On the Labor Market Consequences of Environmental Taxes

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Abstract

This paper uses individual-level data to estimate the labor market consequences of environmental policies. The focus on workers rather than industries as the unit of analysis allows me to investigate previously unobserved outcomes, such as (i) unemployment rates, labor force participation rates, and the natures of layoffs and new hires and (ii) the distributional effects of these policies, both of which are critical to understanding costs and their distributions associated with environmental policies. Exploiting the introduction of a revenue-neutral carbon tax in British Columbia, the results suggest that environmental taxes, although revenue-neutral, are regressive: they tax away jobs disproportionately, more likely from less-educated workers. In particular, the policy increases the unemployment rates of medium- and low-educated males by 1.4 and 2.4 percentage points, respectively. The policy is implemented mainly at the expense of the low-educated: because of these job losses, some engage in temporary and part-time jobs, and, eventually, some, being discouraged workers, leave the labor force, with the caveat that the effects could, though unlikely, be partially driven by an unobserved differential shock to BC's labor market.

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1 Introduction

Environmental policies tend to decrease manufacturing employment (Greenstone, 2002; Kahn and Mansur, 2010; Walker, 2011; Curtis, 2014), incurring strong resistance from policymakers.[1] These policies may shift employment from manufacturing to other sectors. Yet we know little of the overall effects and the distributional effects of these policies in the labor market.

This paper first analyzes how a revenue-neutral environmental tax affects the labor market and how such effects are distributed across demographic groups.[2] Next, it investigates the economic causes and consequences of these distributional effects. As noted by Rausch and Schwarz (2016), the public acceptance of environmental taxes depends on their distributional consequences. Examining the distributional effects speaks directly to the public concern and enhances our understanding of how an environmental tax as a policy tool (or a sectoral demand shock) affects labor market activities. Meanwhile, policymakers could also be interested in the distributional effects to redistribute tax revenues accordingly.

Economic theory makes ambiguous predictions of the impact of revenue-neutral environmental taxes on unemployment. Environmental taxes decrease firms' profits and thus labor demands. On the other hand, the tax revenues are recycled through other tax reductions, which may then increase firms' profits, thereby increasing labor demand. The ambiguous effect on labor demand and thus unemployment leaves economists with an empirical question on how revenue-neutral environmental taxes affect unemployment and other labor market outcomes.[3]

To study the impacts of similar revenue-neutral tax policies on labor market activities, I use a unique opportunity provided by the introduction of a revenue-neutral carbon tax policy in British Columbia (BC). A difference-in-differences (DID) approach is used to capture the causal effect of the policy, which was unique in North America because of the revenue neu-

[1] For example, manufacturing employment is found to decline substantially under the Clean Air Act (Greenstone, 2002; Walker, 2011) and a cap-and-trade program (Curtis, 2014). Meanwhile, Berman and Bui (2001) and Martin et al. (2014) find no support that the reduction in manufacturing employment is large under stringent environmental regulations in Los Angeles, the United States, or a higher carbon tax in the United Kingdom.

[2] Revenue-neutral environmental taxes recycle taxes on polluting activities to lower other taxes. Readers who are interested in the literature on "double dividend hypothesis" of revenue-neutral environmental taxes are referred to Manresa and Sancho (2005), Chiroleu-Assouline and Fodha (2006), Bento and Jacobsen (2007), Dissou and Sun (2013), and Williams et al. (2015).

[3] A more rigorous theoretical foundation on the employment effect of revenue-neutral environmental taxes can be found in Hafstead and Williams III (2016) and Sun and Yip (2017). A theoretical model that assesses the incidence of environmental taxes with household heterogeneity can be found in Rausch and Schwarz (2016).

trality of the carbon tax. This policy is suitable for a policy evaluation because it provides numerous control provinces that are unaffected by the policy. I use individual-level data from the Canadian Labor Force Survey (LFS) to answer the questions. This detailed microdata allows me to estimate the impacts on not only the unemployment rate and other labor market outcomes but also the distributional effects across demographic groups, providing a complete picture of the channels through which a carbon tax policy affects labor market activities.

A number of substantive findings emerge from this paper. First, the policy raises the overall unemployment rate by 1.3 percentage points. Second, unemployment effects appear regardless of gender or educational categories and are significantly different across demographic groups. Males of lower educational levels suffer the brunt of unemployment effects: an increase of 0.6, 1.4, and 2.4 percentage points for high-, medium-, and low-educated males. Third, the proportion of job losers increases regardless of gender or educational categories, suggesting that the unemployment induced by the policy is largely involuntary. Fourth, layoffs for the medium-educated male unemployed are more likely temporary after the policy. Fifth, while the policy does not incline layoffs towards temporary or part-time workers, there are more temporary and part-time new hires of medium- and low-educated males after the policy.

Sixth, the policy eventually reduces the labor force participation (LFP) rate of the low-educated. Perhaps because the unemployment effect is substantial and persistent (lasting seven years) for the low-educated, their LFP rate drops by two percentage points after the second year of the policy. No statistically significant effects are found for medium- or high-educated groups. Seventh, the heterogenous effects may be attributed to the distribution of the energy-intensity of employment. Manufacturing industries that are more energy-intensive experience larger impacts; the medium- and the low-educated engage in manufacturing positions that are 16.3-20.7 and 17.5-26.0 percent more energy-intensive than their high-educated counterparts. These seven results enhance our understanding of the channels through which a carbon tax policy affects labor market activities, including the incidence of unemployment, the natures of layoffs and new hires, and the LFP decision.

Many studies document the environmental improvements of this policy (Elgie and McClay, 2013; Murray and Rivers, 2015). This paper, in contrast, suggests that environmental taxes, even though revenue-neutral, are implemented at the expense of the low-educated: they suffer more job losses, some engage in temporary and part-time jobs, and, eventually, some, being discouraged workers, leave the labor force. While the literature on environmental injustice

expresses concerns about whether environmental regulations shift pollution into regions with more traditionally disadvantaged groups (Banzhaf and Walsh, 2008; Kaswan, 2008; Gamper-Rabindran and Timmins, 2011), this paper raises concerns on the labor market analogues of the environmental injustice.

This paper also contributes to the literature on the regressivity of environmental taxes (Grainger and Kolstad, 2010; Williams et al., 2015; Williams III, 2016). These taxes could be regressive on the use side of income because the poorer tend to spend a larger proportion of their income on polluting goods (Metcalf, 1999; Fullerton et al., 2011). Since these taxes reduce returns to factors of production that are disproportionately owned by the rich, these taxes dampen the regressivity from the source side of income (Fullerton and Heutel, 2007; Araar et al., 2011). This paper reveals the regressivity of environmental taxes from another source side of income: they tax away jobs disproportionately, more likely from the less-educated.

This paper is closely related to Yamazaki (2017). Using industry-level data, Yamazaki (2017) finds that BC's carbon tax increases overall employment. Closely following his estimation strategy, this paper is able to replicate the positive employment effect in the online Appendix. However, this paper provides evidence that the common trend assumption is unlikely satisfied in Yamazaki (2017).

The employment trends in BC and the rest of Canada are parallel in the pre-policy period (i.e., July 2001-June 2008) except July 2004-June 2005. I find an unobserved differential shock in BC during July 2004-June 2005: there was a sharp increase in BC's employment level relative to the rest of Canada. Using July 2001-June 2008 as the pre-policy period (as in Yamazaki (2017)), the average BC's employment in the pre-policy period falls off greatly the parallel trend, leading to biased estimates. Meanwhile, the parallel employment trend during July 2005-June 2008 does provide a solid support on the common trend assumption immediately following the implementation of the policy (i.e., July 2008). Hence, this paper adds to this literature by pointing out the appropriate pre-policy period (i.e., July 2005-June 2008) when other effects of the same policy are examined. As a caveat, since there is no direct test on the common trend assumption, the results of this paper could, though unlikely, be partially driven by an unobserved differential shock similar to the one during July 2004-June 2005. I relegate the details of this analysis to the online Appendix.

In addition to these contributions, at least two major differences distinguish this paper from Yamazaki (2017). First, this paper examines the policy effects on different labor mar-

ket outcomes. Yamazaki (2017) investigates the effects on the overall employment and wage levels. In contrast, this paper focuses on workers rather than industries as the unit of analysis to investigate previously unobserved outcomes, such as unemployment rates, LFP rates, and the natures of layoffs and new hires. Second, the individual-level data allows me to estimate the distributional effects across demographic groups. These two contributions are critical to understanding costs and their distributions associated with the policy and are revelent to policymakers redistributing the tax revenues.

The paper is structured as follows. Section 2 presents the details of BC's carbon tax policy. Section 3 describes data and identification methods. Section 4 presents the labor market consequences of the policy. Section 5 discusses the results and Section 6 concludes the paper.

2 The Revenue-Neutral Carbon Tax in British Columbia

The Ministry of Finance officially announced on February 19, 2008, that a carbon tax would be imposed on July 1, 2008, in BC. The tax, based on greenhouse gas emissions generated from burning fuels, was applied to the consumption of fossil fuels in BC (households and industries).

The policy is revenue-neutral: all carbon tax revenues are returned to residents and firms by the reductions in corporate taxes, personal income taxes, and lump-sum transfers.[4] This feature distinguishes my study from prior works on the impact of a simple carbon tax policy. The carbon tax rate was initially $10 per tonne of carbon dioxide equivalent (CDE) emissions in 2008, increased by $5 per tonne annually until reaching $30 per tonne of CDE on July 1, 2012, and stayed at $30 from that time onwards. Compared to other rates in North America, the initial increase in BC's carbon tax rate was sufficiently high to provide a signal to fossil fuel users so that industries could optimize with respect to the incentives created by the tax with little concern on tax salience (Chetty et al., 2009).[5] In fact, Rivers and Schaufele (2015) find that the tax reduced carbon dioxide emissions from gasoline consumption by 2.4 million

[4] Elgie and McClay (2013) give the details in the distribution of the tax revenues. The carbon tax revenues were used to lower the corporate income tax rate and the two lowest personal income tax rates by five percent. Meanwhile, the government funds a low-income tax credit and a rebate of up to $200 for northern and rural homeowners.

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

tonnes during 2008-2012, suggesting that industries did respond to the policy.[6]

This policy is expected to affect the labor market through two channels, namely the tax effects and the re-distribution effects. First, the carbon tax increases a marginal cost, thereby decreasing firms' output and thus labor demands. This tax effect increases the overall unemployment rate. Since the rise in the marginal cost increases with an energy intensity, this effect is more pronounced in more energy-intensive industries.

This tax effect could also be heterogeneous across demographic groups. In subsection 4.5, I provide evidence that less-educated workers tend to engage in more energy-intensive manufacturing industries in the pre-policy period. Since this effect is expected to be more pronounced in energy-intensive manufacturing industries, I expect the effect is concentrated on less-educated workers. In sum, the tax effect increases the overall unemployment rate, and the effect is larger in more energy-intensive industries, as noted in Yamazaki (2017). This paper adds to the literature by documenting the heterogeneous effects across educational groups.

Since the policy is revenue-neutral, the tax revenues are returned to firms and workers by the reductions in corporate taxes and personal income taxes, potentially stimulating labor demand and labor supply, respectively. The impact on the labor demand may not be highly correlated with the energy-intensity. However, the reduction of five percent in the first two personal income tax rates in BC may incent less-educated workers to work more, leaving the labor supply of better-educated workers unaffected. Hence, this re-distribution effect could mitigate the overall unemployment effect, and this re-distribution effect is homogeneous across industries and is more pronounced for less-educated workers.

The final outcome of these two opposite forces, the tax and the re-distribution effects, is ambiguous from economic theory. However, if the policy is not revenue-neutral (i.e., there is no re-distribution effect), the overall unemployment effect is expected to be larger. In fact, the BC government returned $318 million more in income tax cuts than it collected to date in 2012. If the refunds do meet the tax revenues in amount, the unemployment effect is even larger and could serve as the lower bound of the tax effect.

Next in line are the heterogeneous effects. While the tax effect is more pronounced in more energy-intensive industries, the re-distributional effect is in principle homogeneous across in-

[6]This finding is consistent with other studies. For example, per capita fuel consumption is found to decrease by 19 percent relative to the rest of Canada (Elgie and McClay, 2013; Murray and Rivers, 2015). Elgie and McClay (2013) show that the GHG emissions per capita drop by nine percent during the same period.

dustries. Therefore, the unemployment effect should be concentrated on more energy-intensive industries. The final outcome of the two forces is again ambiguous: while the tax effect is more pronounced for less-educated workers, the re-distribution effect tends to lessen the impact on the poor. Likewise, if the policy is not revenue-neutral, the policy could result in the unemployment effect that is even more disproportionate.

3 Data Descriptions and Identification Methods

3.1 Data Descriptions

The present study utilizes the public-use files of the Canadian LFS (July 2001-June 2015). The Canadian LFS is a monthly household survey, which includes approximately 100,000 individuals. The main purpose of the Canadian LFS is to generate data for official labor force statistics and is similar in nature to the United States Current Population Survey.

In addition to demographic information, such as age, gender, the highest education attained, marital status, etc., this survey includes detailed labor market variables, including labor force status, the number of weekly working hours, employment mode (i.e., part-time versus full-time), the reasons for being unemployed (i.e., job loser versus job leaver), etc.[7] The weekly working hour is defined as the actual weekly working hours on the main job throughout this paper. Table 1 reports summary statistics on the number of weekly working hours, the unemployment rate, and the LFP rate in BC and the rest of Canada, except Manitoba (MB), during the pre- and post-policy periods, which are July 2005-June 2008 and July 2008-June 2015, respectively.[8]

Two points emerge from the table. First, trends in working hours and LFP are similar between BC and other provinces.The statistics indicate that the number of weekly working hours and the LFP rate slightly decline in both groups. Second, trends in unemployment are significantly different between BC and other provinces. Probably because of the recession, the unemployment rates increases by 1.35 and 0.54 percentage points in BC and other provinces; the increase in BC is 0.81 percentage points higher than that in other provinces. The statistics seem to suggest that the carbon tax policy increases BC's unemployment rate, but the increase

[7] I exclude observations with a missing value in any of the dummies for age, education, and marital status.

[8] I show in the online Appendix that MB experienced a sharp increase in its employment level during the post-policy period relative to the rest of the control provinces. The results are qualitatively similar if MB is included.

Table 1: Summary Statistics

	BC	Rest of Canada
A. Weekly Working Hours		
July 2005-June 2008	35.61	36.62
July 2008-June 2015	34.80	35.93
Difference	-0.81	-0.69
B. Unemployment Rates (in %)		
July 2005-June 2008	3.18	4.46
July 2008-June 2015	4.53	5.00
Difference	1.35	0.54
C. LFP Rates (in %)		
July 2005-June 2008	65.03	65.71
July 2008-June 2015	63.94	64.90
Difference	-1.09	-0.81

Notes: *Unemployed* equals one if a respondent is unemployed, and zero otherwise. *LFP* equals one if a respondent participates in the labor market, and zero otherwise. Samples are restricted to the employed and the labor force participants Panel A and B, respectively. They include all survey respondents in Panel C. Data come from the Canadian LFS July 2005-June 2015.

could be driven partly or entirely by changes in the demographic composition of the sample.

3.2 Identification Methods

A multivariate regression analysis is used to isolate the effects of changes in the demographic composition of the sample. In particular, I adopt a DID approach to capture the causal effect of the carbon tax policy. Since the policy was implemented in BC, I define the treatment group as survey respondents in BC. Their counterparts in other provinces, except Manitoba (MB), serve as a control group. Using the DID approach, the causal effect of the policy can be estimated by a regression model as follows:

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

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

BC_j equals one if a respondent lives in BC, and zero otherwise. $Post_t$ equals one in July 2008 or later, and zero otherwise. Therefore, the term $BC_j \times Post_t$ equals one if a respon-

dent lives in BC after the policy, and zero otherwise. I also estimate the model by replacing $BC_j \times Post_t$ with $BC_j \times Tax_t$, where Tax_t equals 0, 0.1, 0.15, 0.2, 0.25, and 0.3 if it is observed during July 2005-June 2008, July 2008-June 2009, July 2009-June 2010, July 2010- June 2011, July 2011- June 2012, and July 2012-June 2015, respectively. This estimation allows me to examine the effect for each Canadian dollar of carbon tax. η_j and δ_t capture the province and the *Year*×*Month* fixed effects, respectively. The online Appendix provides evidence that the employment trends in BC and the rest of Canada are parallel during the three years prior to the policy (i.e., July 2005-June 2008). Therefore, I define the pre- and the post-policy period as July 2005-June 2008 and July 2008-June 2015, respectively.

The coefficient β_1 is the DID estimate of primary interest because it captures the average treatment effect on the treated of the policy. This estimation method requires several identifying assumptions. First, it requires a common trend assumption (i.e., the trends of a dependent variable in BC and the control provinces are parallel from the pre-policy to the post-policy period.). Since this estimation method heavily relies on this assumption, I provide a rigorous analysis on the validity of this assumption in subsection 4.3 (and in the online Appendix).

Second, the policy could not influence labor markets outside of BC. If this policy shifts labor demand from BC towards any control provinces, the unemployment effect may reflect both the rise in BC's unemployment rate and the fall in the unemployment rate in the control provinces. The documented effect could, at best, serve as the upper bound of the true effect. Nevertheless, there exists no direct test on this "leakage effect". If the production shifts away from BC, jobs likely shift to Alberta (AB), the adjacent province of BC. Hence, I exclude the observations in AB to conduct the same analyses. The results are qualitatively similar.[9]

Third, there is no anticipatory effect. If an economy responds to the policy before its implementation, the documented effects could, at best, serve as a lower bound. I provide support on this assumption in subsection 5.1. Fourth, the effects are not driven by the recession of 2008-2009. Although this hypothesis is not directly testable, evidence on this assumption is provided in subsection 5.2.

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

[9]Results are not presented here because of space considerations and are available upon request.

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. The standard errors are therefore clustered at the level of nine provinces, two genders, 12 age groups, and two marital statuses, providing us with 432 clusters. I also estimate the standard errors using a bootstrap procedure proposed by Cameron et al. (2008). Since the main conclusions of this paper do not change using the two estimation methods, I only report the standard errors from the former method.

4 The Labor Market Consequences of the Carbon Tax

This section comprises five subsections. Subsection 4.1 explores the aggregate effects of the policy and is followed by the investigation of the heterogeneous effects across demographic groups in subsection 4.2. In subsection 4.3, the dynamics of the effects are examined. Subsection 4.4 explores the impacts on the natures of layoffs and new hires. Lastly, subsection 4.5 investigates the cause of the heterogeneous effects.

4.1 The Aggregate Effects of the Carbon Tax

Table 2 presents the aggregate effects of the policy from the estimation of equation (1). Three variables are of our interest: the number of working hours, the incidence of unemployment, and the LFP rate. Their averages are 35.61 hours, 3.18 percentage points, and 65.03 percentage points, respectively, in BC in the pre-policy period. Each column shows estimates from a different model specification. All specifications include the province and the *Year*×*Month* fixed effects. Specifications in columns (2) and (4) also include dummies for gender, age, educational level, and marital status.

The results in Panel A suggest that the effect of the policy on the intensive margin of employment is weak. The estimates in columns (1) and (2) indicate that BC's number of weekly working hours, on average, declines by 0.7-0.9 percent subsequent to the policy. The results in columns (3)-(4) indicate that each Canadian dollar of the carbon tax is associated with a 0.018-0.024 percent reduction in the number of weekly working hours. They are statistically insignificant with the exception of the estimate in column (2) that would be considered significant at the 10 percent level but not by stricter criteria.

Table 2: The Effects on the Number of Working Hours, the Unemployment Rate, and the LFP Rate

Difference-in-Differences Analysis				
	(1)	(2)	(3)	(4)
A. Dependent Variable: ln(Weekly Working Hours)				
BC×Post	-0.007	-0.009*		
	(0.005)	(0.005)		
BC×Tax			-0.018	-0.024
			(0.025)	(0.022)
B. Dependent Variable: Unemployed				
BC×Post	0.012***	0.013***		
	(0.003)	(0.003)		
BC×Tax			0.043***	0.046***
			(0.008)	(0.010)
C. Dependent Variable: LFP				
BC×Post	-0.002	-0.003		
	(0.005)	(0.005)		
BC×Tax			-0.026	-0.021
			(0.023)	(0.020)

Notes: *Unemployed* equals one if a respondent is unemployed, and zero otherwise. *LFP* equals one if a respondent participates in the labor market, and zero otherwise. All specifications include the province and the *Year×Month* fixed effects. Specifications also include the dummies for gender, age, educational level, and marital status in columns (2) and (4). Samples are restricted to the employed in Panel A, the labor force participants in Panel B, and all respondents in Panel C. BC is the treatment group. The rest of Canada, excluding MB, is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations are 6,316,260, 7,459,551, and 11,470,678 in Panel A, B, and C, respectively. Adjusted R^2s range 0.005-0.009 in columns (1) and (3). They are 0.172, 0.032, and 0.379 in column (2) of Panel A, B, and C, respectively. Adjusted R^2s in columns (2) and (4) are identical. Robust standard errors in parentheses are clustered at the level of province, gender, educational level, and marital status. Significance levels: ***=1%, **=5%, *=10%.

The policy increases the unemployment rate but does not change the LFP rate. The estimates in Panel B indicate that the policy increases the unemployment rate by 1.2-1.3 percentage points and each Canadian dollar increase in the carbon tax raises the unemployment rate by 0.043-0.046 percentage points. These estimates are statistically significant at one percent level. The results in Panel C suggest that the effects on the LFP rate are all materially and statistically insignificant.

In summary, the carbon tax policy depresses labor demand, which reduces employment mainly from its extensive margin. While the policy cuts the intensive margin by less than one percent, the unemployment rate increases by 1.3 percentage points, accounting for a 41 percent increase. An extensive literature documents the adverse effects of various environmental poli-

cies mainly on manufacturing employment; in contrast, the results in this section speak directly on the overall unemployment effect. These findings largely cohere with empirical regularities: cyclical variations in the total working hours in a labor force arise mainly from changes in the number of employed workers (Shimer, 2010).

4.2 The Distributional Impacts of the Carbon Tax

This subsection investigates the heterogeneous policy effects across gender and educational categories. This analysis is informative for two reasons. First, it is important to understand the distribution of the costs associated with the policy, which is revelent to policymakers redistributing tax revenues. Second, it serves as an internal validity check on the results above. If BC's incidence of unemployment happens to increase for some demographic groups and decrease for others, it may be reasonable to assume that the adverse effects arise from other unobserved factors that are likely unrelated to the carbon tax policy.

Table 3 presents the effects of the carbon tax policy from the estimation of equation (1). Male (Female) samples are examined in columns of odd (even) numbers. Samples of high-, medium-, and low-educated workers are examined. All specifications include the province and the $Year \times Month$ fixed effects and the dummies for age and marital status.

A number of points emerge from the table. First, the results suggest that the policy does not change the number of working hours regardless of gender or educational level. The estimates of $BC_j \times Post_t$ and $BC_j \times Tax_t$ are all materially and statistically insignificant. Second, the results suggest that unemployment rates increase regardless of gender or educational level. In columns (3) and (4), all estimates are positive, with 10 out of the 12 estimates statistically significant at conventional levels. These results rule out the possibility in which the incidences of unemployment increase for some demographic groups but decrease for others.

Third, the unemployment effects are concentrated on medium- and low-educated males. The estimates indicate that the unemployment rates of female workers increase by one percentage point or below. The unemployment effect on high-educated males is materially and statistically insignificant. The unemployment rates of medium- and low-educated males increase by 1.4 and 2.4 percentage points; their unemployment rates increase by 46.6 and 58.5 percent.[10] The results in columns (3) and (4) indicate that each Canadian dollar increase in the

[10] BC's unemployment rates are 3.0 and 4.1 percentage points for medium- and low-educated males during the

Table 3: The Heterogeneous Effects on the Number of Working Hours, the Unemployment Rate, and the LFP Rate

Difference-in-Differences Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	**ln(Working Hours)**		**Unemployed**		**LFP**	
Gender	M	F	M	F	M	F
			Panel A			
			High-Educated			
BC×*Post*	-0.005	-0.009	0.006	0.007***	-0.003	0.007
	(0.006)	(0.009)	(0.004)	(0.001)	(0.005)	(0.006)
Adjusted R^2	0.060	0.039	0.008	0.005	0.394	0.290
			Medium-Educated			
BC×*Post*	-0.008	-0.004	0.014***	0.009***	-0.008	0.000
	(0.006)	(0.008)	(0.005)	(0.002)	(0.007)	(0.006)
Adjusted R^2	0.101	0.079	0.024	0.008	0.373	0.321
			Low-Educated			
BC×*Post*	-0.018	-0.005	0.024***	0.010**	-0.009	0.002
	(0.011)	(0.015)	(0.007)	(0.004)	(0.009)	(0.011)
Adjusted R^2	0.228	0.183	0.046	0.028	0.356	0.313
			Panel B			
			High-Educated			
BC×*Tax*	-0.015	-0.032	0.023	0.025***	-0.014	0.020
	(0.023)	(0.036)	(0.014)	(0.006)	(0.019)	(0.024)
Adjusted R^2	0.060	0.039	0.008	0.005	0.394	0.290
			Medium-Educated			
BC×*Tax*	-0.031	0.006	0.045***	0.033***	-0.039	0.000
	(0.026)	(0.036)	(0.015)	(0.010)	(0.029)	(0.025)
Adjusted R^2	0.101	0.079	0.024	0.008	0.373	0.321
			Low-Educated			
BC×*Tax*	-0.048	-0.006	0.073***	0.046***	-0.058	-0.004
	(0.047)	(0.063)	(0.026)	(0.018)	(0.042)	(0.049)
Adjusted R^2	0.228	0.183	0.046	0.028	0.356	0.313

Notes: *Unemployed* equals one if a respondent is unemployed, and zero otherwise. *LFP* equals one if a respondent participates in the labor market, and zero otherwise. All specifications include the province and the *Year*×*Month* fixed effects and the dummies for age and marital status. Samples are restricted to the employed and the labor force participants in columns (1)-(2) and (3)-(4). They include all survey respondents in columns (5) and (6). M and F denote male and female samples. BC is the treatment group. The rest of Canada, excluding MB, is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008-June 2015. The numbers of observations are about 0.7-1.1 million, 1.4-2.4 million, and 0.9-2.5 million for the samples of high-, medium, and low-educated, respectively. Robust standard errors in parentheses are clustered at the level of province, age, and marital status, providing us with 216 clusters. Significance levels: ***=1%, **=5%, *=10%.

carbon tax is associated with 0.025, 0.045, 0.033, and 0.046 percentage point increases in the unemployment rates of high-educated females, medium-educated males, medium-educated females, and low-educated males, respectively. The impact on low-educated males is substantial: each Canadian dollar of the carbon tax increases their unemployment rate by 0.073 percentage points.

Fourth, the LFP decision is basically unaffected. The estimates in columns (5) and (6) indicate that the effects on the LFP decision are economically and statistically insignificant at any conventional level. In subsection 4.3, I will show that the LFP rate of low-educated workers declines by about two percentage points subsequent to the second year of the policy.

In summary, this subsection suggests that the impacts of the policy are not uniform in the labor market. They appear mainly among medium- and low-educated males. This subsection reveals the regressivity of environmental taxes: they tax away jobs disproportionately, more likely from the less-educated. Furthermore, it highlights the importance in understanding the distributional effects of these policies. If one considers the 41 percent increase in the overall unemployment rate as a disaster to the labor market, this subsection reveals the worse situation for low-educated males, with the 58 percent increase in their unemployment rate.

4.3 The Dynamics of the Effects of Carbon Taxes

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

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

$$Y_{ijt} = \alpha + \sum_t \beta_t (BC_j \times d_t) + X_{ijt}^T \gamma + \eta_j + \delta_t + \varepsilon_{ijt}, \tag{2}$$

pre-policy period.

where d_t equals one between July in year *t* and June in year *t+1* for all years between 2005 and 2014 except 2007, and zero otherwise. July 2007-June 2008 is excluded because it serves as a reference year. I also investigate the dynamics of the aggregate effects and the heterogeneous effects by educational level. All specifications include the province and the *Year*×*Month* fixed effects and the dummies for age and marital status. The models, which estimate the aggregate effect, also control for educational level. Similar to the analysis above, the estimates $\hat{\beta}_t$ are of our interest. It measures the difference in a dependent variable between BC and control provinces during July in year t- June in year $t+1$, relative to the reference year (i.e., June 2007-July 2008).

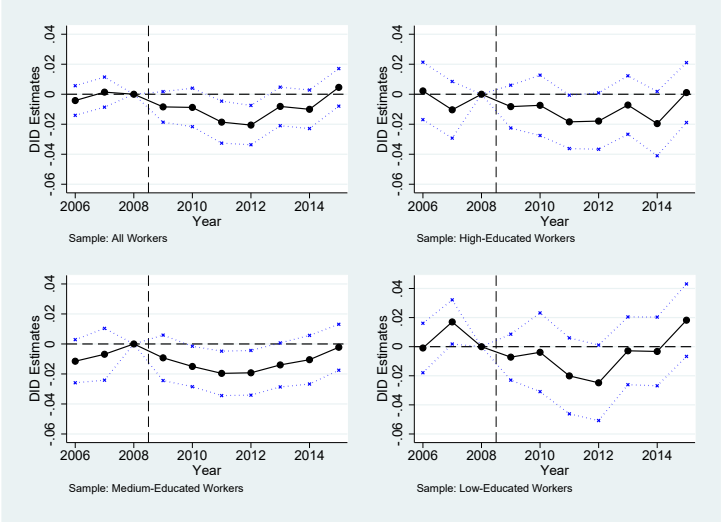
Given the results in the previous subsection, several features of the estimates are expected. First, if the interested variables in BC and the control provinces do follow a parallel trend in the prior-policy period, it is likely that their trends are parallel in the post-policy period. Therefore, one should expect that the difference in these variables between BC and the control provinces prior to the reference year is close to the difference in the reference year: all the $\hat{\beta}_t$ prior to the reference year should not be statistically different from zero.

Second, if the carbon tax policy does increase the incidence of unemployment, it is expected that the corresponding $\hat{\beta}_t$ for the incidence of unemployment are positive in the post-policy period. Third, the results in subsection 4.1 and subsection 4.2 indicate that the unemployment effect increases with the carbon tax rate. Since the carbon tax increases gradually from 2008 to 2012, it is expected that the size of the unemployment effect $\hat{\beta}_t$ increases gradually in the first few years.

Figures 1, 2, and 3 display the $\hat{\beta}_t$, where the dependent variables are the number of weekly working hours, the incidence of unemployment, and the LFP rate, respectively. Being a reference year, the estimate corresponding to July 2007-June 2008 is normalized to zero. The first dot represents the estimate $\hat{\beta}_t$ of the period July 2005-June 2006, which captures the average differences in the interested variables between BC and the control provinces relative to the differences in the reference year. The vertical line represents the first month of the carbon tax policy (i.e., July 2008).

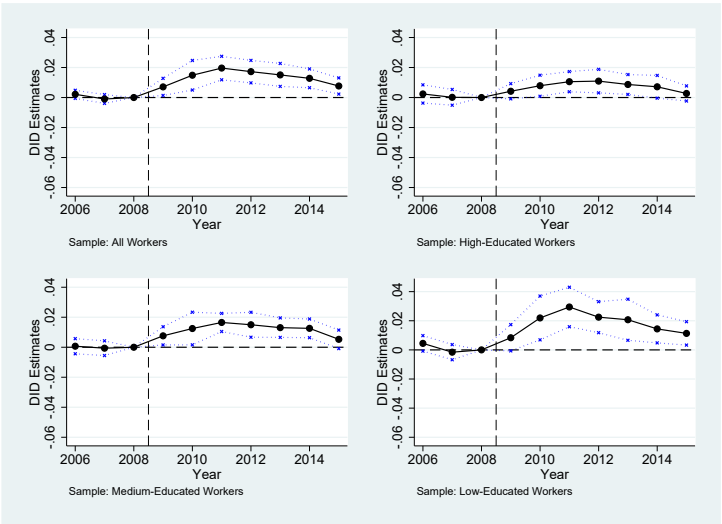
A number of points emerge from these figures. First, the trends in these variables between BC and control provinces are largely parallel in the pre-policy period. Using the entire samples, the $\hat{\beta}_t$ are all close to zero prior to the reference period as shown in each of the top left figures. These suggest that the difference in these variables between BC and the control provinces are

Figure 1: The Dynamic Effects on the Number of Working Hours



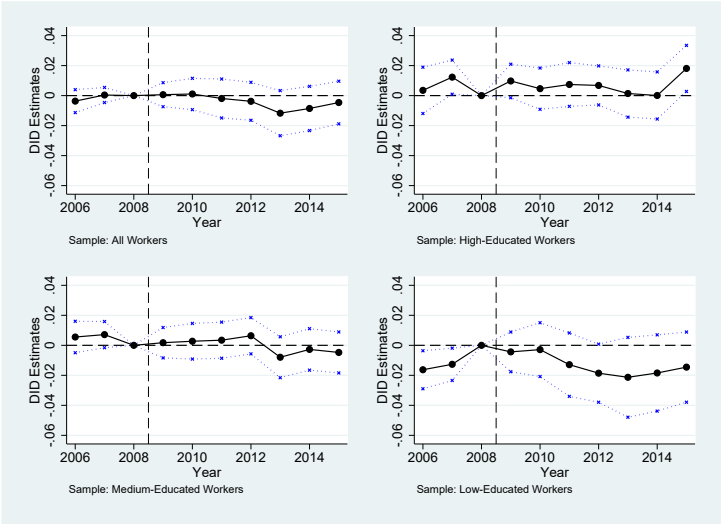
Notes: The dependent variables are ln(weekly working hours). Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (2) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure 2: The Dynamic Effects on the Incidence of Unemployment



Notes: The dependent variables are Unemployed. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (2) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

Figure 3: The Dynamic Effects on the LFP Rate



Notes: The dependent variables are LFP. Data are from the Canadian LFS July 2005-June 2015. The reference period is July 2007-June 2008. Each dot represents the main DID estimate from equation (2) in the corresponding year. For example, the first dot represents the main DID estimate of the period July 2005-June 2006. The vertical line represents the first month of the carbon tax policy (July 2008). BC is the treatment province. The dashed line represents the 95 percent confidence interval.

similar among these years. In other words, these variables share similar trends in BC and the control provinces during July 2005-June 2008, providing solid evidence for the common trend assumption upon which the DID method heavily relies on.

Moreover, I show in the online Appendix that employment trends between BC and each of the control provinces are also parallel during July 2005-June 2008 but not July 2004-June 2005. It is likely that their trends are also parallel immediately following the implementation of the policy. Meanwhile, the online Appendix also reveals the importance of the common trend assumption. The results suggest that relative to the reference year (i.e., July 2007-June 2008), BC's average employment level during July 2001-June 2005 is substantially smaller than that in the control provinces, revealing the unparallel employment trends between BC and the control provinces. Hence, causal effects is likely obtained if the observations between July 2005 and June 2008 are considered. The inclusion of the observations during July 2001-June 2005 as part of the pre-policy period could lead to the estimates that are quantitatively different from the true ones.

Second, the adverse effects on the number of weekly working hours are the strongest in

the third and the fourth years of the policy. Figure 1 shows that the number of working hours for the medium-educated decreases steadily until the third and the fourth years, at which the numbers of working hours are about two percent lower than the pre-policy level. Since then, the estimate has gradually rebounded to its pre-policy level. The dynamics of the effects are similar between high- and low-educated workers: the numbers of working hours are close before and after the policy, except for the sudden declines in the third and the fourth years.

Third, the dynamics of the unemployment effects are similar regardless of educational level. Figure 2 shows that they all first rise and then decline gradually. As discussed in subsections 4.1 and 4.2, the unemployment effect increases with the carbon tax rate. As the rate increased gradually until July 1, 2012 and stayed at the same level afterward, the sizes of these unemployment effects increased in the first few years and declined gradually afterwards.

Fourth, the unemployment effects are more pronounced for less-educated workers throughout the entire period of examination. At their peaks, the effects for the medium- and the low-educated double and triple the effect of the high-educated, respectively. This finding is consistent with the results above: the policy taxes away jobs disproportionately, more likely from the less-educated. This subsection adds to this result by showing that this effect holds in each of the post-policy year.

Fifth, the LFP rate of the low-educated, once it drops, remains low for five years. Figure 3 shows that using the entire sample, BC's LFP rate remains stable over 10 years relative to the control provinces. Nevertheless, this estimate may hide the impacts on the low-educated. The figure shows that trends in the LFP rates are similar between BC and the control provinces using the high- and the medium-educated samples, suggesting that the policy has no impacts on their LFP decisions. The LFP rate of the low-educated is also unaffected in the first two years of the policy but drops by about two percentage points in the third year and remains low afterward. Probably because the unemployment effects are substantial and persistent, some low-educated workers get discouraged and leave the labor force. In fact, if the unemployed are more likely to leave the labor force than their employed counterparts, the true unemployment effect for low-educated workers subsequent to the second year of the policy should be larger than what the estimate suggests. In other words, if the discouraged workers had stayed in the labor market, the estimated unemployment effect on the low-educated males would have been larger than 2.4 percentage points.

In summary, this subsection traces the dynamics of the impacts on the three labor market

Table 4: The Heterogeneous Effects on Unemployed Workers

Difference-in-Differences Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	**Job Losers**		**Job Leavers**		**Temporary Layoffs**	
			A. High-Educated			
BC×Post	0.060**	0.015	-0.006	-0.004	0.013	0.032
	(0.029)	(0.026)	(0.030)	(0.029)	(0.015)	(0.020)
Observations	31,452	34,117	19,561	23,669	11,891	10,448
			B. Medium-Educated			
BC×Post	0.042***	0.016	-0.003	-0.015	0.052***	0.008
	(0.016)	(0.014)	(0.014)	(0.015)	(0.012)	(0.021)
Observations	117,321	93,541	58,756	62,296	58,565	31,245
			C. Low-Educated			
BC×Post	0.012	0.056***	0.007	0.000	-0.022	0.008
	(0.014)	(0.017)	(0.016)	(0.017)	(0.017)	(0.025)
Observations	159,143	109,649	94,837	79,924	64,306	29,725

Notes: *Job Losers* (*Job Leavers*) equals one if an unemployed worker is a job loser (a job leaver), and zero otherwise. *Temporary Layoffs* equals one if a job loser experienced a temporary layoff, and zero otherwise. All specifications include the province and the *Year×Month* fixed effects and the dummies for age and marital status. Samples are restricted to unemployed workers in columns (1) and (2), the unemployed except job losers in columns (3) and (4), and job losers in columns (5) and (6). Male (Female) samples are examined in columns of odd (even) numbers. BC is the treatment group. The rest of Canada, except MB, is the control group. Data come from the Canadian LFS July 2005-June 2015. The post-policy period is defined as July 2008 afterward. Adjusted R^2s range 0.023-0.072 and 0.022-0.100 in Panels A and B, and they range 0.137-0.154 and 0.021-0.077 in columns (1)-(3) and (4)-(6) in Panel C. Robust standard errors in parentheses are clustered at the level of province, age, and marital status, providing us with 216 clusters. Significance levels: ***=1%, **=5%, *=10%.

consequences. The results suggest that each of these variables follows a parallel trend in BC and the control provinces during the pre-policy period. Moreover, these results confirm that the impacts are more pronounced for less-educated workers.

4.4 The Impacts on the Natures of Layoffs and New Hires

This subsection explores the impacts of the policy on the natures of layoffs and new hires. It answers whether the layoffs induced by the policy are more likely involuntary and temporary and whether the new hires are more likely permanent and full-time positions. Beyond unemployment, this subsection provides additional information on the labor market consequences.

This analysis is informative. If the unemployment effects are driven by voluntary layoffs, it may be reasonable to argue that the documented unemployment effects are benefits, not costs.

This case may happen because the tax revenue is recycled to reduce corporate taxes. If the reductions in the corporate taxes happen to exceed the carbon tax payment, firms may benefit from the carbon tax policy and expand their businesses. The unemployment is voluntary and temporary and may simply reflect a transition to a better job. More-educated workers tend to accumulate more sector- and firm-specific human capital, reducing their incentives to leave. This rationale reconciles with the results above, explaining why the unemployment effects decrease with educational level if the layoffs are more likely voluntary and temporary.

There are three types of unemployed workers in the data: job losers, job leavers, and others. Others include new entrants and re-entrants. I consider job losers and job leavers as involuntary and voluntary unemployment, respectively. Table 4 reports the impact on the nature of layoffs from the estimation of equation (1). Columns (1) and (2) present the effects on the proportion of job losers in unemployment. The dependent variable is a dummy variable for job losers and the sample of examination includes only unemployed workers. I exclude job losers from the sample and estimate the impact on the proportion of job leavers. The results are reported in columns (3) and (4). Layoffs are either temporary or permanent. I construct a dummy variable for temporary layoffs, estimate the effects on this variable, and report the corresponding estimates in columns (5) and (6). All specifications include the province and the *Year*×*Month* fixed effects and the dummies for age and marital status. The pre- and post-policy periods are defined as July 2005-June 2008 and July 2008-June 2015.

The results in columns (1) and (2) suggest that the incidence of involuntary unemployment increases regardless of gender or educational level. The estimates are all positive, suggesting that the policy increases the proportions of job losers by 1.2-6.0 percentage points. However, the estimates are not statistically significant for high- and medium-educated females and low-educated males. According to subsection 4.3, the LFP rate of low-educated workers declines by two percentage points subsequent to the second year of the policy. If job losers are more likely to be discouraged and leave the labor force than job leavers, the effect on the proportion of job losers for low-educated males would be much larger than what the estimate suggests.

Columns (3) and (4) present the effect on the likelihood of being job leavers in unemployment. The results suggest that there are no effects on this likelihood regardless of gender or educational level. The estimates are all materially and statistically insignificant at any conventional level. These results complement the unemployment effects in subsections 4.1 and 4.2. Workers quit jobs not for a better opportunity; the unemployment is involuntary: environmen-

tal taxes, even though revenue-neutral, depress the aggregate labor demand.

The last two columns reveal the heterogeneity in the nature of layoffs. The estimates suggest that because of the policy, the proportion of temporary layoffs increases by 5.2 percentage points for medium-educated males. No statistically significant impacts are found for the other groups. While the results in subsection 4.2 show that the unemployment effect is larger at a lower educational level, this subsection indicates that the natures of the layoffs also differ by educational level: layoffs are more likely temporary for medium-educated workers.

Next, I investigate whether the job natures of new hires are affected subsequent to the policy. Since the policy results in a severe unemployment problem and the unemployment is involuntary, workers may seek temporary and part-time jobs as stepping stones. To understand these effects, I estimate the following triple differences model:

$$\begin{aligned} Y_{ijt} &= \alpha + \beta_1(BC_j \times \mathbb{I}_{ijt}(\text{Tenure}\leq 12 \text{ Months}) \times Post_t) + \beta_2(BC_j \times Post_t) \\ &\quad + \beta_3(\mathbb{I}_{ijt}(\text{Tenure}\leq 12 \text{ Months}) \times Post_t) + \beta_4(BC_j \times \mathbb{I}_{ijt}(\text{Tenure}\leq 12 \text{ Months})) \\ &\quad + \mathbb{I}_{ijt}(\text{Tenure}\leq 12 \text{ Months }) + X_{ijt}^T\gamma + \eta_j + \delta_t + \varepsilon_{ijt}, \end{aligned} \tag{3}$$

where $\mathbb{I}_{ijt}(\text{Tenure}\leq 12 \text{ Months})$, a dummy variable for new hires, equals one if an employed worker is hired in a year, and zero otherwise. Two dependent variables are of our interest: *Permanent Jobs* and *Full-Time Jobs*. The former (latter) variable equals one if an employed worker engages in a permanent (full-time) position, and zero otherwise.

I exclude the observations during July 2008-June 2009 and the respondents with years of tenure between 13 and 24 months in this analysis. The observations during July 2008-June 2009 are excluded to ensure that the respondents with years of tenure less than one year in the post-policy period are hired after the implementation of the policy. By excluding the respondents with years of tenure between 13 and 24 months, the samples other than the new hires are incumbents: they join their respective firms prior to the policy. Hence, the incumbents and the new hires are defined as those whose years of tenures are, respectively, longer than two years and shorter than one year. In this exercise, the pre- and the post-policy periods are defined as July 2005-June 2008 and July 2009-June 2010, respectively. The results for various estimation windows are qualitatively similar but are not presented here because of space considerations. While BC's new hires make up the treatment group, there are two control groups: (i) BC's incumbents and (ii) the new hires in the control provinces.

The coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ are of our interest. Table 5 reports these two coefficient estimates from the estimation of equation (3). The effects on *Permanent Jobs* and *Full-Time Jobs* are reported in columns (1)-(2) and (3)-(4), respectively. All specifications include the province and the *Year*×*Month* fixed effects and the dummies for age and marital status.

The results suggest that the carbon tax policy does not alter the distributions of the permanent and the full-time workers among the incumbents regardless of gender or educational level. In columns (1) through (4), the estimates $\hat{\beta}_2$ are all materially and statistically insignificant at any conventional level, suggesting that the likelihoods that the layoffs occur in the permanent and the full-time positions remain unchanged subsequent to the policy. In other words, the policy does not incline the layoffs towards temporary or part-time workers.

The job natures of new hires are affected among medium- and low-educated males. While the estimates $\hat{\beta}_1$ suggest that the proportions of the permanent and the full-time new hires decline by 4.9 and 3.4 percentage points for medium-educated males and 6.0 and 2.8 percentage points for low-educated males, respectively, they are statistically insignificant at any conventional level for high-educated males.[11] These results reveal that more temporary and part-time new hires of medium- and low-educated males are seen subsequent to the policy. Nevertheless, this analysis does not reveal whether this phenomenon arises because the available temporary and part-time jobs are increasingly popular (i.e., the supply side) or because they cannot find any full-time positions and engage in these types of jobs as stepping stones (i.e., the demand side). Prior research finds that earnings losses associated with sectoral reallocation could be significant (Walker, 2013). One possibility is that workers who experience involuntary unemployment for a prolonged period of time may engage in temporary and part-time positions, which are known to be associated with earnings penalty (Connolly and Gregory, 2008; Manning and Petrongolo, 2008; Fernández-Kranz and Rodríguez-Planas, 2011). If it is the case, this paper complements the literature by revealing that it is medium- and low-educated males, not high-educated males, who bear the significant reallocative costs.

4.5 The Cause of the Heterogeneous Effects

This section has shown the heterogeneous effects on labor market consequences. In what follows, I explore the cause of the heterogeneity; I investigate whether employment in more

[11] The proportions of the permanent and the full-time new hires also decline by 6.5 and 6.2 percentage points for high-educated females.

Table 5: The Heterogeneous Effects on Employed Workers

Difference-in-Differences Analysis

	(1)	(2)	(3)	(4)
Dependent Variable	**Permanent Jobs**		**Full-Time Jobs**	
Gender	M	F	M	F
A. High-Educated				
$BC \times \mathbb{I}(\text{Tenure} \leq 12 \text{ Months}) \times Post$	-0.004	-0.065**	-0.006	-0.062***
	(0.029)	(0.031)	(0.023)	(0.024)
$BC \times Post$	-0.002	-0.002	-0.009	0.005
	(0.005)	(0.007)	(0.008)	(0.013)
Observations	186,217	223,774	243,068	254,982
Adjusted R-squared	0.116	0.125	0.091	0.047
B. Medium-Educated				
$BC \times \mathbb{I}(\text{Tenure} \leq 12 \text{ Months}) \times Post$	-0.049**	-0.024	-0.034**	-0.030**
	(0.019)	(0.017)	(0.016)	(0.012)
$BC \times Post$	-0.001	-0.003	0.002	-0.004
	(0.006)	(0.005)	(0.005)	(0.011)
Observations	458,733	477,816	565,246	542,291
Adjusted R^2	0.147	0.122	0.139	0.083
C. Low-Educated				
$BC \times \mathbb{I}(\text{Tenure} \leq 12 \text{ Months}) \times Post$	-0.060***	-0.015	-0.028*	0.002
	(0.015)	(0.013)	(0.016)	(0.018)
$BC \times Post$	-0.001	-0.003	-0.004	0.013
	(0.005)	(0.006)	(0.006)	(0.011)
Observations	384,335	335,351	486,971	382,708
Adjusted R^2	0.156	0.130	0.304	0.183

Notes: Dependent variables are *Permanent Jobs* and *Full-Time Jobs* in columns (1) and (2) and (3) and (4) respectively. *Permanent Jobs* (*Full-Time Jobs*) equals one if an employed worker engages in permanent jobs (full-time jobs), and zero otherwise. All specifications include the province and the *Year*×*Month* fixed effects and the dummies for age and marital status. Samples are restricted to employed respondents with tenure below one year and those with tenure over two years. M and F denote male and female samples. BC is the treatment group. The rest of Canada, except MB, is the control group. Data come from the Canadian LFS July 2005-June 2008 and July 2009-June 2010. The post-policy period is defined as July 2009-June 2010. Robust standard errors in parentheses are clustered at the level of province, age, and marital status, providing us with 216 clusters. Significance levels: ***=1%, **=5%, *=10%.

Table 6: The Heterogeneous Effects of the Policy and the Distribution of Energy-Intensity

	(1)	(2)	(3)	(4)
BC×*Post*	-0.028**	-0.028**		
	(0.012)	(0.014)		
Medium-Educated			0.163**	0.207**
			(0.078)	(0.077)
Low-Educated			0.175**	0.260***
			(0.084)	(0.087)
Adjusted R^2	0.125	0.096	0.063	0.061

Notes: Dependent variables are $\ln$(Energy Intensity). I merge the LFS data with three-digit industry energy intensity from NBER Productivity Database. Energy intensity is measured by the total cost of electric and fuels divided by the total value of shipments in the corresponding industry in 2007 in columns (1) and (3). In columns (2) and (4), it is measured by the total cost of electric and fuels divided by the total value added in the corresponding industry. Samples are restricted to manufacturing employed respondents. BC is the treatment group. The rest of Canada, except MB, is the control group. Data come from the Canadian LFS July 2005-June 2015 in columns (1) and (2). The post-policy period is defined as July 2008-June 2015. Data come from from the Canadian LFS 2007 in columns (3) and (4). The number of observations is 702,704 and 8,293 in columns (1) and (2) and (3) and (4), respectively. All specifications include the Year×Month fixed effect, the dummies for gender, age, marital status, and industry. They also include the province fixed effect and the dummy for educational level in columns (1) and (2). Robust standard errors in parentheses are clustered at the level of province, age, and marital status, providing us with 432 clusters in columns (1) and (2). Significance levels: ***=1%, **=5%, *=10%.

energy-intensive manufacturing industries is affected more and, if yes, whether less-educated workers tend to engage in more energy-intensive manufacturing industries in the pre-policy period. This analysis is informative for two reasons. First, it provides an opportunity to estimate the heterogeneous effects across industries and provides suggestive evidence on the cause of the distributional effects of environmental taxes, both of which are relevant to policymakers redistributing the tax revenue accordingly. Second, 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 unemployment effects arise from an unobserved factor that is likely unrelated to BC's carbon tax policy.

I first examine whether the impact on more energy-intensive industries is larger. However, I could not estimate heterogeneous unemployment effects across industries simply by estimating equation (1) and replacing the treatment variable BC with *BC*× *Industry*, where *Industry* is a dummy variable for each industry. With a repeated cross-sectional data, I could not observe the industry an unemployed worker belongs to or the industry of an unemployed worker in his previous position; I could only rely on the sample of employed workers.

Hence, I investigate whether the energy intensity of positions, on average, decreases sub-

sequent to the policy. In response to the carbon tax, firms in energy-intensive industries may shrink employment or lower their energy intensity more, both of which reduce the energy intensity of employment. Since I am concerned only with the employment effect, I construct an index that measures an industry-specific energy intensity in 2007, the year prior to the policy.

I follow Curtis (2014) to construct an energy intensity index as follows. After obtaining the Canadian LFS data, I merge three-digit-industry energy intensity data from the 2007 NBER Productivity Database. I divide total industry energy expenditure by total value of shipments for the industry to construct an energy intensity index for the 19 different three-digit manufacturing industries. Hence, the index of any particular industry in any year is identical to its 2007 level. To serve as a robustness check, I also conduct another measure of energy intensity: the cost of energy expenditure per unit of value added for the industry. Since the NBER Productivity Database has the information on the energy expenditure for manufacturing industries only, I restrict the sample to manufacturing employment in this analysis.

I obtain the policy effect on the energy intensity of employment from the estimation of equation (1), with a dependent variable $\ln(\text{Energy Intensity})$. While BC and the rest of Canada serve as the treatment and the control groups, the pre- and the post-policy periods are defined as July 2005-June 2008 and July 2008-June 2013. $\hat{\beta}_1$ measures the average change in the energy intensity of employment (in percent) subsequent to the policy. Such a change in the energy intensity results from the change in the employment level in more energy-intensive industries, relative to the employment level in their less energy-intensive counterparts. As mentioned in Section 2, the impacts of the policy should be concentrated on industries that are more energy-intensive; therefore, I expect $\hat{\beta}_1$ is negative.

Table 6 reports the estimates $\hat{\beta}_1$ for the two measures of energy intensity in columns (1) and (2). The results suggest that the impacts of the policy are larger for more energy-intensive industries. The estimates indicate that the energy intensity decreases by 2.8 percent in manufacturing employment subsequent to the policy. Such a decrease arises not from shifting towards less-energy intensive inputs because the energy intensity index of an industry does not vary over time. The findings suggest that the employment of more energy-intensive industries tend to shrink more after the policy, consistent with the discussion in Section 2.

Next, I proceed to examine whether less-educated workers tend to engage in manufacturing industries that are more energy-intensive prior to the policy. I regress the two measures of the energy intensity, $\ln(\text{Energy Intensity})$, on the dummies for the medium- and the low-educated

and other controlled variables. This analysis only includes the observations in 2007 because it is concerned only with the pre-policy situation. Since only the situation in BC is of our concern, samples are restricted to BC's manufacturing employees. I report the coefficient estimates for the two dummy variables in Table 6. The former (latter) measure of the energy intensity is used as the dependent variable in column (3) (column (4)).

The results suggest that less-educated workers tend to engage in more energy-intensive manufacturing industries. The estimates indicate that in the pre-policy period, the low-educated engage in the manufacturing industries that are 17.5-6.0 and 1.2-5.3 percent more energy-intensive than those in which the high- and the medium-educated engage, respectively. Also, the medium-educated engage in the manufacturing industries that are 16.3-20.7 percent more energy-intensive than those in which the high-educated engage.

In brief, it is evident that the impacts of the carbon tax policy are larger for more energy-intensive manufacturing industries. Probably because less-educated workers tend to engage in more energy-intensive industries, the adverse effects on the less-educated are more pronounced. The two pieces of evidence provide one of the channels through which the unemployment effect and their consequences differ across educational levels.

5 Discussions

This paper uncovers the overall picture of the carbon tax policy in the labor market. It depresses the aggregate labor demand, leading to waves of involuntary layoffs. These layoffs do not incline towards temporary or part-time workers but are concentrated on medium- and low-educated males. While the overall unemployment rate increases by 1.3 percentage points, the increases reach 1.4 and 2.4 percentage points for medium- and low-educated males.

These unemployment effects appear immediately following the implementation of the policy. The size of the unemployment effect increases gradually with the carbon tax rate in the first few years. While the tax rate maintains at the same level from July 2012 onwards, the size of unemployment effect has decreased gradually since then. In the meantime, the unemployed induced by the policy continue to seek jobs. This paper discovers that the new hires are more likely temporary and part-time positions for medium- and low-educated males, not for their high-educated counterparts. Perhaps because the unemployment effects are substantial and persistent, more low-educated workers engage in temporary and part-time position and

eventually, some, being discouraged, leave the labor force. This paper shows that the impacts are larger on employment in the more energy-intensive manufacturing industries. Probably because the less-educated tend to engage in these industries, the unemployment effect and its consequences are more pronounced for the less-educated.

To interpret the documented effect as a causal effect of the policy, the four assumptions, as stated in subsection 3.2, are required. The solid evidence on the common trend assumption (the first assumption) and the discussion on the "leakage effect" (the second assumption) are provided in subsection 4.3 and subsection 3.2, respectively. I will discuss the rest of the identifying assumptions in this section.

5.1 Assessing the Anticipatory Effect

The analyses of this paper assume no anticipatory 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 of time limits the extent to which firms and workers respond to the policy. To ease the doubt on this claim, I estimate the announcement effect on BC's unemployment rate.

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 gap in the incidence of unemployment between the pre- and the post-announcement periods. However, this difference may not capture the causal effect of the announcement because of seasonality. That is, the unemployment rate between March and June could be different from that in January. Therefore, I control the seasonal unemployment effect using the observations in 2005, 2006, and 2007, serving as the second difference. Lastly, BC's unemployment effect in 2008 could be country- and year-specific because of the recession. Hence, I include the observations from the other provinces (except MB) to control the country-specific effect. Therefore, the difference in the unemployment effect between BC and the other provinces serves as the

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

Difference-in-Differences Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Education	**High-Educated**		**Medium-Educated**		**Low-Educated**	
Gender	M	F	M	F	M	F
BC×2008×Mar-Jun	0.004	0.009	0.008	0.007	0.010	-0.001
	(0.007)	(0.007)	(0.005)	(0.007)	(0.007)	(0.009)
BC×2008	0.004	-0.021***	-0.006	-0.008	-0.005	-0.012
	(0.006)	(0.007)	(0.006)	(0.008)	(0.005)	(0.009)
BC×Mar-Jun	-0.002	-0.005	0.002	-0.004	-0.001	-0.010*
	(0.005)	(0.004)	(0.003)	(0.004)	(0.007)	(0.006)
2008×Mar-Jun	0.002	0.005*	0.008***	-0.003	0.007*	0.008*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Adjusted R^2	0.007	0.005	0.026	0.012	0.046	0.032

Notes: Dependent variables are *Unemployed*, which equals one if a respondent is unemployed, and zero otherwise. All specifications include the province, the year, and the month fixed effects, and the dummies for age and marital status. Samples are restricted to labor force participants. M and F denote male and female samples. The treatment group is BC's respondents between March and June in 2008. There are two control groups: the rest of Canada between March and June in 2008 and BC's respondents between March and June during 2005-2007. Data come from the Canadian LFS January, March, April, May, and June 2005-2008. The numbers of observations are 113,896, 119,000, 278,888, 267,493, 254,358, and 200,973 in columns (1) through (6), respectively. Robust standard errors in parentheses are clustered at the level of province, age, and marital status, providing us with 216 clusters. Significance levels: ***=1%, **=5%, *=10%.

third difference. The estimates can be obtained from the following regression model:

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

The dependent variable is *Unemployed*, which equals one if a labor force participant is unemployed, and zero otherwise. 2008_t equals one if a respondent is observed in 2008, and zero otherwise. $March\text{-}June_t$ equals one if a respondent is observed between March and June, and zero otherwise. Samples are restricted to the labor force participants in BC and the other control provinces in January, March, April, May, and June during 2005-2008. All specifications include the province, the year, and the month fixed effects and the dummies for age and marital status. Again, standard errors are clustered at the level of province, age groups, and marital status, providing us with 216 clusters.

Table 7 reports the estimates of our interest from the estimation of equation (4). The

columns of odd (even) numbers report the estimates from the examination of male samples (female samples). The samples of the high-, medium- and low-educated samples are examined in columns (1) and (2), (3) and (4), and (5) and (6), respectively.

Two points emerge from the table. First, the result suggests no anticipatory effect regardless of gender or educational level. The estimates $\hat{\beta}_1$ are all statistically insignificant at any conventional level, suggesting that BC's unemployment rate remains unchanged after the announcement. 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), except high-educated males and medium-educated females, experience a small increase in unemployment rates in the first half of 2008. These increases are likely attributed to the recession.[12] Since the estimates $\hat{\beta}_1$ are all statistically no different from zero, it is safe to conclude that the trends in unemployment rate for BC and the control provinces are parallel in the first half of 2008. While subsection 4.3 provides evidence that the annual trends in unemployment rates are parallel in BC and the control provinces during the three years prior to the policy, this subsection provides support for a common trend in unemployment rates during the six months prior to the policy. Furthermore, it provides support that the recession does increase unemployment rates in Canada and the impact of the recession is not specifically larger on BC's labor market. This support will be discussed in the next subsection. In sum, this subsection finds no evidence on any anticipatory effect between the announcement and the implementation dates of the policy.

5.2 Are the Effects Driven by the Carbon Tax or the Recession?

This paper documents that BC's unemployment rates increase subsequent to July 2008. This finding may indicate that the carbon tax policy causes BC's unemployment rate to rise by 1.3 percentage points, or it may reflect the existence of other negative shocks that specifically affect BC's labor market. Despite no implementation of any new environmental or labor market policy other than the carbon tax policy in BC around July 2008, the estimated unemployment effects could be driven by the recession of 2007-2009. The recession affects the unemployment rate of the Canadian economy as a whole. But if the negative effect is larger in BC's economy, this unparallel impact on BC could explain the negative estimates in the post-policy period.

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

I argue that the likelihood that the documented unemployment effects are driven from the recession is low for two reasons. First, the unemployment effects are consistent with the implementation date of the carbon tax policy, not the recession. The previous subsection shows that the recession does increase unemployment rates in BC and the control provinces, and such increases are statistically identical between BC and the control provinces during the recession. However, the increases in unemployment rates are more pronounced in BC subsequent to the implementation of the policy. These results cast doubt on the claim that the documented unemployment effects are driven from the recession but not the carbon tax policy.

Second, the unemployment effect, as shown in subsection 4.5, is concentrated in more energy-intensive manufacturing industries. Furthermore, this paper finds larger unemployment effects for the medium- and low-educated than the high-educated. Meanwhile, I provide suggestive evidence that the medium- and the low-educated engage in the manufacturing industries that are more energy-intensive than those in which the high-educated engage. These documented heterogenous unemployment effects are consistent with the tax structure of the carbon tax policy, not the aggregate shock in recession.

In sum, the unemployment effects do not fit well with the timing of the recession, and the features of the unemployment effects cohere with the carbon tax structure. This subsection concludes that the documented unemployment effects are attributed to BC's carbon tax policy.

6 Conclusion

An extensive literature documents the adverse employment effect of environmental policies. However, these policies may induce sectoral reallocation: unaffected sectors may absorb some of the unemployed. Yet we know little of the overall effects and the distributional effects of these policies in the labor market. This paper exploits the introduction of a revenue-neutral carbon tax in BC to investigate the overall and the distributional effects of environmental taxes.

This paper uncovers the overall picture of an environmental tax policy in the labor market. It finds that the policy is regressive: it taxes away jobs disproportionately, more likely from the less-educated. It is evident that the increased unemployment is likely involuntary. Moreover, evidence suggests that the layoffs induced by the policy are more likely temporary for medium-educated males. Also, the new hires are more likely temporary and part-time positions for medium- and less-educated males. Probably because of the unemployment effect is substantial

and persistent, some low-educated males get discouraged and, eventually, leave the labor force.

This paper also investigates the cause of the heterogeneity. Suggestive evidence indicates that the impacts on more energy-intensive manufacturing industries are larger, in line with the environmental tax policy structure. Furthermore, the evidence suggests that the effects are more pronounced for less-educated workers because they tend to engage in more energy-intensive manufacturing industries in the pre-policy period.

This paper completes the analysis on the overall and the distributional effects on unemployment across demographic groups. However, as a caveat, this paper notes that there was an unobserved differential shock to BC's labor market a few years prior to the policy and hence, the results of this paper could, though unlikely, be partially driven by a similar unobserved differential shock. This paper proposes the appropriate pre-treatment period and the appropriate control groups in evaluating the same policy. It will be a fruitful and interesting research project to explore the impacts of the same policy on wages and their distribution.

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