there is no urge to compensate incumbent workers. In contrast, many new hires were laid off in the first place because of carbon taxes.

Once employed, they receive lower hiring wages. The government could utilize carbon tax revenues to provide firms with hiring subsidies to speed up hiring processes and to provide new hires with tax credits to mitigate potential after-tax wage losses created by environmental policies.

This paper makes primary contributions to the literature on environmental economics. This paper provides one of the most complete pictures of how environmental policies shape labor markets in this empirical literature. Earlier literature on the labor market outcomes of environmental policies focuses mainly on manufacturing sectors (Greenstone, 2002; Walker, 2011; Kahn and Mansur, 2013). Yet, these policies may shift employment from manufacturing to other sectors (Hafstead and Williams, 2018; Castellanos and Heutel, 2019) and may create green jobs (Wagner, 2005; Vona et al., 2018; Marin and Vona, 2019), both of which absorb unemployment and make the overall employment effect ambiguous.

This paper is related to recent studies that explore the overall employment effect of environmental policies. For example, Yamazaki (2017) and Carbone et al. (2020) find that the employment effects of BC's carbon tax are heterogeneous across sectors: several sectors expand their employment, and many sectors shrink. Yip (2018) documents a significant unemployment effect of the same policy. Nevertheless, the analyses of the dynamics and mechanisms of unemployment effects are rarely seen in this literature. Moreover, none of the aforementioned studies examine the policy impacts on job transition rates.[1]

To the best of my knowledge, this paper is the first to quantify the unemployment effect of environmental policies through each of the job-loss and job-finding channels. Not to mention, the analyses on the dynamics of the two channels are important. For example, Ferris et al. (2014) find little evidence on the employment effect of an environmental regulation over a prolonged post-policy period. When disaggregating the effect by year, they find the employment effect strong but short-lived. Moreover, Yip (2018) finds a significant unemployment effect in the first two years of a carbon tax policy, but Azevedo et al. (2020) find little evidence on the average employment effect in the first sixth year of the same policy. The quantification of the job-loss and job-finding effects in this paper complements this literature: it helps economists understand why the unemployment effect is short-lived and why the literature may fail to identify the significant unemployment effects of environmental policies.

This paper also contributes to recent literature that documents the adverse earnings effects of environmental policies (Walker, 2013; Curtis, 2017). A majority of this literature studies the earnings effects on the employees of affected firms and industries. With existing evidence in this literature, economists and policymakers still have a hard time understanding the associated earnings and wage losses in the aggregate labor markets. This paper fills the gap by estimating the wage effects and the underlying mechanisms at the aggregate level. Moreover, environmental policies are known to create job losses—the extensive margin of employment. It remains unclear whether the documented earnings losses in the literature stem from the reduction in wage rates or labor hours—the intensive margin of employment. In contrast, this paper speaks directly to the wage and unemployment effects of environmental policies, documents the differences in their dynamics, and provides an explanation for the differences.

[1] Walker (2013) and Curtis (2017) also study the impacts of environmental policies on the job-separation and job-finding rates of affected firms. This paper studies the impacts on the economy as a whole.

Despite the empirical setting pertaining to carbon taxes, this analysis is able to lend insight into labor market adjustments to sector-specific shocks. BC's carbon tax was based on greenhouse gas emissions generated from burning fuels. It increases the marginal cost of production through energy use, potentially creating cost shocks that are especially large in the manufacturing sectors. Hence, this study provides micro-evidence that complements a large body of literature on labor market adjustments to sectoral shocks such as oil crises and other environmental policies.

For example, this paper is related to the literature on employment flows (Darby et al., 1986; Hall, 2005a,b,c; Davis et al., 2006; Fujita and Ramey, 2009; Elsby et al., 2009; Shimer, 2012; Elsby et al., 2013). The seminal paper of Darby et al. (1986) claims that unemployment fluctuations are almost entirely driven by a job-loss effect (i.e., variations in separation rates). While this belief has been prevalent for many years, Hall (2005c) says that "*in the modern US economy, recessions do not begin with a burst of layoffs*", Shimer (2012) concludes that separation rates are comparatively acyclic, and many recent theoretical studies refer to Hall (2005a,b,c), Shimer (2005, 2012), and Hagedorn and Manovskii (2008) to motivate their treatment of separation rates as acyclical. Recent studies of Elsby et al. (2009) and Elsby et al. (2013) conclude that the inflows and outflows of unemployment are essential in explaining unemployment effects. In contrast to correlations documented in this macro-labor literature, this paper provides causal micro-estimates supporting that (i) the separation rate increases in response to sectoral shocks, (ii) both job-loss and job-finding effects are keys to understanding unemployment effects, and (iii) the impacts on job-finding effects last longer than job-loss effects.

Moreover, this paper sheds light on the literature on nominal wage rigidity (Goette et al., 2007; Martins et al., 2012; Haefke et al., 2013; Barattieri et al., 2014; Elsby et al., 2016; Elsby and Solon, 2018). A large body of this literature measures wage rigidity by comparing individual wage growths (i.e., either the magnitude or the likelihood of wage growth, or both) between two or multiple points of time. This approach utilizes lagged wages as reference levels to measure wage rigidity.

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

Using this approach, this paper provides micro-evidence reconciling the sticky wage hypothesis with recent documentation of wage procyclicality in the literature (Martins et al., 2012; Haefke et al., 2013; Elsby et al., 2016). While incumbent wages are found rigid, the average hiring wage plunge lowers the average wage of an economy. Moreover, this paper provides new insights into the mechanism of wage rigidity. One of the "*textbook*" explanations of the sticky wage theory is that wages are not easily adjusted under labor contracts and are adjusted through the renegotiation of contracts (Barro, 1977; Fischer, 1977; Thomas and Worrall, 1988). Nevertheless, this paper finds that average wages are completely rigid among incumbent workers in the first five years of the shock, over which many incumbent workers are expected to renegotiate contracts at least once.[2] This finding (i) suggests that downward nominal wage rigidity lasts longer than many economists expect, (ii) casts doubt on labor contracts explaining rigidity and on wage adjustments operating through renegotiation during contract renewals, and (iii) provides suggestive evidence that average wages are slowly and mainly adjusted through new contracts during hiring processes.

Before proceeding to the next section, it is worth highlighting that this paper has no intention to support or object to any environmental policies. While Fried et al. (2018) pinpoints that "*policymakers must pay careful attention to not only the long-run outcomes but also to the transitional welfare effects*", this paper calls for attention to the transitional labor market adjustments to environmental policies.

The paper is structured as follows. Section 2 presents the details of BC's carbon tax. Section 3 describes identification strategies. Section 4 presents results and their policy implications. Section 5 discusses the credibility of the results. Section 6 summarizes findings.

2 Revenue-Neutral Carbon Tax in British Columbia

BC's carbon tax policy was officially announced in Budget 2008 on February 19 in 2008. The Carbon Tax Act was enacted by the legislative assembly between April and May and the policy was implemented on July 1 of the same year. This tax is based on greenhouse gas emissions generated from burning fuels and it is applied to the consumption of fossil fuels in BC including industries and households. For example, the consumption of gasoline is required to pay the carbon tax of 2.34¢ per liter in July 2008.[3]

BC's carbon tax provides an ideal platform to study labor market adjustments to environmental policy for four reasons. First, the policy provides numerous control labor markets. Many OECD countries introduced nationwide carbon tax policies such as Finland in 1990, Norway in 1991, Sweden in 1991, Denmark in 1992, etc., making it difficult to find appropriate control labor markets. BC's carbon tax was introduced in one of the Canadian provinces only, providing numerous control labor markets from the ROC.[4] Second, the arrival date of the tax is definite. BC's carbon tax policy was implemented on July 1, 2008. Since then, BC's labor market has been influenced by the policy. This policy provides the exact arrival date to define pre- and post-policy periods.

Third, the policy is salient. It is crucial to ensure the public is aware of the policy so that fossil fuel users could optimize with respect to the incentives created by the policy. Otherwise, we would be uncertain whether labor markets generally do not respond to environmental policies or those policies are unnoticeable. For example, a statistically insignificant wage effect can be attributable to wage rigidity, but the insignificant wage effect may simply reflect that the shock is unnoticeable. Initially, BC's carbon

[2] According to Elsby et al. (2016), "*...many workers, including ourselves, have their nominal wages reset only once a year*".

[3] Readers who are interested in the carbon tax by fuel type are referred to Table A1 in Appendix A.

[4] Readers, who are interested in the geographical location of BC, are referred to Figure A1 in Appendix A.

tax rate was CAD$10 per tonne of carbon dioxide equivalent (CDE) emissions in 2008, which was one of the highest in the history of North America. For example, the initial carbon taxes were CAD$3.50, USD$0.04, and USD$5 per tonne of CDE in Quebec in 2007, the San Francisco Bay Area, California, in 2008, and Maryland in 2010, respectively. Moreover, the coverage of BC's carbon tax is among the broadest and most comprehensive on the globe because it is applied to virtually all fossil fuels consumed by households and firms. This board-based feature is rarely seen; for example, Denmark's carbon tax was first imposed only on households in 1991, an environmental tax was only introduced to the transportation sector in the United Kingdom in 1993, a carbon tax is only imposed on energy producers in Quebec in 2007. These two features make BC's carbon tax salient, allowing us to study labor market adjustments to an environmental policy.

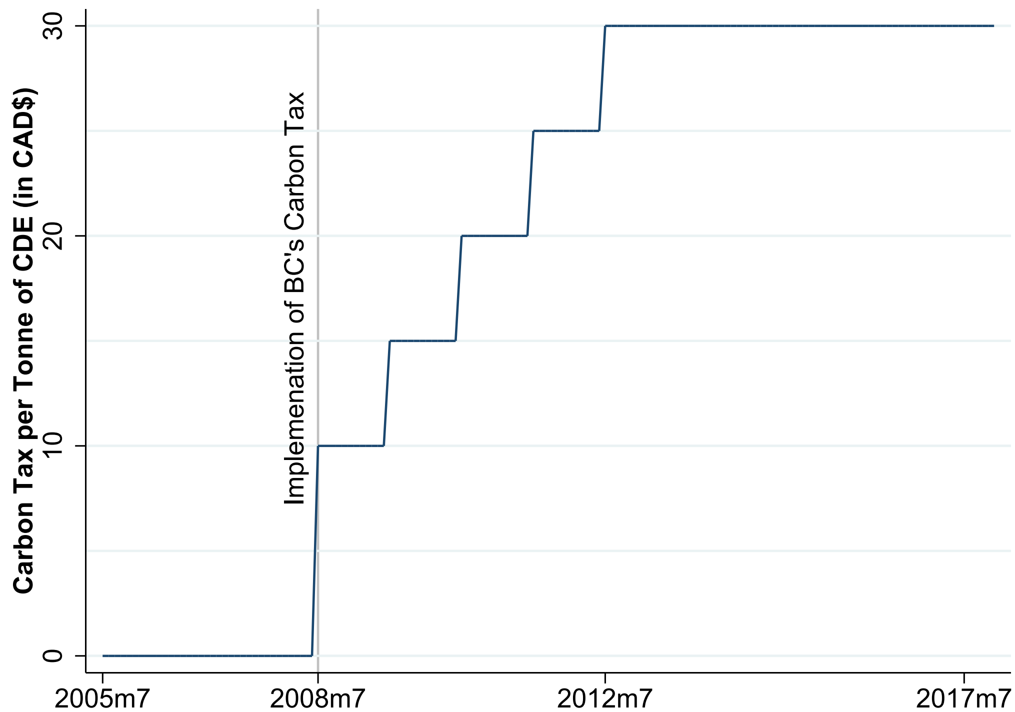
In fact, the literature also documents the public responses to BC's carbon tax. Rivers and Schaufele (2015) find that BC's carbon tax policy decreases carbon dioxide emissions from gasoline consumption by 2.4 million tonnes during 2008-2012, suggesting that industries did respond to the policy. This finding is consistent with other studies. For example, per capita fuel consumption in BC is found to decrease by 19 percent relative to the ROC (Elgie and McClay, 2013; Murray and Rivers, 2015). Elgie and McClay (2013) shows that the GHG emissions per capita drop by nine percent during the same period. The documentations of the public responses suggest that fossil fuel users are sensitive to the policy.

To further support the claim on the salience of BC's carbon tax, I provide additional evidence on the public awareness of carbon tax policies. Figure A2 in Appendix A demonstrates the Google Trend of "*carbon tax*" in BC between January in 2004 and December 2019. Google Trend measures the frequency people in BC search the keyword of "*carbon tax*" using Google Web Search. It normalizes the highest frequency to 100 as to why the measure ranges from zero to a hundred. The figure shows that people living in BC search the keyword of "*carbon tax*" the most frequently in June 2008, one month before the implementation of BC's carbon tax. Moreover, the frequency of this month is at least ten times more than any month before 2008. It suggests that the public in BC is aware of the carbon tax policy. Indeed, search frequencies were high in BC even though carbon taxes were introduced in other Canadian provinces such as Alberta (AB) in January 2017, and a couple of provinces in April 2019.

In fact, similar behaviors can be found in the Canadian provinces of AB, Manitoba (MB), New Brunswick (NB), Ontario (ON), and Saskatchewan (SK). Figure A3 in Appendix A shows the Google Trend of "*carbon tax*" in theses provinces. For example, people in these provinces also searched the keyword of carbon tax the most frequently when the tax was introduced in their provinces, and their search frequencies were also high when carbon taxes were introduced in other Canadian provinces. The evidence suggests that the public in general is aware of carbon tax policies, especially in their own province.

Fourth, BC's carbon tax provides a novel source of exogenous variations in the stringency of the shock. As shown in Figure 1, BC's carbon tax rate increased annually by CAD$5 from 2008 until it reached CAD$30 in 2012. The rate stayed at CAD$30 from July 1, 2012, onwards. I expect the severity of the shock from BC's carbon tax increases with the carbon tax rate. If the market does not fully respond to future rate adjustments, I expect the sizes of the effects to grow with the tax rate in the first few years.

Figure 1: BC's Carbon Tax Rate Over Time



Indeed, economists and policymakers are interested in BC's carbon tax because it was unique in North American due to its revenue-neutral nature. Carbon tax revenues were returned to firms and residents through the reductions in corporate tax taxes, personal income taxes, and lump-sum transfers. For example, the corporate tax rate was cut from 12% to 11% in July 2008, and down to 10% in January 2011. However, this rate went up again to 11% in April 2013. The four lowest personal income tax rates were reduced in April 2008. Since then, the personal income tax rates remain unchanged. The historical BC's corporate income tax and personal income tax rates are illustrated in Figure A4 in Appendix A, and the corresponding carbon tax revenues and tax cuts can be found in Table A2 in Appendix A.

Nevertheless, this policy is by no means perfect for identification. The public may react to this policy prior to the implementation. Therefore, estimates may 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 policy. Note that BC's carbon tax policy was announced on February 19 and was implemented on July 1 of the same year. The short period between the dates of announcement and implementation mitigates the potential anticipation effects of the policy, allowing us to capture the entire policy effects on labor market adjustments. I will present in section 5.2 that no statistical evidence on the anticipation effects on BC's carbon tax can be found.

3 Identification Strategies

3.1 Identification Methods

Identification of the labor market effects of BC's carbon tax requires workers that have similar characteristics to those affected workers in BC. While BC's unemployment and average wage rates fluctuate over time, a comparison of BC's labor market outcomes between pre- and post-policy periods unlikely yields unbiased estimates. Similarly, a naive comparison of labor market outcomes between BC and the ROC at any point of time likely gives biased estimates because labor market outcomes, including the unemployment and average wage rates, are different across Canadian provinces prior to the policy.[5]

I exploit spatial and temporal variations in BC's carbon tax to solve the identification problem. First, I include a set of province-level fixed effects to control for time-invariant, observed and unobserved, factors that account for the differences in the unemployment and average wage rates between BC and the ROC. The inclusion of the province-level fixed effects ensures that estimates are derived from time-varying factors. Second, I add a set of time-fixed effects to control for time-varying factors that affect the Canadian economy as a whole at any given point of time such as the 2008 financial crisis and variations in the exchange rate between Canadian dollars and US dollars.

The inclusion of these two sets of fixed effects allows me to identify the effects of BC's carbon tax by comparing the differences in labor market outcomes between BC and the ROC and between the pre- and post-policy periods. The estimates will be free from biases that arise from the time-invariant fundamental differences in labor market outcomes between BC and the ROC and arise from countrywide fluctuations in the labor market outcomes over time. This research design amounts to a standard DID regression estimator, with treatment and control groups consisting of workers in BC and the ROC, respectively.

To estimate the causal effect of BC's carbon tax, I complement the DID approach with the CEM method. A matching method is adopted to make worker samples in BC and the ROC observably similar and to make inferences less model-dependent, reducing statistical bias (Ho et al., 2007). I use the CEM method because it dominates commonly used matching methods, including propensity score and Mahalanobis matching, by reducing the imbalance, model dependence, estimation error, bias, variance, etc. (Iacus et al., 2011, 2012). In what follows, I will brief the procedure that combines the CEM method with the DID approach.

The CEM is applied to balance between 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 any sample in either BC or 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 the weight equal to one to each worker of the treatment group in remaining cells and returns weights $n_t^j/n_c^j \times N_c/N_t$ to the control group in each of the remaining cells j, where n_t^j and n_c^j are the sample sizes of BC and the control group in cell j. N_t and N_c are the total numbers of observations in the matched samples of BC and the control group, respectively. These weights are used to rescale the

[5] I report and discuss the unemployment and average hourly wage rates in BC and the ROC in Figure 2 in section 3.2.

weight of each observation in the matched control sample to balance the empirical distributions of the matching covariates between BC and control samples in the DID regression model.

Using the DID approach, the causal effect of the policy can be estimated by a regression model as follows:

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

where Y_{ijt} is an economic outcome of individual *i* in province *j* at time *t*. BC_j equals one if an individual 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 an individual lives in BC after the policy, and zero otherwise.

X^T_{ijt} is a vector of individual characteristics, including dummies for gender, age group, the highest qualification attained, marital status, and industries. These regressors control for variations in the sample composition. I will show that results are robust to model specification. In other words, the conclusion of this paper is independent of the choices of individual characteristics in the model. η_j captures the province fixed effect; δ_t captures the *Year*×*Month* fixed effects.

The coefficient β_1 is the DID estimate of our primary interest. This key DID estimate captures the average treatment effect on the treated. It is estimated by a weighted least squares method, where the weight of each observation is obtained by the CEM method. This estimation method requires a common trend assumption. That is, the trends of a dependent variable in BC and the control provinces are parallel in the absence of BC's carbon tax. Since this estimation method relies heavily on the common trend assumption, I will discuss the validity of this assumption in detail in section 5.

Yip (2018) provides evidence that there was a shock to BC's labor market (relative to the labor market in the ROC) in 2004 and suggests that observations prior to 2005 should be excluded from the pre-policy period. He shows that the unemployment trends are parallel between BC and the ROC during the three years prior to the policy (i.e., July 2005-June 2008). Here, I provide suggestive evidence on the source of the shock and discuss potential remedies to the problem. I believe the change in BC's unemployment is largely related to the 2010 Winter Olympics and the 2010 Winter Paralympics in BC. Vancouver won the bid on the host of the 2010 Winter Olympics on July 2, 2003. Since then, a series of infrastructure had been upgraded and newly built to prepare for the events.[6] Figure F2 in Appendix F provides suggestive evidence on the expansion of BC's construction industry. While many industries in BC expanded in

[6]The associated construction was completed piece by piece between 2005 and 2009. For example, the Whistler Sliding Centre was constructed during June 2005-December 2007, costing CAD$105 million. Whistler Olympic Park was constructed between 2005 and 2007, costing nearly CAD$120 million. Pacific Coliseum completed the upgrade of its seating, heating, ventilation, air conditioning, ice surface, and a scoreboard in the summer of 2007. The construction of Richmond Olympic Oval began in 2006 and ended in 2008, costing CAD$178 million. Thunderbird Sports Centre was reconstructed for the 2010 Winter Olympics. The refurbishment of an ice hockey facility and the construction of a new practice began in April 2006 and ended in 2008, costing CAD$47.8 million. The 2010 Olympic Village was constructed from March 2007 to Summer 2009, costing CAD$875 million. BC Place Stadium held the opening and closing ceremonies of the Winter Olympics 2010. It upgraded its seating, washrooms, concessions and reinforced its ring beam between May 2008 and October 2009, costing over CAD$150 million. In addition to these sporting facilities and housing, many highways were upgraded to connect among sporting facilities, public infrastructure, and housing since 2005.

2004-2005 (i.e., the solid line), BC saw the largest increase in its construction industry: its employment nearly doubled in size. In Appendix F, I use a synthetic control method (SCM) to construct a synthetic industry for each of 18 industries in BC. By this approach, BC's construction industry is the only industry whose employment path cannot be replicated from the ROC (i.e., the dash line in Figure F2). The results suggest that the unparallel employment trend between BC and the ROC during 2004-2005 documented in Yip (2018) is likely attributable to the Olympic shock.

The shock reduced BC's unemployment rate, potentially contaminating the DID estimates of the unemployment effect. Naturally, there are three empirical strategies to address the effect of the Olympic shock. First, a DID approach can be conducted by excluding construction workers from the sample of examination. However, this strategy is infeasible in all analyses focusing on unemployed workers. The reason is that the industry of unemployed workers is unknown. A lot more of unemployed workers than the employed do not report their industries in many Canadian data such as the Canadian Labour Force Survey (LFS). If an unemployed worker reports his industry, the reported industry could be the last industry an unemployed worker belongs to and could be also an industry the unemployed is currently seeking jobs from. This strategy is not an ideal approach to the estimate the unemployment effect but is feasible to estimate the wage effect. As such, I restrict the sample to manufacturing workers to estimate the wage effects in section 5.1 and the results cohere with the main findings of this paper.

Because of the unparallel trends in employment, another remedy is the SCM. The SCM is ideal because it relaxes the parallel trend assumption. Nevertheless, this assumption is relaxed only if a synthetic group exists (Abadie et al., 2010). Hence, I follow Azevedo et al. (2020) to adopt the SCM to conduct the analysis at the industry level. The advantage of this approach is to increase the number of donors from nine provinces to 162 province-specific industries, increasing the chance to create the synthetic group. While the SCM works well for the analysis of employed workers, I use the SCM to estimate the employment and wage effects and report results in Figures F4 and F5 in Appendix F. The results are largely consistent with the conclusion of this paper. Nevertheless, the SCM cannot be applied for the analysis of unemployed workers for the reason explained above and is not an ideal approach to estimate the unemployment effects, which are focuses of this paper.

Another possibility is to analyze with matching. I use the CEM method to re-weight the sample of ROC. Figure F1 in Appendix F presents the unemployment rates of BC and the re-weighted sample of ROC. The figure suggests that the re-weighted sample of ROC does not share a parallel unemployment trend with BC in the pre-policy period because of the drop in BC's unemployment rate between July 2003 and June 2005. The matching approach and the SCM probably share the same problem, in which nine donors are insufficient to replicate BC's employment trend. Therefore, I follow Yip (2018) to define the pre-policy period as July 2005-June 2008 and discuss potential caveat of the findings in section 5.4.

The post-policy period is defined as July 2008-June 2015, the first seven years of the policy. The sample after June 2015 is excluded because a series of climate change policies were proposed and introduced after the second half of 2015. For example, Alberta's carbon tax policy was announced in November 2015 and was implemented in January 2017. The Paris Agreement was adopted by consensus in December 2015. According to the agreement, each country, including Canada, must plan to mitigate global warming. Ontario Climate Change Mitigation and Low-Carbon Economy Act was passed in May 2016. The Greenhouse Gas Pollution Pricing Act was passed in the Parliament of Canada in June 2018—new carbon tax policies were introduced to a couple of Canadian provinces in 2019. I extend the sample period to June 2019 to conduct a comparable analysis. Appendix D provides support that the key conclusion remains unchanged.

The remaining issue relates to the estimation of standard errors. Bertrand et al. (2004) raises concerns about the correlation of the regressors within clusters in the DID estimation. Accordingly, the cluster-robust standard errors are estimated to generalize the Huber-White sandwich estimates of ordinary least squares standard errors to the clustered setting to account for possible heteroscedasticity and within non-treated group dependence of standard errors. According to Bertrand et al. (2004) and Angrist and Pischke (2008), standard errors should be clustered by 42 levels or more. In this paper, standard errors are clustered at the level of ten provinces, two genders, nine age groups, three educational levels, and two marital statuses, providing us with 1080 clusters. I also estimate the standard errors using a wild bootstrap procedure proposed by MacKinnon and Webb (2017) and Roodman et al. (2019).[7]

3.2 Data Descriptions

I utilize the public-use files of the Canadian LFS. 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. These public-use files are used so that the results of this paper can be easily replicated without accessing the data in Statistics Canada in Ottawa.

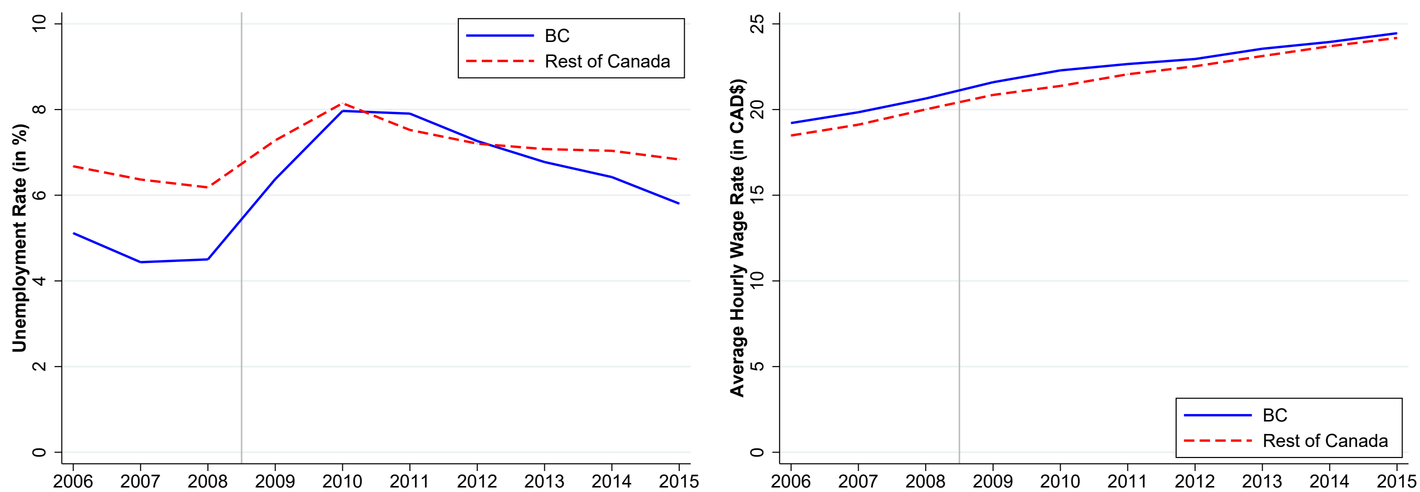
The LFS is appropriate for the current study because of two reasons. First, it provides detailed labor market information to support the in-depth study of labor market adjustments. Information on labor market statuses and hourly wage rates allow me to explore the policy impacts on unemployment and wages. It also provides information on the length of tenure, the duration of unemployment spells, industry, etc., allowing me to investigate previously unobserved outcomes in the literature. For example, the length of tenure allows me to distinguish new hires from incumbent workers to explore whether wages are adjusted through hiring or incumbent wages. When appropriate, I will further discuss how these variables are used and why they are important to the present study.

Second, the LFS provides detailed demographic information for the CEM method. Ideally, the matching covariates between BC and the ROC are exogenous to the policy; however, labor market variables are likely endogenized. While demographic variables such as age, gender, the highest educational attainment, and marital status, are shown to be key determinants of labor market outcomes, they are also expected to be exogenous to BC's carbon tax policy. Therefore, I utilize two genders (i.e., male and female), three age groups (i.e., 15-29, 30-44, and 45-59), three educational levels (i.e., low-, medium-, and high-educated workers), and two marital statuses (i.e., married/common-law and others) as matching covariates to make samples observably similar in terms of demographic composition.

[7] I use the command of boottest in Stata 14 to execute the wild bootstrap procedure with seed 1000.

Throughout the analysis, I exclude workers aged 60 or above from my sample to avoid potential biases from retirement decisions. When I explore unemployment effects, I restrict the sample to labor force participants. I exclude unemployed workers from the sample to investigate wage effects. Since samples may vary from analyses to analyses, I apply the CEM algorithm to obtain a different set of weights in each analysis.

Figure 2: The Unemployment Rate and The Hourly Wage Rate in BC and the ROC during 2005-2015



Note: Data are from the Canadian LFS July 2005-June 2015. Year t indicates the period between July in year t-1 and June in year t. The vertical line represents the month of implementation of BC's carbon tax, which was July 2008. While the averages in BC are calculated from the sample in the LFS, the averages in the ROC are calculated using the re-weighted sample.

Figure 2 presents the unemployment and average hourly wage rates in BC and the ROC during July 2005- June 2015.[8] Two points deserve mentioning. First, the figure presents the first visual evidence of this paper to support that a carbon tax policy will increase unemployment. On average, the unemployment rates were 4.68 and 6.41 percentage points in BC and the ROC between July 2005 and June 2008. The rates were about 1.7 percentage points lower in BC than the ROC during the three successive years prior to the policy, revealing a pre-policy parallel trend in unemployment between BC and the ROC. Once the carbon tax was introduced, BC caught up with the ROC—their unemployment rates reached as high as eight percentage points in 2010. When the carbon tax rate stopped to increase in 2012, BC's unemployment rate fell below the unemployment rate in the ROC again. I will provide several pieces of statistical evidence to further support that this unemployment effect is driven by BC's carbon tax, and I will explain why the unemployment effect decays once the tax stopped to increase.

Second, the wage growth slowed down in BC years after the policy. Prior to the policy, the average hourly wage rate was about CAD$19.9 in BC, CAD$0.7 higher than in the ROC. This difference was steady before the policy. While wages in BC and the ROC grew during the entire period of examination,

[8]The averages in BC are identical in the sample of the LFS and the re-weighted sample. In Figure 2, I calculate the averages in the ROC using the re-weighted sample.

the growth started to slow down in BC roughly three years after the implementation of BC's carbon tax. The wage gap got closer; in 2015, their average wages are almost identical. Although the slowdown happened years after the policy, I will provide evidence to support that the adverse wage effect is driven by BC's carbon tax, and I will explain why the wage effect emerges with lags.

Next, I present the descriptive statistics of demographic information in Appendix B. Since I utilize genders, age groups, educational levels, and marital statuses as matching covariates, samples in BC and the ROC are matched so well that the distributions of these variables in BC's sample and the re-weighted ROC's sample are visually and statistically identical. The empirical distributions associated with the ROC's re-weighted sample are referred to the corresponding distributions of BC's raw sample. Hence, I calculate the statistics using the raw data of LFS, not the re-weighted one.

The demographic compositions in the two samples are similar during July 2005-June 2015. For example, as shown in Figure B1, about half the sample is male; the difference is statistically insignificant between the two samples. As demonstrated in Figure B1, about 58-59 percent of the samples are married/common-law in the ROC. This proportion is about 1.3-1.5 percent lower in BC, and the difference in this proportion between BC and the ROC is steady. Look at Figure B2. About 30 percent of the two samples are 15-29 years old. Both samples become aged: the proportion of respondents aged 45-59 rises from 36 to 38 percent. The age structures of the two samples are close. As shown in Figure B3, the two samples become more educated: their shares of low-educated survey respondents fall steadily from 41 to 38 percent, and the shares of their high-educated respondents increase by 1.7 percent. These figures suggest that (i) the samples in BC and the ROC are largely comparable and (ii) BC's carbon tax has negligible effects on the demographic composition in BC.

Next, I present industry structures in the two samples in the pre-policy period. BC and the ROC rely on similar industries. The three major industries are wholesale & retail trade, health care & social assistance, and manufacturing. Wholesale & retail trade is the major industry in the two economies, accounting for 17.29 and 16.86 percent of the samples in BC and the ROC, respectively. About 11.05 and 11.75 percent of the samples work in health care & social assistance in BC and the ROC. 10.40 percent of BC's sample and 13.54 percent of ROC's sample work in manufacturing. These three industries account for 38.74 and 42.15 in BC and the ROC prior to BC's carbon tax policy. I would like to highlight that the shares of finance, insurance, real estate, & leasing industry are close in BC and in the ROC, amounting to 5.20 and 5.03 percent. The similar shares of the financial industry in the two samples ease our concern that the 2008 financial crisis may affect the two economies disproportionately. I will provide rigorous statistical analyses on this point later. In sum, BC and the ROC are by no means identical in their demographic and industry structures; however, they are close enough that we can learn a valuable lesson by the comparison between them.

4 Results

The results are presented by four subsections. Section 4.1 presents the unemployment and wage effects of BC's carbon tax. Sections 4.2-4.3 present the central findings of this paper—(i) the dynamics of the

Table 1: The Unemployment and Wage Effects of BC's Carbon Tax

Difference-in-Differences Analysis

	(1)	(2)	(3)
	A. Unemployment Effect (N=7,405,598)		
BC×Post	0.013***	0.013***	
	(0.002)	(0.003)	
Adjusted R^2	0.005	0.034	
	B. Wage Effect (N=5,450,559)		
BC×Post	-0.024***	-0.024***	-0.024***
	(0.008)	(0.004)	(0.004)
Adjusted R^2	0.050	0.393	0.479
Province Fixed Effect	yes	yes	yes
Year×Month Fixed Effect	yes	yes	yes
Gender		yes	yes
Age		yes	yes
Educational Level		yes	yes
Marital Status		yes	yes
Industry			yes

Note: BC's unemployment rate and hourly wage rate are 4.72% and CAD$19.91 in the pre-policy period. Dependent variables are a dummy variable for unemployment and ln(hourly wage rate) in Panel A and B, respectively. Samples are restricted to all LFP in Panel A and all employees at work in Panel B. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Significance level: ***=1%.

unemployment and wage effects and (ii) the mechanisms generating the dynamics. This section ends with the discussion on appropriate labor market policies along with environmental policies in section 4.4.

4.1 The Unemployment and Wage Effects of Carbon Tax

This section explores the unemployment and wage effects of BC's carbon tax. I estimate these effects from equation (1) and report the estimates $\hat{\beta}_1$ in Table 1.

In Panel A, a dummy variable of being unemployed is used as a regressand to estimate the unemployment effect. Samples are restricted to all respondents in the labor force. All specifications include the province and the *Year×Month* fixed effects. The specification in column (2) also includes dummy variables for gender, age, the highest educational attainment, marital status. For space consideration, I only report estimates associated with *BC×Post*, which is the parameter of our interest $\hat{\beta}_1$.

The results suggest that the unemployment effect of BC's carbon tax is moderate. The estimates indicate that the unemployment rate increases by 1.3 percentage points subsequent to BC's carbon tax, and the estimates are insensitive to the additional controls of individual characteristics. I report standard errors in parentheses that are clustered at the level of province, gender, the highest educational attainment, and marital status. I also estimate standard errors using a wild bootstrap procedure. All the estimates are statistically significant at the one percent level regardless of the procedure used to estimate the standard

Table 2: The Average Incumbent Wage Effects of BC's Carbon Tax

Difference-in-Differences Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A			
BC×*Post*	-0.001	-0.001	-0.003	-0.003	-0.004	-0.007	-0.009
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Adjusted R^2	0.034	0.038	0.042	0.046	0.050	0.055	0.058
				Panel B			
BC×*Post*	-0.002	-0.001	-0.003	-0.004	-0.005	-0.008	-0.010*
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R^2	0.361	0.324	0.300	0.284	0.275	0.269	0.266
				Panel C			
BC×*Post*	-0.003	-0.003	-0.003	-0.002	-0.004	-0.007	-0.010**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Adjusted R^2	0.444	0.409	0.386	0.370	0.360	0.352	0.347
Average Wage	21.64	22.62	23.25	23.75	24.11	24.46	24.76

Note: Dependent variables are ln(hourly wage rate). The sample of examination is restricted to employees with over t years of tenure in Column (t), where $t \in \{1, 2, ..., 7\}$. The pre-policy period is defined as July 2005-June 2008 in each column. The post-policy period is defined as the first tth year of the policy in Column (t). The numbers of observations range from 1,646,120 in Column (1) to 1,945,157 in Column (7). Panel A includes province and *Year*×*Month* fixed effects. Panel B also includes dummy variables for gender, age, the highest educational attainment, and marital status. I include a dummy variable for industry in Panel C. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. I report the average wage of the corresponding sample in BC during the pre-policy period at the bottom of the table. Significance level: **=5% & *=10%.

error.

I conduct analogous analyses to estimate the average wage effect, and the results can be found in Panel B. The logarithm of an hourly wage rate at the main job is used as a regressand. Samples are restricted to employees at work. Specifications in Columns (1) and (2) are identical to the specifications of the corresponding columns in Panel A. I also include a dummy variable for the industry in Column (3) to control for variations in industry structures. Again, I only report the parameter of our interest $\hat{\beta}_1$ for space consideration. Similar to the unemployment effect, the results suggest that the average wage effect is moderate. The estimates reveal that BC's average wage falls by 2.4 percent following the policy. Again, the estimates are robust to model specification and are statistically significant at the one percent level regardless of procedures used to estimate the standard error.

Next, I explore the mechanism through which the adverse wage effect operates. According to the sticky wage theory (Keynes, 1936; Fischer, 1977; Taylor, 1980), incumbent wages are downward rigid. Part of the reason is that wages are specified on labor contracts. Wages are adjusted upon contract renewals. Hence, the shock of BC's carbon tax may end up depressing labor demands without cutting incumbent wages. If the incumbent wage effect of BC's carbon tax happens to be negligible, the adverse wage effect documented in Table 1 is likely attributable to new hires.

In light of the wage rigidity, I explore the wage effects on incumbent workers and new hires, separately. While the documentation of wage losses created environmental policies is rare in the literature, this analysis provides an opportunity to estimate the incumbent and hiring wage effects of an environmental policy. Meanwhile, this analysis provides an opportunity to investigate the mechanism of wage adjustments. In particular, it answers whether wages are adjusted through contract renewals with incumbent workers or new contracts with new hires. Both the documentation of the wage effects and the revelation of the underlying mechanism enhance our understanding of labor market responses to environmental policies.

Moreover, most of the contributions in the literature identify nominal wage rigidity and wage procyclicality from recessions in the macro-labor literature (Goette et al., 2007; Carneiro et al., 2012; Martins et al., 2012; Haefke et al., 2013; Barattieri et al., 2014; Elsby et al., 2016; Elsby and Solon, 2018) or firm-level idiosyncratic shocks in the literature on job displacement (Jacobson et al., 1993; Hijzen et al., 2010; Schmieder and Von Wachter, 2010; Walker, 2013; Curtis, 2017; Farber, 2017). The present analysis bridges between the two strands of literature: it studies wage responses to an aggregate shock of a carbon tax as in the macro-labor literature, and it zooms into one episode—BC's carbon tax—to estimate the causal effects as in the literature on job displacement.

However, the analysis of the incumbent wage effects could be tricky. Ideally, the analysis restricts the sample to incumbent workers—employees who were hired prior to the policy. Nevertheless, the analysis is not as straightforward as it sounds. For example, when I restrict the sample to employees with at least one year of tenure, the entire sample in the first year of the policy is hired prior to the policy. In the second year, a fraction of the sample is hired after the policy because the sample includes employees working with their current employers shorter than two years (but definitely over a year). In general, if the sample is restricted to employees with at least t years of tenure, the same problem exists from the $t+1$th year of the policy onwards.

Hence, I conduct multiple analyses to solve the problem. In each analysis, I restrict the sample to employees with at least t years of tenure to analyze the average incumbent wage effect up to the tth year of the policy. Therefore, the sample period includes the three years before the policy and the first tth years of the policy. I follow a similar procedure as before to estimate the average incumbent wage effects from equation (1) and report the estimate of $\hat{\beta}_1$ in Column (t) of Table 2. For example, in Column (3), the sampled employees had worked over three years with their employers at the time of the survey, and the samples were collected between July 2005 and June 2011. In this way, we are certain that the entire sample of examination is hired prior to the policy.

Moreover, I conduct analogous analyses to ensure results are insensitive to model specifications. While Panel A only controls for province and *Year*×*Month* fixed effects, specifications in Panel B include dummy variables for gender, age, the highest educational attainment, and marital status. In addition to these individual characteristics, Panel C also controls for a dummy variable for the industry.

The analysis provides evidence on incumbent wage rigidity. For example, the estimates are all materially insignificant in Columns (1) through (6) in Panels A, B, and C: they are all less than 0.008 in magnitude, suggesting that BC's carbon tax, on average, reduces the wage of incumbent workers no more than 0.8 percent. Meanwhile, these estimates are all statistically insignificant at any conventional level,

Table 3: The Average Hiring Wage Effects of BC's Carbon Tax

Difference-in-Differences Analysis

	(1)	(2)	(3)
BC×*Post*	-0.049***	-0.049***	-0.044***
	(0.013)	(0.006)	(0.006)
Adjusted R^2	0.078	0.382	0.482
Province Fixed Effect	yes	yes	yes
Year×*Month* Fixed Effect	yes	yes	yes
Gender		yes	yes
Age		yes	yes
Educational Level		yes	yes
Marital Status		yes	yes
Industry			yes

Note: BC's hourly wage rate are CAD$15.38 in the pre-policy period. Dependent variables are ln(hourly wage rate). Samples are restricted to all employees at work. $N = 1,171,228$. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Significance level: ***=1%.

providing no statistical support on the adverse wage effects on incumbent workers in the first six years of the policy. Although the estimates in Column (7) are statistically significant at the ten percent and five percent levels in Panels B and C, the statistical significance is largely dependent on model specifications and the estimates are economically small, less than 0.01. The adverse incumbent wage effect, if exists, is tiny and emerges after seven years of the policy. Therefore, it is safe to conclude that this analysis finds no evidence on any incumbent wage effects at least in the first six years of the policy.

One may wonder that the negligible wage effect may simply reflect the weak economic impacts of environmental policies. Nevertheless, the results in Table 1 do reveal the wage losses created by BC's carbon tax. While such the wage effect cannot be found among incumbent workers, it is important to estimate the wage effect on new hires, the counterpart of incumbent workers in employment.

Next, I explore the wage effect of BC's carbon tax on new hires. I follow Carneiro et al. (2012) to define new hires as workers with less than one year of tenure. According to this definition, the sample of new hires in the post-policy period may be hired prior to the policy. For example, workers, who are observed in January 2009, may have worked with their employers over six months (but definitely less than a year)—they are hired prior to the policy. In other words, a proportion of the sample between July 2008 and June 2009 are hired prior to the policy. Therefore, I exclude the sample between July 2008 and June 2009 from my analysis to ensure that new hires in the post-policy period are hired after the policy. I estimate the average hiring wage effects using equation (1).

The result provides strong evidence on the adverse hiring wage effect. Table 3 reports the estimates of our interest $\hat{\beta}_1$. These estimates are statistically significant at one percent level regardless of model specifications and procedures used to estimate the standard error; meanwhile, they are economically significant, ranging from -0.049 to -0.044, revealing the average hiring wage losses of 4.4-4.9 percent.

The analyses on incumbent and hiring wage effects are important for three reasons. First, it, once again, confirms that environmental policies have strong impacts on the aggregate labor market. In addition to the extensive documentation of job losses created by environmental policies in the literature, the result indicates that the associated wage losses are significant as well. Second, the documented hiring wage effect supports incumbent wage rigidity as a source of the negligible wage effect. Since BC's carbon tax results in hiring wage plunges, the negligible incumbent wage effect is unlikely due to the weak labor market impacts of environmental policies. Instead, the negligible effect is more likely attributable to the nominal wage rigidity among incumbent workers.

Third, the analysis uncovers the mechanism through which the wage effect operates. Although an environmental policy depresses labor demands, incumbent wages are unaffected due to the nominal wage rigidity. It is important to highlight that the rigidity lasts at least six years, over which typical labor contracts are expired and renegotiated multiple times. The long-lasting rigidity casts doubt on the widespread view of a nominal wage rigidity (Barro, 1977; Fischer, 1977; Thomas and Worrall, 1988): it casts doubt on labor contracts as a cause of nominal wage rigidity and casts doubt on the renegotiation of labor contracts upon contract renewals as a channel of wage adjustments. On the contrary, the analysis provides sound evidence on hiring wage plunges: wage adjustments to environmental policies operate mainly (if not exclusively) through labor turnovers.

4.2 The Dynamics of Unemployment Effects

This section explores the dynamics of the unemployment effect. The purpose of this exercise is twofold. First, it provides us with an opportunity to estimate the extent to which the unemployment effects of environmental policies 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 the trends in the unemployment rate are parallel between BC and the ROC prior to the policy, the trend likely remains parallel immediately following the policy. In other words, this analysis provides important information on the validity of the common trend assumption. Moreover, if the documented unemployment effect is driven by BC's carbon tax, I expect the unemployment effect appears following the implementation of the policy and the effect may grow in the first few years because the tax rate increases annually. Hence, this analysis provides evidence on the source of the documented unemployment effect.

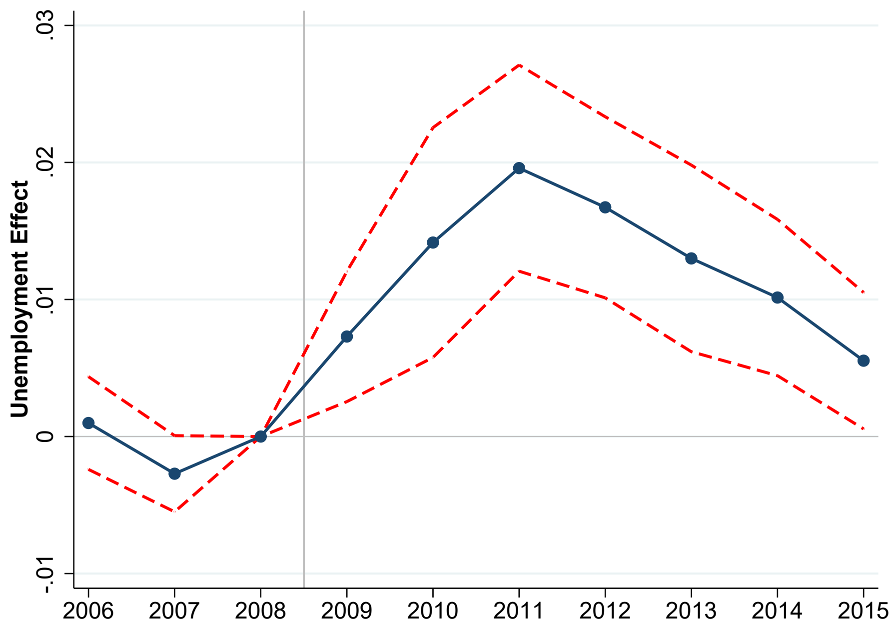
To estimate the dynamic effects, equation (1) is extended by replacing $BC_j \times Post_t$ with a full set of $BC_j \times d_t$ interaction terms as follows:

$$Y_{ijt} = \alpha + \sum_{t \neq 2008} \beta_t (BC_j \times d_t) + X_{ijt}^T \gamma + \eta_j + \delta_t + \varepsilon_{ijt}. \tag{2}$$

Since BC's carbon tax was introduced in the mid of 2008, I define a year beginning from July one year earlier. For example, the year of 2006 is defined as the period between July 2005 and June 2006. d_t is a dummy variable for year t. The year of 2008 is excluded because the period of July 2007-June 2008 serves as a reference year. The inclusion of a constant term α and the province and the *Year*$\times$*Month* fixed effects normalizes the difference in Y_{ijt} between BC and the ROC to zero in the reference year.

I control for individual characteristics including age, gender, the highest educational attainment, and marital status. The conclusion remains unchanged without the additional controls of the individual characteristics. For space consideration, I report the estimates in Table C1 in Appendix C. Standard errors are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Similar to the analyses above, we are interested in the estimates $\hat{\beta}_t$.

Figure 3: The Dynamics of Unemployment Effects



Note: The regressand is a dummy variable of being unemployed. Samples are restricted to the labor force. The vertical line represents the first month of the carbon tax policy (July 2008). The dashed line represents a 95 percent confidence interval.

Figure 3 displays the estimates of $\hat{\beta}_t$. The vertical line represents the first month of the policy (i.e., July 2008). Each dot represents the main DID estimate $\hat{\beta}_t$ from equation (2) in the corresponding year. The dot is zero in 2008 because the difference in the unemployment rate is normalized to zero in a reference year.

A number of points emerge from the figure. First, the figure provides suggestive evidence on the common trend assumption. The dots prior to the policy are economically and statistically not different from zero, suggesting that the differences in the unemployment rate between BC and the ROC are similar. In other words, BC and the ROC share a parallel trend in their unemployment rates prior to the policy. And the trend likely remains parallel subsequent to the policy. Second, BC's unemployment rate increases subsequent to the policy. The estimates are all positive and are statistically significant at the five percent level in the post-policy period. The result reveals that BC's unemployment rate starts to grow immediately following the policy. Third, the estimates increase in the first few years, suggesting that the unemployment effect grows over time. This increasing unemployment effect coheres with the tax incentive created by the policy because the carbon tax rate grows annually in the first few years. Altogether, these three features provide evidence that (i) the common trend assumption is likely satisfied, and (ii) the documented unemployment effect is likely driven by BC's carbon tax policy. In other words, we can interpret the estimates as the causal effect of BC's carbon tax on the unemployment rate.

Nevertheless, it is puzzling that the unemployment effect decays in the later stage of the policy. When the carbon tax rate stopped to increase, firms are still required to pay the tax. What makes the unemployment effect decay? I will answer this question by exploring whether the unemployment is increased because employees are more likely laid off (i.e., the job-loss effect) or because unemployed workers find it harder to get hired (i.e., the job-finding effect). I decompose the total unemployment effect into the job-loss and job-finding effects. That is, I quantify the unemployment effect that is solely driven by each of the two effects.

I perform this decomposition exercise for two reasons. First, it helps economists understand the functioning of labor markets by answering how unemployment is created and why the unemployment effect decays. Second, it serves as guidance on the design of social benefits to redistribute tax revenues by answering who is suffered from the unemployment effect. If the unemployment effect happens to be driven mainly by the job-loss effect, the carbon tax increases the likelihood of being laid off among employees. In contrast, if the carbon tax increases unemployment through the job-finding effect, it decreases the chance of being hired among unemployed workers.

Here, I brief the procedure of the decomposition. According to the literature on a job search theory (Pissarides, 2000; Shimer, 2012), the evolution of unemployment is given by $(1-u)Lx - uLf$, where L is the stock of labor force and u is the unemployment rate. $(1-u)L$ and uL are the stocks of employment and unemployment. x and f are a job-separation and a job-finding rate. $(1-u)Lx$ and uLf measure the stocks of workers flowing out of and flowing into employment, respectively. The law of motion equation for an unemployment rate can be written by $\triangle u = (1-u)x - uf$, where $\triangle u$ is the variation in the unemployment rate. When $\triangle u = 0$, the steady-state unemployment rate u is given by

$$u = \frac{f}{x+f}.$$

To obtain the unemployment effect of BC's carbon tax, I totally differentiate the above equation with respect to BC's carbon tax policy $\mathcal{P}$ and rearrange terms to yield the following equation:

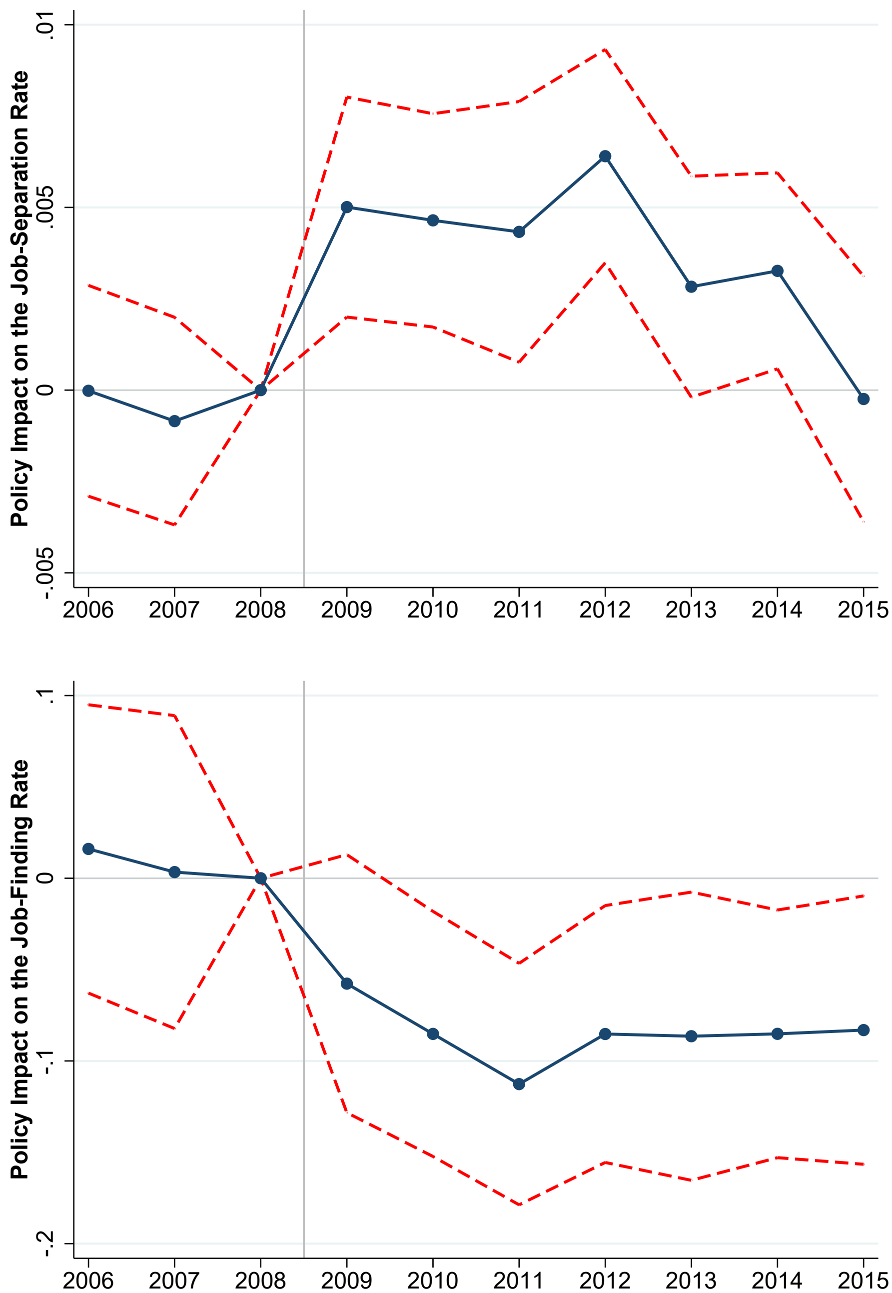
$$\frac{du}{d\mathcal{P}} = u(1-u)\left(\frac{dx}{d\mathcal{P}} - \frac{df}{d\mathcal{P}}\right), \tag{3}$$

where $dx/d\mathcal{P}$ and $df/d\mathcal{P}$ are the policy impacts on job-separation and job-finding rates. When f, x, and u are set to their pre-policy levels, $du/d\mathcal{P}$ can be interpreted as the total unemployment effect of the policy $\mathcal{P}$. To obtain the unemployment effect that is solely driven by a job-finding effect, I set $dx/d\mathcal{P} = 0$ to shut down the channel through which BC's carbon tax increases unemployment through a job-loss channel. Similarly, I obtain the job-loss effect by setting $df/d\mathcal{P} = 0$ to shut down the channel through which BC's carbon tax increases unemployment through a job-finding channel.

This decomposition exercise requires policy impacts on two job transition rates. I first follow Shimer

(2012) to obtain the two monthly job transition rates. I brief the procedure to execute the estimation of job transition rates in Appendix E. Next, I estimate the policy impacts on the two transition rates from equation (2), with a job-separation rate and a job-finding rate being regressands. I report the parameter of our interest $\hat{\beta}_t$ in Figure 4.

Figure 4: The Dynamics of the Policy Impacts on Job Transition Rates



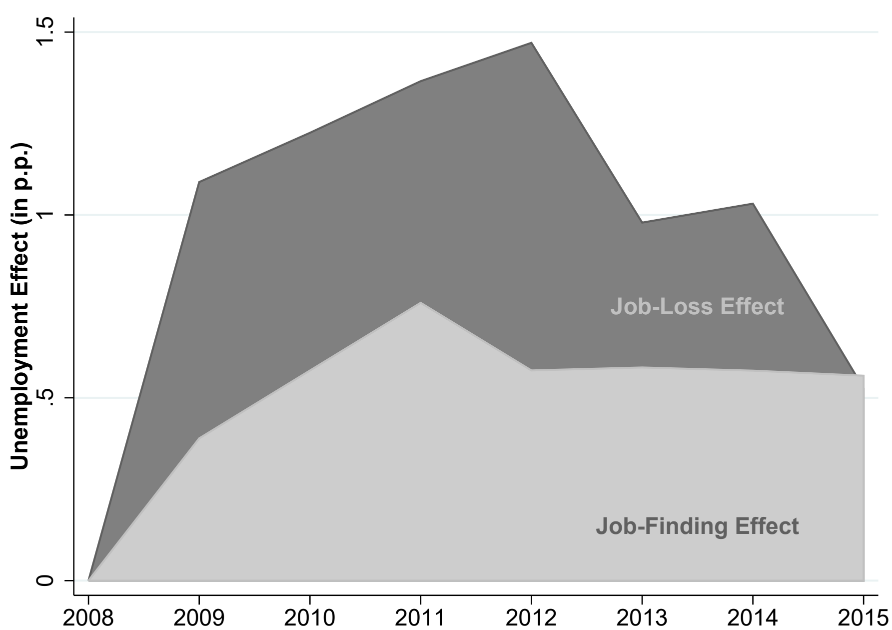
Note: A job-separation rate and a job-finding rates are regressands in the top and the bottom panels, respectively. The vertical line represents the first month of the carbon tax policy (July 2008). The dashed line represents a 95 percent confidence interval.

The results can be summarized into three parts. The first part enhances our understanding of why BC and the ROC follow a parallel unemployment trend. During three successive years prior to the policy, the estimates are all economically and statistically close to zero, suggesting that the differences in the two rates between BC and the ROC are similar. In other words, BC and the ROC share parallel pre-policy trends in the two job transition rates. According to equation (3), an unemployment rate depends on two job transition rates. While the trends in the transition rates are parallel in BC and the ROC, it is unsurprised that their unemployment trends are also parallel.

Second, the policy impacts on the two rates emerge immediately following the policy. While the job-separation rate increases right after the policy, her job-finding rate falls at the same time. Third, these policy impacts follow an increasing trend in the first couple of years, consistent with the tax incentives created by BC's carbon tax. Once BC's carbon tax rate stopped to increase, the policy impacts on the two rates diverge. The high job-separation rate returns to its pre-policy level, whereas the job-finding rate remains low. However, it is difficult to quantitatively interpret the estimates. What does it mean by the increase in the job-separation rate by 0.005 and the decrease in the job-finding rate by 0.1? How much does unemployment increase through each of the two channels?

To answer these questions, I quantify the job-loss and the job-finding effects in terms of the unemployment effect using equation (3). I calibrate f, x, and u to the job-finding rate, the job-separation rate, and the unemployment rate in BC during July 2007-June 2008, the reference year in the above regression analysis. They are 0.6376, 0.0307, and 0.0450, respectively. I plug in the policy impacts $\hat{\beta}_t$ into equation (3) and yield Figure 5.

Figure 5: The Flow Decomposition of Unemployment Effects



Note: The light and dark gray areas measure the unemployment effect through the job-finding and job-loss channels.

Altogether, this analysis provides a more complete picture of the unemployment effect. First, both job-loss and job-finding effects are indispensable components of the total unemployment effects. The estimates reveal that each of the two effects accounts for half of the total effect in the early stage of the policy. Each of them is quantitatively essential in explaining the total unemployment effect. In other words, the carbon tax increases unemployment because job losses become increasingly common, and finding a job becomes harder.

Second, the result reveals that the unemployment rate increases through both channels. While the total unemployment effect grows in the first few years, the two unemployment effects also follow increasing trends. The total effects grow in the first few years because the increase in the carbon tax rate intensifies the unemployment effects through the two channels.

Third, the unemployment effect decays mainly because of the job-loss effect. While the total unemployment effect decays, the job-loss effect decays at the same time. In contrast, the job-finding effect stays the same. This finding answers our previous question—the unemployment effect decays almost entirely because the job-loss effect is short-lived. A small unemployment effect persists mainly because the job-finding effect is long-lived.

The results make economic sense. Intuitively, a carbon tax increases the marginal cost of production through energy use, reducing labor demand. Hence, many positions become unprofitable. Workers are laid off without lags or until their contracts expire. When the unprofitable positions are gone, firms have no more incentive to lay off workers, explaining why the job-loss effect is short-lived, and why the unemployment effect quickly decays. Meanwhile, firms take a carbon tax into account to make hiring decisions, explaining why the job-finding effect emerges once the carbon tax rate begins to rise. The increase in the carbon tax rate tightens the labor market, making job-hunting harder. As a result, the unemployment effect through the job-finding channel increases. Although the carbon tax rate stops to increase, this shock is permanent. The job-finding rate remains low, explaining why the job-finding effect is long-lived and why the small unemployment effect persists.

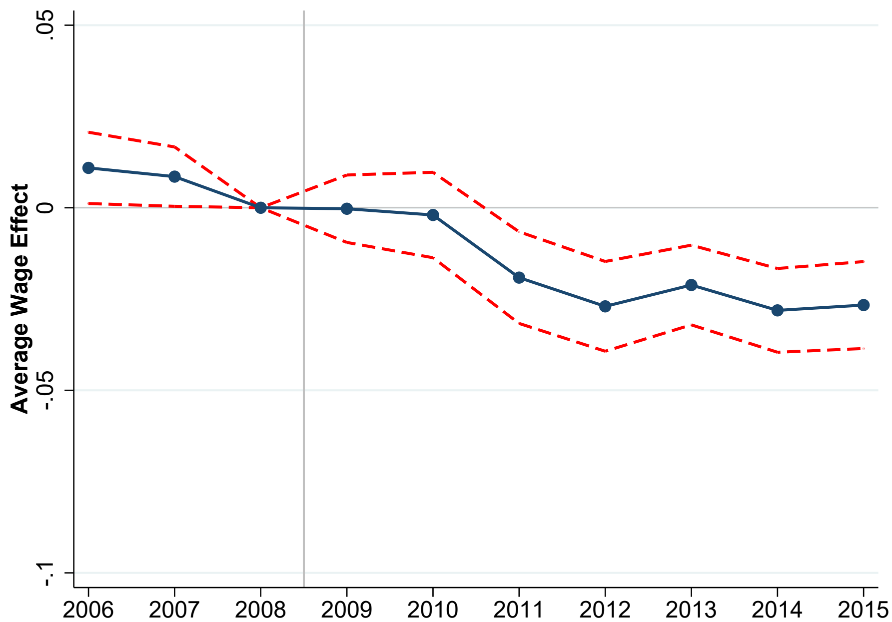
To conclude, this section documents the dynamics and mechanisms of the unemployment effect. It concludes that a complete understanding of the unemployment effect requires an explanation of employment flows—the job-loss and job-finding effects.

4.3 The Dynamics of Wage Effects

This section studies the dynamics of wage effects. It first uncovers the dynamics, followed by the exploration of the mechanisms generating the observed wage changes. I will show that the average wage effect emerges with lags. I will provide evidence that a complete understanding of the observed wage changes requires an explanation of employment flows.

I follow the same procedure as in section 4.2 to uncover the dynamics of wage effects from equation (2). The treatments of model specification and standard errors are the same. The sample is restricted to employees at work. Similar to section 4.2, the estimates are insensitive to the additional control of individual characteristics. For space consideration, I report the estimates from models with various specifications in Table C2 in Appendix C.

Figure 6: The Dynamics of the Average Wage Effect



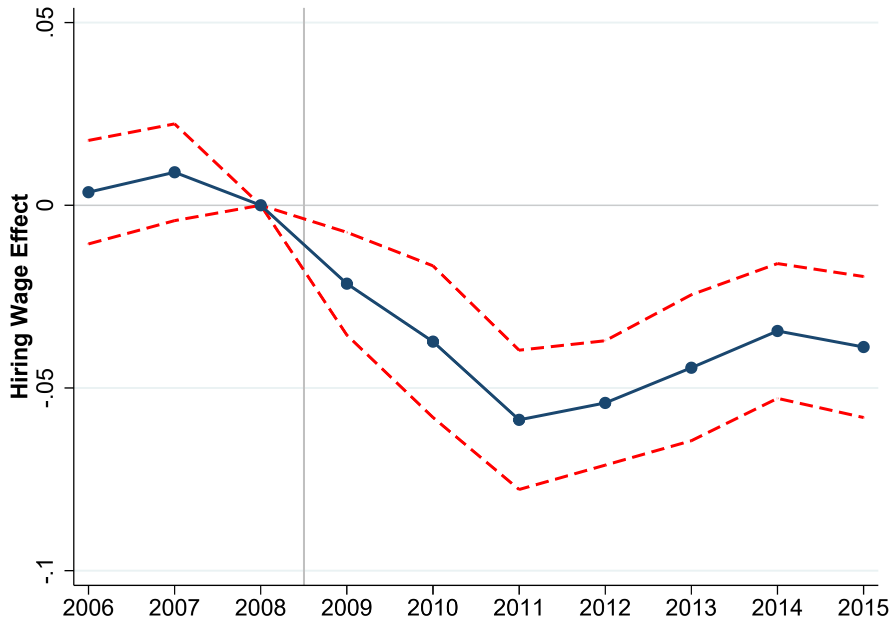
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to employees at work. The vertical line represents the first month of the carbon tax policy (July 2008). The dashed line represents a 95 percent confidence interval.

Figure 6 presents the parameter of our interest $\hat{\beta}_t$. Prior to the reference year, the estimates are marginally statistically significant at the five percent level but not by stricter criteria. These estimates are materially small, about one percent. In the first two years of the policy, the estimates are all economically and statistically not different from zero at any conventional level. This analysis cannot find any statistical evidence on the adverse wage effect in the early stage of the policy. The result also suggests that the adverse wage effect, if exists, is negligible in magnitude. In the later stage of the policy, the estimates drop significantly: the results suggest that BC's average wage drops by two to three percent in the later period.

A natural question arises: why should I believe that the adverse wage effect in the later stage of the policy is driven by BC's carbon tax but not unobserved shocks that are orthogonal to the policy? The rest of this subsection will answer this question; it provides evidence to explain why the wage effect of an environmental policy comes with lags. Answering this question is informative for two reasons. First, of course, it provides support that the documented wage effects are driven by BC's carbon tax. Second, it may help the literature untie the knot. If their wage effects of environmental policies generally come with lags, the traditional DID approach, by comparing a couple of years before and after the policy, naturally fails to find any meaningful wage effect as in Figure 6. Hence, this section helps us understand what hinders the literature from identifying the wage effects of environmental policies.

Inspired by section 4.1, I explore the dynamics of hiring and incumbent wage effects, separately. I uncover the dynamics from equation (2), with the sample restricted to new hires and incumbent workers at work, respectively.

Figure 7: The Dynamics of Hiring Wage Effects



Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to new hires at work. The vertical line represents the first month of the carbon tax policy (July 2008). The dashed line represents a 95 percent confidence interval.

Using the sample of new hires, I estimate and report $\hat{\beta}_t$ in Figure 7.[9] The figure reveals that BC's carbon tax reduces the average hiring wage. First, the estimates suggest that BC and the ROC share a parallel trend in the average hiring wage prior to the policy. Look at the first two estimates. They are statistically insignificant at any conventional level, providing no statistical support on the significant differences in the hiring wage gap between BC and the ROC before the policy. Moreover, the estimates are materially small, revealing that the divergence of their hiring wage trends, if exists, is negligible in magnitude. Therefore, it is safe to conclude that the hiring wage trends between BC and the ROC are parallel during the three successive years before the policy, suggesting that the trend likely remains parallel afterward.

Once the carbon tax is introduced, the estimates turn negative and are all statistically significant, suggesting that the carbon tax policy does reduce the average hiring wage immediately after the policy. Moreover, the estimates grow in the first few years. With the increasing carbon tax rate, the marginal cost of production continues to grow, further depressing labor demands. As a result, the labor market continues to adjust the average hiring wage downwards in the early stage of the policy. Once the carbon tax rate stopped to increase in 2012, BC's average hiring wage stayed low afterward. Since 2012, the

[9] I report the values of the estimates in Table C3 in Appendix C.

average hiring wage has been cut by about four percent. The long-lasting feature of the wage cut may reflect wage adjustments after the re-optimization of firms' profit functions under a rather permanent shock of BC's carbon tax.

These results are astonishing. The entire dynamics of the hiring wage effect largely cohere with the tax incentives created by BC's carbon tax, providing credible support that environmental policies, such as BC's carbon tax, cut hiring wages. So, what masks the average wage effect? Why are the estimated average wage effect negligible in the early stage of the policy as shown in Figure 6? I will answer this question by uncovering the dynamics of the incumbent wage effect.

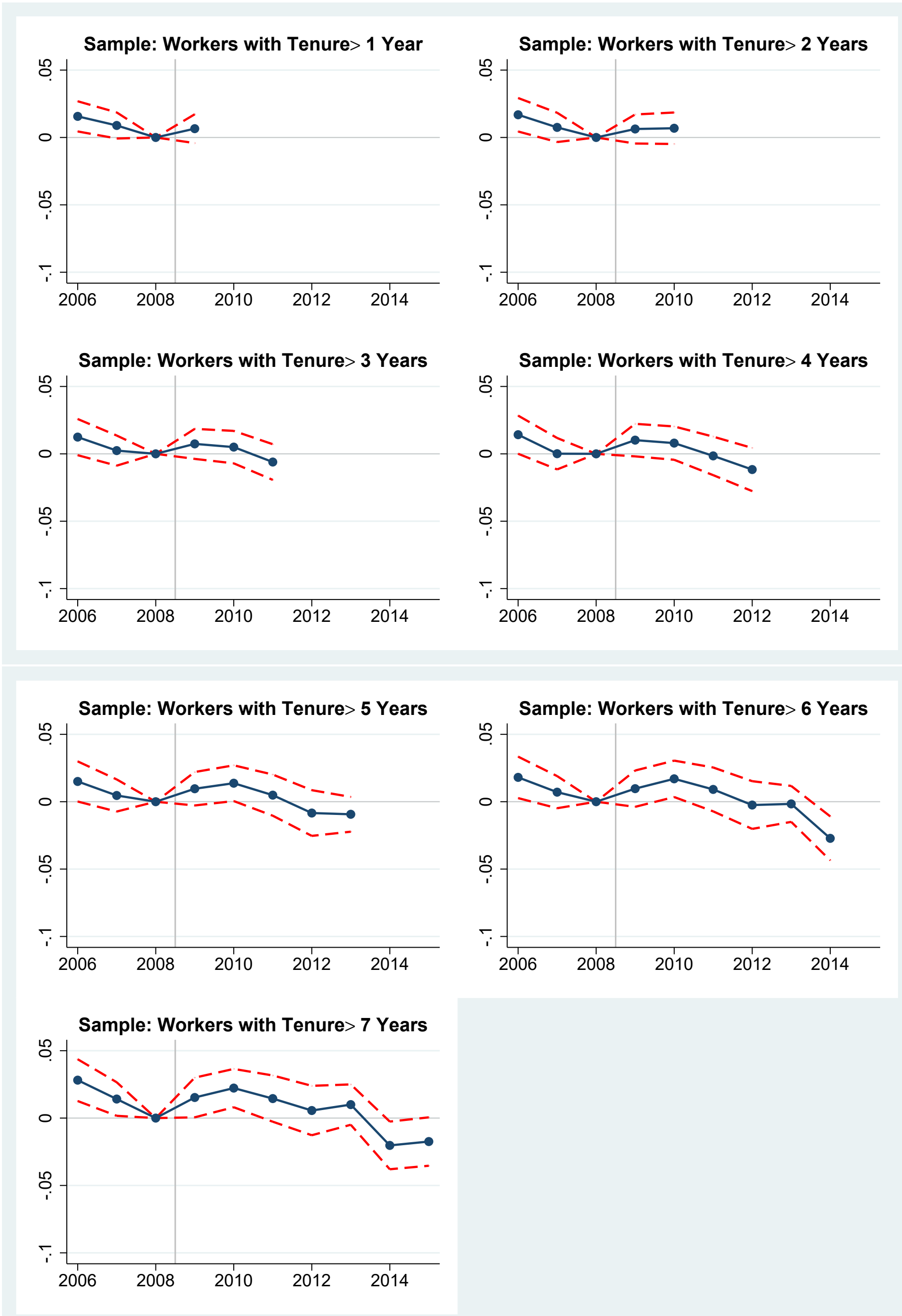
I use the seven different samples and follow a similar procedure as before to uncover the dynamics of incumbent wage effects from equation (2). The estimates from seven different samples are reported in Figure 8. It is important to note that most of the estimates are economically and statistically insignificant at conventional levels, with a caveat that the estimate is negative in 2014, five years after the policy. The estimates, once again, confirm that statistical evidence on any incumbent wage effect is at best weak at least in the first five years of the policy, revealing the nominal wage rigidity for quite a long period.

Now, it becomes clear why the average wage effect delays and grows gradually. Figure 9 presents the composition of new hires and incumbent workers in BC's employment. By comparing months of tenure and the number of months after the policy, I define new hires as employees who are hired after July 2008 and classify the rest of the employment as incumbent workers. Initially, a majority of employees are incumbent workers in employment; they are hired before the policy. While their wages are completely rigid, no statistical evidence on the average wage effect can be found in the early stage of the policy. Meanwhile, the average hiring wage is found to plunge as shown in Figure 7 and the share of new hires grows gradually as shown in Figure 9. As a result, the average wage continues to decrease with the gradual increase in the proportion of new hires in employment, explaining why the average wage effect emerges later and grows gradually.

Intuitively, a carbon tax policy increases the marginal cost of production, thereby depressing labor demands. Firms may desire to cut wages. For reasons that are out of the scope of this paper, firms are unwilling to cut incumbent wages; instead, they lay off workers, explaining why the separation rate is found to increase after the policy as shown in section 4.2. Meanwhile, firms recruit new hires at lower hiring wages. Wage adjustments operate through this slow process of labor turnover.

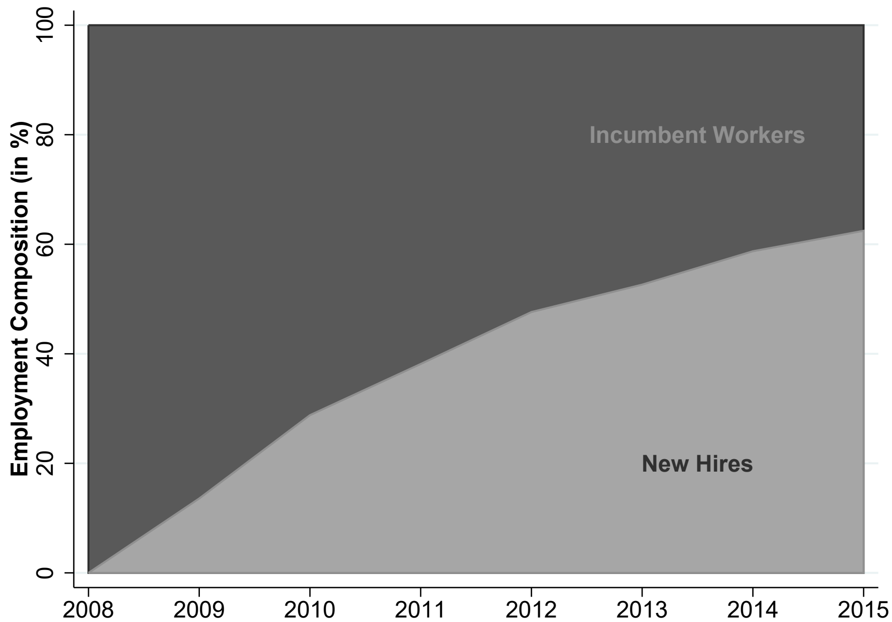
In sum, this section documents the wage dynamics: the average wage effect is absent in the early stage of the policy, emerges later, and grows gradually. I present evidence on a long period of the incumbent wage rigidity, the substantial hiring wage effect, and the gradual increase in the share of new hires in the employment. Altogether, the evidence enhances our understanding of the functioning of labor markets by answering how wage adjustments operate through labor turnovers—employment flows. Moreover, the documented hiring wage plunge points out why the public is concerned with substantial wage losses potentially created by environmental policies. Meanwhile, this section highlights the difficulty in the estimation of the average wage effect of environmental policies. Since wage adjustments operate through a slow process of labor turnover, the average wage effect emerges with lags. This slow process of wage adjustments partly explains why the literature may fail to capture the average wage

Figure 8: The Dynamics of Incumbent Wage Effects



Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. The vertical line represents the first month of the carbon tax policy (July 2008). The dashed line represents a 95 percent confidence interval.

Figure 9: The Composition of Employment in BC



Note: Employees who are hired after July 2008 are defined as new hires. The rest of the employment is defined as incumbent workers.

effect, especially in the short run.

4.4 Policy Implications

Thus far, this paper has documented evidence on the labor market adjustments to BC's carbon tax and revealed the mechanisms of the adjustments. This section discusses possible labor market policies to accommodate with environmental policies.

For long, the literature suggests extending the entitlement periods of unemployment benefits in economic downturns (Jung and Kuester, 2015; Mitman and Rabinovich, 2015; Landais et al., 2018a,b). The rationale is to smooth the consumption of unemployed workers over a prolonged unemployment period. For example, the United States Congress increased the benefits from a maximum of 26 to as many as 99 weeks in the 2008 recession. While BC's carbon tax could increase the unemployment rate by as high as two percentage points, should policymakers generally give unemployment insurance a longer lifeline along with environmental policies? I will answer this question.

To begin with, let me introduce two measures of unemployment. Following the literature, I define short-term unemployment as unemployment with spells less than or equal to 26 weeks and define the rest of unemployment as long-term unemployment. Hence, the short-term unemployment rate measures the proportion of short-term unemployed workers in the labor force. In light of section 4.2, it is unclear whether the unemployment effect is mainly driven by short- or long-term unemployment. The job-loss effect increases the share of short-term unemployment among unemployed workers. In contrast, the job-finding effect prolongs the average duration of unemployment, thereby increasing unemployment through long-term unemployment. While the two effects also increase unemployment, the extension of benefit periods helps mainly in the latter case scenario. The findings of the unemployment effects from sections 4.1 and 3 are insufficient to draw any conclusion concerning the length of benefit periods. It is, therefore, necessary to explore the short- and long-term unemployment effects of BC's carbon tax to address the policy question.

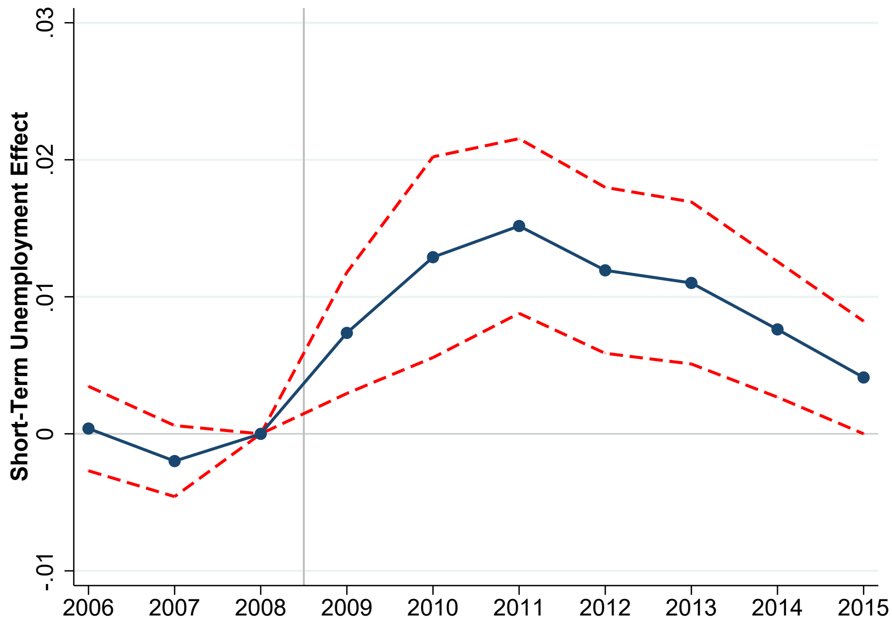
I estimate the two effects from equation (2). When estimating the short-term unemployment effect, I use a dummy variable for being a short-term unemployed worker as a regressand. Similarly, a dummy variable for being a long-term unemployed worker is used as a regressand to estimate the long-term unemployment effect. I follow the same procedure that estimates the dynamics of unemployment effects in section 4.2 so that their treatments of model specification, standard errors, and the sample of the examination are all identical. I report the estimates of $\hat{\beta}_t$ in Figure 10 and Tables C4 and C5 in Appendix C.

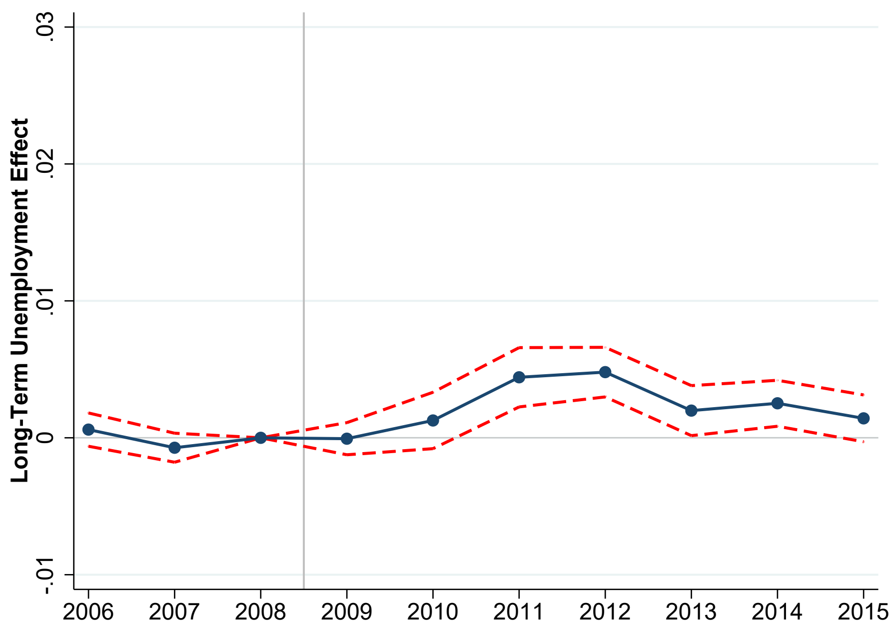
Several points emerge from the figure. First, the dynamics of the short- and long-term unemployment effects are similar to the dynamics of the overall unemployment effects—they increase in the first few years and decrease after 2012. Second, the short-term unemployment effect at least triples the long-term one. While the short-term unemployment effect peaks at 1.5 percentage points, the long-term unemployment effects are no more than 0.5 percentage point. The carbon tax increases unemployment mainly through short-term unemployment—the job-loss effect is more dominating in this regard. However, the comparison with the pre-policy composition of unemployment may tell a slightly different story. Short-term unemployment constitutes 88 percent of unemployment in the pre-policy period, whereas it, on average, accounts for 82 percent of the increased unemployment. That is, about 88 out of every 100 unemployed workers were short-term unemployed before BC's carbon tax. When the tax creates 100 unemployment, 82 of them are short-term unemployed. The two proportions are comparable: the job-loss effect is neck and neck with the job-finding effect in creating unemployment. Although the unemployment effect of an environmental policy could be significant, the policy seems to increase mainly short-term unemployment.

To better answer this question, I further breakdown the unemployment effect of BC's carbon tax by unemployment spell. I calculate the unemployment effects for workers with the unemployment durations of 1-3 months, 4-6 months, 7-9 months, 10-12 months, and over a year. Notice that the first two unemployment effects add up to the short-term unemployment effect, and the sum of the rest yields the long-term unemployment effect. I present the breakdown in Figure 11.

Several points deserve to be highlighted. First, over half of the increased unemployment can find a job within three months. During the seven years of the post-policy period, workers unemployed for 1-3 months account for over 52 percent of the increased unemployment. On average, they account for 61 percent of the unemployment effect. Second, a majority of the increased unemployment find a job within half a year. On average, only 20 percent of the increased unemployment fails to find a job in six months. While the standard time-lengths of unemployment benefits cover 8.2 months in Canada and six months in the United States, these coverages help out 80 percent of the increased unemployment to smooth

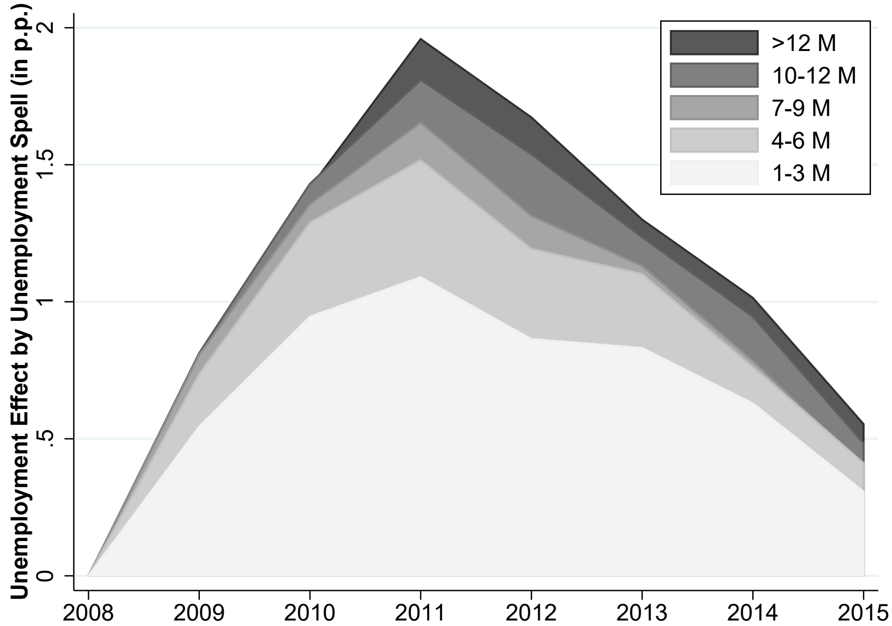
Figure 10: The Dynamics of Short- and Long-Term Unemployment Effects





Note: The regressands are dummy variables of being short-term unemployed and long-term unemployed. Samples are restricted to the labor force. The vertical line represents the first month of the carbon tax policy (July 2008). The dashed line represents a 95 percent confidence interval.

Figure 11: Unemployment Effects by Unemployment Spell



Note: Data are from the Canadian LFS July 2005-June 2015.

their consumption over the entire period of unemployment. Third, only 13 percent of unemployment fails to get a job in nine months. In other words, the Canadian standard has smoothed the consumption of about 87 percent of the unemployment spells created by BC's carbon tax. Fourth, nearly all new unemployment is hired within a year—less than three percent fails to get a job. Altogether, the current standards of unemployment benefits will cover 80 percent of complete unemployment spells and cover a large proportion of unemployment spells of the rest 20 percent.

This analysis reveals that the unemployment effect is mainly attributable to the short-term unemployment effect. In other words, BC's carbon tax increases unemployment mainly through the number of unemployment spells, not the duration of unemployment spells. Although the unemployment effect is significant, there is no urge to accommodate environmental policies with the substantial extension of unemployment benefit periods.

Other policy implications are derived from the analyses on the wage effects. Notice that employees are defined as incumbent workers because they keep their jobs. Meanwhile, their wages are unaffected by the policy as indicated in section 4.3. There is no urge to compensate incumbent employees. However, many unemployed workers were laid off in the first place. Once hired, they receive lower hiring wages because of the policy. One should notice that carbon tax policies create revenues for the government. For example, BC's carbon tax generates hundreds of millions of government revenues.[10] Therefore, the government could make use of carbon tax revenues to provide firms with hiring subsidies to speed up hiring processes and to provide new hires with tax credits to reduce potential after-tax wage losses

[10]Readers, who are interested in the carbon tax revenues of BC's policy, are referred to Table A2 in Appendix A.

created by environmental policies.

5 Are the Results Credible?

This section discusses the credibility of the results. BC's carbon tax increases the marginal cost of production through energy use. If the documented labor market responses are driven by BC's carbon tax, I expect the effects are concentrated on energy-intensive industries. I assess the heterogeneous effects across industries in section 5.1. To interpret the documented effects as the causal effects of BC's carbon tax, identifying assumptions are required. I assess the anticipatory effect of the policy in section 5.2 and discuss the validity of the common trend assumption in section 5.3. Section 5.4 discusses and provides further evidence on the sources of the documented effects.

5.1 Assessing the Heterogeneous Effect

This section assesses the heterogeneous effects across industries. This analysis is informative for two reasons. First, it serves as an internal validity check on the results above. If the effects happen to be larger in less energy-intensive industries, it may be reasonable to assume that the documented effects result from unobserved factors that are likely unrelated to the carbon tax policy. Second, it provides an opportunity to estimate the heterogeneous effects across industries, which is relevant to policymakers redistributing carbon tax revenues accordingly.

This assessment requires information on energy intensity. Since the Canadian LFS has no such information, I follow Curtis (2017) to construct an energy intensity index. Notice that the energy intensity is likely endogenized. In response to the carbon tax policy, firms may reduce their energy intensity in the post-policy period. Therefore, I construct the index using an industry-specific energy intensity at their 2007 level, where 2007 is the year prior to the policy.

Following Curtis (2017), I merge three-digit-industry energy intensity data from the 2007 NBER Productivity Database to the Canadian LFS. I follow Curtis (2017) to divide total industry energy expenditure by the total value of shipments for the industry to construct an energy intensity index for 19 different manufacturing industries. As such, the energy intensity index in any given year is identical to its 2007 level. Hence, this index is unaffected by BC's carbon tax policy. Since the NBER Productivity Database only has the information on the energy expenditure for manufacturing industries, I restrict the sample to manufacturing employment in this analysis. To serve as a robustness check, I construct two other measures of energy intensity: the costs of energy expenditure per unit of value-added and per unit of capital expenditure, both of which can be found from the NBER Productivity Database.

To examine the heterogeneous wage effect, I adopt a triple differences approach:

$$\begin{aligned} \mathit{ln(Wage)}_{ijkt} \;=\;& \alpha + \beta_1(\mathit{ln(EI)}_k \times \mathit{BC}_j \times \mathit{Post}_t) + \beta_2(\mathit{ln(EI)}_k \times \mathit{Post}_t) + \beta_3(\mathit{ln(EI)}_k \times \mathit{BC}_j) \\ & + \beta_4(\mathit{BC}_j \times \mathit{Post}_t) + X_{ijkt}^T\gamma + \psi_k + \eta_j + \delta_t + \varepsilon_{ijkt}, \end{aligned} \tag{4}$$

where *Wage* measures the nominal hourly wage rate of respondent i in industry k in province j at time t.

X^T_{ijkt} 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 the 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 is concentrated on industries that are more energy-intensive, $\hat{\beta}_1$ is expected to be negative. It is noteworthy to highlight that $\hat{\beta}_2$ and $\hat{\beta}_3$ also provide important information to us. While $\hat{\beta}_2$ captures the heterogeneous wage effect across industries in the ROC subsequent to BC's carbon tax, $\beta\beta_3$ measures the wage elasticity with respect to an energy intensity in BC prior to BC's carbon tax.

I use the three measures of energy intensity to estimate $\hat{\beta}_1$, $\hat{\beta}_2$, and $\hat{\beta}_3$ from equation (4) and report the estimates in Table 4. The dependent variables are the logarithm of an hourly wage rate. Energy intensity is measured by energy expenditure per unit value of shipments, per unit of value-added, and per unit of capital expenditure in Panels A, B, and C, respectively. Samples are restricted to all employees in manufacturing industries. The average hourly wage of BC's employees in manufacturing industries was CAD$19.02 in the pre-policy period. All specifications include the province, the *Year*×*Month*, and industry fixed effects. The specification in column (2) also includes individual characteristics such as dummies for gender, age, the highest educational attainment, and marital status.

Several points deserve comments. First, the results suggest that the average wage generally increases with an energy intensity. According to Columns (1) and (2), $\hat{\beta}_3$ are positive regardless of the measure of an energy intensity. The estimates are all statistically significant at one percent level. The result indicates that prior to BC's carbon tax, a one percent higher in the energy intensity will increase the average wage by 5.1-6.1 percent.

Second, the results reveal that the heterogeneous wage effects cohere with the tax incentives created by BC's carbon tax. *ln(EI)*×*BC*×*Post* are statistically significantly negative at the one- to the five-percent level of significance. In particular, the estimates indicate that the carbon tax policy reduces the wage elasticity with respect to energy intensity by 2.0-3.1 percent. For each percent higher in the energy intensity of an industry, BC's carbon tax will cause the average wage to drop by 2.0-3.1 percent, bringing down the elasticity from 5.1-6.1 percent to 2.1-3.4 percent. These results provide another sound support that the labor market responses documented in this paper are attributed to BC's carbon tax.

Third, a similar response cannot be found in the ROC. The coefficient estimates of *ln(EI)*×*Post* are materially and statistically insignificant at any conventional level, with the only exception in Column (1) of Panel B. This exception is marginally significant at the ten percent level but not by stricter criteria. The inclusion of individual characteristics shrinks these estimates: they are at least six times smaller than the corresponding coefficient estimates of *ln(EI)*×*BC*×*Post*. I find no statistical evidence that moving from the pre- to the post-policy period, the change in the wage elasticity is associated with the energy intensity of an industry in the ROC. The difference in the wage response between BC and the ROC provides further support that the heterogeneous wage effects are related to BC's carbon tax, not country-specific shocks.

One may concern that the heterogeneous effects across industries are driven by the 2008 financial crisis, not BC's carbon tax. For example, financial crises may hit capital-intensive industries more. If

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

Difference-in-Differences Analysis

Dependent Variable: ln(Hourly Wage)			
	(1)	(2)	(3)
A. EI=Energy Expenditure/Value of Shipments			
ln(EI)×*BC*×*Post*	-0.031***	-0.026***	-0.033***
	(0.011)	(0.010)	(0.013)
ln(EI)×*Post*	-0.006	-0.001	0.002
	(0.004)	(0.003)	(0.004)
ln(EI)×*BC*	0.052***	0.053***	0.076***
	(0.013)	(0.010)	(0.012)
Adjusted R^2	0.188	0.382	0.382
B. EI=Energy Expenditure/Value Added			
ln(EI)×*BC*×*Post*	-0.022**	-0.020**	-0.039***
	(0.011)	(0.009)	(0.015)
ln(EI)×*Post*	-0.008*	-0.003	0.005
	(0.004)	(0.003)	(0.004)
ln(EI)×*BC*	0.051***	0.054***	0.080***
	(0.013)	(0.010)	(0.013)
Adjusted R^2	0.188	0.382	0.382
C. EI=Energy Expenditure/Capital Expenditure			
ln(EI)×*BC*×*Post*	-0.030**	-0.027**	-0.025*
	(0.015)	(0.013)	(0.013)
ln(EI)×*Post*	-0.005	-0.003	-0.003
	(0.005)	(0.004)	(0.004)
ln(EI)×*BC*	0.057***	0.061***	0.058***
	(0.018)	(0.014)	(0.013)
Adjusted R^2	0.188	0.382	0.382
Province Fixed Effect	yes	yes	yes
Year×*Month* Fixed Effect	yes	yes	yes
Industry	yes	yes	yes
Gender		yes	yes
Age		yes	yes
Educational Level		yes	yes
Marital Status		yes	yes
ln(CI)×*BC*×*Post*			yes
ln(CI)×*Post*			yes
ln(CI)×*BC*			yes

Notes: Capital intensity (CI) is measured by capital stock per unit value of shipments, capital stock per unit value added, and capital stock per unit of capital expenditure in Panels A, B, and C, respectively. Capital stock is the sum of structure capital and equipment capital. N=635,765. Samples are restricted to all manufacturing employees. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Significance levels: ***=1%, **=5%, *=10%.

industries with higher levels of energy intensity happen to be more capital-intensive, the estimates may capture the partial wage responses to the financial crisis. Nevertheless, $\hat{\beta}_2$ are found materially and statistically insignificant, providing no statistical evidence on the wage response to the financial crisis through the energy intensity in the ROC. The result suggests that if the heterogeneous wage responses to the financial crisis across energy intensity are similar in BC and the ROC, our estimates are unbiased.

However, what if the wage responses to the financial crisis are different in BC than the ROC? Tightening credit constraints in the 2008 financial crisis may hinder technology upgrading. Hence, industries with tighter credit constraints may not be able to reduce their emission intensity: they are required to pay a higher carbon tax payment. The empirical support for a similar mechanism can also be found in Andersen (2016) and Andersen (2017). As a result, industries facing tighter credit constraints in 2008 may be affected more by BC's carbon tax because of the higher tax payment. The negativity of $\hat{\beta}_1$ may reflect a partial or a full wage response to BC's carbon tax through tigher financial constraints in the 2008 crisis.

To ease the concern, I conduct the analysis by adding the interaction terms of *ln(CI)*$\times$*BC*, *ln(CI)*$\times$*Post*, and *ln(CI)*$\times$*BC*$\times$*Post* in my regression model, where *ln(CI)* is the logarithm of the industry-specific capital intensity at the 2007 level. The inclusion of the interaction terms removes the wage effect through the channel of capital intensity. Table 4 presents the key estimates in Column (3). While the capital intensity is measured by total capital per unit value of shipments, per unit of value-added, and per unit of capital expenditure in Panels A, B, and C, total capital is measured by the sum of structure capital and equipment capital. The conclusion remains unchanged—the adverse wage effects are concentrated on industries with higher levels of energy intensity in BC, not in the ROC.

This section provides evidence on the coherence between the documented heterogeneous effects and the tax incentives created by BC's carbon tax. This robust evidence suggests that the documented labor market responses are driven by BC's carbon tax.

5.2 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 before the policy, the estimates will end up capturing only the effects of the shocks in the post-policy period, not the entire effects. 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 limits the extent to which firms and workers respond to the policy. To further ease the doubt on the anticipatory effects, I estimate the anticipatory effect on BC's hourly wage rate and unemployment rate.

Here is the procedure to assess the anticipatory effects. The policy was announced on February 19, 2008, and was implemented on July 1 of the same year. Hence, I define the pre- and the post-announcement periods as January and the months between March and June in 2008, respectively. The observations in February are excluded because half of the month indeed belongs to the pre-announcement period, and the other half belongs to the post-announcement period. In this way, the first difference arises from the gaps in wage and unemployment rates between the pre- and post-announcement periods.

Table 5: The Anticipatory Effects of BC's Carbon Tax

Difference-in-Differences Analysis

	(1) **Unemployment Effect**	(2) **Wage Effect**
BC×2008×Mar-Jun	-0.002	0.001
	(0.004)	(0.010)
BC×2008	0.006	-0.008
	(0.004)	(0.008)
BC×Mar-Jun	0.004	-0.007
	(0.003)	(0.007)
2008×Mar-Jun	0.004***	-0.006*
	(0.002)	(0.003)
Adjusted R^2	0.035	0.406

Notes: The regressands are a dummy variable for being unemployed in Column (1) and the logarithm of hourly wage in Column (2). All specifications include province and year fixed effects, and the dummy variables for age, gender, the highest educational attainment, and marital status. Samples are restricted to all employees and LFP in column (1) and (2), respectively. N=623,490 and N=467,807 in Columns (1) and (2), respectively. The treatment group is BC's respondents between March and June in 2008. There are two control groups: the ROC between March and June in 2008 and BC's respondents between March and June in 2007. Data come from the Canadian LFS January, March, April, May, and June 2007-2008. Robust standard errors in parentheses are clustered at the level of province, age, gender, and marital status. Significance levels: ***=1%, **=5%, *=10%.

However, this difference may not capture the causal effect of the announcement because of seasonality. That is, the unemployment and wage rates between March and June could be different from that in January. I control this seasonality using the observations in 2007, serving as the second difference. Lastly, BC's wage and unemployment effects in 2008 could be country- and year-specific. Hence, I include the observations from the other provinces to control the country-specific effect. Therefore, the difference between BC and the other provinces serves as the third difference. The estimates can be obtained from the following regression model:

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

2008_t equals one if a respondent is observed in 2008, and zero otherwise. *March-June_t* equals one if a respondent is observed between March and June, and zero otherwise. Samples are restricted to workers in BC and the other control provinces in January, March, April, May, and June during 2007-2008. All specifications include province and year fixed effects and the dummy variables for age, gender, the highest educational attainment, and marital status. Again, standard errors are clustered at the level of province, age, gender, the highest educational attainment, and marital status, providing us with over a thousand clusters. I also estimate standard errors using a wild bootstrap procedure. Conclusions are insensitive to the procedure used to estimate the standard errors.

Table 5 reports the estimates of our interest from the estimation of equation (5). Two points emerge from the table. First, the result suggests that the anticipatory effects are negligible. The estimates $\hat{\beta}_1$

are close to zero and are statistically insignificant at any conventional level, providing no statistical evidence on changes in BC's unemployment and wage rates after the announcement. It is therefore safe to conclude that the anticipatory effects are economically and statistically negligible. While sections 4.2 and 4.3 provide evidence that the annual unemployment and wage trends are parallel between BC and the control provinces during the three years before the policy, this subsection provides support for the parallel trends in the unemployment and wage rates during the six months before the policy.

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

5.3 Assessing the Common Trend Assumption

The DID approach requires a common trend assumption. That is, BC and the ROC share parallel trends in the unemployment and wage rates during the period of examination. Despite no direct test on this assumption, I summarize suggestive evidence on the validity of this assumption found in this section.

The unemployment trend is likely parallel between BC and the ROC. First, Figure 3 in section 4.2 shows that the unemployment trend is parallel between BC and the ROC before the policy, suggesting that this trend likely remains parallel afterwards. Second, BC and the ROC also share parallel trends in the underlying components of the unemployment rate. While the literature on a job search theory indicates the dependence of the steady-state unemployment rate on the job-separation and job-finding rates, Figure 4 in section 4.2 shows that the trends in the two rates are also parallel before the policy. Moreover, Figure 10 in section 4.4 decomposes the unemployment effect into the short- and long-term unemployment effects. BC and the ROC share parallel trend in the short- and long-term unemployment rates. While the pre-policy trends in the short- and long-term unemployment rates are parallel in BC and the ROC, it is unsurprised that BC and the ROC share a parallel trend in the unemployment rate.

The wage trend is likely parallel between BC and the ROC. Section 4.3 decomposes the average wage effect into the hiring and incumbent wage effects. Apparently, BC and the ROC share a parallel hiring wage trend before the policy as shown in Figure 7. I use various samples to explore the dynamics of the incumbent wage in Figure 8. Most of the estimates are small in magnitude and they move along the zero line. The finding suggests that BC and the ROC do share a parallel trend in the average incumbent wage from the pre-policy period to the post-policy period. It reveals that if a labor market variable is less likely affected by the policy, the post-policy trend of the variable is likely parallel between BC and the ROC.

All these results point to the same conclusion: the common trend assumption is likely satisfied. Although the chance is low, what if the identifying assumption does not hold? To ease the doubt, I further the analysis using the SCM. This method remedies our problem at hand because it relaxes the common trend assumption. Moreover, Azevedo et al. (2020) argue that the DID approach is inappropriate in identifying the causal effect of BC's carbon tax because the common trend assumption may not be satisfied. Hence, I conduct the analysis with the SCM to identify the causal effect of BC's carbon tax on labor market outcomes.

It is, however, worth highlighting that the SCM is by no means perfect. This method creates a synthetic BC using the information from the ROC so that BC has the synthetic BC to compare with. To create such a synthetic group, it requires a large number of donors from the ROC (i.e., the control provinces). With only nine donors (i.e., the nine provinces) from the ROC, such a synthetic BC may not exist. In other words, the SCM may fail to find a convex combination of the nine donors that replicate the pre-policy path of BC's labor market outcome. To remedy the problem, I create the synthetic control (SC) group for each of the 18 industries in BC instead of creating the synthetic BC at the aggregate level. I allow each of the 18 industries in each of the nine control provinces as a donor to contribute a weight to construct each synthetic industry in BC. As such, I have 162 donors from the 18 industries in the ROC rather than nine donors from the ROC at the provincial level.

I relegate the details of the analysis to Appendix F. This analysis yields an important yet unsurprised results—when the common trend assumption is relaxed, the adverse labor market effects of BC's carbon tax remain significant. I find that this SCM allows us to create 18 synthetic industries in BC. Most of the industries and the corresponding synthetic group share similar paths of the employment, average hiring wage, and average incumbent wage levels between July 2001 and June 2008, the seven years before the policy.[11] For many industries, the paths are so close that they overlap throughout the entire pre-policy period. While BC and the SC groups share similar paths of the employment and average hiring wage levels prior to the policy, many industries fall off the respective synthetic groups in their paths of the employment and average hiring wage levels after the policy. Moreover, the finding of the analysis suggests that the average incumbent wage level drops significantly in BC with delays. These results cohere with the findings of this paper, providing additional evidence that an environmental policy does create job and wage losses.

5.4 Assessing Other Possible Sources

This section assesses possible sources other than BC's carbon tax that can explain the documented labor market responses in BC. The first concern is related to the Olympic shock as discussed in section 3.1. One may wonder if the unemployment and wage effects in the post-policy period are driven by the Olympic shock. Here are several reasons why the Olympic shock cannot be the major driver of the documented effects.

First, the dynamics of the unemployment effect does not cohere with the construction timeline in BC. Since the infrastructure was completed in the second half of 2009, construction workers are expected to be released to BC's labor market around the same period. Figure F2 in Appendix F also tells the same

[11] I did not conduct a similar analysis on the causal effect on the unemployment rate, the job-separation and job-finding rates, and the short- and long-term unemployment rates because I cannot tell which industry a worker belongs to when s/he becomes unemployed.

story: the employment of the construction industry plummeted in the second year of BC's carbon tax policy. Since then, the number of employed workers has been stable for three years. However, BC's unemployment rate started to rise in the first year of BC's carbon tax and continued to rise until June 2011 as shown in Figure 3. Once the tax rate stopped to increase in 2012, the unemployment effect started to decay. The entire dynamics of the unemployment effect is more consistent with the dynamics of the tax incentive of BC's carbon tax than the construction timeline in BC.

Second, the adverse wage effect is inconsistent with the construction timeline. As shown in Figure 7, the adverse hiring wage effect fell immediately after BC's carbon tax, a year before most of the construction works were completed. The hiring wage effect continued to enlarge in the first few years of the policy, over which the carbon tax rate increased annually. The dynamics of the adverse wage effect, again, coheres with the tax incentives created by BC's carbon tax, not the Olympic shock.

Third, the adverse wage effect is concentrated on manufacturing industries with higher energy intensity. While firms with higher energy-intensity are expected to pay more carbon tax, I expect that the wage effect is related to energy intensity. Section 5.1 restricts the sample to manufacturing industries to explore the heterogeneous wage effect across manufacturing industries. As such, the sample of construction workers is excluded from the sample of examination. The result reveals that the adverse wage effect is concentrated on manufacturing industries with higher energy intensity, providing additional support that the documented labor market responses after 2008 are not driven by the Olympic shock.

Another concern is related to the financial crisis in 2008. If the crisis happens to affect BC more than the ROC, the documented unemployment and adverse wage effects may reflect the labor market responses to the crisis, not necessarily BC's carbon tax. Although there exists no direct test to verify the claim that the estimates capture the labor market effects of BC's carbon tax, the documented effects are more consistent with the tax incentive created by BC's carbon tax than the financial crisis.

For example, section 5.2 indicates that the crisis increases unemployment and decreases the average wage rate in Canada in the first half of 2008. Meanwhile, there exists no statistical evidence on larger unemployment or wage effects in BC than the ROC during the same period. The findings cast doubt on the claim that the 2008 financial crisis affects BC's labor market more than the ROC. Moreover, section 5.1 documents that the adverse wage effects are concentrated on industries with higher energy intensities in BC, in line with the tax incentives. If wage losses created by the financial crisis are generally more pronounced among industries with higher energy intensities, I expect to see a similar heterogeneity in the ROC. Nevertheless, no statistical evidence on a similar wage heterogeneity can be found in the ROC. These results suggest that the adverse wage response in BC is unlikely driven by the financial crisis.

Next in line is the concern on trade shocks from the United States. Unfortunately, there does not exist any direct test to disprove the claim. To control for variations in the exchange rate of Canadian dollar to United States dollar could, to a certain extent, ease the doubt. The inclusion of *Year*$\times$*Month* fixed effect has already taken the variations in the exchange rate into account in all models of this paper. However, a few may worry that the impacts of trade shocks are heterogenous across provinces. If the trade shock happens to disproportionately affect BC more than the ROC, the inclusion of the *Year*$\times$*Month* fixed effect will not remove the trade effects from all of the estimates in this paper. Hence, I re-conduct all the analyses of this paper by controlling *BC×Monthly Exchange Rate* and report the tables in Appendix C and figures in Appendix G. All these tables and figures suggest that the conclusions of this paper are insensitive to the exchange rate fluctuations.

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

The last concern is related to an interprovincial migration between BC and the ROC. While BC's unemployment rate remains elevated between 2008 and 2012, workers may seek jobs from other provinces. If the migrants and the rest of the labor force in BC are different in their characteristics, changes in the sample composition may contaminate the estimates of this paper. To address this issues, I first assess the possibility of the interprovincial migration caused by BC's carbon tax. I show in Appendix I the two time series of migrants into and out of BC. The two series are quite steady in the past 20 years, with no signs of increases due to BC's carbon tax. Indeed, if job losses are the concern leading to migrations, it is unsurprised that BC's carbon tax does not significantly induce migrations. As shown in section 4.4, BC's carbon tax increases mainly short-term unemployment. A majority of unemployed workers can find a job within half a year, over which the unemployed are protected by standard employment insurance. Moreover, I presented evidence in section 3.2 that the demographic characteristics in BC and the ROC, such as the distributions of gender, age, marital status, educational levels, etc., are quite stable throughout the entire period of examination, casting doubt on the changes in the sample composition. Therefore, it is safe to conclude that the interprovincial migration may not be an issue.

On the contrary, the documented effects cohere with the incentives created by BC's carbon tax. For example, the effects fit well with the timing of the policy. While the tax was introduced in 2008, I expect to observe the effects immediately following the policy. Moreover, the tax rate grows in the first few years. If the public happens not to fully respond to the increasing tax rate in advance, I also expect the policy impacts grow in the first few years. The results in sections 4.2-4.4 support these two arguments. The unemployment effect in Figure 3, the policy impacts on the two transitional rates in Figure 4, the average hiring wage effect in Figure 7, and the short- and long-term unemployment effects in Figure 10 share two common features: these effects emerge after the second half of 2008, and the effects grow in the first few years. These results suggest that BC's carbon tax policy is the major source of the documented labor market responses.

6 Discussion

6.1 How Does the Carbon Tax Shape the Labor Market?

The carbon tax increases the marginal cost of production through energy use. While the energy is one of the input factors, the increase in the margin cost will decrease the derived demands for other input factors including labor hours. Firms re-optimize their profit functions: they cut their labor demands and cut wages. However, firms are reluctant to adjust incumbent wages. As a result, they would like to replace incumbent workers with new hires at lower hiring wages. Therefore, many incumbent workers are laid off until firms find it unprofitable to replace the remaining incumbent workers with new hires. Hence, unemployment increases through a job-loss channel—job losses become increasingly common. This mechanism coheres with the sticky wage theory (Keynes, 1936; Fischer, 1977; Taylor, 1980): (cyclical) unemployment arises because of nominal wage rigidity.

Meanwhile, firms take the carbon tax into account to make hiring decisions. According to a canonical job search model (McCall, 1970; Pissarides, 2000), a higher production cost requires matches between a firm and an unemployed worker a higher match-specific productivity level to formulate a production unit. The tightened requirement makes unemployed workers harder to find a job, thereby increasing unemployment through a job-finding channel. Since hiring wages are adjusted downward, firms find it unprofitable to lay off new hires. As a result, the job-loss effect vanishes: the job-separation rate returns to its pre-policy level, causing the unemployment effect to decay. Since BC's carbon tax is a permanent shock, the job-finding rate remains low. The long-lasting job-finding effect explains why the unemployment rate remains slightly higher than its pre-policy level in the long run.

Wages are cut through labor turnover. On the one hand, incumbent wages are rigid. On the other hand, a majority of employment is incumbent workers at the beginning of the policy. As a consequence, the average wage effect is initially negligible. While hiring wages are adjusted downward, the average wage continues to decrease as a gradual increase in the proportion of new hires in employment.

6.2 What Can We Learn from BC's Carbon Tax?

This article makes several primary contributions. To the best of my knowledge, this paper is the first to uncover the differences in the dynamics between the unemployment and wage effects of environmental policies. Whereas the unemployment effect arrives without lags but decays quickly, the wage effect emerges with lags but grows gradually.

The study on the dynamics uncovers the significant labor market responses to an environmental policy. My analysis on the dynamics suggests that the labor market responses could be significant: while the unemployment effect peaks at two percentage points, the carbon tax could reduce the average wage by three percent. These significant labor market responses recognize the public concern on potential job and wage losses created by environmental policies, which plays a central role in political debates over environmental regulations.

Surprisingly, recent literature finds weak or no evidence on labor market responses to environmental policies (Berman and Bui, 2001; Martin et al., 2014; Hafstead and Williams, 2018; Castellanos and Heutel, 2019; Azevedo et al., 2020). A few studies, especially the earlier ones, document the significant employment effects of environmental policies in affected firms and sectors (Greenstone, 2002; Walker, 2011; Kahn and Mansur, 2013; Curtis, 2017). Nevertheless, proponents of environmental regulations argue that environmental policies may shift employment from affected sectors to others, making the unemployment effect ambiguous. While the empirical evidence on such sectoral reallocation can be found in this literature (Walker, 2011, 2013), Hafstead and Williams (2018) and Castellanos and Heutel (2019) show that the unemployment effect of environmental policies is at best weak due to labor reallocation.

Also, 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*". This bias may arise from labor reallocation: the key estimate of the DID approach ends up capturing both the shrinkage of regulated firms and the expansion of unregulated firms, casting doubt on the job killing feature of environmental regulations documented in the earlier literature. Moreover, as highlighted by Walker (2013), the use of newly regulated firms as treatment groups, which is commonly seen in the earlier literature, makes it difficult to conclude the overall economic effects of environmental policies on the aggregate labor market. Using the entire province in BC as a treatment labor market, this paper takes sectoral reallocation within BC's labor market into account and speaks directly to the aggregate labor market adjustments to environmental policies. Having considered the potential labor reallocation, this paper finds that the unemployment effect of environmental policies remains strong.

In addition to labor reallocation, another popular argument is related to the weak Porter hypothesis. This hypothesis implies that environmental policies may induce innovation. As such, these policies create green jobs and thus increase labor demands, absorbing unemployment and making the unemployment effect ambiguous. The evidence on the growing green jobs induced by environmental policies is well documented in the literature (Vona et al., 2018; Marin and Vona, 2019). Despite no evidence for or against the hypothesis, this paper finds that the weak Porter hypothesis is so "weak" that even if the innovation effect of an environmental policy creates jobs, the policy increases unemployment.

This paper also contributes to the literature on revenue-neutral carbon taxes. This literature suggests that carbon taxes depress unemployment (Bovenberg and van der Ploeg, 1998; Wagner, 2005). But if the carbon tax revenue is redistributed to the public by reducing payroll tax rates, the policy will provide financial incentives to create vacancies and thus depresses unemployment. In particular, Hafstead and Williams (2018) and Castellanos and Heutel (2019) calibrate search equilibrium models to US economy; their simulation exercises suggest that the unemployment effect of revenue-neutral carbon taxes is at best weak. While BC's carbon tax was the first revenue-neutral carbon tax in North American jurisdiction, the causal estimates of this paper give a lesson to this literature—a carbon tax, even though revenue-neutral, results in significant unemployment and adverse wage effects.

Meanwhile, the findings of this paper provide valuable information to studies that carefully calibrate a staggered-wage model to examine the unemployment effects of environmental regulations (Hafstead and Williams, 2018; Castellanos and Heutel, 2019). This paper reveals that it takes longer than our expectation to have wages fully adjusted: incumbent wages remain rigid after the usual length of labor contracts. Nevertheless, this literature often assumes a shorter period of wage rigidity than empirical regularity. This divergence partly explains why the unemployment effects of environmental regulations are much stronger in empirical frameworks like this paper than calibrated models in prior literature. Incorporating persistent wage rigidity into search equilibrium models will help this literature improve their model predictability of unemployment volatility.

In this paper, the study on the dynamics of the labor market responses unties two knots in the literature. First, this paper reveals that any studies, if estimating the unemployment effects of environmental policies in the long run or averaging their effects over prolonged post-policy periods, may conclude with weak unemployment effects because the significant unemployment effect decays quickly. This reason partly explains why Yip (2018) finds a significant unemployment effect in the first two years of a carbon tax policy, but Azevedo et al. (2020) finds no evidence on the unemployment effect in the sixth year of the same policy.

Second, any estimated average wage effects of environmental policies are likely weak in the short run because wage effects come with lags. Quite often, this literature employs a DID approach to identify the labor market effects of environmental policies. The approach compares labor market outcomes between pre- and post-policy periods. If the post-policy period is not long enough, it is unsurprised that prior literature finds no statistical evidence on the wage effect. For example, this paper finds the average wage effect statistically negligible in the first two years of the policy. Meanwhile, it provides solid support that BC's carbon tax reduces the average wage through a slow process of labor turnover. This reason partly explains the lack of conclusive empirical evidence on the adverse wage effect of environmental policies in the literature. In sum, this paper bridges between the public and the literature: it explains why the public is so concerned with the significant labor market effects of environmental policies even though an extensive literature finds the effects weak.

Moreover, this paper uncovers the mechanisms explaining the dynamics of the labor market responses. This paper is the first in this literature to decompose the unemployment effect into job-loss and job-finding effects. I find that the initial unemployment effect is significant because of the two effects. However, the unemployment effect quickly decays solely because the job-loss effect is short-lived. These findings highlight the importance of the decomposition exercise that explains the dynamics of the unemployment effect—it arrives without lags and decays quickly, enhancing our understanding of unemployment adjustments to environmental policies.

Inspired by prior empirical literature on wage rigidity, this paper studies the average wage effect, along with hiring and incumbent wage effects. I find that an environmental policy, like BC's carbon tax, causes hiring wages to plunge, leaving incumbent wages unaffected.[12] One of the explanations of this wage-cutting process is that workers lose industry- and firm-specific human capitals after reemployed in other sectors, in line with the significant earnings losses due to sectoral reallocation documented in Walker (2013). The hiring wage plunge and the incumbent wage rigidity suggest that wages are adjusted through a slow process of labor turnover, explaining why the wage effect comes with lags and grows gradually. This paper provides strong evidence to conclude that a complete understanding of labor market adjustments (i.e., the unemployment and wage adjustments) requires an explanation of employment flows—the inflow and outflow of employment.

Furthermore, the study on the mechanisms sheds light on the distribution of costs associated with environmental policies in the labor market. In particular, incumbent workers bear no associated cost

[12] Walker (2013) and Curtis (2017) find that earnings losses are found mainly among the new hires, not the incumbent workers, of newly regulated firms.

in the labor market: they kept their jobs with their wages unaffected. If the policy is revenue-neutral so that payroll tax rates are cut, incumbent workers benefit from an increased after-tax labor income. Hence, there is no urge to compensate incumbent workers. In contrast, unemployed workers bear the brunt of the cost: a majority of them lose their jobs because of the policy, and they, once re-employed, receive lower hiring wages. Therefore, carbon tax revenues could be redistributed wisely to compensate unemployed workers: hiring subsidies can be provided to firms to speed up a hiring process and tax credits can be provided to new hires so that their after-tax hourly wage rates are not cut that much. While the distributive costs of environmental policies in the labor market are often neglected in the literature (Hazilla and Kopp, 1990; Bovenberg et al., 2005; Ryan, 2012; Fullerton and Monti, 2013; Williams et al., 2015; Rausch and Schwarz, 2016), the findings of this paper contribute to this literature by enhancing our understanding of how the associated costs are distributed in the labor market and by providing guidance to redistribute carbon tax revenues.

This paper also discusses another policy implication related to unemployment benefits. To the best of my knowledge, this paper is the first to decompose the unemployment effect of environmental policies into short- and long-term unemployment. My analysis reveals that about 80% of the unemployment effect is attributable to short-term unemployment. That is, a majority of unemployed workers can find a job within half a year, suggesting that BC's carbon tax increases unemployment mainly through the number of unemployment spells, not the duration of unemployment spells. The literature on the optimal unemployment benefits design often suggests to extend the entitlement period of unemployment benefits at high unemployment rates to smooth the consumption of unemployed workers over a prolonged period of unemployment (Røed and Zhang, 2003; Lalive, 2008; Caliendo et al., 2013; Jung and Kuester, 2015; Mitman and Rabinovich, 2015; Kolsrud et al., 2018; Landais et al., 2018a,b). This paper reveals that despite the significant unemployment effect, there is no urge to accommodate environmental policies with substantial extension of benefit periods.

In addition to the literature on environmental economics, this paper also gives lessons on labor economics. For example, this paper speaks to the literature on job displacement (Jacobson et al., 1993; Hijzen et al., 2010; Davis et al., 2011; Walker, 2013; Curtis, 2017; Farber, 2017; Krolikowski, 2017; Flaaen et al., 2019). While an extensive literature pays attention to earnings losses, my study on the hiring wage dynamics complements this literature. In particular, Walker (2013) and Curtis (2017) document that an intensity standard and a cap-and-trade program create earnings losses among the new hires of regulated firms. While these policies also reduce employment (i.e., the extensive margin of employment), it is unclear whether the earnings losses result from the reduction in wages (i.e., returns to labor hours) or the reduction in labor hours (i.e., the intensive margin of employment).

This paper paints the overall picture of the earnings effects documented in this literature. My findings reveal that although a large portion of the earnings losses are attributable to wage losses, an earnings recovery is completely unrelated to the recovery of wages. I find that an environmental policy, such as BC's carbon tax, could reduce the average hourly wage of new hires by three percent, slightly less than 3.5 percent earnings losses documented in Curtis (2017). The small divergence is attributable to the reduction in the labor hours of new hires.[13] Moreover, Walker (2013) leaves an unresolved question: what causes a quick earnings recovery from environmental regulations? This paper gives a hint—the hiring wage effect shows no recovery sign over 10 years (See Figure D3 in Appendix D.); hence, the earnings recovery is completely attributed to the recovery of labor hours.

Also, this paper sheds light on the literature on business cycles. It provides micro-evidence that harmonizes the sticky wage theory and the wage procyclicality documented in prior literature. While incumbent wages are found rigid, the hiring wage cut is the main source of the adverse average wage effect. Moreover, this paper provides new insights into the mechanism of wage rigidity. One of the "*textbook*" explanations of the sticky wage theory is that wages are not easily adjusted under labor contracts and are adjusted upon contract renewals (Barro, 1977; Fischer, 1977; Thomas and Worrall, 1988). Nevertheless, this paper finds that the wage rigidity lasts longer than the usual length of labor contracts, casting doubt on labor contracts explaining the wage rigidity and on wage adjustments through renegotiation upon contract renewals.

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

Moreover, the findings of this paper uncover the processes through which sectoral shocks increase unemployment (Hall, 2005a,c; Davis et al., 2006; Fujita and Ramey, 2009; Elsby et al., 2009; Shimer, 2012; Elsby et al., 2013). The sticky wage theory predicts that wage rigidity creates (cyclical) unemployment through increased job-separation rates. According to the job search theory, the increased production cost reduces job-finding rates. Both the increased job-separation rate and the decreased job-finding rate jointly increase unemployment.

This paper contributes to this literature by providing micro-evidence on the dynamic effects on the job transition rates. On the one hand, many incumbent workers were laid off without substantial lags until firms find it unprofitable to further lay off remaining incumbent workers. On the other hand, hiring wages are easily adjusted so that the shock will not increase the job-separation rate of new hires. As a result, the causal effect on the job-separation rate is short-lived: this rate falls to its pre-shock level. This paper provides empirical evidence on the short-lived job-separation effect, in line with the calibrated model in Coles and Moghaddasi Kelishomi (2018). This paper also finds that the causal effect on the job-finding rate lasts long. One of the explanations is that firms take a carbon tax shock into account to make hiring decisions and the carbon tax is a permanent shock. While the literature mainly focuses on the contemporary effects on the two transition rates, this paper uncovers the entire dynamics of the two rates.

[13] Chi Man (2020) provides evidence that BC's carbon tax increases the likelihood of new hires working part-time, supporting this claim.

During the entire process, over 40 percent of the unemployment effect is attributable to the increased job-separation rate, and over half the effect arises from the decreased job-finding rate. 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, challenging the widespread view that job-separation rates are acyclical (Hall, 2005a,c; Shimer, 2012).

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Appendix A: The Background of BC's Carbon Tax

Table A1: Carbon Tax by Fuel Type

Fuel Type	July 08-June 09	July 09-June 10	July 10-June 11	July 11-June 12	July 12-June 13
Aviation Fuel	2.46¢/L	3.69¢/L	4.92¢/L	6.15¢/L	7.38¢/L
Diesel	2.69¢/L	4.04¢/L	5.11¢/L	6.39¢/L	7.67¢/L
Gasoline	2.34¢/L	3.51¢/L	4.45¢/L	5.56¢/L	6.67¢/L
Heavy Fuel Oil	3.15¢/L	4.73¢/L	6.30¢/L	7.88¢/L	9.45¢/L
Jet Fuel	2.61¢/L	3.92¢/L	5.22¢/L	6.53¢/L	7.83¢/L
Natural Gas	1.90¢/cm^3	2.85¢/cm^3	3.80¢/cm^3	4.75¢/cm^3	5.70¢/cm^3
Propane	1.54¢/L	2.31¢/L	3.08¢/L	3.85¢/L	4.62¢/L

Note: Data come from Ministry of Finance 2016.

Table A2: Carbon Tax Revenue and Tax Cuts

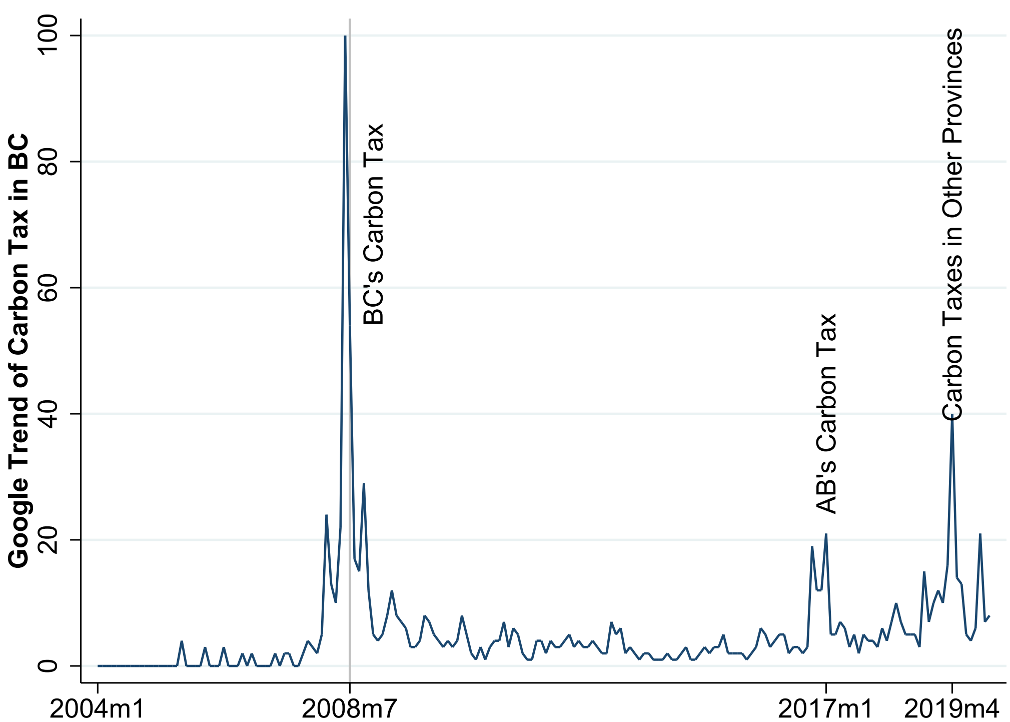
(in CAD$ Millions)	2008/09	2009/10	2010/11	2011/12	2012/13	2013/14	2014/15
Carbon Tax Revenue	306	542	741	959	1,120	1,222	1,198
Personal Tax Cuts	213	359	391	470	546	522	565
Corporate Tax Cuts	100	370	474	671	834	710	959
Total Tax Cuts	313	729	865	1,141	1,380	1,232	1,524

Note: Data come from Ministry of Finance 2016.

Figure A1: The Map of Canada and the Location of BC

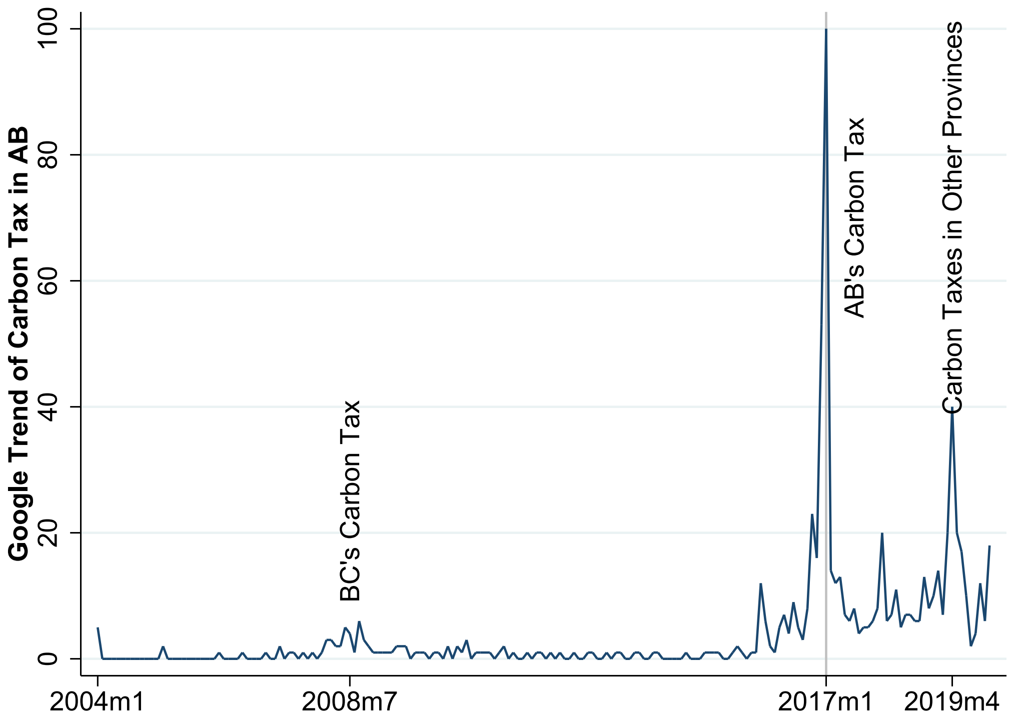
Note: The colored region is the Canadian province of BC.

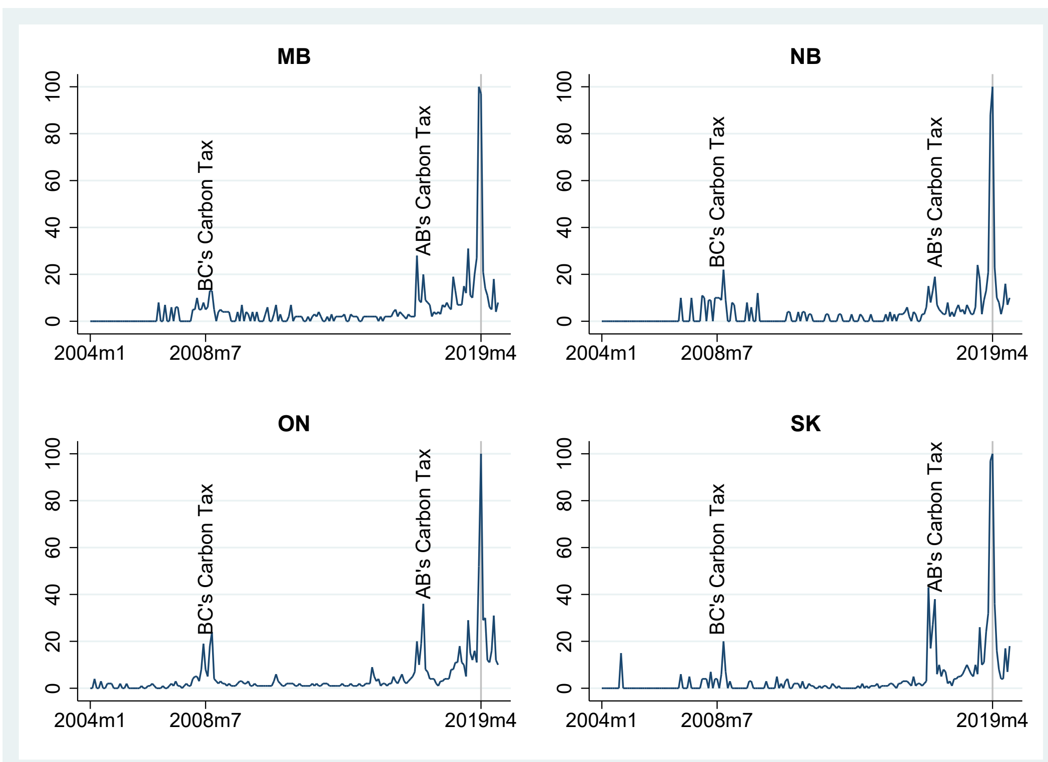
Figure A2: Google Trend of Carbon Tax in BC



Note: This figure shows the frequency people in BC search the key word of Carbon Tax using Google. Google Trend normalizes the highest frequency to 100. The vertical line indicates the month of implementation of BC's carbon tax. Carbon taxes were implemented in BC in July 2008, in AB in January, 2017, and in a couple of provinces in April 2019.

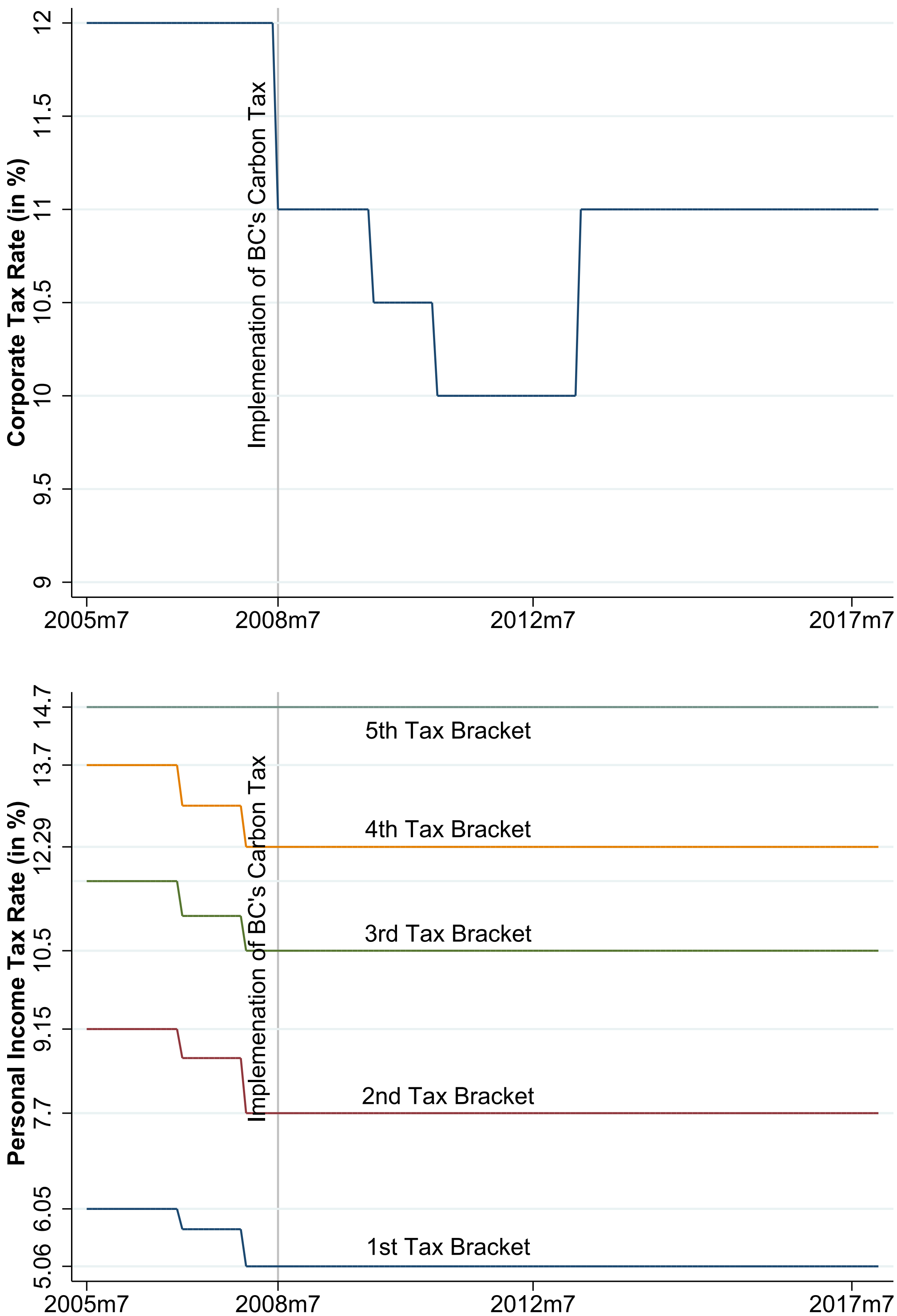
Figure A3: Google Trend of Carbon Tax in Other Canadian Provinces





Note: These figures show the frequency people in various Canadian provinces search the key word of Carbon Tax using Google. Google Trend normalizes the highest frequency to 100. The vertical line indicates the month of implementation of carbon taxes in the corresponding province. Carbon taxes were implemented in BC in July 2008, in AB in January, 2017, and in a couple of provinces in April 2019.

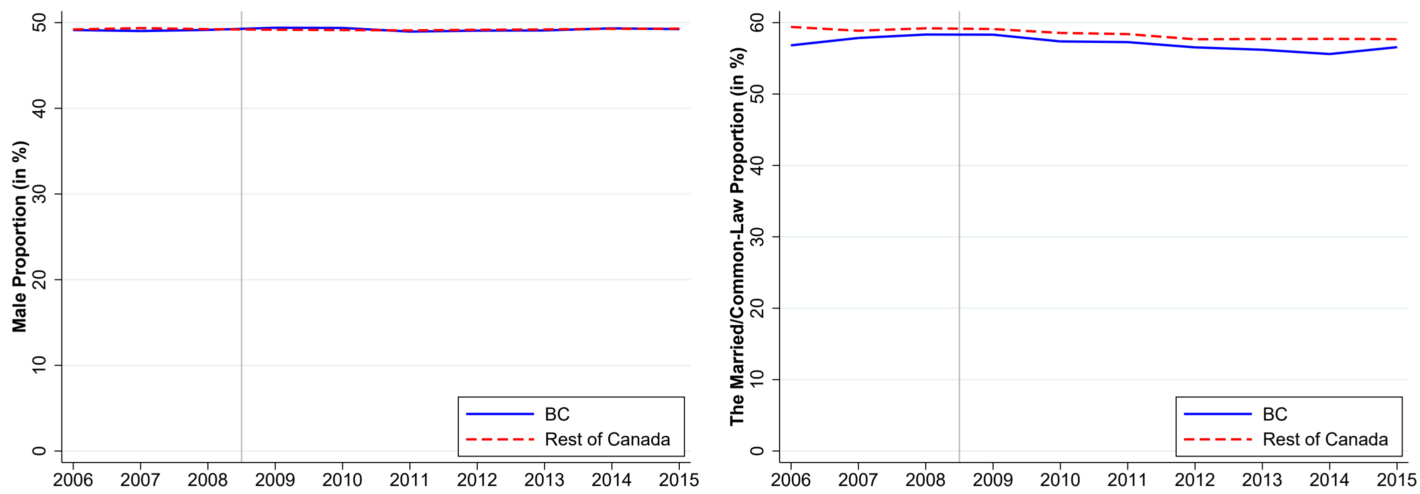
Figure A4: BC's Corporate Income Tax and Personal Income Tax Rate



Note: Data are obtained from the official website of the Government of British Columbia.

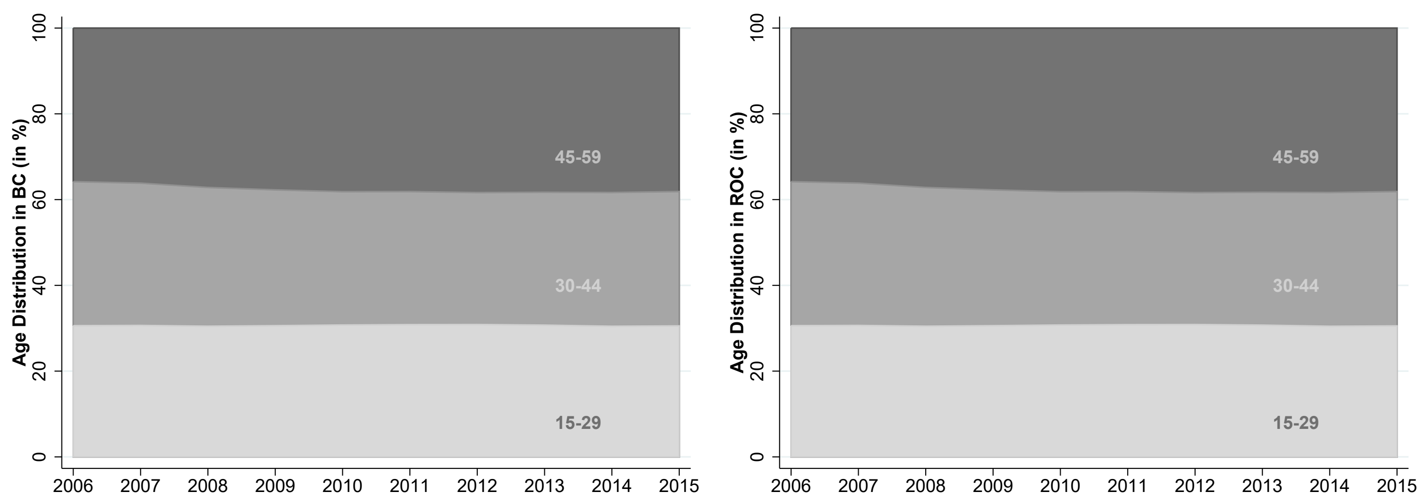
Appendix B: Descriptive Statistics

Figure B1: The Shares of Male and Married/Common-Law Respondents in BC and the ROC



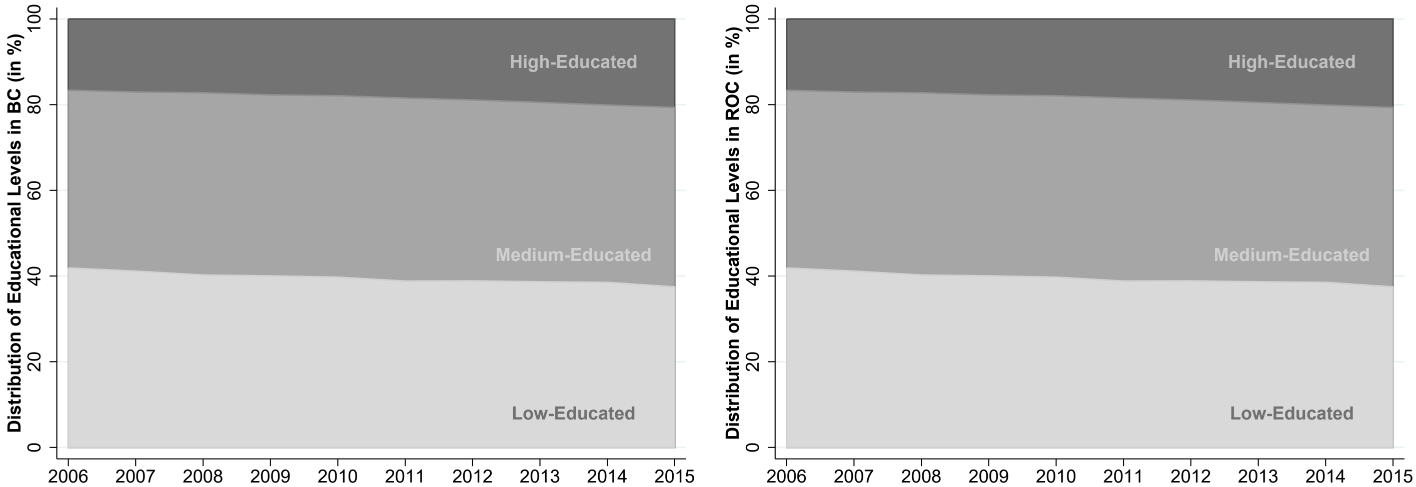
Notes: Data are from the Canadian LFS July 2005-June 2015.

Figure B2: The Age Distribution in BC and the ROC



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

Figure B3: The Distribution of Educational Levels in BC and the ROC



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

Table B1: Industry Structure in the Pre-Policy Period

	BC	ROC
Agricultures	1.33	1.27
Forestry, Fishing, Mining, Oil & Gas	2.21	2.98
Utilities	0.58	1.06
Construction	7.25	5.57
Manufacturing: Durables	6.39	7.55
Manufacturing: Non-Durables	3.91	6.04
Wholesale Trade	3.59	3.31
Retail Trade	13.70	13.55
Transportation & Warehousing	5.44	4.57
Finance, Insurance, Real Estate, & Leasing	5.20	5.03
Professional, Scientific & Technical	4.99	4.04
Management, Administrative & Other Support	3.72	3.68
Educational Services	7.28	7.71
Health Care & Social Assistance	11.05	11.75
Information, Culture & Recreation	4.76	4.15
Accommodation & Food Services	9.37	7.52
Other Services	3.45	3.51
Public Administration	5.78	6.71

Notes: Data are from the Canadian LFS July 2005-June 2008. Samples are restricted to employees at work.

Appendix C: Estimates in Tables

Table C1: The Unemployment Effects of BC's Carbon Tax

Difference-in-Differences Analysis

Dependent Variable: Dummy Variable For Being Unemployed			
	(1)	(2)	(3)
BC×2006	0.001	0.001	0.003
	(0.002)	(0.002)	(0.002)
BC×2007	-0.002	-0.003*	-0.001
	(0.001)	(0.001)	(0.002)
BC×2009	0.008***	0.007***	0.010***
	(0.002)	(0.002)	(0.002)
BC×2010	0.015***	0.014***	0.015***
	(0.004)	(0.004)	(0.004)
BC×2011	0.020***	0.020***	0.019***
	(0.003)	(0.004)	(0.004)
BC×2012	0.017***	0.017***	0.017***
	(0.003)	(0.003)	(0.003)
BC×2013	0.013***	0.013***	0.013***
	(0.003)	(0.003)	(0.003)
BC×2014	0.010***	0.010***	0.011***
	(0.002)	(0.003)	(0.003)
BC×2015	0.006***	0.006**	0.008***
	(0.002)	(0.003)	(0.003)
Adjusted R^2	0.006	0.034	0.034
Province Fixed Effect	yes	yes	yes
Year×Month Fixed Effect	yes	yes	yes
Gender		yes	yes
Age		yes	yes
Educational Level		yes	yes
Marital Status		yes	yes
BC×Exchange Rate			yes

Note: The sample is restricted to labor force participants. BC's unemployment rate is 4.50% in the reference year, which is July 2007-June 2008. N=7,405,598. *Exchange Rate* is the monthly average exchange rate between Canadian dollar and US dollar. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Significance level: ***=1%, **=5%, *=10%.

Table C2: The Average Wage Effects of BC's Carbon Tax

Difference-in-Differences Analysis

Dependent Variable: ln(**Hourly Wage Rate**)			
	(1)	(2)	(3)
BC×2006	0.007	0.014***	0.012**
	(0.006)	(0.005)	(0.005)
BC×2007	0.005	0.007	0.006
	(0.005)	(0.004)	(0.004)
BC×2009	-0.003	-0.001	-0.002
	(0.006)	(0.004)	(0.004)
BC×2010	-0.005	-0.002	-0.002
	(0.008)	(0.006)	(0.005)
BC×2011	-0.022**	-0.015***	-0.015***
	(0.011)	(0.006)	(0.006)
BC×2012	-0.029***	-0.023***	-0.023***
	(0.011)	(0.006)	(0.006)
BC×2013	-0.022*	-0.022***	-0.022***
	(0.012)	(0.005)	(0.005)
BC×2014	-0.029**	-0.029***	-0.030***
	(0.014)	(0.005)	(0.005)
BC×2015	-0.029**	-0.027***	-0.029***
	(0.012)	(0.005)	(0.006)
Adjusted R^2	0.051	0.482	0.482
Province Fixed Effect	yes	yes	yes
Year×Month Fixed Effect	yes	yes	yes
Gender		yes	yes
Age		yes	yes
Educational Level		yes	yes
Marital Status		yes	yes
Industry		yes	yes
BC×Exchange Rate			yes

Note: The sample is restricted to employees at work. BC's average wage is CAD$20.64 in the reference year, which is July 2007-June 2008. N=5,450,559. *Exchange Rate* is the monthly average exchange rate between Canadian dollar and US dollar. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Significance level: ***=1%, **=5%, *=10%.

Table C3: The Hiring Wage Effects of BC's Carbon Tax

Difference-in-Differences Analysis			
Dependent Variable: ln(**Hourly Wage Rate**)			
	(1)	(2)	(3)
BC×2006 BC_2006	-0.001	0.009	0.013
	(0.009)	(0.006)	(0.008)
BC×2007	0.005	0.010	0.013*
	(0.008)	(0.007)	(0.007)
BC×2009	-0.025***	-0.016**	-0.012
	(0.010)	(0.006)	(0.007)
BC×2010	-0.042***	-0.033***	-0.032***
	(0.013)	(0.010)	(0.010)
BC×2011	-0.062***	-0.048***	-0.048***
	(0.016)	(0.008)	(0.008)
BC×2012	-0.056***	-0.048***	-0.049***
	(0.014)	(0.008)	(0.008)
BC×2013	-0.046**	-0.039***	-0.039***
	(0.019)	(0.008)	(0.008)
BC×2014	-0.038**	-0.030***	-0.029***
	(0.019)	(0.009)	(0.009)
BC×2015	-0.043***	-0.031***	-0.026***
	(0.016)	(0.008)	(0.009)
Adjusted R^2	0.074	0.481	0.481
Province Fixed Effect	yes	yes	yes
Year×Month Fixed Effect	yes	yes	yes
Gender		yes	yes
Age		yes	yes
Educational Level		yes	yes
Marital Status		yes	yes
Industry		yes	yes
BC×Exchange Rate			yes

Note: The sample is restricted to new hires at work. BC's average hiring wage is CAD$16.10 in the reference year, which is July 2007-June 2008. N=1,307,939. *Exchange Rate* is the monthly average exchange rate between Canadian dollar and US dollar. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Significance level: ***=1%, **=5%, *=10%.

Table C4: The Short-Term Unemployment Effects of BC's Carbon Tax

Difference-in-Differences Analysis			
Dependent Variable: Short-Term Unemployment			
	(1)	(2)	(3)
BC×2006	0.001	0.000	0.002
	(0.002)	(0.002)	(0.002)
BC×2007	-0.002	-0.002	-0.000
	(0.001)	(0.001)	(0.002)
BC×2009	0.008***	0.007***	0.009***
	(0.002)	(0.002)	(0.002)
BC×2010	0.013***	0.013***	0.013***
	(0.003)	(0.004)	(0.004)
BC×2011	0.016***	0.015***	0.015***
	(0.003)	(0.003)	(0.003)
BC×2012	0.012***	0.012***	0.012***
	(0.003)	(0.003)	(0.003)
BC×2013	0.011***	0.011***	0.011***
	(0.002)	(0.003)	(0.003)
BC×2014	0.008***	0.008***	0.008***
	(0.002)	(0.003)	(0.003)
BC×2015	0.004**	0.004**	0.006***
	(0.002)	(0.002)	(0.002)
Adjusted R^2	0.005	0.033	0.033
Province Fixed Effect	yes	yes	yes
Year×Month Fixed Effect	yes	yes	yes
Gender		yes	yes
Age		yes	yes
Educational Level		yes	yes
Marital Status		yes	yes
BC×Exchange Rate			yes

Note: The sample is restricted to labor force participants. BC's unemployment rate is 3.99% in the reference year, which is July 2007-June 2008. N=7,405,598. *Exchange Rate* is the monthly average exchange rate between Canadian dollar and US dollar. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Significance level: ***=1%, **=5%, *=10%.

Table C5: The Long-Term Unemployment Effects of BC's Carbon Tax

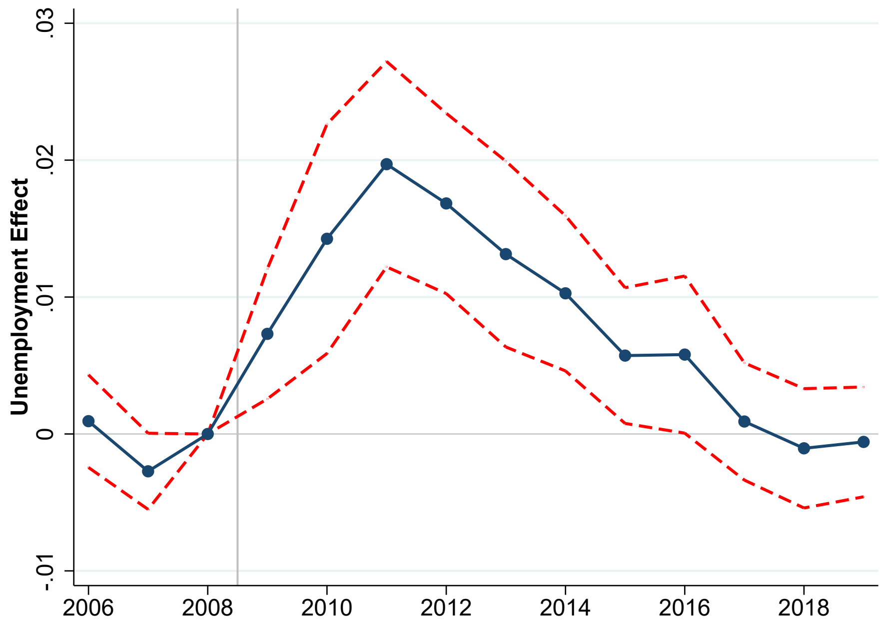
Difference-in-Differences Analysis

Dependent Variable: Long-Term Unemployment			
	(1)	(2)	(3)
BC×2006	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
BC×2007	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.001)
BC×2009	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)
BC×2010	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
BC×2011	0.005***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)
BC×2012	0.005***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)
BC×2013	0.002**	0.002**	0.002**
	(0.001)	(0.001)	(0.001)
BC×2014	0.003***	0.003***	0.003***
	(0.001)	(0.001)	(0.001)
BC×2015	0.001*	0.001	0.002*
	(0.001)	(0.001)	(0.001)
Adjusted R^2	0.002	0.006	0.006
Province Fixed Effect	yes	yes	yes
Year×Month Fixed Effect	yes	yes	yes
Gender		yes	yes
Age		yes	yes
Educational Level		yes	yes
Marital Status		yes	yes
BC×Exchange Rate			yes

Note: The sample is restricted to labor force participants. BC's unemployment rate is 0.51% in the reference year, which is July 2007-June 2008. N=7,405,598. *Exchange Rate* is the monthly average exchange rate between Canadian dollar and US dollar. Robust standard errors in parentheses are clustered at the level of province, age, gender, the highest educational attainment, and marital status. Significance level: ***=1%, **=5%, *=10%.

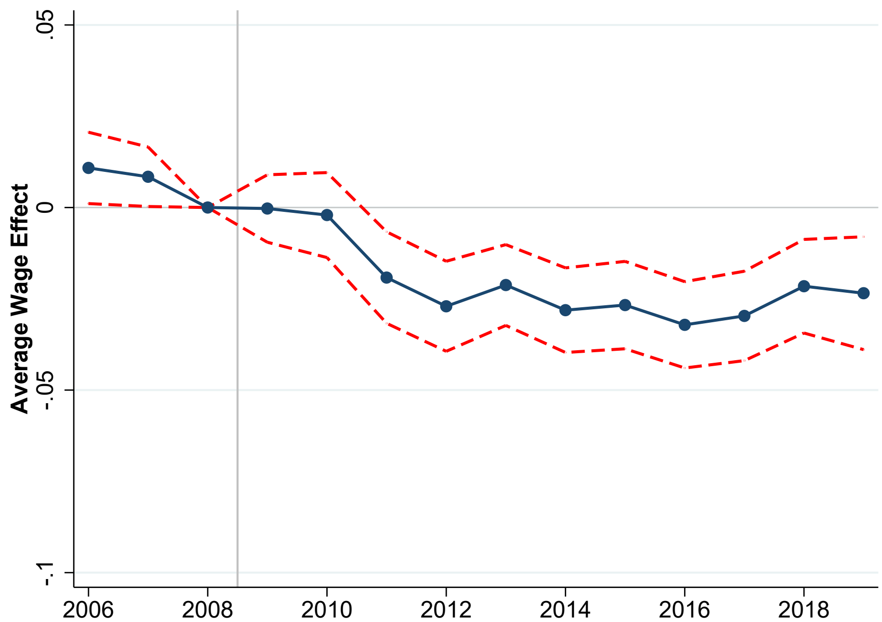
Appendix D: Results Extension to 2019

Figure D1: The Dynamics of Unemployment Effects



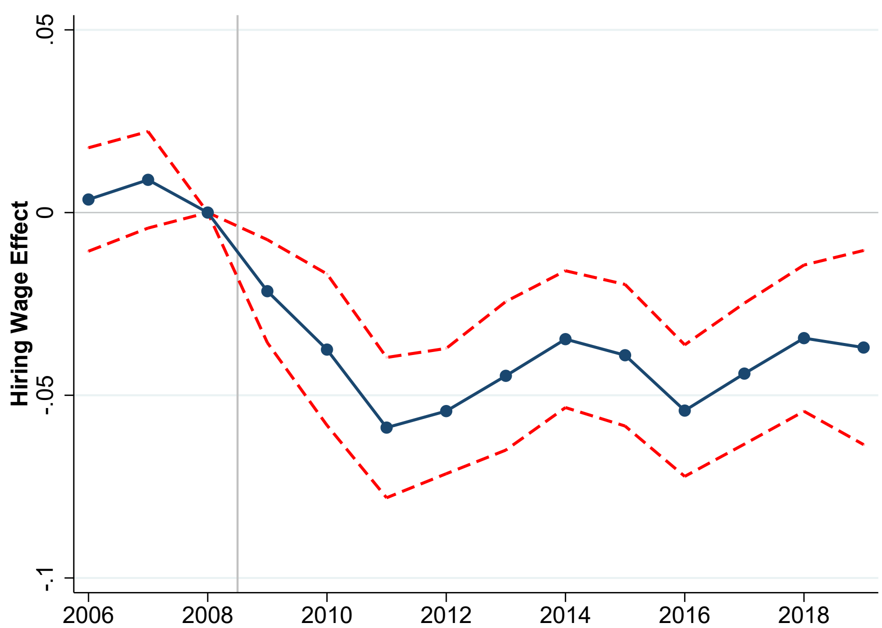
Note: The regressand is a dummy variable of being unemployed. Samples are restricted to the labor force. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

Figure D2: The Dynamics of Average Wage Effects



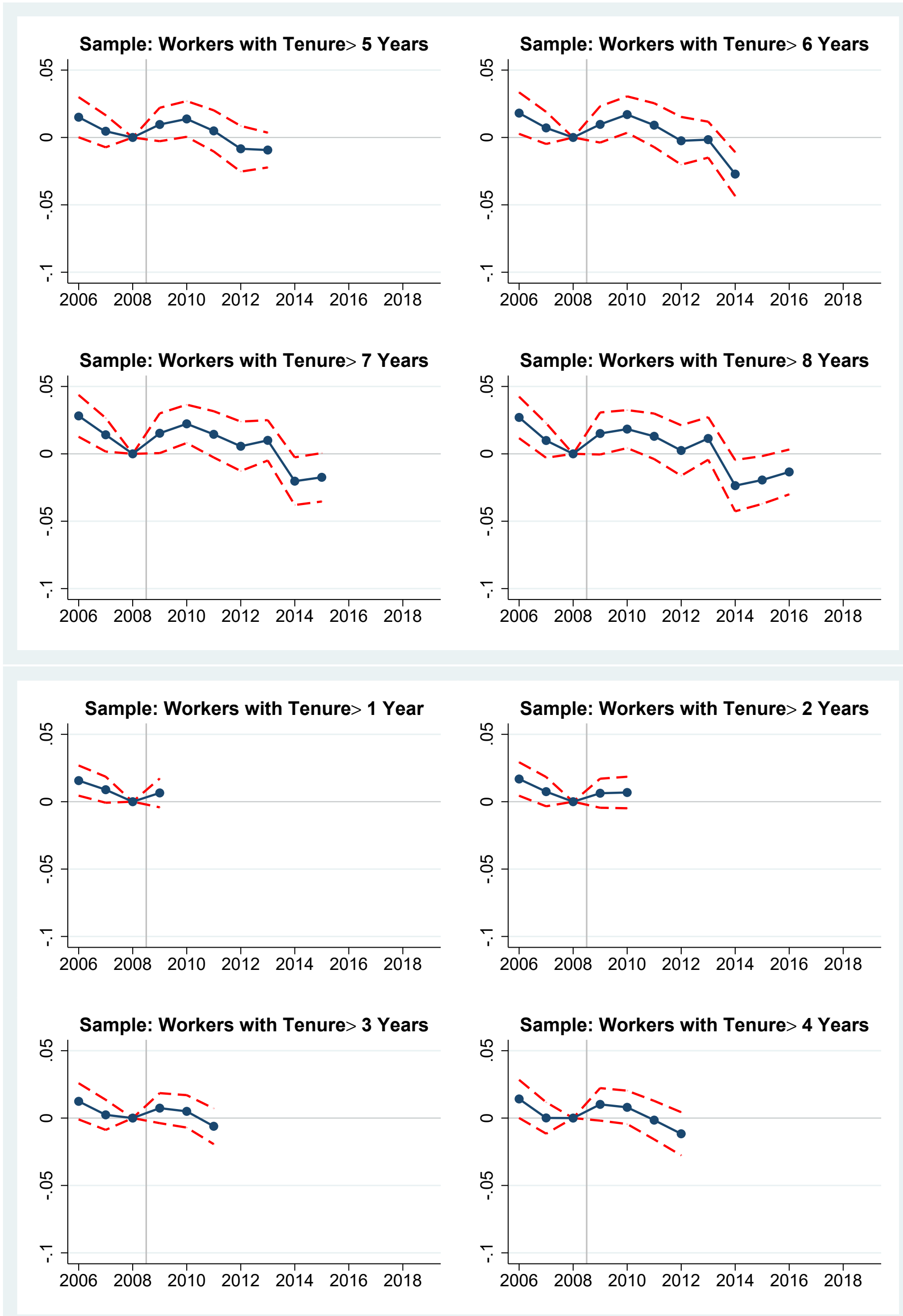
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to employees at work. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

Figure D3: The Dynamics of Hiring Wage Effects



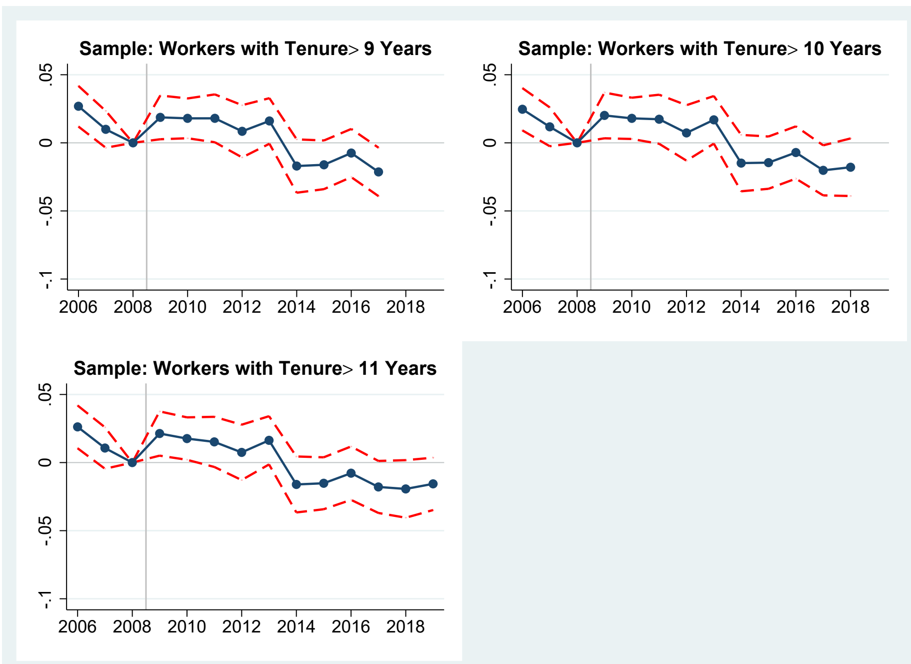
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to new hires at work. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

Figure D4: The Dynamics of Incumbent Wage Effects



Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

Figure D4: The Dynamics of Incumbent Wage Effects (Continued)



Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

Appendix E: The Estimation of Job Transition Rates

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

Appendix F: Synthetic Control Method

This paper combines the CEM method with the DID approach to identify the causal effect of BC's carbon tax on labor market outcomes. This identification strategy requires the common trend assumption. Although this paper provides many suggestive evidence to support the assumption, this assumption is difficult to verify. For example, if I prolong the pre-policy period, the trends in the unemployment are unparallel between BC and the re-weighted sample of ROC in the pre-policy, as shown in Figure F1. Hence, I adopt the SCM to estimate the causal effect because this method relaxes the common trend assumption. This appendix briefs the procedure of the SCM and presents the key results.

The SCM purposes to resemble a synthetic BC that share similar levels and paths of labor market outcomes with BC in the pre-policy period. It requires a large number of control provinces (i.e., donors) to construct the synthetic BC as a weighted average of donors from the ROC. Since BC and the synthetic BC share similar levels and paths in labor market outcomes prior to BC's carbon tax, the paths are likely similar immediately following the policy. Hence, the divergence of BC's labor market outcome from the synthetic BC's outcome after the policy captures the causal effect of BC's carbon tax.

Nevertheless, this method does not guarantee the existence of the synthetic BC. With only nine donors (i.e., the nine provinces) from the ROC of Canada, this method may fail to find a convex combination of donors that replicate the pre-policy paths of BC's labor market outcomes. I expand the donor list by creating the SC group for each of the 18 industries in BC instead of the BC economy as a whole. Meanwhile, I allow each of the 18 industries in each of the nine control provinces as a donor to resemble each of the 18 synthetic industries in BC. As such, the number of donors increases from nine to 162, largely increasing the likelihood of creating each of the synthetic industries. Each analysis involves the treatment industry in BC and the 162 industries from the ROC, indexed by $j = 1$ and $j = 2, 3, ..., 163$, respectively.

Moreover, I extend the pre-policy period from July 2005-June 2008 to July 2001-June 2008. Yip (2018) shows that BC's employment increased sharply between 2004 and 2005. Hence, he suggests that observations before 2005 should be dropped; otherwise, the common trend assumption may not be satisfied. Defining the pre-policy period as 2001-2007, Azevedo et al. (2020) argues that the employment effect of BC's carbon tax is found weak because the SC method relaxes the common trend assumption. I restrict the sample to observations between July 2001 and June 2015 so that this analysis is largely comparable to Azevedo et al. (2020). Following the main content of this paper, I define year t as the period between July in year $t-1$ and June in year t. In each analysis, samples are observed for time periods $t = 2002, 2003, ..., 2015$, where the pre- and post-policy periods are 2002-2008 and 2009-2015, respectively.

A particular synthetic industry of BC is constructed as a weighted average of the 162 donors. Let $\mathbf{W} = (w_2, w_3, ..., w_{163})^T$ be a vector of weights, where $w_j \in [0, 1]$ for all $j \in \{2, 3, ..., 163\}$ and $\sum_{j=2}^{163} w_j = 1$. Let $\tilde{\mathbf{Y}}$ be the outcome variable of our interest in the pre-policy period and $\mathbf{X}$ be a set of characteristics (i.e., predictor variables) in the pre-policy period. Let V be a matrix indicating the relative significance of each predictor variable. Following Abadie et al. (2010), $\mathbf{V}$ is chosen to minimize the prediction error of the outcome between the particular industry in BC and the synthetic industry, and $\mathbf{W}$ is chosen to minimize the pre-treatment root mean squared error with a given weighted average of donors, $||\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}||_{\mathbf{V}}$, where $\mathbf{X}_1$ is a vector of predictor variables of the industry in BC and $\mathbf{X}_0$ is a vector of predictor variables of the donors.

Each analysis uses the outcome variable of our interest in the first five years of the pre-policy period and the predictor variables to train the data. Ideally, the industry of our interest and the corresponding synthetic industry share a similar path of the outcome variable during the first five years of the pre-policy period. The outcome variable of the synthetic industry in the rest of the sample periods is calculated by the weighted average of the donors with weights equal to the optimal vector of weights $\mathbf{W}^*$. If the synthetic industry is able to replicate the path of the outcome variable of the industry of our interest in the rest of the pre-policy period, the synthetic industry is believed be able to replicate the counterfactual path of the outcome variable in the corresponding industry in BC in the post-policy period, in which the carbon tax policy had not been implemented. The divergence of the outcome variables between the synthetic industry and the corresponding industry captures the causal effect of BC's carbon tax.

This analysis mainly examines the employment and wage effects. If my goal is to identify the causal effect of BC's carbon tax on the unemployment rate, it requires me to measure the unemployment rate at the industry level so as to construct the synthetic group for each of the 18 industries in BC. Nevertheless, it is unclear which industry an unemployed worker belongs to. Our approach cannot be used to estimate the unemployment effect. Instead, I estimate the employment effect of BC's carbon tax. I aggregate employment at the industry level by province to construct the outcome variable of our interest.

I also examine the wage effects of BC's carbon tax. The main content of this paper finds that BC's carbon tax cuts the average hiring wage, not the average incumbent wage. Hence, this analysis will estimate the average wage effect among new hires and incumbent workers, respectively. Following the main content of this paper, I define new hires as workers with tenure less than a year and define incumbent workers as those with tenure over seven years. I construct the variables of interest by averaging the wage rate by industry in each province among the sample of new hires and incumbent workers, respectively.

Each of the analyses uses the same set of predictor variables. These predictor variables include the shares of male and female workers, the shares of high-, medium-, and low-educated workers, the shares of married and single workers, and the shares of workers aged 15-19, 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, and 55-59 in the labor force by province. These 16 predictors are calculated by averaging the corresponding shares in the pre-policy period. The shares are obtained from the Canadian LFS data.

The paths of the employment levels, the average hiring wages, and the average incumbent wages of the 18 industries in BC (solid line) and the corresponding synthetic industries (dash line) in Figures F2, F3, and F4, respectively. In each figure, the vertical grey line represents the year BC's carbon tax was implemented. The industries in BC and the corresponding synthetic industries share similar paths of the three outcome variables of our interest in the pre-policy period. As highlighted above, their paths are expected to be close with each other for a couple of years in the pre-policy period.[14] Since the paths are

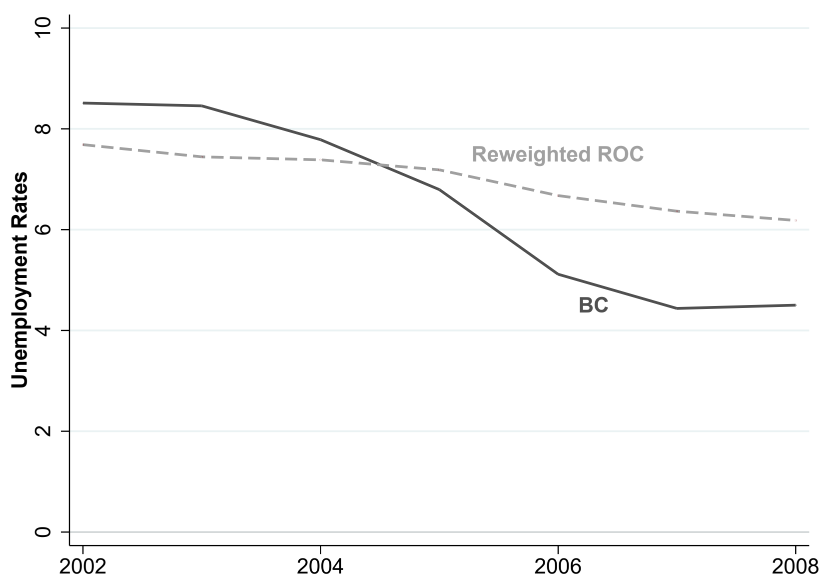
[14]The only exception happens in the construction sector. Although the paths of employment are sightly different between the construction sector and her synthetic group, they share similar paths of the average hiring and incumbent wages.

close throughout the entire pre-policy period, the paths of the 18 industries can be replicated by those of the synthetic industries in a couple of years of the pre-policy periods. Therefore, I expect the paths to follow similar paths immediately following BC's carbon tax. In other words, the synthetic industry is believed be able to replicate the counterfactual path of the outcome variable in the corresponding industry in BC in the post-policy period, in which the carbon tax policy had not been implemented. Nevertheless, many industries fall off their synthetic groups in the paths of the employment level, the average hiring wage rate, and the average incumbent wage rate.

Figures F5 and F6 resemble the aggregate labor market of BC from the 18 industries. BC's employment level is computed by adding up the employment level of all the industries. I calculate the average hiring and incumbent wages of BC using their weighted averages, with the weights of each industry equal to her shares in BC in 2007, a year before the policy. The shares of various industries are different and the shares of new hires and incumbent workers within the same industry can also be different. It is clear in Figure F5 that BC's employment falls off the paths of the employment of the synthetic BC after BC's carbon tax. A similar result can also be seem in the average hiring wage presented in Figure F6. The decline in the incumbent wage is not obvious in the first few years, but such a decline becomes significant in the later stage of the policy.

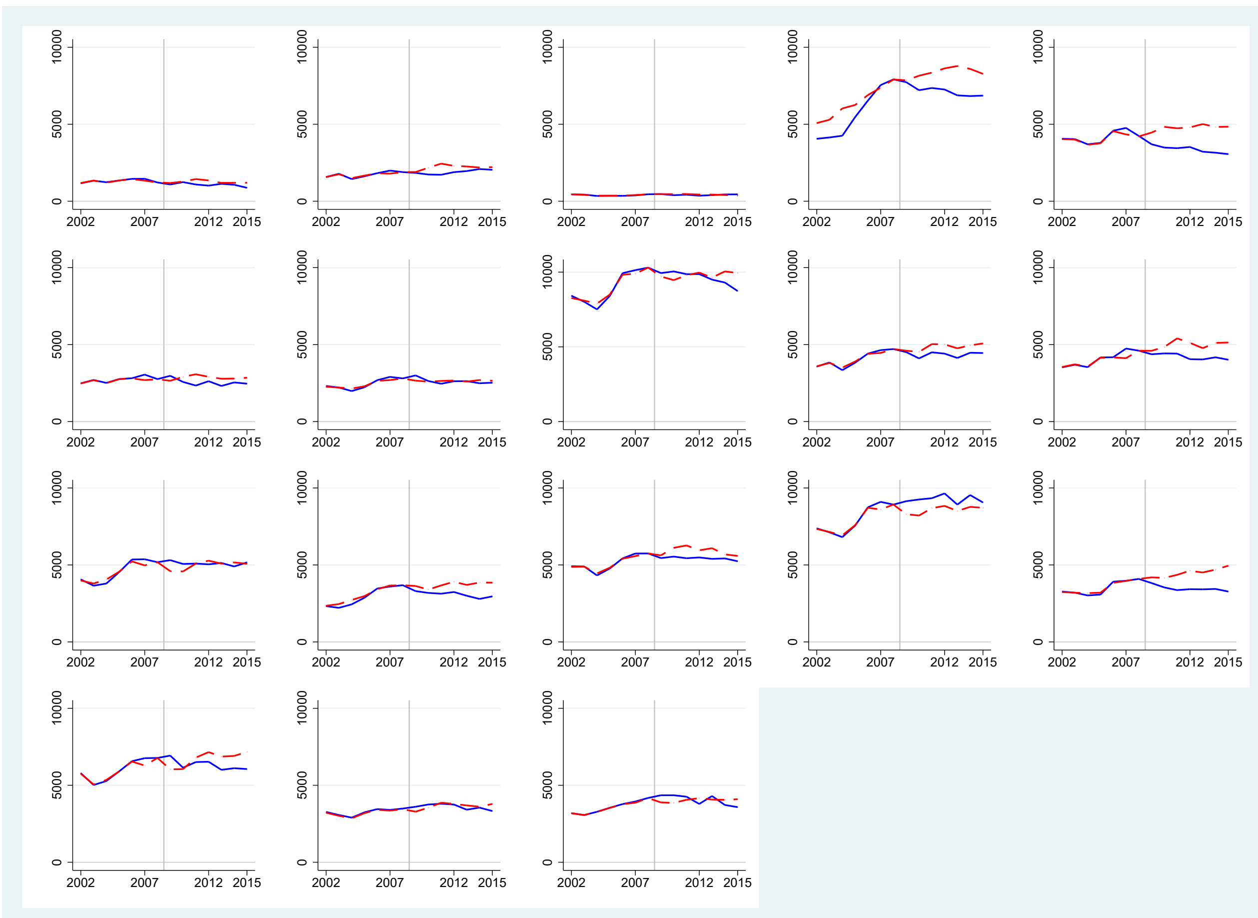
The findings of this appendix suggest that the results in the main content of the paper are robust when the common trend assumption is relaxed.

Figure F1: The Dynamics of Unemployment Rates



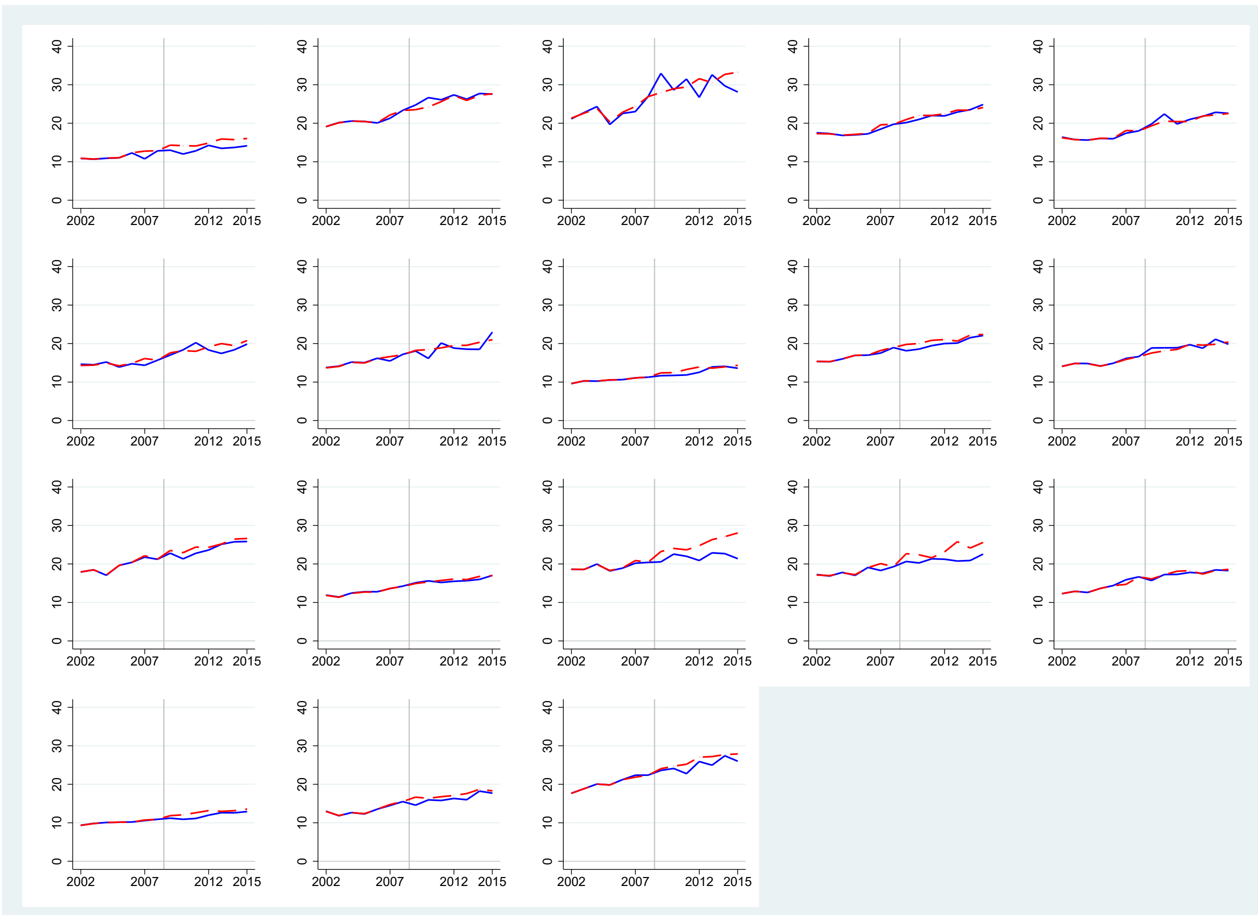
Note: This graph presents the paths of the unemployment rate in BC (solid line) and the re-weighted sample of ROC (dash lines).

Figure F2: The Dynamics of Employment Effects by Industry



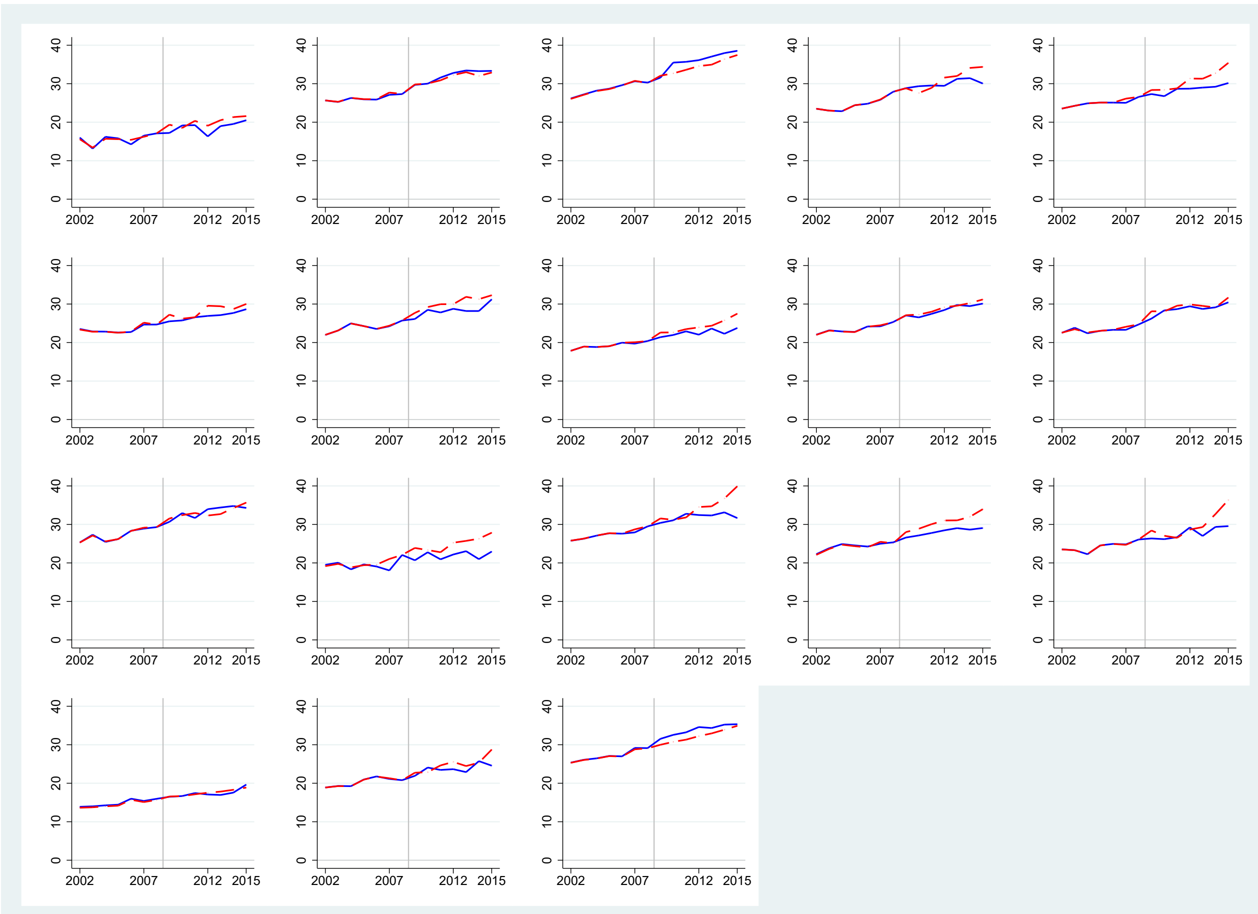
Note: This graph presents the employment paths of 18 industries in BC (solid line) and the corresponding synthetic industries (dash line). From top to bottom and from left to right, industries are (1) agriculture, (2) forestry, fishing, mining, oil, & gas, (3) utilities, (4) construction, (5) manufacturing: durables, (6) manufacturing: non-durables, (7) wholesale trade, (8) retail trade, (9) transportation & warehousing, (10) finance, insurance, real estate & leasing, (11) professional, scientific, & technical services, (12) management, administrative. other support, (13) educational services, (14) health care & social assistance, (15) information, culture, & recreation, (16) accommodation & food services, (17) other services, and (18) public administration.

Figure F3: The Dynamics of Hiring Wage Effects by Industry



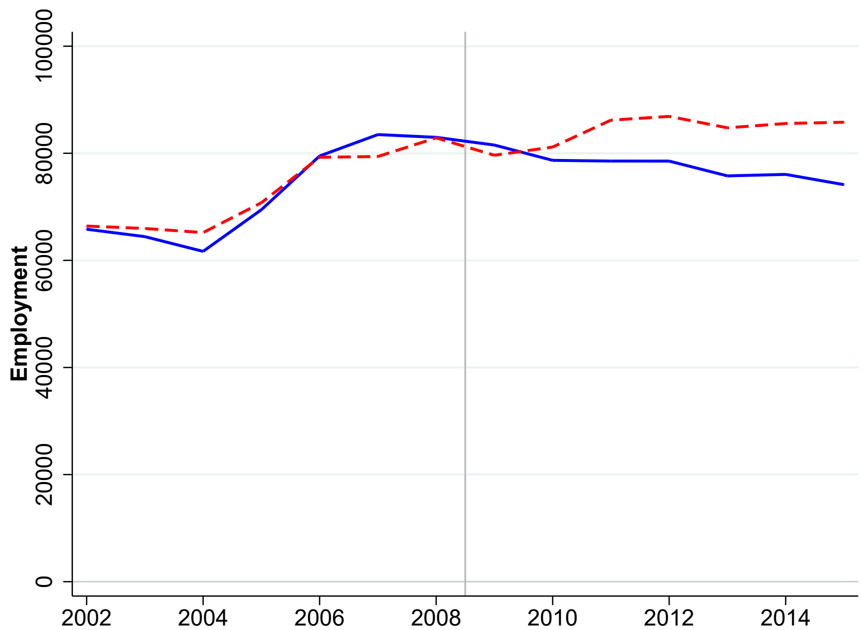
Note: This graph presents the paths of the average hiring wage in 18 industries in BC (solid line) and in the corresponding synthetic industries (dash line). From top to bottom and from left to right, industries are (1) agriculture, (2) forestry, fishing, mining, oil, & gas, (3) utilities, (4) construction, (5) manufacturing: durables, (6) manufacturing: non-durables, (7) wholesale trade, (8) retail trade, (9) transportation & warehousing, (10) finance, insurance, real estate & leasing, (11) professional, scientific, & technical services, (12) management, administrative. other support, (13) educational services, (14) health care & social assistance, (15) information, culture, & recreation, (16) accommodation & food services, (17) other services, and (18) public administration.

Figure F4: The Dynamics of Incumbent Effects by Industry



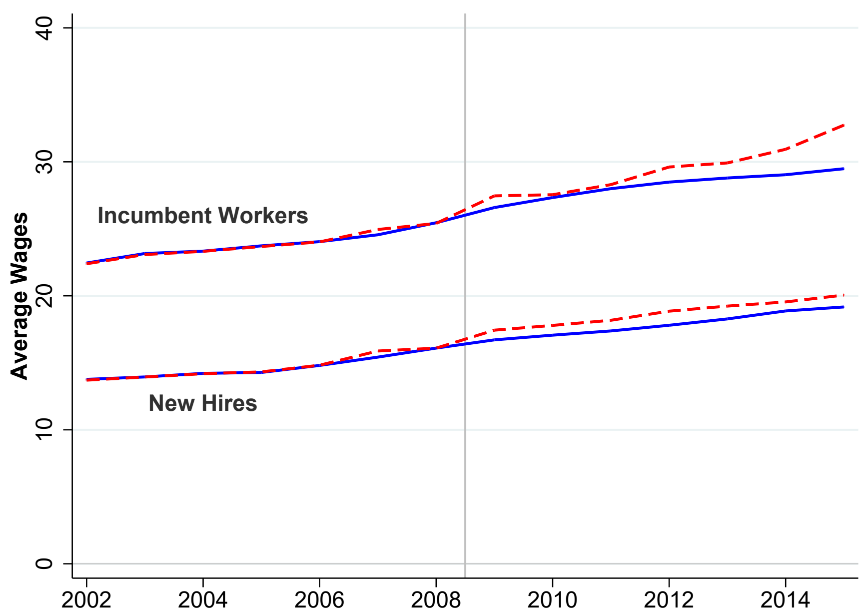
Note: This graph presents the paths of the average incumbent wage in 18 industries in BC (solid line) and in the corresponding synthetic industries (dash line). From top to bottom and from left to right, industries are (1) agriculture, (2) forestry, fishing, mining, oil, & gas, (3) utilities, (4) construction, (5) manufacturing: durables, (6) manufacturing: non-durables, (7) wholesale trade, (8) retail trade, (9) transportation & warehousing, (10) finance, insurance, real estate & leasing, (11) professional, scientific, & technical services, (12) management, administrative. other support, (13) educational services, (14) health care & social assistance, (15) information, culture, & recreation, (16) accommodation & food services, (17) other services, and (18) public administration.

Figure F5: The Dynamics of Employment Effects



Note: This graph presents the paths of employment in BC (solid line) and in the synthetic BC (dash line).

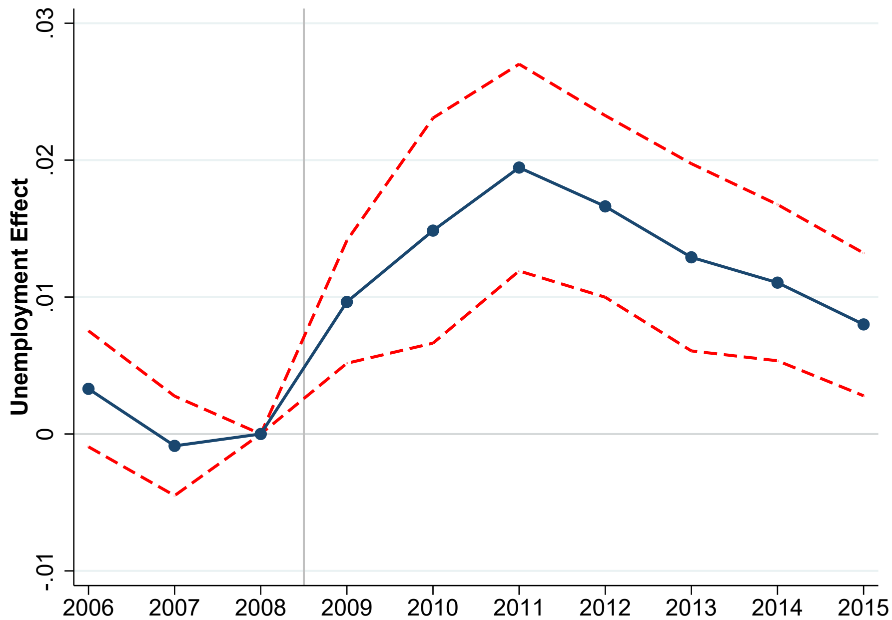
Figure F6: The Dynamics of Hiring Wage and Incumbent Wage Effects



Note: This graph presents the paths of the average hiring and incumbent wages in BC (solid line) and in the synthetic BC (dash line).

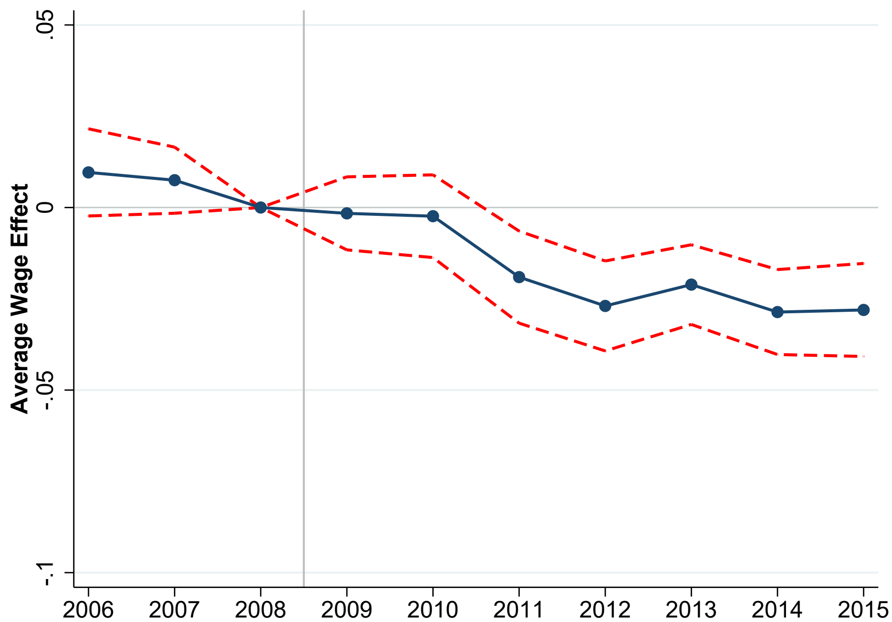
Appendix G: Robustness Check: Trade Shocks

Figure G1: The Dynamics of Unemployment Effects



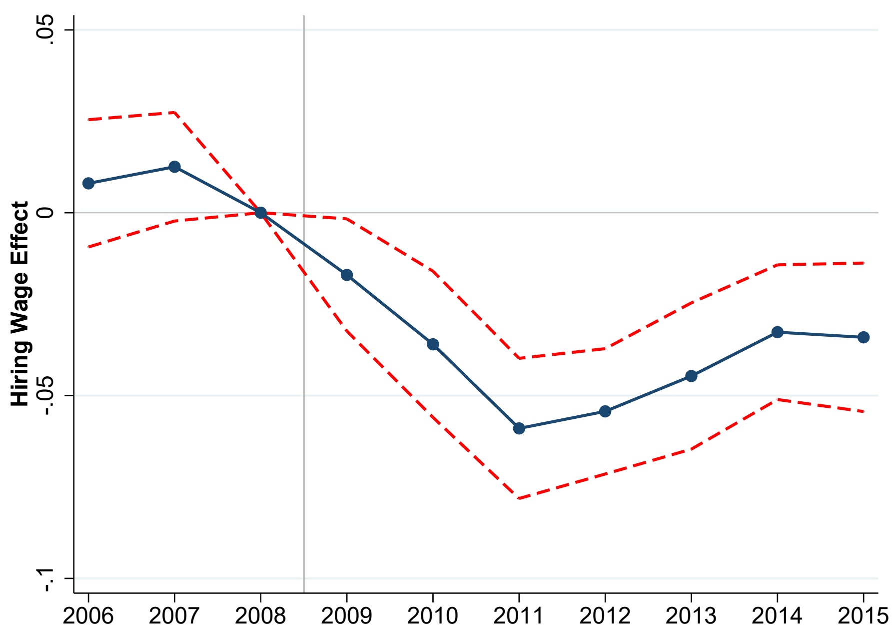
Note: The regressand is a dummy variable of being unemployed. Samples are restricted to the labor force. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

Figure G2: The Dynamics of Average Wage Effects



Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to employees at work. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

Figure G3: The Dynamics of Hiring Wage Effects



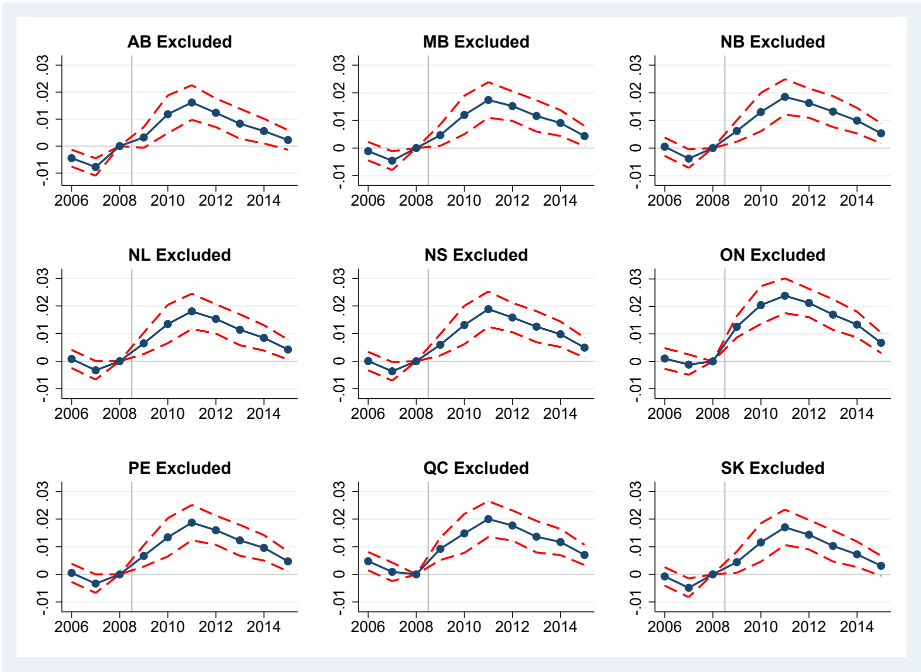
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to new hires at work. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

Figure G4: The Dynamics of Incumbent Wage Effects

Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. The vertical line represents the first month of the carbon tax policy. The dashed line represents a 95 percent confidence interval.

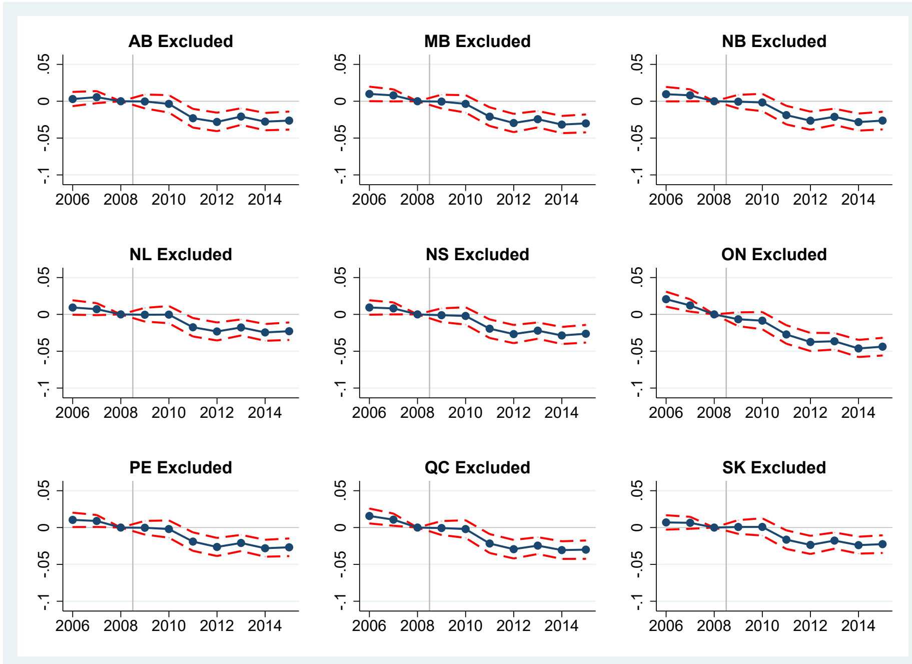
Appendix H: Insensitivity to Choices of Control Provinces

Figure H1: The Dynamics of Unemployment Effects



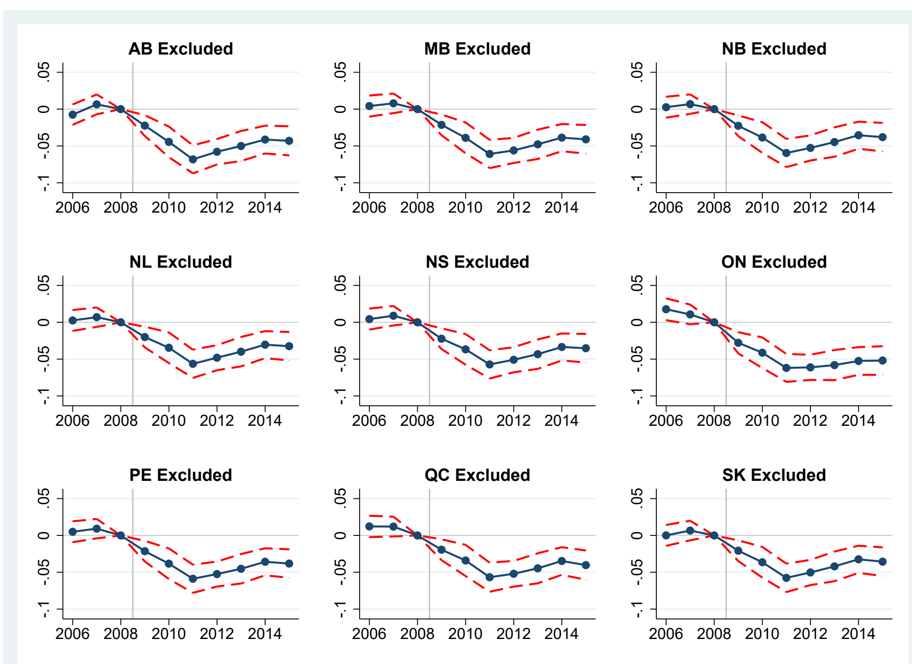
Note: The regressand is a dummy variable of being unemployed. Samples are restricted to the labor force. In each figure, one control province is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H2: The Dynamics of Average Wage Effects



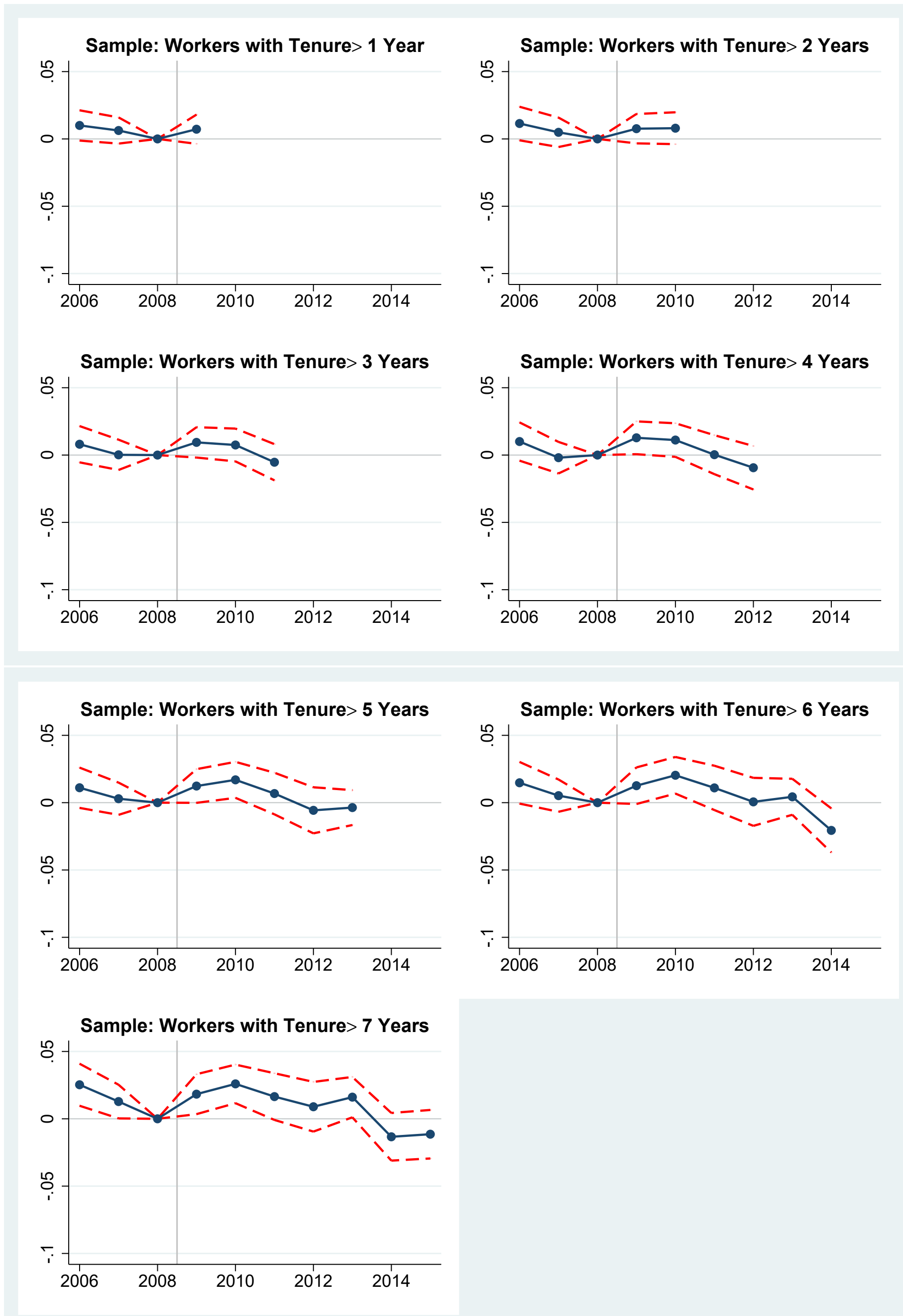
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to employees at work. In each figure, one control province is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H3: The Dynamics of Hiring Wage Effects



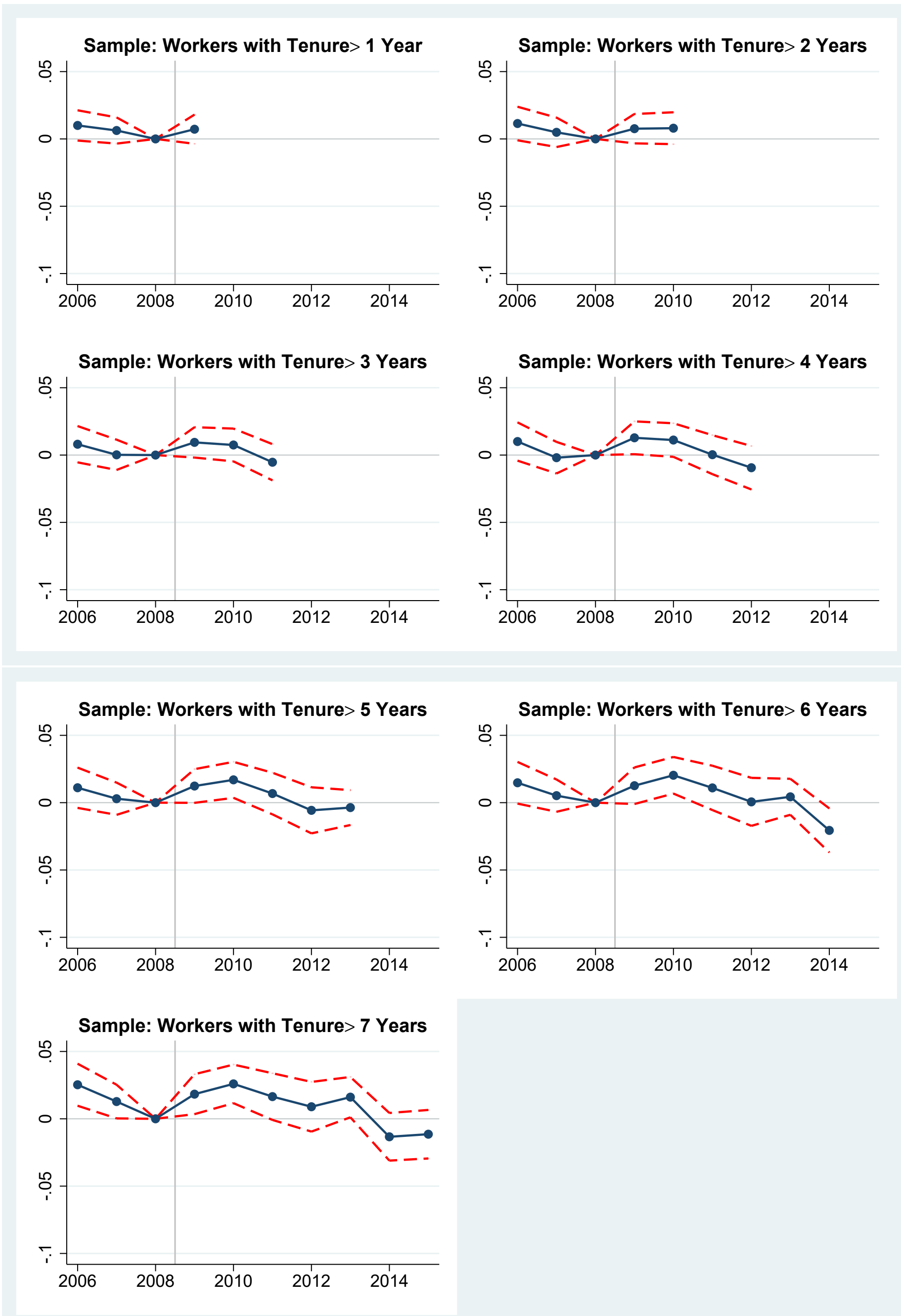
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to new hires at work. In each figure, one control province is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H4: The Dynamics of Incumbent Wage Effects with AB Excluded



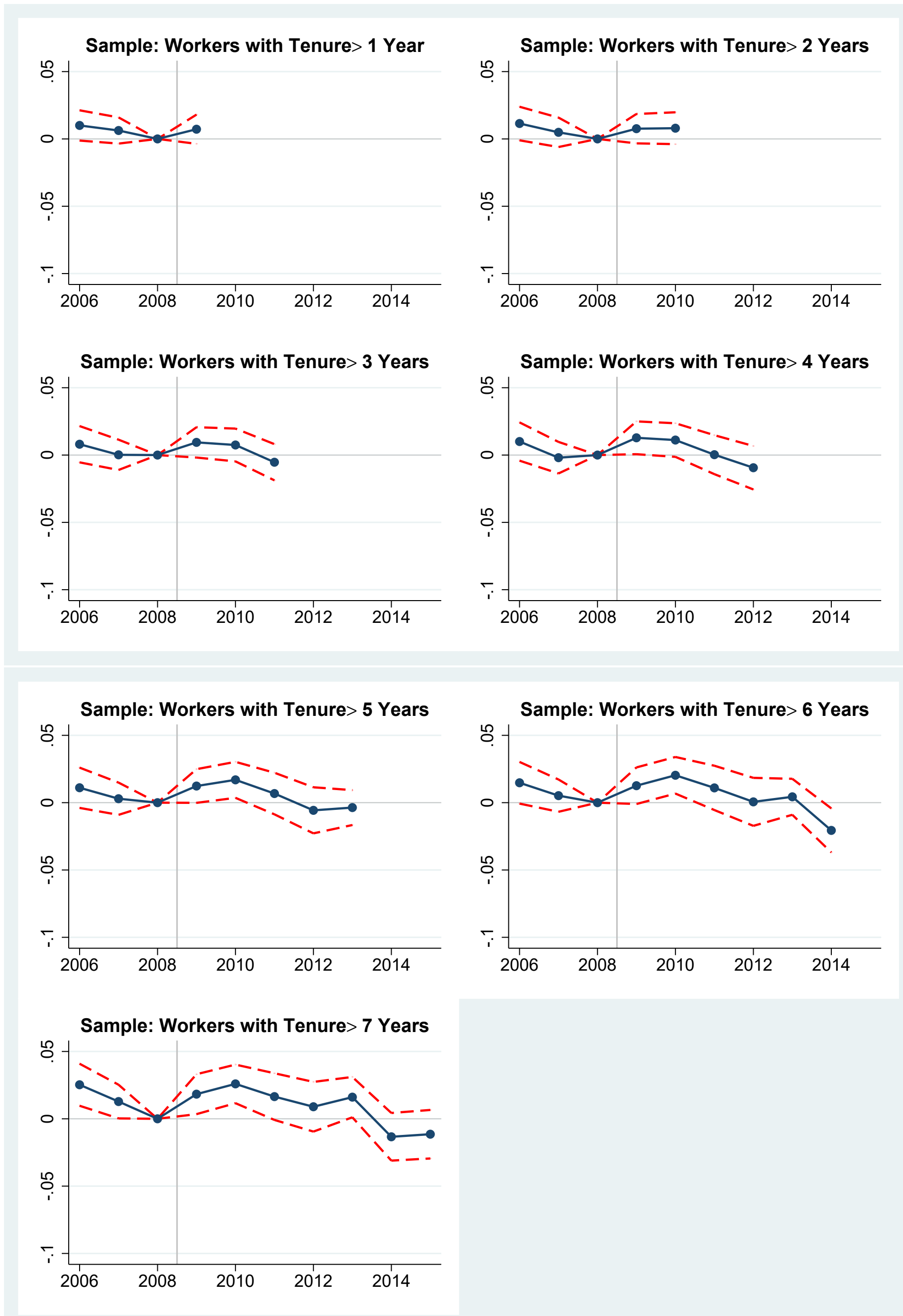
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. AB is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H5: The Dynamics of Incumbent Wage Effects with MB Excluded



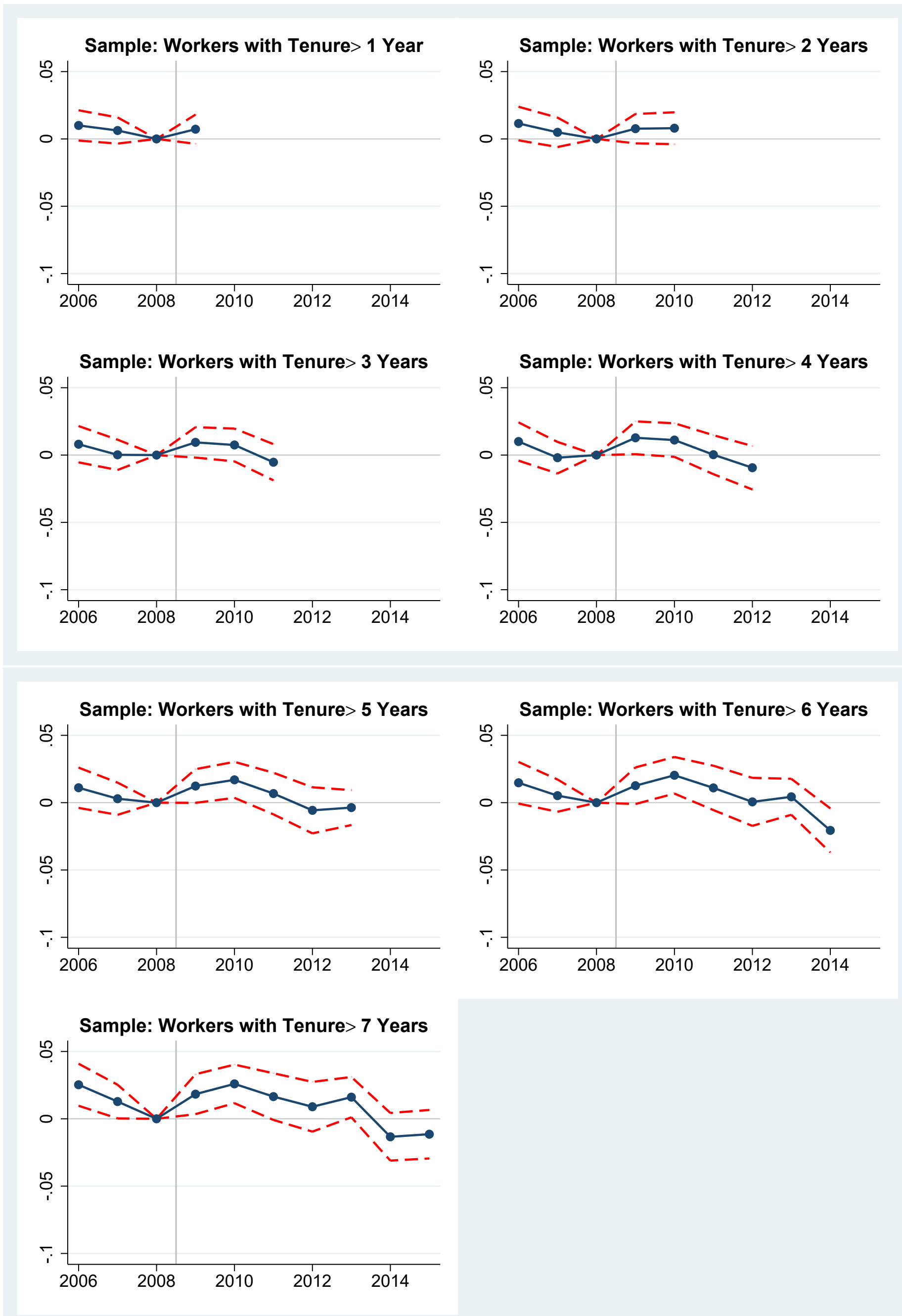
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. MB is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H6: The Dynamics of Incumbent Wage Effects with NB Excluded



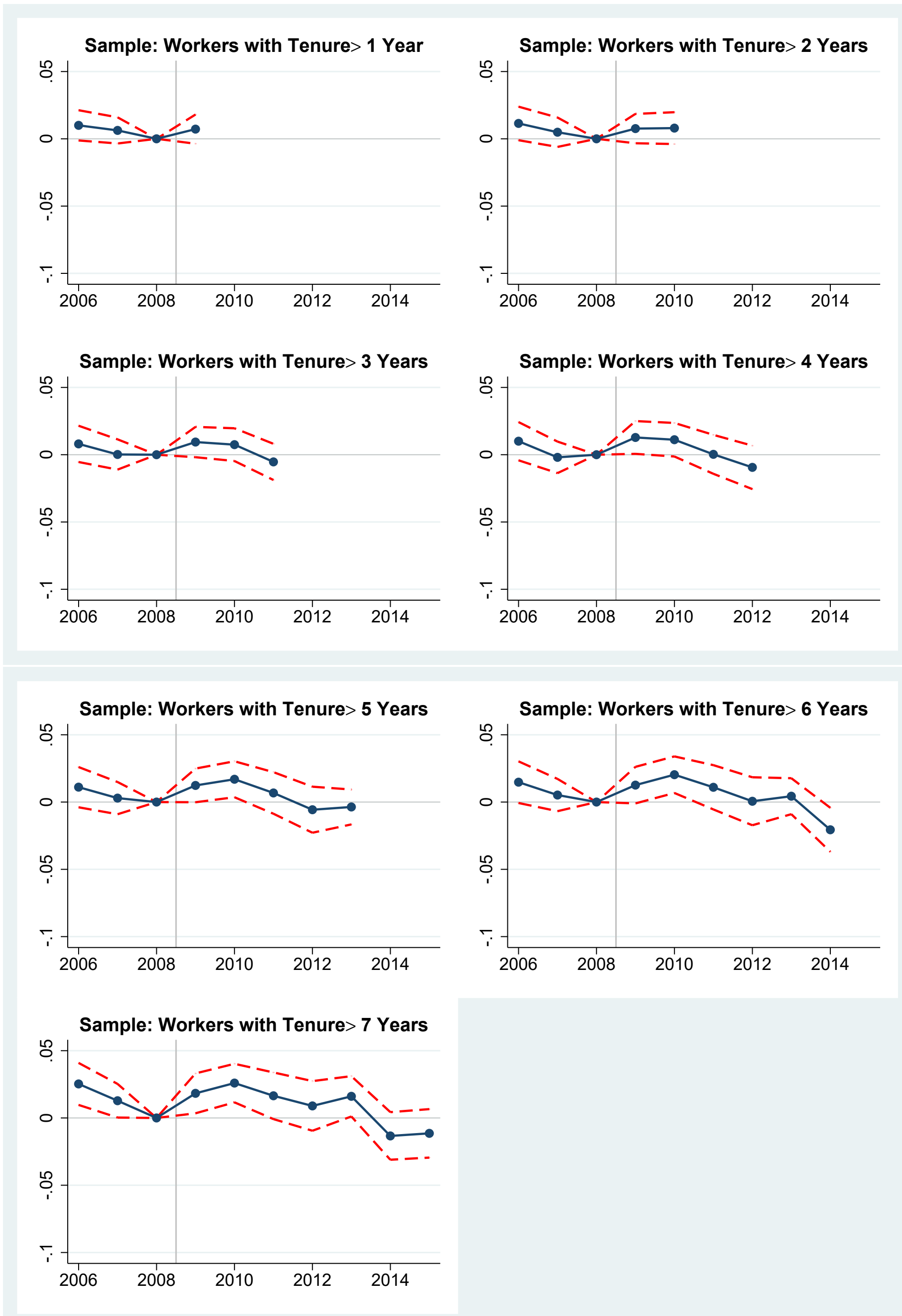
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. NB is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H7: The Dynamics of Incumbent Wage Effects with NL Excluded



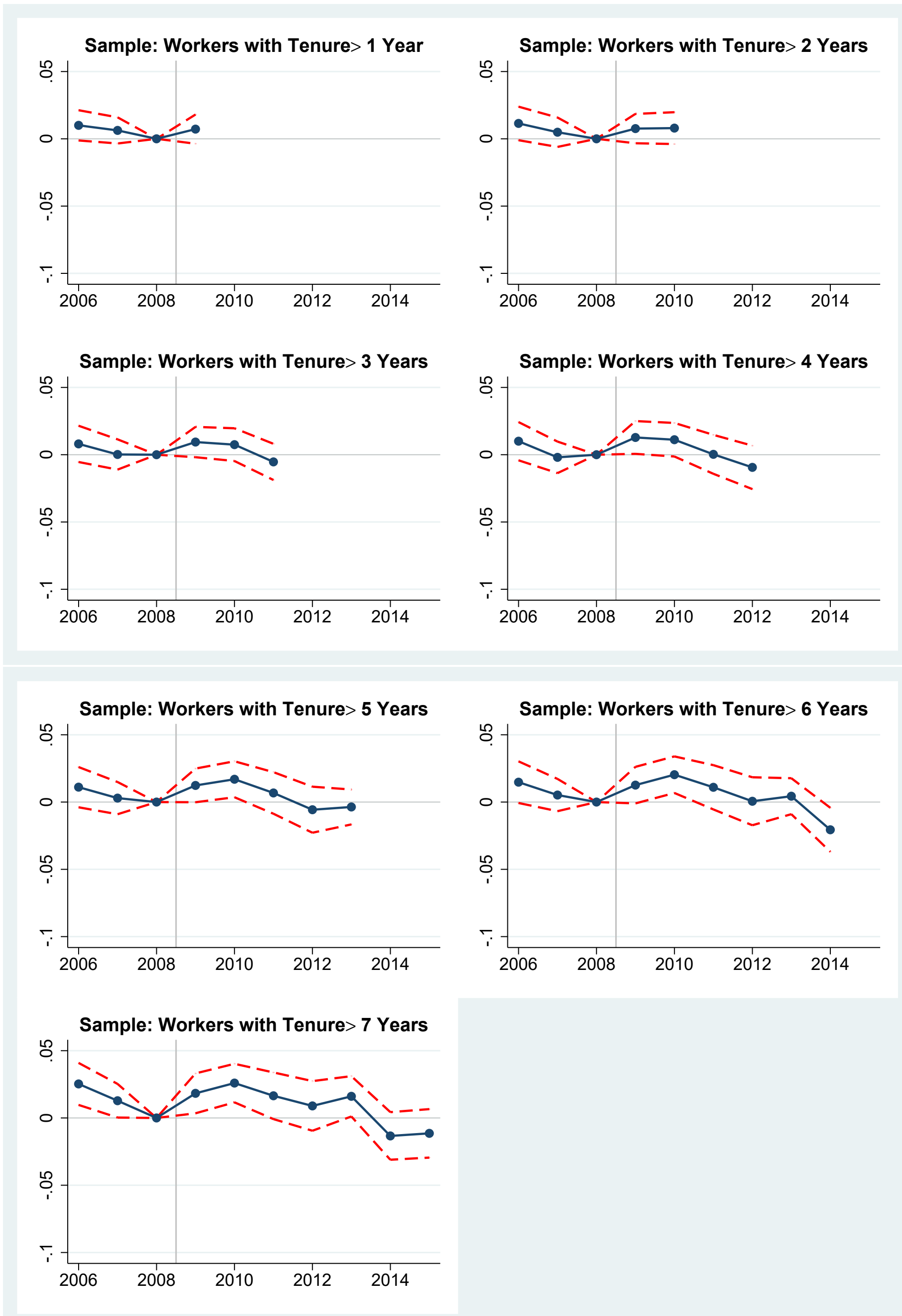
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. NL is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H8: The Dynamics of Incumbent Wage Effects with NS Excluded



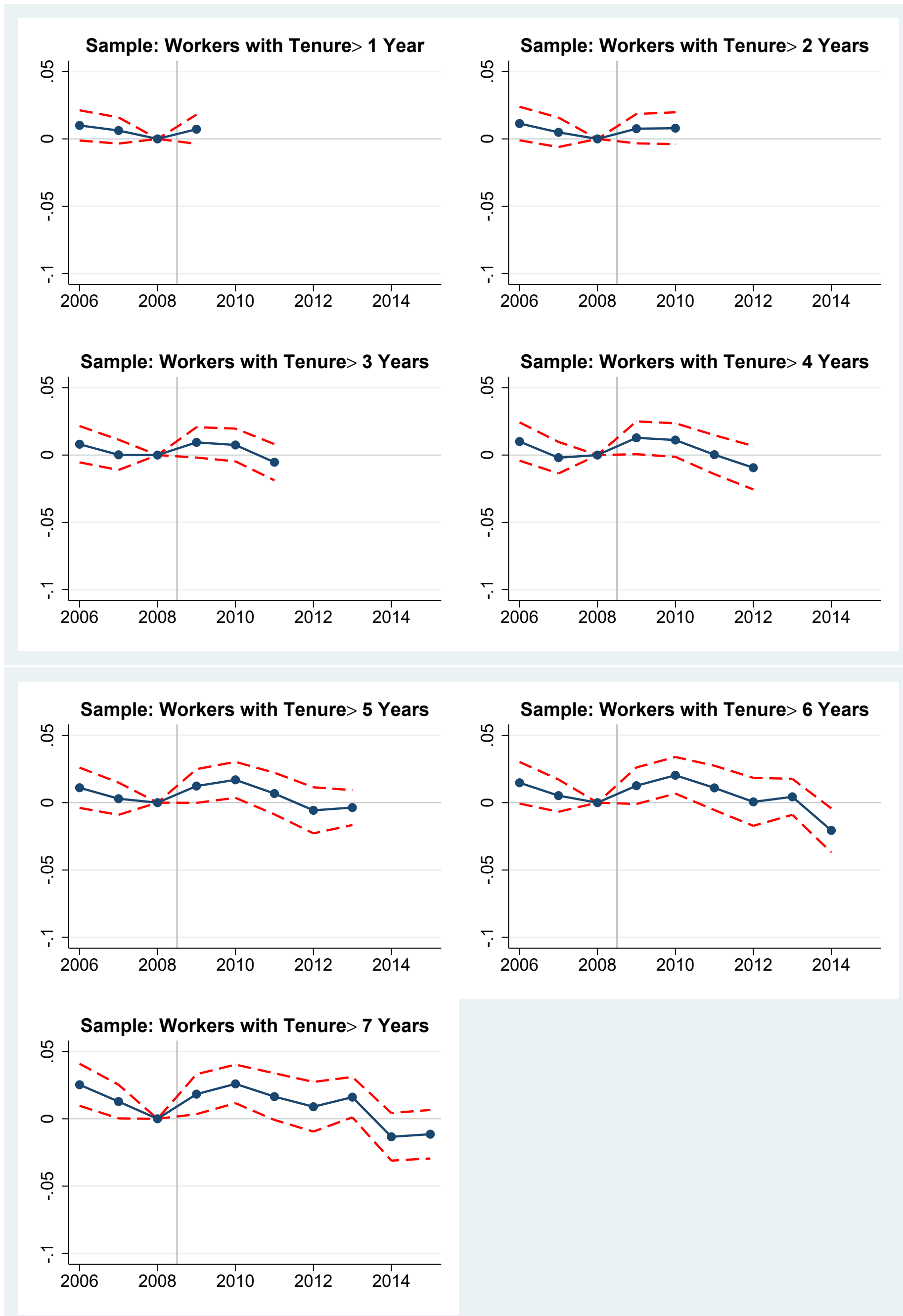
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. NS is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H9: The Dynamics of Incumbent Wage Effects with ON Excluded



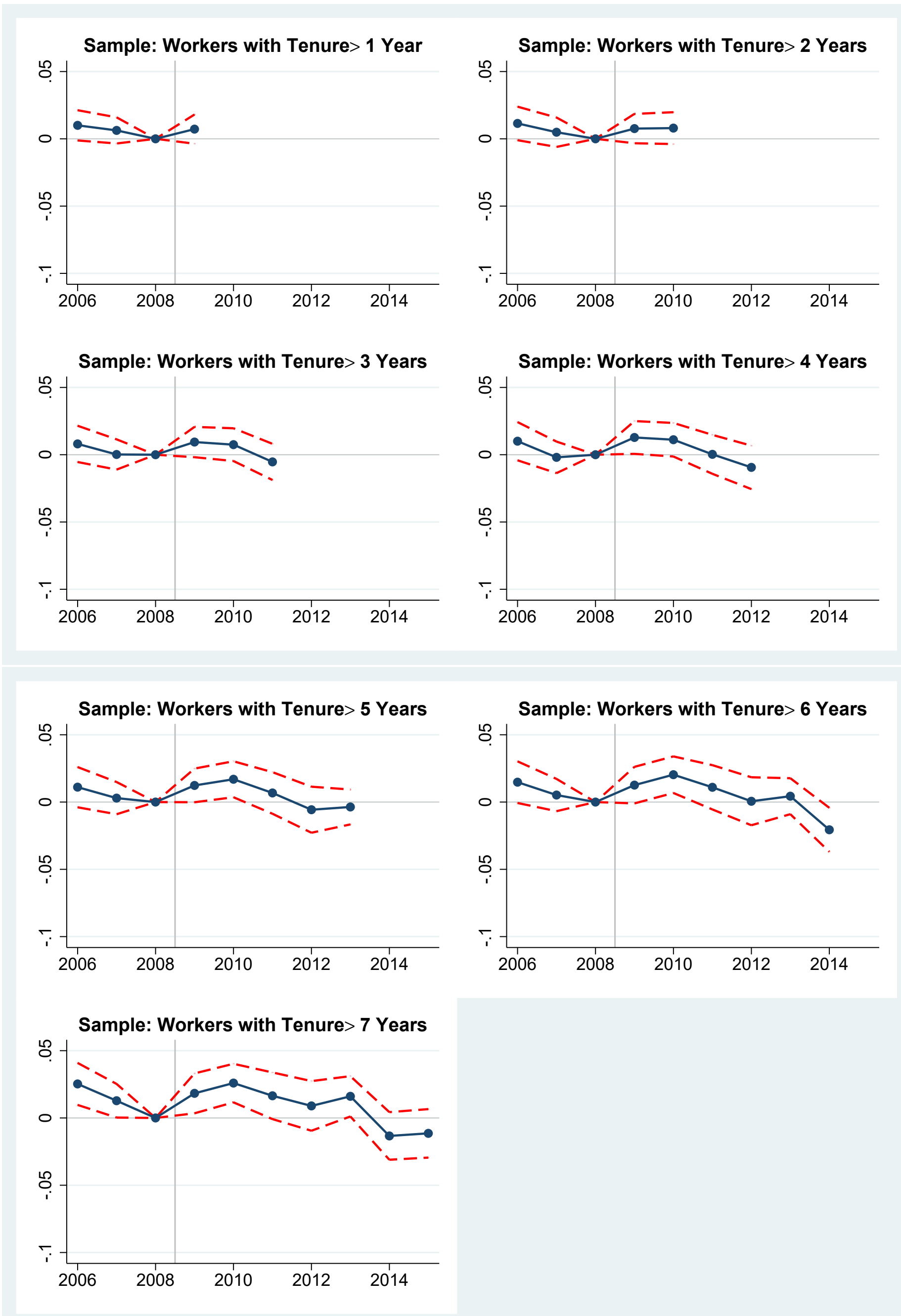
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. ON is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H10: The Dynamics of Incumbent Wage Effects with PE Excluded



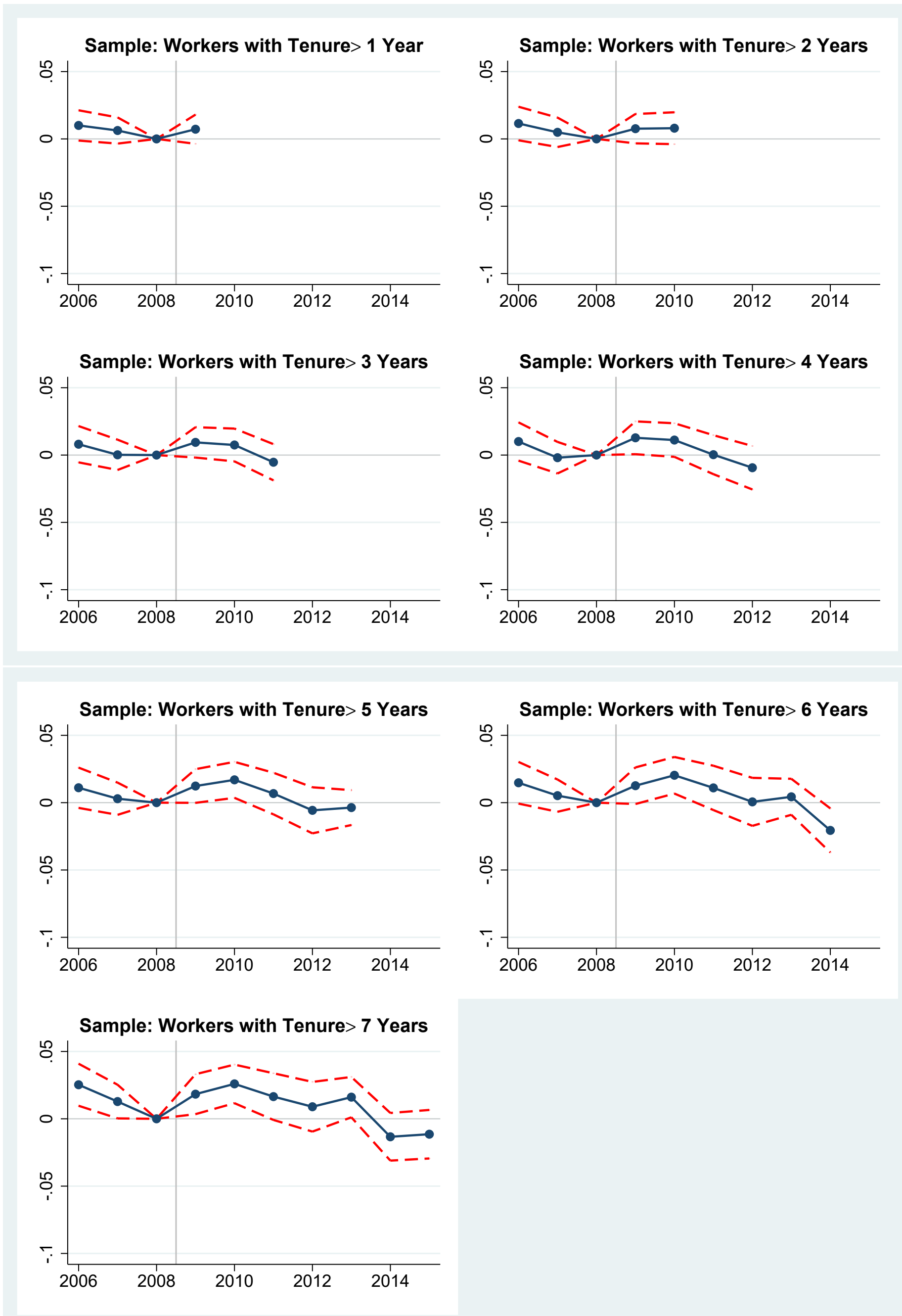
Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. PE is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Figure H11: The Dynamics of Incumbent Wage Effects with QC Excluded



Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. QC is excluded from the sample. The dashed line represents a 95 percent confidence interval.

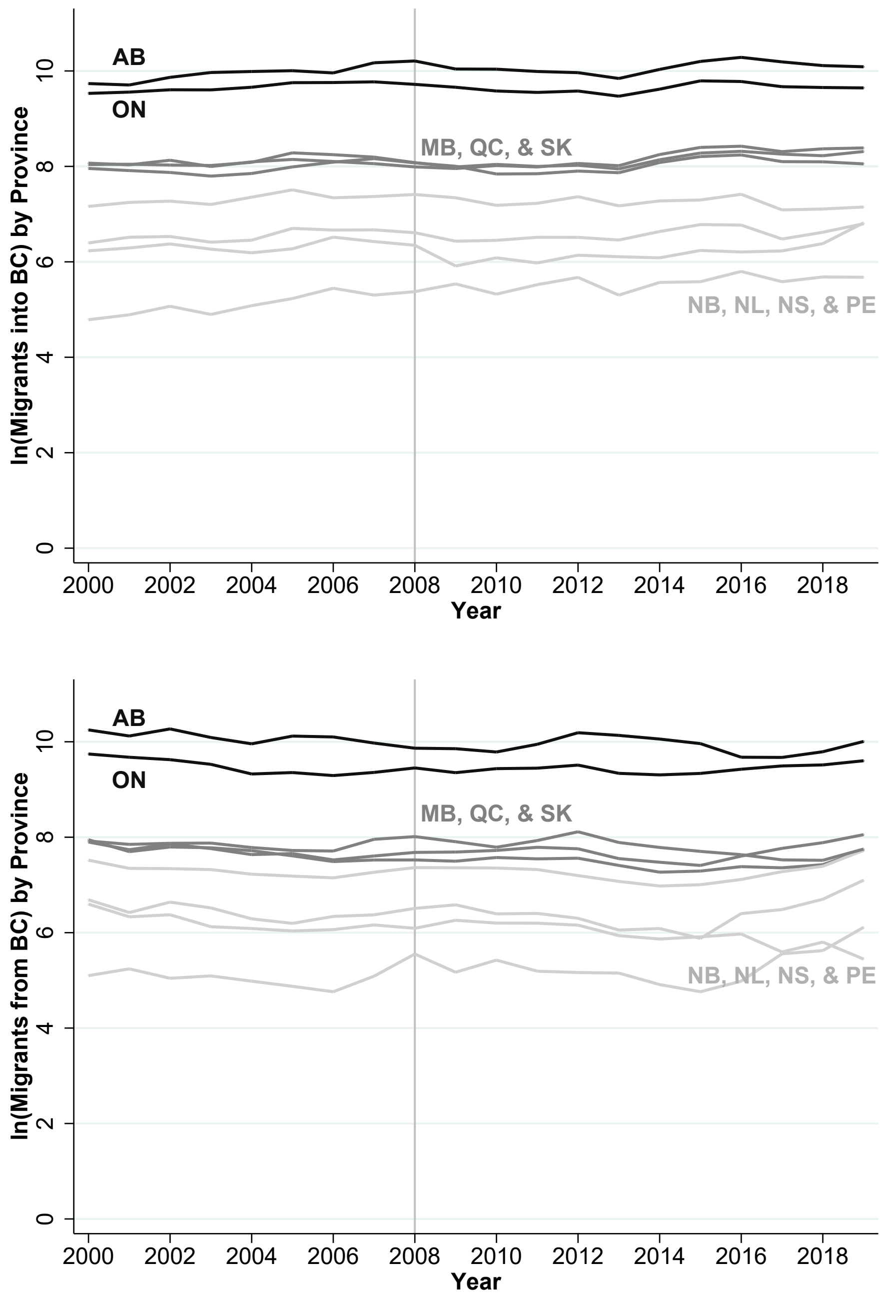
Figure H12: The Dynamics of Incumbent Wage Effects with SK Excluded



Note: The regressand is the logarithm of an hourly wage rate in the main job. Samples are restricted to incumbent workers at work. SK is excluded from the sample. The dashed line represents a 95 percent confidence interval.

Appendix I: Interprovincial Migrants by Province

Figure I1: The Dynamics of Migrants into and out of BC by Province



Note: Data is downloaded from Statistics Canada.