What's in a Name? The Incidence of Gasoline Excise Taxes versus Gasoline Carbon Levies

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Abstract

Legislators often attach specific names to individual taxes to promote their purpose, increase transparency, and ease public backlash over tax increases. While having political benefits, does the simple act of naming and promoting a tax for a specific purpose have a more meaningful effect in the marketplace? Do consumers respond differently to tax-induced price increases depending on the name and purpose of the tax? In this article, a natural experiment is used to compare consumer responsiveness and tax incidence after the introduction of two new gasoline taxes in Alberta – 1) an increase in the generic excise tax and 2) an environmentally-targeted "carbon levy". While similar in magnitude, the taxes were very different in name, purpose, and transparency. Results show that the naming and marketing of a tax can indeed matter – responses were lower and incidence higher for the carbon levy than the less transparent excise tax. Implications are discussed.

JEL Classification Codes: L11, L15, L81, L91, M38, Q31

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1 Introduction

Legislators around the world have a long history of attaching specific names to individual taxes, such as Canada's "Fair Share Health Care Levy" or China's "Environmental Protection Tax". The simple act of naming a tax, and collecting it as a separate line item from general income or sales taxes, can make the tax more palatable to taxpayers, since the purpose of the tax and the benefits it is expected to produce are more transparent. It can also deflect some of the negative sentiment that is often associated with tax increases, provided the purpose of the tax is deemed worthy.

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This stands in contrast to general tax increases which, while servicing many of the same types of programs and public services, are a bit of a black box and are often met with more opposition. For example, when George H.W. Bush famously promised "Read my lips, no new taxes" in the 1988 presidential campaign, but then later agreed to a budget bill that included general tax increases, public reaction was strong and negative - irrespective of the social programs the new taxes might be used for - and that broken promise played an important role in Bush's failed re-election bid four years later.

One type of tax with a specific name attached to it, and one that has received a great deal of recent attention is the so-called "carbon tax" or "carbon levy". Carbon levies, most commonly imposed on sales of gasoline and other fuels derived from fossil fuels, are designed to reduce carbon emissions and simultaneously fund research and development of more environmentally friendly sources of renewable energy. As of the time of this writing, the United States is one of the few industrialized countries without a carbon levy, but fifteen U.S. states in the West and Northeast are actively considering one to combat climate change. In January 2019, dozens of prominent economists published a letter in the Wall Street Journal urgently calling for the introduction of a federal carbon levy.[1] Elsewhere, carbon levies are in force in several Canadian provinces and in numerous other jurisdictions around the world. A small but growing literature has emerged to evaluate the effects of carbon levies on carbon emissions. Effects can occur either through short run substitution away from the now more expensive fossil fuels, or through longer term substitution towards more cost-effective alternative energy sources whose development has been funded by the levy. Early results generally show that carbon levies are effective at reducing carbon emissions.

Unlike the bulk of studies in the literature, we are not interested in estimating the effects of a carbon levy here. Rather, we are interested in estimating the effects of *calling it* a carbon levy. What's in a name? We want to know if consumers respond differently to a tax when that tax is given a specific name and marketed for a transparent purpose that consumers care about, in comparison to a generic tax increase which, while potentially funding many of the same programs, is given no specific name and does not have as clear or transparent a purpose. Does the simple act of naming

[1] Wall Street Journal (2019). "Economists' Statement on Carbon Dividends". January 16, 2019. Availalble at https://www.wsj.com/articles/economists-statement-on-carbon-dividends-11547682910 last accessed January 25, 2019.

and marketing the purpose of the tax matter? Is a tax dollar worth a dollar to consumers regardless of its use or does knowing where that tax money goes matter to them in their decisionmaking?

In this article, we examine how the simple naming and marketing of a tax can affect the incidence of that tax. Our application will focus on the recent introduction of a carbon levy on sales of gasoline in Canada. Our prior is that the naming of a tax should not matter since, from a financial perspective, a dollar is worth a dollar. But marketing and purpose can potentially matter. It is well known that consumers are often willing to act in a more inelastic way – purchasing products at a premium price – when that product embodies certain benefits to society during production, e.g. more environmentally friendly, union made, humane, or animal-friendly. Similarly, to the extent that consumers are more accepting of a carbon levy whose stated purpose is agreeable to them and whose expected benefits are more transparently communicated, consumers may also respond in a more inelastic way. This would ironically mean that consumers would reduce their short run gasoline consumption by less under a carbon levy than under an equivalent tax whose eventual disposition is less transparent, such as a general excise tax increase. However, it would also mean that revenues from the carbon levy would initially remain high, so that the long run development of more environmentally friendly fuels could occur more quickly. The potential for differential consumer responses to taxes of different names can in principle be tested by comparing the degrees of consumer incidence across them, provided comparable examples of both can be found.

While the issue has been discussed and debated in policy circles, efforts to establish a link between the naming of a tax and the incidence of that tax has been difficult due to an identification problem. Basically, a tax either has a catchy name or it does not, so finding a comparable benchmark tax, similar in all ways except in its naming and marketing, can be difficult. Identification must otherwise come from comparing more distant taxes, perhaps on different products, or comparing taxes of very different amounts, or comparing taxes on items purchased by different kinds of consumers, in different states, or in relatively distant time periods.

Fortunately, and largely unique to the literature, we solve this identification problem by exploiting a fortuitous natural experiment in the retail gasoline industry in the province of Alberta, Canada. The provincial government there implemented two new taxes on the sale of gasoline, one in April 2015 and another in January 2017, twenty-one months apart. The two taxes were of roughly equal sizes but came with very different names and very different marketing campaigns and stated purposes.

The first, imposed in April 2015, was a simple increase in the usual excise tax on gasoline of 4.0 cents per liter (or approximately 11.4 U.S. cents per gallon). The increase was significant (on gasoline prices of about a dollar per liter) and was used to supplement general provincial revenues which in turn contributed to a variety of general government services, such as administration, health care, education, infrastructure and regulation. While the tax would ultimately fund important services, there was no special name or marketing campaign that came with the tax, and the link between it and the specific benefits that it was expected to provide was less transparent.

In contrast, the second tax, imposed in January 2017, came with a special name, purpose, and marketing pitch. The introduction of the so-called "carbon levy" was specifically marketed and advertised as a tax to help the environment, reduce greenhouse gas emissions, invest in renewable energy projects and energy efficiency programs, and promote other environment conservation projects (Government of Alberta, 2018). The tax was similar in size to the excise tax increase, at 4.49 cents per liter (or approximately 12.8 U.S. cents per gallon), but this time the link between the tax and the specific benefits that it was supposed to provide was more clearly connected.

The taxes – similar in size and identical in location but very different in name and stated purpose – presents a unique opportunity to examine the relationship between tax incidence and the naming and marketing of a tax, while holding all else equal.

One additional nice feature of our natural experiment setting is the fact that Alberta was the only province in Western Canada to change its gasoline taxes during our sample period from 2013 to 2017. Gasoline taxes elsewhere in Western Canada did not change at all during this time, meaning that cities in Western Canada are available to serve as control cities. Noting that Western Canadian cities are all supplied from the same sources and serviced through the same pipelines, they can control for any changes in wholesale or retail prices or in retail margins that may occur during the sample.

Using panel data on wholesale and retail gasoline prices, along with tax information, for both regular and premium grade gasoline, for Western Canadian cities over a five year period, we estimate the consumer incidence of the tax increases, or as it is commonly called in the gasoline literature, the degree of pass-through. Pass-through, or consumer incidence, is complete if retail prices in the treatment cities (i.e. cities in Alberta) increase by 4.0 cents per liter after April 1, 2015 (when provincial fuel tax increased from 9 to 13 cents per liter) and an additional 4.49 cents per liter after January 1, 2017 (when the carbon levy was first introduced). Pass-through is incomplete if either tax change is less than introduced tax increase. While the recent literature on gasoline tax incidence agree that taxes are mostly borne by consumers, there is less agreement on whether pass-through is complete or not in response to wholesale cost changes, and there is relatively little research at all on pass-through in response to tax changes.

Next, we turn to our main question of whether the degree of pass-through of a tax can be differentially affected by the naming and marketing of that tax. We find that it can. We find that consumers were less responsive to the introduction of the carbon levy than to the similarly-sized generic excise tax increase, i.e. that the incidence of the carbon levy fell more on consumers and its degree of pass-through was more complete than it was for the generic excise tax increase.

Another novel feature in our study is that we estimate effects not only for regular grade gasoline but for premium grade gasoline as well. Few other studies include premium grade gasoline in their analysis, but in framing their conclusions about "gasoline" generally, implicitly assume that their results on regular grade gasoline should carry over to higher octane grades as well. Our study shows that this assumption is premature. We search for differential incidence across the two kinds of taxes, first for regular and then again for premium, and we find an interesting difference across the grades that has important distributional implications.

Setting aside the identification benefits afforded by our natural experiment design, there are several other reasons why dynamic pricing in the retail gasoline industry is so well studied. First, gasoline and other fuels are core inputs to many consumer goods and services in the economy. Tax policies that change the price of gasoline ultimately affects the overall price level in the economy. Second, gasoline expenditures can represent a considerable fraction of a household budget, especially among lower income households, which implies regressivity of gasoline taxes (Chernick and Reschovsky, 1997; Sammartino, 1990). Given inelastic demand especially in the short run, gas price increases can reduce consumers' disposable income and have significant welfare effects. Finally, gasoline prices are a rare example of fully tax-inclusive prices. What you see is what you pay. On one hand, consumers may be less aware of tax changes hidden in the posted price (Krishna and Slemrod, 2003) but on the other, consumers tend to be more tuned in to gasoline price changes than to price changes in other industries, and often have strong, if not always correct, convictions about what causes them. These are all reasons the gasoline industry is a particularly interesting one for exploring the effects of tax changes.

The remainder of this paper proceeds as follows. Section 2 provides background information and review of relevant literature, Section 3 discusses the data and our empirical strategy. Section 4 presents the results, and Section 5 provides some concluding remarks.

2 Literature and Background

Economists study the incidence of tax – the distribution of the tax burden between buyers and sellers – to better understand the welfare implications of tax policy reforms. Standard textbook theory on tax incidence suggests that the price elasticities of demand and supply together determine the degree of tax passed through to consumers and producers. The tax burden falls more strongly upon consumers when supply is more elastic than demand, and more strongly on producers when demand is more elastic than supply. In cases with exceptionally inelastic demand – arguably including the retail gasoline industry – the tax burden can be potentially passed through completely to consumers.

A simple model posits that demand is given by $D(p(\tau))$ and supply is given by $S(p(\tau) - \tau)$, where p is the tax-inclusive price and τ is the specific (per unit) tax. Note that demand in this model responds only to the post-tax price and supply responds only to the pre-tax price. Simple algebra shows that the increase in the tax-inclusive price paid by the consumer per unit of tax is given by

$$\frac{dp}{d\tau} = \frac{\partial S/\partial p}{(\partial S/\partial p - \partial D/\partial p)} = \frac{\eta}{\eta - \varepsilon}$$

where η is the aggregate elasticity of supply and ε is the aggregate elasticity of demand. The change in the tax-exclusive price is given by

$$\frac{d(p-\tau)}{d\tau} = \frac{\partial S/\partial p}{(\partial S/\partial p - \partial D/\partial p)} - 1 = \frac{\partial D/\partial p}{(\partial S/\partial p - \partial D/\partial p)} = \frac{\varepsilon}{\eta - \varepsilon}$$

The terms $dp/d\tau$ and $d(p-\tau)/d\tau$ thus represent the incidence of the tax to consumers and producers, respectively. Incidence to consumers is greater with more elastic supply and more inelastic demand, with full burden corresponding to either perfectly elastic supply or perfectly inelastic demand. Incidence to producers is greater with more inelastic supply and more elastic demand, with full burden corresponding to perfectly inelastic supply or perfectly elastic demand.

It is well known that demand elasticities for retail gasoline are generally low and that supply elasticities are high (Brons et al. (2008); Hughes et al. (2008); Levin et al. (2017); Lin and Prince (2013); Noel and Roach, (2016)). One thus expects the burden of tax increases to fall disproportionately on consumers, and this is generally what the literature finds. However, there is little consensus on the extent of consumer incidence and whether consumer incidence is complete (i.e. 100%) or incomplete. Whether the naming and marketing of a tax can affect the degree of consumer incidence by influencing consumer elasticities, which is of particular interest to us, has received little attention. It is a surprising omission in light of the practice of assigning special names to taxes in an effort to make them more transparent and palatable to taxpayers.

The current literature on gasoline tax incidence can be divided into two broad categories, based on the outcome they find. The first group of studies finds that consumer incidence, or the degree of pass-through of taxes into retail consumer prices, is incomplete. The second group of studies finds that it is complete. In the latter case, the tax burden falls entirely on consumers.

Included in the first group of studies is Barron et al. (2004), who examine a reduction in the excise gasoline tax in Connecticut after a large price spike in 2000. The authors find that only about two-thirds of the tax reduction was ultimately passed through to consumers. In a similar study, Doyle and Samphantharak (2008) examine the temporary moratorium on the 5% gasoline tax in Illinois and Indiana in the summer of 2000, and show that 70% of the tax suspension and 80–100% of tax reinstatement were passed through to retail prices. Less than complete tax pass-through was also found by Silvia and Taylor (2014). They examine the Washington State Nickel Funding Package, which implemented a five–cent increase in excise tax on gasoline and diesel fuel in the state of Washington, and argue that incomplete pass-through may have resulted from competitive pressures from hypermarket entry. Chouinard and Perloff (2004), in a broader analysis of gasoline tax incidence that included the 48 contiguous US states over the period of 1989–1997, calculate that the incidence of the federal gasoline tax fell equally on consumers and wholesalers, while that state taxes fell almost entirely on consumers.

The second group of studies in the literature find that pass-through of gasoline taxes into retail prices is largely complete. Alm et al. (2009) find complete passing of state gasoline excise taxes to consumers based on monthly price data for all 50 US states from 1984 to 1999. Pass-through was higher in urban areas than rural ones, which they attribute to differences in market structure and competition. Analyzing state taxes on gasoline and diesel from 1983 to 2003, Marion and Muehlegger (2011) find that tax changes were fully incorporated into the retail price within about a month. Similar results were found in Spain. Bello and Contin- Pilart (2012) find that regional gasoline taxes were fully passed into retail prices, while Stolper (2016) finds complete pass-through on average but with meaningful heterogeneity across stations.

A related strand of literature focuses not so much on the overall degree of pass-through but rather on short term dynamics. These studies generally focus on shocks to upstream wholesale prices, which are common, as opposed to the changes in tax rates we examine here, which are less common. Most take a special interest in searching for asymmetry – i.e. whether downstream prices rise after a cost increase more quickly than they fall after a cost decrease. There are many such "rockets and feathers" studies using different cities or countries, different time periods, different methodologies, and results vary. Early studies documenting the phenomenon include Bacon (1991), Borenstein et al. (1997), and Bachmeier and Griffin (2003). Verlinda (2008) provides evidence that gas stations with certain site and local market characteristics have higher price response asymmetry than stations without. Deltas (2008) finds that gasoline markets with high retail-wholesale margin tend to have higher cost response asymmetries, and Lewis (2011) finds that imperfect information together with consumer search are a factor underlying the asymmetry.[2] Noel (2009) finds that Edgeworth price cycles can potentially lead to a spurious rockets and feathers finding, all else equal, and Lewis and Noel (2011) find that, in practice, the speed of actual cost pass-through is faster in markets with Edgeworth cycles than those without due to the fluid nature of pricing during cycles.

[2] Other theoretical descriptions of rockets and feathers include Yang and Ye (2008), Tappata (2009) and Cabral and Fishman (2012).

In this study, we are less concerned about asymmetries in pass-through or its short run dynamics, but rather on the overall degree of tax pass-through. We are particularly interested in how that overall degree of pass-through differs between two very differently-named and differently-marketed taxes, the usual excise tax and the so-called carbon levy. Alberta was not the first province in Canada to introduce a carbon levy, and a small literature has developed to examine the effects of targeted taxes in other areas. British Columbia (B.C.) was among the first jurisdictions in North America to adopt a carbon levy, which it did in 2008. The levy was implemented in stages over the next four years. Rivers and Schaufele (2015) show that it reduced demand and CO_2 emissions by a non-negligible amount, and Elgie and McClay (2013) find a similar decrease.[3] Beck et al. (2015) analyze the social welfare implications of the B.C. carbon tax using computable general equilibrium model, and surprisingly conclude that the tax was a progressive tax, i.e. that it fell more on higher income households than lower income ones, while reducing carbon emissions.

There is also evidence that the transparency, or saliency, of a tax can affect incidence and pass-through. Outside of gasoline markets, Chetty et al. (2009) find that consumers reduce grocery consumption significantly more when a tax increase is included in posted prices than if applied later at the register.[4] At first glance, price transparency would appear to not be an issue with gasoline markets, since gasoline prices are always posted as fully tax-inclusive prices and, presumably, consumers would be equally harmed by a one cent increase regardless of the final destination of that penny. Yet Li et al. (2014) find that components that make up the gasoline price can individually matter. Differentiating between changes in excise taxes and changes in tax-exclusive prices (due to wholesale price changes), they argue that consumers respond more to the former than the latter, attributing the difference to the permanent nature of tax increases in contrast to the transitory nature of crude and wholesale price changes.

[3] More modest results were observed by Davis and Kilian (2011), who conclude that imposing a 10-cent higher gasoline tax in the US would decrease demand by 1.43%, while reducing transportation CO_2 emissions by 0.48%. The authors suggest: (i) widespread adoption of environmental gasoline taxes across international arenas, (ii) imposing taxes on other branches of economy (e.g. electricity production), and (iii) adopting higher gasoline taxes (as high as $1 per gallon).

[4] This can be a motivation for the recent surge in mandatory hotel resort fees, since consumers tend to compare hotels on the basis of advertised "top-line" prices rather than final "bottom-line" prices.

3 Data and Methodology

To our knowledge, ours is the first study to contrast two state-level gasoline taxes that were very similar in magnitude, but vastly different in name and stated purpose, all in a natural experiment setting. Our natural experiment focuses on two distinct gasoline tax increases in the province of Alberta, in the western part of Canada. On April 1, 2015, the Albertan government increased its usual and generic excise tax by four cents per liter, from 9 cents per liter to 13 cents per liter. Then on January 1, 2017, it created a new per-liter tax under the pseudonym of a "carbon levy", equal to an additional 4.49 cents per liter. As it turns out, these two tax increases were the only two in all of Western and Central Canada, at both the provincial and federal level, between 2013 and 2017. This lends itself well to a straightforward difference-in-differences analysis in which major cities in Alberta serve as the treatment group and major cities in nearby provinces serve as the control group. We can compare the degree of pass-through of the two tax increases, or their incidence, holding unobserved time-invariant factors fixed and unobserved city-invariant factors fixed as well.

The treatment group consists of four impacted cities in Alberta (Calgary, Edmonton, Lethbridge, and Red Deer) and the control group consists of nine unimpacted cities in the three other provinces of Western Canada – the provinces of British Columbia, Saskatchewan and Manitoba. There are four control cities in British Columbia (Vancouver, Victoria, Kamloops and Kelowna), three in Saskatchewan (Regina, Saskatoon, Prince Albert) and two in Manitoba (Winnipeg, Brandon). The treatment and control groups are relatively well balanced geographically with four cities in Alberta and either four or five control cities on either side of it (British Columbia being to the west and Saskatchewan and Manitoba being to the east). They are also relatively well balanced in terms of size, with the population of the four treatment cities ranking second, third, ninth and tenth out of the total thirteen cities in the sample. All cities in the sample - whether in the treatment or control groups - face similar supply shocks and conditions throughout the period, since they are all connected to a common supply (via pipeline from major oil crude reserves and refineries in Western Canada). To the extent there are city-specific shocks to wholesale costs that are unrelated to any tax change, we control for these with city-specific wholesale prices.

We employ wholesale and retail price information, along with excise, carbon levy, and ad valorem tax information, from Kent Marketing Ltd. between January 2013 and December 2017. The data consists of weekly average retail prices and the corresponding average weekly wholesale prices ("rack prices"), for each city, for regular grade gasoline and premium grade gasoline.[5] Our study is a rare example of a study that incorporates premium grade gasoline into the analysis, rather than focusing on regular grade gasoline alone, and its inclusion produces some interesting comparative results that are new to the literature. We also have comprehensive information on excise taxes, sales taxes, and other applicable taxes on gasoline at the federal, provincial, and local level, along with changes in those taxes over time. The only tax changes in our sample cities during our sample period was in Alberta. Summary statistics are reported in Table 1.

We perform a series of analyses to examine incidence and pass-through. First, we estimate the overall degree of tax pass-through, i.e. consumer incidence, of the two taxes combined. Pass-through is complete if retail prices in the treatment cities (regular and premium grade) increase by 4.0 cents per liter shortly after April 1, 2015 and an additional 4.49 cents per liter shortly after January 1, 2017. It is incomplete otherwise. We perform the analysis separately for regular grade gasoline and premium grade gasoline.

We then contrast the degree of pass-through following the excise tax increase to the degree of pass-through after the carbon levy, and discuss any differences we find by and across grades. While our prior is that a one cent per liter increase in an excise tax should be equivalent to a one cent increase in a carbon levy (and equivalent to a one cent tax increase under any other name), if consumers respond to these differently-named and differently-marketed taxes in different ways, pass-through will also differ. As carbon levies are marketed as a specific effort to curb pollution and protect the environment, to the extent that consumers are more accepting of a tax earmarked for this purpose (viewing the price increase net of social benefit to be lower), it could result in a more muted consumer response, i.e. a more inelastic response, and higher pass-through.

In each case, we consider the effect that the taxes have on both prices and margins. Our basic

[5] If a rack price for a given city is not published, we use the rack price for the closest city with a published rack price within the province.

estimating equation for price is given by:

$$
\begin{aligned}
PRICE_{gct} \quad = \quad & \beta^p_{0g} + \beta^p_{1g} EXCISETAX_t + \beta^p_{2g} CARBONLEVY_t + \beta^p_{3g} TREATMENT_c \\
& + \beta^p_{4g} TREATMENT_c \times EXCISETAX_t \\
& + \beta^p_{5g} TREATMENT_c \times CARBONLEVY_t \\
& + \beta^p_{6g} RACK_{gct} + \sum_{c=2}^{13} \phi^p_{cg} + \sum_{d=2}^{12} \xi^p_{dgt} + \sum_{y=2014}^{2016} \zeta^p_{ygt} + \varepsilon^p_{gct} \qquad (1)
\end{aligned}
$$

where $PRICE_{gct}$ is the retail price of gasoline of gasoline grade g in city c at time t. The two grades are regular grade gasoline and premium grade gasoline. The dichotomous variable $EXCISETAX_t$ is equal to one after April 1, 2015 when the additional excise tax was in effect, and $CARBONLEVY_t$ is a dichotomous variable equal to one after January 1, 2017 when both the additional excise tax and the carbon levy were in effect. The dichotomous variable $TREATMENT_c$ is equal to one for each of the four cities in the province of Alberta. The variable $RACK_{gct}$ is the posted wholesale price of gasoline grade g in city c at time t and controls for wholesale price changes. The ϕ^p_{cg}, ξ^p_{dgt} and ζ^p_{ygt} are sets of city fixed effects, month fixed effects, and year fixed effects respectively. Note that in specifications where the ϕ^p_{cg} are included, the $TREATMENT_c$ indicator function is omitted since $TREATMENT_c$ is nothing more than a collection of time-invariant city fixed effects.[6] The omitted month fixed effect is January, and there are two omitted year fixed effects – the usual omitted year fixed effect is 2013 and the second omitted year fixed effect is 2017 (since this is collinear with the $CARBONLEVY_t$ main variable, given that the carbon tax went into effect on January 1 of that year). The p superscripts denote coefficients in the price equation. The ε^p_{gct} are normally distributed error terms, potentially containing within-market correlation. In all specifications, we calculate and report robust standard errors, clustered at the city level, to account for the correlation.

[6] Equivalently, one could retain the $TREATMENT_c$ main effect and exclude two city fixed effects, one fixed effect for a treatment city and one for a control city. Results are mathematically identical either way.

The corresponding estimating equation for margins is given by:

$$
\begin{aligned}
MARGIN_{gct} \quad = \quad & \beta^m_{0g} + \beta^m_{1g} EXCISETAX_t + \beta^m_{2g} CARBONLEVY_t + \beta^m_{3g} TREATMENT_c \\
& + \beta^m_{4g} TREATMENT_c \times EXCISETAX_t \\
& + \beta^m_{5g} TREATMENT_c \times CARBONLEVY_t \\
& + \sum_{c=2}^{13} \phi^m_{cg} + \sum_{d=2}^{12} \xi^m_{dgt} + \sum_{y=2014}^{2016} \zeta^m_{ygt} + \varepsilon^m_{gct} \qquad (2)
\end{aligned}
$$

where $MARGIN_{gct}$ is defined as $PRICE_{gct} - RACK_{gct}$ for grade g in city c at time t, and other variables are as previously defined. The m superscripts denote the margin equation. The margin equation is isomorphic to the price equation but with β^p_{6g} constrained to be equal to one.

In both equations, the variables of interest are the interaction terms $TREATMENT_c \times EXCISETAX_t$ and $TREATMENT_c \times CARBONLEVY_t$. The coefficients on first interaction, β^p_{4g} and β^m_{4g} show the overall degree of pass-through from the additional excise tax and the coefficients on the second, β^p_{5g} and β^m_{5g}, show the overall degree of pass-through from the carbon levy.

In addition to our main price and margin regressions, we perform a large number of robustness checks using alternate specifications that serve to confirm our results. These include Dynamic OLS models, Vector Autoregressive Error–Correction Models (VAR–ECM), Regression Discontinuity models, models involving expanded sets of controls, models of real versus nominal prices, models with varying levels of time-aggregation to address concerns of overrejection bias, and models exploring the potential biases caused by the inclusion or exclusion of ad valorem taxes on gasoline. We provide details of each specification later in the results section as we introduce them.

In our main specifications, we use nominal prices instead of real prices so we can directly compare our figures to the 4.0 and 4.49 cent per liter nominal tax increases. Our results are very similar when using real prices, as we show in the robustness section, unsurprising in light of the short time period involved (twenty-one months between taxes) and low rates of inflation during this period. We use prices exclusive of the national ad valorem tax since its inclusion which would otherwise inflate our pass-through estimates due to the tax-on-tax nature of gasoline ad valorem taxes. In the robustness section later, we show how our results change in the expected ways if we were to include the ad valorem tax, and discuss the importance of handling ad valorem taxes carefully in pass-through analyses. This is not always clearly done in the literature, and is especially problematic in Europe where ad valorem rates are high and ad-valorem-inclusive and ad-valorem-exclusive pass-through can greatly differ. In our setting, with a four to a four-and-a-half cent per liter tax increase and a low national ad valorem rate of 5%, there is little difference between the two calculations.

We begin our empirical analysis with a series of preliminary diagnostics. We perform a series of Augmented Dickey–Fuller (ADF) unit root tests on retail prices, rack prices, and margins, separately for each city and for each grade of gasoline and for each of the three distinct periods in our study – January 1, 2013 to March 31, 2015 (before the two tax changes went into effect), April 1, 2015 to December 31, 2016 (after the additional excise tax went into effect but before the carbon levy went into effect) and January 1, 2017 to December 31, 2017 (after both tax changes went into effect). There are 78 individual unit root tests in total (two grades x three periods x thirteen cities) for each type of price series (retail, rack, and margins). If unit roots are present and the price series is non-stationary, regressions of these variables can lead to spurious results unless the variables are themselves cointegrated. Not surprisingly, we cannot reject the null hypothesis of a unit root in either the retail price series or in the rack price series individually in many cases (we reject the null 21 times out of 78 for the retail price series and 22 times out of 78 for the rack price series). However, when taking the difference between retail prices and rack prices and performing unit root tests on price–cost margins, we reject the null hypothesis of a unit root at the 5% level in every case.[7] Phillips–Perron (PP) unit root tests also agree – we reject the null hypothesis of a unit root in margins at the 5% level in each city and period for each grade – a total of 78 null rejections in 78 tests.[8] Together, the ADF and PP tests show that margins are stationary. Since margins are simply retail minus rack prices, retail and rack prices are thus cointegrated with a cointegrating coefficient close to one.

To confirm the cointegrating relationship, we also perform Engle-Granger cointegration tests on retail and rack prices, for each city, grade, and time period (for a total of 78 tests). Engle-Granger tests agree with the margin unit root tests – we reject the null hypothesis of no cointegration in

[7] We reject the null 70 out of 78 times at the 1% level.
[8] We reject the null 74 out of 78 times at the 1% level.

each case at better than the 1% level in each case.[9] Johansen trace statistic cointegration tests also agree, rejecting the null hypothesis at the 5% level or better in each case. We conclude that retail and rack prices are cointegrated as could be expected. Since our regressions all contain either retail and rack prices in combination, or the difference in retail and rack prices, i.e. margins, or a series of first differences of retail prices and rack prices, spurious results from trending or random walk variables in our price and margin regressions is not a concern.

To further demonstrate the point, we test for the existence of different pre-existing trends in either prices or margins across our treatment and control cities, both before the implementation of the first tax increase (prior to April 1, 2015), and between the implementation of the first and second tax increase (April 1, 2015 to December 31, 2016). If there are differential pre-existing trends across the treatment and control cities that are expected to continue into the treatment period, that can generate a spurious difference-in-differences result, since the result may simply reflect the continued convergence or divergence of the trends. In all four cases (2 grades x 2 pre-existing trend periods), we find no evidence of differential pre-existing trends, with highly insignificant coefficients on the trend-treatment interaction term and a median p-value of 0.56. We conclude that our data is well-behaved and we may proceed.

4 Results

We now turn to the main results. In short, we have three interesting findings. First, we find that pass-through of the two taxes are generally high. On average across the two taxes and our various specifications, approximately 90% of the tax increases on regular grade gasoline are passed through to consumers, and thus borne by them. The finding is consistent with the low aggregate elasticities of demand for gasoline estimated in the literature, and is near the center of the range of tax pass-through estimates in the gasoline tax literature.

Second, and central to our analysis, we find a meaningful difference between the pass-through of the additional excise tax on one hand and the carbon levy on the other. In the case of the

[9] The Engle–Granger test is based on Augmented Dickey–Fuller tests using the residuals of the cointegrating regression (of retail prices on rack prices) rather than on prices themselves, and using further adjusted critical values as derived by Phillips and Ouliaris (1990).

carbon levy, we find complete pass-through, indistinguishable from 100%, with point estimates that are within a few tenths of a cent of the 4.49 cent per liter increase. We never reject complete pass-through in any specification. In contrast, pass-through for the additional excise tax tend to be noticeably lower, in the neighborhood of 75%, with point estimates generally around 3.0 cents per liter on the 4.0 cent per liter tax. In some specifications, we can reject complete pass-through (while falling just shy of statistical significance in others). The difference in pass-through is economically meaningful and consistent with our hypothesis that consumers may view the environmentally-marketed carbon levy differently than the equivalent generic excise tax increase. This leads to a less elastic and muted response, translating into higher pass-through.

Third, we find an interesting difference when we perform the same comparison for premium grade gasoline instead of regular grade gasoline. It is well known that premium grade gasoline tends to be purchased by wealthier consumers and that these consumers tend to be more inelastic. Consistent with this, we find almost complete pass-through for premium grade prices, but it is now complete for both kinds of taxes. Instead of 75% pass-through for regular grade gasoline, we find 88-96% pass-through on the excise tax increase for premium grade gasoline, across specifications. Meanwhile, pass-through on the carbon levy continues to be topped out at 100%, as it was for regular grade gasoline. The result is that consumers of premium grade gasoline bear the highest consumer incidence from the two taxes collectively – as much as 98% overall – with noticeably less heterogeneity in pass-through across the two taxes. It shows that the naming of the tax has the greatest effect on consumers of regular grade gasoline, while there is little additional advantage for consumers of premium grade gasoline, who are already almost perfectly inelastic to price increases.

The empirical details are presented in Table 2. The first two specifications estimate tax pass-through on regular grade gasoline prices, and the second two estimate tax pass-through on premium grade gasoline prices. The final two estimate tax pass-through on regular and premium grade margins respectively. All six specifications are based on nominal prices and remove all ad valorem taxes. We relax these assumptions later.

Specification (1) is the basic difference-in-differences specification for regular grade gasoline prices, with Albertan cities as the treatment group (Alberta) and other Western Canadian cities outside of Alberta as the control group. There are two treatment periods ($CARBONLEVY$ equal to one after April 1, 2015 and $EXCISETAX$ equal to one after January 1, 2017). We include a control for contemporaneous rack prices as well. The point estimates show that pass-through of the two different taxes are high and that consumers bear the greatest burden of the tax. The coefficient on the $TREATMENT \times EXCISETAX$ interaction variable in Specification (1) of 2.96 shows that regular grade gasoline prices in Albertan cities rose approximately three cents per liter after the increase in the excise tax than before, 74% pass-through on the 4.0 cent per liter increase. In contrast, the coefficient on the $TREATMENT \times CARBONLEVY$ interaction variable shows that regular grade gasoline prices in Albertan cities increased another 4.68 cents per liter with the introduction of the carbon levy twenty-one months later, very close to 100% pass-through.

Not only is pass-through for regular grade gasoline high in general, it is noticeably different across the two taxes. The difference in the point estimates is approximately 25 percentage points, an economically meaningful amount that is suggestive of a potential difference in the way these differently-named and marketed taxes are viewed. Though the difference in our simple static specifications is statistically significant at only the 15% level (given the relatively small size of the tax increases against the backdrop of highly variable gasoline prices), we find that it is a persistent difference that recurs throughout our various specifications and robustness checks. In our dynamic specifications later, the effect is statistically significant at better than the 1% level.

Specification (2) adds individual city indicator variables and year and month indicator variables as well. (The $TREATMENT$ variable is subsumed by the city indicator variables in this specification.) Pass-through in this specification is very similar to the last one – 2.73 cents per liter (68%) for the excise tax and 4.33 cents per liter (97%) for the carbon levy, resulting in a similar degree of economic and statistical significance.

Specification (3) considers premium grade gasoline prices instead of regular grade gasoline prices. We find high pass-through for all taxes including a notably higher pass-through for the excise tax in particular. The coefficient on the $TREATMENT \times EXCISETAX$ variable is 3.84, corresponding to 96% pass-through on the 4.0 cent per liter excise tax increase (compared with 75% pass-through for regular grade gasoline). The coefficient on the $TREATMENT \times CARBONLEVY$ variable continues to be high, at 4.65 cents per liter, and corresponds again to essentially complete pass-through of about 100% on the 4.49 cent per liter carbon levy. The results are consistent with our expectations that premium grade gasoline consumers tend to be more inelastic and pass-through is more complete for this grade regardless of the naming of the tax. In other words, when the carbon levy was marketed as a socially beneficial tax to support environmental initiatives, consumers of regular grade gasoline tended to behave less elastically and became more similar to the way that consumers of premium grade gasoline normally act.

Specification (4) repeats Specification (3) but with city, year, and month indicator variables instead of a main $TREATMENT$ variable, and produces similar results. We find a 3.52 cent per liter price increase following the excise tax increase (88% pass-through) and a 4.31 cent per liter additional price increase following the carbon levy (97% pass-through).

Specifications (5) and (6) use regular grade and premium grade margins, respectively, instead of prices on the left hand side. Results are again similar. For regular grade gasoline, the pass-through of the additional excise tax into margins is 2.85 cents per liter, or about 71%, and the pass-through of the carbon levy into margins is 4.51 cents per liter, or almost exactly 100%. For premium grade gasoline, the pass-through of the additional excise tax now rises to 3.77 cents per liter, or 94%, and pass-through of the carbon levy is 4.57 cents per liter, essentially 100% again. The results show that the degree of pass-through into prices and margins are high generally and that the burden of the tax is largely borne by consumers. Our results (for regular grade gasoline and ignoring the difference in the taxes) are similar to Alm et al. (2009), Marion and Muehlegger (2011) and Bello and Contin-Pilart (2012) which find a high degree of tax pass-through, and less similar to Barron et al. (2004) which finds a lower degree of tax pass-through. The results on premium grade gasoline are new.

Also new is the estimated difference in pass-through based on the naming and marketing of the tax. The point estimates, for regular grade gasoline, show economically meaningfully higher pass-through on the carbon levy, a tax named and promoted to fund environment programs, than on the additional excise tax, whose ultimate disposition, although important, is less clearly connected.

To test the robustness of our results, we perform a series of additional regressions. First, we consider various subsets of our control group and our treatment group, and then re-estimate the model each time, to see if any one control or treatment city is disproportionately affecting results. We find that this is not the case. The results are similar with any subset of treatment and control cities, and all conclusions continue to hold.[10]

Second, as an additional robustness check, we stretch our control group to reach outside Western Canada and include cities in the next closest Canadian province to the east, Ontario. The advantage of including this additional province is that it doubles the size of our control group, from nine cities to nineteen cities. The disadvantage is that Ontario may not be as comparable to Alberta as the other provinces in Western Canada. Ontario is geographically further removed from Western Canada and, notably, is not supplied out of Western Canada but from U.S. and overseas sources. While we still control for local wholesale prices, arguably the more distant province is less comparable in terms of demand shocks.[11] With this caveat in mind, we add the cities of Toronto, Ottawa, Hamilton, London, Kingston, Thunder Bay, Sault Ste. Marie, Sudbury, Timmins and North Bay to the expanded control group.[12]

We report results in Table 3. The six specifications in the table correspond to the six specifications of Table 2, but with the larger control group. We find slightly noisier but similar results. In Specification (1), using regular grade gasoline prices, the coefficient on the $TREATMENT \times EXCISETAX$ variable is equal to 3.11, corresponding to 78% pass-through. The coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.35 cents per liter, corresponding to 97% pass-though. In Specification (2), which again examines regular grade gasoline but includes city and time indicator variables as well, the coefficients are 3.06 cents per liter and 3.96 cents per liter respectively, corresponding to 77% and 88% pass-through on the excise tax and carbon levy, respectively. In Specifications (3) and (4), which repeat the analysis for premium grade gasoline instead, the coefficients on the $TREATMENT \times EXCISETAX$ variable are both equal to 3.5 cents per liter, and are higher than the corresponding estimates on regular grade gasoline. The coefficients on the $TREATMENT \times CARBONLEVY$ variable also remain high, ranging from 3.7 cents per liter to 4.0 cents per liter, and show very similar pass-through across the two taxes in the case of premium grade gasoline.

Specification (5) considers regular grade gasoline margins instead of prices, and yields coefficients

[10] Results not shown to conserve space.

[11] There are five more provinces in Canada even further east, but each is subject to various forms of gasoline price regulation, and we exclude them.

[12] For reference, most cities in Ontario are north of Ohio or New York State, whereas cities in Alberta are north of Montana and Idaho.

equal to 3.09 cents per liter and 4.18 cents per liter, corresponding to 77% and 93% pass-through on the excise tax and carbon levy, respectively. Specification (6) considers premium grade gasoline margins and yields coefficients equal to 3.52 cents per liter and 4.01 cents per liter, corresponding to 88% and 89% pass-through respectively. Again, we see that with premium grade gasoline, pass-through on the excise tax is now higher and pass-through on both taxes are now more similar. We conclude that, even with this expanded and potentially less ideal control group, our main results continue to hold.

Third, we perform a large number of alternate methodological specifications to test the robustness of our results. We report the results in Tables 4A and 4B. Table 4A is for regular grade gasoline and Table 4B is for premium grade gasoline. Each row in each table corresponds to a separate specification, which is described by the note at the beginning of that row. We report four main results of interest for each specification – the $TREATMENT \times EXCISETAX$ and $TREATMENT \times CARBONLEVY$ coefficients when using retail prices on the left hand side, and again when using retail margins on the left hand side. All specifications use the Western Canada control group and all include city, year, and month indicator variables, as well as the rack price, as additional controls.

We begin with regular grade gasoline in Table 4A. In the first specification ("CPI Adjusted Prices"), we replace nominal prices with real prices, using 2015Q1 as the base quarter. Since the sample period is short and inflation is low, we expect this to have little impact, and we find this to be the case. In the price regression, we find a coefficient on the $TREATMENT \times EXCISETAX$ variable of 3.04 cents per liter, corresponding to 76% pass-through and a coefficient on the $TREATMENT \times CARBONLEVY$ variable of 4.56, corresponding to 101% pass-through. The corresponding estimates in the margin regression are 3.17 cents per liter (79%) and 4.70 cents per liter (104%). Whether using nominal or real prices, our conclusions carry through.

In the second specification, we add short run dynamics to the model. While our interest lies in long run pass-through and not in short run dynamics per se, accounting for short run dynamics recognizes the superconsistency in the cointegrating relationship and implicitly corrects the standard errors to account for this superconsistency (Stock and Watson (1993)). Table 2 does not do this and, as a result, we expect our previous standard errors are biased upwards, understating statistical significance. Superconsistency-corrected standard errors are estimated using the dynamic ordinary least squares (DOLS) specification of Stock and Watson (1993), which simply adds lagged and lead rack price first differences to Equation (1):

$$\begin{aligned} PRICE_{gct} \;=\;& \beta^p_{0g} + \beta^p_{1g} EXCISETAX_t + \beta^p_{2g} CARBONLEVY_t + \beta^p_{3g} TREATMENT_c \\ &+ \beta^p_{4g} TREATMENT_c \times EXCISETAX_t \\ &+ \beta^p_{5g} TREATMENT_c \times CARBONLEVY_t \\ &+ \beta^p_{6g} RACK_{gct} + \sum_{s=-S}^{S} \gamma^p_{sg} \Delta RACK_{gct} \\ &+ \sum_{c=2}^{13} \phi^p_{cg} + \sum_{d=2}^{12} \xi^p_{dgt} + \sum_{y=2014}^{2016} \zeta^p_{ygt} + \varepsilon^p_{gct} \end{aligned} \quad (3)$$

where S is the lag/lead length, and the p superscripts denote the price equation.[13] The margin equation is similar with the p superscripts replaced with m superscripts, and $\beta^m_{6g} = 1$.

The results ("Dynamic OLS Model") yield very similar coefficients as before, but now with substantially reduced standard errors as expected. In the price regression, the coefficient on the $TREATMENT \times EXCISETAX$ variable is 2.996 cents per liter, corresponding to 75% pass-through and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.65, corresponding to 103% pass-through. Carbon levy pass-through is as always indistinguishable from 100%. But we can now reject complete pass-through on the excise tax increase at the 1% level. We can also statistically reject that pass-through on the additional excise tax and the carbon levy are equal in this specification, also at the 1% level. Carbon levy pass-through is complete, the excise tax pass-through is not. Results are similar in the margin regression as well. The coefficient on the $TREATMENT \times EXCISETAX$ variable is 2.91 cents per liter, corresponding to 73% pass-through and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.54, corresponding to 101% pass-through. The former is statistically distinguishable from complete pass-through and the two are again statistically significantly different from one another.

In the third specification, we incorporate short run dynamics using a vector-autoregressive error

[13] The lag/lead length S is determined by the testing down method. We use $S = 5$, though our coefficients of interest are very similar for longer or shorter lag/lead lengths.

correction model (VAR-ECM) in the spirit of Engle and Granger (1987). VAR-ECMs have been used to evaluate gasoline prices in numerous past studies, including Borenstein et al. (1997), Noel (2009), Lewis and Noel (2011), and many others. We are interested neither in speed nor asymmetry, but the model allows retail prices to respond to changes in wholesale prices with a lag and potentially asymmetrically for increases and decreases. Deviations from the long run cointegrating relationship are allowed, and the model allows for a gradual reversion back to the long run relationship over time.

We estimate two forms of the model, a one step version (Banerjee et al. (1993); Borenstein et al. (1997)) and a two step version (Bachmeier and Griffin (2003); Engle and Granger (1987)). In the two step version, the first step is to simply re-estimate Equation (1) and the second step is to use the residuals from that regression in a second regression of retail price changes on lagged rack and retail price changes:

$$\begin{aligned} \Delta PRICE_{gct} \;=\; & \sum_{s=0}^{S} \gamma_{gs}^{p+} \Delta RACK_{gc,t-s}^{+} + \sum_{s=0}^{S} \gamma_{gs}^{p-} \Delta RACK_{gc,t-s}^{-} \\ & + \sum_{r=1}^{R} \delta_{gr}^{p+} \Delta PRICE_{gc,t-r}^{+} + \sum_{r=1}^{R} \delta_{gr}^{p-} \Delta PRICE_{gc,t-r}^{-} \\ & + \theta^{p+} z_{gc,t-1}^{+} + \theta^{p-} z_{gc,t-1}^{-} \end{aligned} \tag{4}$$

where

$$\begin{aligned} z_{gct} \;=\; & PRICE_{gct} - \beta_{0g}^{p} - \beta_{1g}^{p} EXCISETAX_t - \beta_{2g}^{p} CARBONLEVY_t \\ & - \beta_{3g}^{p} TREATMENT_c - \beta_{4g}^{p} TREATMENT_c \times EXCISETAX_t \\ & - \beta_{5g}^{p} TREATMENT_c \times CARBONLEVY_t \\ & - \beta_{6g}^{p} RACK_{gct} - \sum_{c=2}^{13} \phi_{cg}^{p} - \sum_{d=2}^{12} \xi_{dgt}^{p} - \sum_{y=2014}^{2016} \zeta_{ygt}^{p} \end{aligned} \tag{5}$$

and $z_{gc,t-1}^{+} = \max(0, \Delta z_{gc,t-1})$ and $z_{gc,t-1}^{-} = \min(0, \Delta z_{gc,t-1})$. The variables $\Delta RACK_{gc,t-i}^{+}$, $\Delta RACK_{gc,t-i}^{-}$, $\Delta PRICE_{gc,t-i}^{+}$, and $\Delta PRICE_{gc,t-i}^{-}$ are similarly defined.[14] The first stage residual $z_{gc,t-1}$ can be taken as known in this set-up (instead of estimated) due to the superconsistency of the cointegrat-

[14] We use $S = 4$ and $R = 8$, though results are unaffected with other reasonable lag lengths.

ing regression and the "+" and "–" distinction allows for different rates of reversion to the long run equilibrium depending on the direction of the initial deviation. In the one step model, Equation (5) is substituted directly into Equation (4) and the system is estimated simultaneously.[15]

We begin with the two-step version. Since the first step of the two-step version is simply a re-estimation of Equation (1), it produces identical interaction coefficients as the ones found in Table 2 (the $TREATMENT \times EXCISETAX$ coefficient equal to 2.96 cents per liter and the $TREATMENT \times CARBONLEVY$ variable equal to 4.68). We thus do not re-report these in the table.

The third specification of Table 4A ("Simple VAR–ECM Model") reports the results from the one-step version. The results are generally similar, though the point estimates are slightly higher in the one-step version than the two-step version, and the difference between them a little less. The table shows a coefficient on the $TREATMENT \times EXCISETAX$ variable equal to 3.41 cents per liter and a coefficient on the $TREATMENT \times CARBONLEVY$ variable equal to 4.77, corresponding to a 21% difference in pass-through across the two taxes (as opposed to a 28% difference in the two-step version). The difference in pass-through is statistically significant just shy of the 10% level. We also perform a margins version of the VAR–ECM model by restricting β^p_{6g} and find similar results. The coefficient on the $TREATMENT \times EXCISETAX$ variable is 3.28 cents per liter and a coefficient on the $TREATMENT \times CARBONLEVY$ variable of 4.57.

The fourth and fifth specifications of Table 4A ("Collapse to Monthly Data" and "Collapse to Yearly Data") address a different concern about potential statistical overrejection, raised by Bertrand et al. (2004). That study shows that serially correlated data in a long panel can potentially lead to downward biased standard error estimates. In Monte Carlo simulations, they show that OLS regressions with no standard error corrections at all can reject the null (at the 5% level) almost 50% time when the null is true.

Fortunately, overrejection is of less concern in our setting because we do make necessary adjustments. In particular, we cluster our standard errors at the city level, with an arbitrary variance-covariance matrix to account for serial correlation in the error term. Bertrand et al. (2004) find that this procedure largely corrects the problem when there are many clusters and does well, though not

[15] The one step version requires $z^+_{mt} = z^-_{mt}$ but maintains direction specific γ^+, γ^-, δ^+, and δ^- in the estimation.

perfectly, for smaller numbers of clusters as well. With ten clusters in their simulations (we have thirteen in our main analysis), the rejection rate in the face of a true null falls to 8% (down from 50%) at a significance level of 5%. When the number of clusters is twenty (we have twenty-three in our expanded analysis), the rejection rate under a true null is almost correct – a 5.8% rejection rate with a significance level of 5%. Since we cluster standard errors in our specification and have a fairly large number of clusters, combined with our relatively short panel and use of relatively infrequent weekly-level data (relative to the speed of pass-through), we expect little downward bias in our clustered standard error calculations. In fact, we expect our specifications that do not take advantage of the superconsistency of the cointegrating relationship yield standard errors that are conservatively high.

Nonetheless, in case any potential for over-rejection remains, we implement the technique that Bertrand et al. (2004) find works best in reducing any Type I error to the correct level even after clustering is done. They suggest simply aggregating the data up to a coarser unit of time, essentially removing much of the time dimension, and thus the serial correlation, from the analysis. The approach reduces Type I error to the correct level even with few clusters, but it comes at the cost of substantially increasing Type II error (not rejecting a null hypothesis that is false) because it throws away both good and bad variation.[16]

We implement the technique two ways. In the fourth specification of Table 4A ("Collapse to Monthly Data"), we collapse the data from the original weekly frequency to a monthly frequency instead. This reduces the number of observations by 77%, from 3,393 city-weekly observations to only 780 city-monthly observations, and reduces the potential for serial correlation in the error terms that might not be captured by our standard clustering technique. In the fifth specification of Table 4B ("Collapse to Yearly Data"), we take this a step further and collapse the data all the way down to a yearly frequency. This substantially reduces the number of observations, by 98%, from 3,393 observations to a paltry 78 observations, one per city per year (and per tax period in the case of 2015 which had a midyear tax increase). The specification is exceptionally demanding

[16] In their simulations, Bertrand et al. (2004) reject the null hypothesis of no effect only 6% of the time when the alternative hypothesis (of a 2% effect on the left hand side variable) is actually true. That is, statistical power is just 6% and Type II error is high at 94%. Fortunately, power increases and Type II error falls the greater is the true effect (under the alternative hypothesis) above 2%.

of the data, as it throws almost all usable variation, significantly inflating the potential for Type II error.

Nonetheless, and in each case, our results hold. In the "Collapse to Monthly Data" price specification, the coefficient on the $TREATMENT \times EXCISETAX$ variable is 2.81 cents per liter (70% pass-through) and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.52 cents per liter (101% pass-through). In the margin specification, the $TREATMENT \times EXCISETAX$ variable is again 2.81 cents per liter (70% pass-through) and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is again 4.52 cents per liter (101% pass-through). All results are statistically significant and standard errors are not noticeably higher than before.

In the "Collapse to Yearly Data" price specification, the coefficient on the $TREATMENT \times EXCISETAX$ variable is 3.08 cents per liter (77% pass-through) and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.47 cents per liter (100% pass-through). In the margin specification, the $TREATMENT \times EXCISETAX$ variable is 3.10 cents per liter (78% pass-through) and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.51 cents per liter (100% pass-through). Again, standard errors are not noticeably higher showing that the potential for inflated standard errors is not a large concern.

In the sixth specification of the table ("+/- One Year Event Window"), we perform a robustness check in which limit the length of the event window, from the entire sample period to the period one year on either side of each tax change. The shorter window removes any confounding events that can affect prices and margins but were further removed from the date of the tax changes. In this specification, we again find similar results, even a little stronger. The coefficient on the $TREATMENT \times EXCISETAX$ variable is 2.60 cents per liter (65% pass-through) and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.42 cents per liter (98% pass-through). In the margin specification, the $TREATMENT \times EXCISETAX$ variable is 2.89 cents per liter (72% pass-through) and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.53 cents per liter (100% pass-through). All results are statistically significant.

In the seventh specification of the table ("Regression Discontinuity") we use the same one-year event window to perform a simple regression discontinuity specification instead of our main difference-in-differences specifications. In other words, we exclude the control group from the analysis and rely on the sudden shock to tax rates at a point in time to identify effects. We find that the point estimates are again similar. The coefficient on the $TREATMENT \times EXCISETAX$ variable is 3.18 cents per liter (79% pass-through) and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.40 cents per liter (98% pass-through). In the margin specification, the $TREATMENT \times EXCISETAX$ variable is 3.10 cents per liter (78% pass-through) and the coefficient on the $TREATMENT \times CARBONLEVY$ variable is 4.40 cents per liter (98% pass-through). The fact that the point estimates are similar to the difference-in-differences shows that the effects are being driven by changes in the treatment cities themselves and not from confounding unknown factors changing in the control cities. The standard errors in these regressions are notably lower now that they do not contain the additional white noise variation from the control cities, and we can again reject complete pass-through of the excise tax into prices and margins. Pass-through of the carbon levy in particular is more precisely estimated and still exceptionally close to complete. Other event windows, whether using a regression discontinuity or difference-in-differences framework, produce similar results.

Finally, in the eighth specification of Table 4A ("Including Ad Valorem Taxes"), we show the effects of not excluding ad valorem taxes in our data. Whether ad valorem taxes are included or not included in the price data depends on the definition of pass-through one is interested in. Both are correct, but they measure different things and the interpretation is different as well. This has not always been well distinguished in the literature. Gasoline is a unique commodity in that both specific (i.e. per liter) taxes and ad valorem taxes often apply, and the ad valorem tax is generally charged on top of the specific-tax-inclusive price. That means that a 20% ad valorem tax added on top of a one cent per liter specific tax increase, assuming complete pass-through, would actually result in a 1.2 cent per liter price increase (and a 1.0 cent per liter margin increase). Because the ad valorem tax in our analysis is rather small (5%) and the tax changes are also relatively small compared to the price of gasoline, the choice of ad-valorem-inclusive or -exclusive prices makes only minor difference in our setting. The coefficient on the $TREATMENT \times EXCISETAX$ variable rises to 2.87 cents per liter and the coefficient on the $TREATMENT \times CARBONLEVY$ variable rises to 4.55 cents per liter, all compared to the otherwise equivalent second specification of Table 2, which holds all else equal except for the ad valorem tax. There, the $TREATMENT \times$

$EXCISETAX$ variable was 2.73 cents per liter and the coefficient on the $TREATMENT \times CARBONLEVY$ variable was 4.33 cents per liter. The 5% ad valorem tax simply appears as a 5% inflation in the estimated pass-through coefficients.

The difference here is small, but our point is a more general one. Ad-valorem-tax-inclusive and ad-valorem-tax-exclusive degrees of pass-through measure different things, and it is important to be explicit about what is being estimated in order to interpret the coefficients properly. Since ad valorem taxes, especially in Europe, are as high as 20% and excise taxes are as high as 91 cents per liter, the difference in measured pass-through using an ad-valorem-tax-inclusive rate or an ad-valorem-tax-exclusive rate can be as high as 18 cents per liter, or 70 U.S. cents per gallon. It is a meaningful difference that has not always been clearly separated in the literature, but needs to be for proper interpretation.

We now turn to our robustness checks for premium grade gasoline. We repeat the entire analysis of Table 4A, but for premium, and report the results in Table 4B. Similar to our robustness checks on regular grade gasoline, our results for premium grade gasoline continue to hold under this battery of robustness checks. We find that pass-through on the additional excise tax are generally higher for premium grade gasoline than for regular grade gasoline (with 90% median pass-through, instead of 75%) and pass-through on the carbon levy remains exceptionally close to 100%. Each is significantly different from zero, insignificantly different from complete, and insignificantly different from one another.

The results for premium grade gasoline supports our initial intuition. Premium grade gasoline is more expensive and tends to be purchased by wealthier, inelastic consumers whose more expensive vehicles recommend and require it. While consumers of regular grade gasoline may view and respond to the two types of taxes differently with different (albeit low) degrees of responsiveness, premium grade gasoline consumers are especially inelastic causing pass-through to top out at 100% after a tax under any name.

5 Conclusion

In this study, we examine two distinct types of gasoline taxes – an increase in the excise tax and the introduction of a new carbon levy – and calculate the consumer incidence of each tax or, equivalently, the degree of tax pass-through. We are particularly interested in exploring how the naming and marketing of a tax, via changes in consumer behavior and elasticities, can affect these outcomes.

There is a small literature examining gasoline tax incidence in response to various tax increases, and the evidence is mixed on whether gasoline taxes are passed through to consumers completely or incompletely. One interesting result in our study is that we find examples of both – we find that the carbon levy is passed through completely and the excise tax is passed through incompletely in our markets. The findings suggest that taxes may not all be homogeneous – even if the dollar amounts are the same – the naming, marketing, and purpose of a tax can matter. This could potentially underlie some of the mixed evidence in the literature on gasoline tax incidence. In our study, the two taxes in question were marketed very differently - the carbon levy was implemented with a campaign to make consumers aware of its purpose and value, while the nearly equivalent-in-size increase in the excise tax came with little fanfare or transparency.

Relatively little has been done to explore different kinds of taxes and their potentially differential effects on tax incidence, and one reason may be the difficulty in reliably identifying effects. Tax changes are relatively infrequent, and when one does occur, it generally comes with a name and a marketing campaign, or it is does not. This forces the researcher to resort to cross-sectional comparisons across jurisdictions or use other benchmarking techniques that are subject to all the usual omitted variables problems. Fortunately, we are able to overcome this problem by exploiting a unique natural experiment in which a single jurisdiction (Alberta) implemented both an excise tax increase and a new carbon levy, at distinctly separate times, but not too far apart, all the while gasoline taxes remained stable in all neighboring jurisdictions.

Using panel data of regular and premium grade gasoline prices from Alberta and other Western Canadian cities from 2013 to 2017, we estimate the degree of pass-through of the tax increases into retail prices. We then contrast the degree of pass-through of the excise tax increase with the degree of pass-through on the carbon levy, two similar tax increases in Alberta presented to consumers under different names and different degrees of transparency.

Across our many econometric specifications, we find that the degree of pass-through across the two taxes are generally high – on average across the two, approximately 90% of the tax increases on regular grade gasoline are passed through to consumers. This is consistent with the low aggregate elasticity of demand for gasoline estimated in the literature, and more similar to the higher pass-through estimates reported in the gasoline tax literature. We then find an interesting difference between pass-through of the additional excise tax on one hand and the carbon levy on the other, for regular grade gasoline. In the case of the carbon levy, our estimated pass-through estimates are especially high, essentially 100%, with point estimates within just a few tenths of a cent of the 4.49 cent per liter levy across our many specifications. In contrast, pass-through for the additional excise tax tend are only in the neighborhood of 75%, with point estimates generally around 3.0 cents per liter on a 4.0 cent per liter tax. The general pattern of coefficients is consistent with the hypothesis that consumers view the environmentally–targeted carbon levy more favorably than an equivalent excise tax increase, leading to less elastic and muted responses, and higher pass-through.

Pass-through on premium grade gasoline, consistent with our intuition, is generally more complete across the board. Pass-through on the carbon levy is again near 100%, but now the pass-through on the excise tax increase increases to as much as 96%, resulting in a smaller difference in pass-through by these generally less elastic consumers.

An interesting implication of this result is that calling the tax a carbon levy, which makes it more transparent and potentially more palatable to consumers, thus makes consumers more inelastic to the price increase it causes. The irony is that by calling it a carbon levy, the tax is unlikely to reduce gasoline use by as much and unlikely to reduce carbon emissions by as much in the short run, compared to a generic tax increase. Of course, in either case, the short run effects via higher prices are small at least for tax increases of this size.[17] After all, gasoline taxes have long been a favorite source of revenue for governments exactly because demand is inelastic and because the reduction in sales and added deadweight loss is small. The value of calling a carbon

[17] One expects a larger short run effect for carbon levies of a greater size, but not for the more modest size passed in Alberta.

levy and a carbon levy lies in the fact that it keeps revenues high, funds the program's energy initiatives with relatively less economic loss, and helps foster the development and introduction of new cost-competitive substitutes more quickly. Thus, the long run effect of a carbon levy would appear to be more on the supply side than on the demand side.

Ours is among the first study to examine gasoline tax incidence and the potentially differential effects based on the type and naming of the tax. Our results suggest that consumers may exhibit more complex preferences towards different taxes and that behavioral considerations can be important in predicting taxation outcomes. We have shown that consumers respond differently to a tax increase that may support something they care about or is (or is at least perceived to be) more transparently spent. Carbon levies were promoted as a specific effort to curb pollution and protect the environment, whereas general excise tax increases are less transparently absorbed into general expenditure accounts. The policy implications of differential pass-through are straightforward - the naming and marketing of a tax can matter. It matters for incidence, the distribution of surplus, and ultimately the efficacy of the tax itself.

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Table 1. Summary Statistics

	Num. Obs.	Mean	Std. Dev.	Minimum	Maximum
Main Dataset - Western Canada					
Regular Grade Retail Price	3393	109.84	15.61	57.9	154.2
Regular Grade Wholesale Price	3393	69.49	12.87	31.0	96.7
Premium Grade Retail Price	3393	124.97	15.33	74.8	169.5
Premium Grade Wholesale Price	3393	77.17	12.55	39.0	104.1
Extended Dataset - Western Canada and Ontario					
Regular Grade Retail Price	6003	112.76	15.38	57.9	154.2
Regular Grade Wholesale Price	6003	69.53	12.65	31.0	96.8
Premium Grade Retail Price	6003	128.04	14.93	74.8	169.5
Premium Grade Wholesale Price	6003	77.44	12.43	39.0	104.8

All prices in Canadian cents per liter.

Table 2. Excise Tax and Carbon Levy Pass-Through

	Retail Prices				Retail Margins	
	Regular (1)	Regular (2)	Premium (3)	Premium (4)	Regular (5)	Premium (6)
TREATMENT*EXCISETAX	2.962**	2.731**	3.841**	3.523**	2.851**	3.766**
	(0.733)	(0.802)	(0.710)	(0.724)	(0.760)	(0.705)
TREATMENT*CARBONLEVY	4.683**	4.331**	4.649**	4.306**	4.515**	4.568**
	(0.788)	(0.771)	(0.795)	(0.772)	(0.824)	(0.834)
EXCISETAX	2.168**	-0.610**	2.219**	-1.077**	-0.972**	-1.601**
	(0.861)	(0.265)	(0.610)	(0.474)	(0.297)	(0.611)
CARBONLEVY	-0.458	0.952	-0.200	2.386	2.304*	4.013**
	(0.572)	(1.663)	(0.585)	(1.553)	(1.293)	(1.157)
TREATMENT	-10.741**		-11.520**			
	(1.803)		(2.505)			
RACK	1.061**	0.933**	1.027**	0.912**		
	(0.037)	(0.036)	(0.031)	(0.038)		
City Indicator Variables	N	Y	N	Y	Y	Y
Month Indicator Variables	N	Y	N	Y	Y	Y
Year Indicator Variables	N	Y	N	Y	Y	Y
Adj. R-Squared	0.832	0.923	0.768	0.916	0.260	0.355
Num. Obs.	3393	3393	3393	3393	3393	3393

** Significant at the 5% level. * Significant at the 10% level. Standard errors in parentheses.

Table 3. Excise Tax and Carbon Levy Passthrough - Extended Control Group

	Retail Prices				Retail Margins	
	Regular	Regular	Premium	Premium	Regular	Premium
	(1)	(2)	(3)	(4)	(5)	(6)
TREATMENT*EXCISETAX	3.113**	3.062**	3.519**	3.446**	3.091**	3.518**
	(0.484)	(0.507)	(0.658)	(0.660)	(0.496)	(0.659)
TREATMENT*CARBONLEVY	4.349**	3.957**	4.015**	3.711**	4.182**	4.011**
	(0.612)	(0.592)	(0.605)	(0.583)	(0.635)	(0.628)
EXCISETAX	1.663**	-0.618	2.064**	-0.651	-0.927**	-1.067**
	(0.710)	(0.404)	(0.514)	(0.448)	(0.379)	(0.474)
CARBONLEVY	-0.083	1.264	0.510	2.947**	2.415**	4.360**
	(0.343)	(1.152)	(0.350)	(1.099)	(0.810)	(0.763)
TREATMENT	-8.639**		-8.403**			
	(1.092)		(1.467)			
RACK	1.042**	0.944**	1.001**	0.925**		
	(0.028)	(0.027)	(0.022)	(0.029)		
City Indicator Variables	N	Y	N	Y	Y	Y
Month Indicator Variables	N	Y	N	Y	Y	Y
Year Indicator Variables	N	Y	N	Y	Y	Y
Adj. R-Squared	0.844	0.932	0.782	0.924	0.203	0.312
Num. Obs.	6003	6003	6003	6003	6003	6003

** Significant at the 5% level. * Significant at the 10% level. Standard errors in parentheses.

Table 4A. Excise Tax and Carbon Levy Pass-through on Regular Grade Gasoline - Alternate Specifications

	Num. Obs.	Regular Retail Prices: Excise Tax	Regular Retail Prices: Carbon Levy	Regular Retail Margins: Excise Tax	Regular Retail Margins: Carbon Levy
CPI-Adjusted Prices	3393	3.043**	4.555**	3.166**	4.700**
		(0.804)	(0.729)	(0.755)	(0.779)
Dynamic OLS Model	3237	2.996**	4.654**	2.913**	4.538**
		(0.321)	(0.423)	(0.319)	(0.424)
VAR-ECM Model	3276	3.413**	4.766**	3.275**	4.568**
		(0.511)	(0.741)	(0.547)	(0.796)
Collapse to Monthly Data	780	2.806**	4.517**	2.808**	4.520**
		(0.768)	(0.803)	(0.751)	(0.823)
Collapse to Yearly Data	78	3.078**	4.473**	3.100**	4.512**
		(0.597)	(0.877)	(0.561)	(0.855)
+/- One Year Event Window	1365	2.603**	4.421**	2.888**	4.533**
		(0.523)	(0.819)	(0.435)	(0.838)
Regression Discontinuity	420	3.175**	4.397**	3.102**	4.398**
		(0.367)	(0.400)	(0.234)	(0.499)
Including Ad Valorem Taxes	3393	2.867**	4.547**	2.904**	4.603**
		(0.843)	(0.810)	(0.824)	(0.868)

** Significant at the 5% level. * Significant at the 10% level.

Table 4B. Excise Tax and Carbon Levy Pass-through on Premium Grade Gasoline - Alternate Specifications

	Num. Obs.	Regular Retail Prices		Regular Retail Margins	
		Excise Tax	Carbon Levy	Excise Tax	Carbon Levy
Real Prices	3393	3.816**	4.579**	4.073**	4.792**
		(0.724)	(0.728)	(0.700)	(0.792)
Dynamic OLS Model	3237	3.851**	4.592**	3.860**	4.601**
		(0.320)	(0.439)	(0.321)	(0.436)
VAR-ECM Model	3276	4.309**	4.521**	4.253**	4.459**
		(0.558)	(0.750)	(0.587)	(0.820)
Collapse to Monthly Data	780	3.660**	4.496**	3.731**	4.571**
		(0.697)	(0.808)	(0.697)	(0.834)
Collapse to Yearly Data	78	3.866**	4.394**	4.009**	4.567**
		(0.598)	(0.882)	(0.606)	(0.863)
+/- One Year Event Window	1365	3.222**	4.496**	3.696**	4.574**
		(0.700)	(0.806)	(0.704)	(0.828)
Regression Discontinuity	420	3.783**	4.190**	3.912**	4.314**
		(0.712)	(0.425)	(0.679)	(0.462)
Including Ad Valorem Taxes	3393	3.699**	4.521**	3.816**	4.648**
		(0.760)	(0.811)	(0.748)	(0.875)

** Significant at the 5% level. * Significant at the 10% level.