

Is Inflation Relief Passed-through? Evidence from Excise Taxes

Michael D. Noel* Travis Roach[†] and Madison Hill[‡]

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Abstract

The combination of ongoing pandemic-related supply chain issues and the commencement of the Ukraine-Russia war triggered a sharp increase in gasoline prices in 2022, occurring alongside a forty-year high in general inflation. In response, five state governments temporarily stopped collecting excise taxes on gasoline in order to provide ‘inflation relief’ to their constituents. In this work, we examine the incidence of this tax relief and assess whether or not the full amount was passed-through to consumers using multiple methodologies in a natural experiment setting. We find that excise tax pass-through was not complete, with only about 73% of the excise tax relief passed down to consumers. Georgia had the lowest pass-through rate of 62.8% while Connecticut had the highest rate at 82%. Our results imply an unintended subsidy to gasoline retailers of more than \$571 million dollars in exchange for only about \$10 in savings for each driver. The results highlight the need for alternative policy measures.

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*Texas Tech University, michael.noel@ttu.edu.

[†]University of Central Oklahoma, troach2@uco.edu.

[‡]University of Oklahoma, madisonhill@ou.edu.

1 Introduction

In the summer of 2022, several Governors and state legislatures elected to remove excise taxes on gasoline to provide “inflation relief” in the midst of a highly inflationary period.¹ Higher gas prices, and inflation more broadly, were being stoked by high pent-up demand post-pandemic, the slow unwind of international supply chain backlogs, and the onset of the Ukraine-Russia war. The root causes of high prices are largely out of the control of state and local officials, however, so targeting gasoline prices and excise taxes on gasoline can make sense from both a practical and political point of view. It can make practical sense because it is one of the few ways a state policymaker can affect consumer prices directly and immediately, and on an important and highly-inelastic product. It can also make political sense, as fuel expenditures make up a non-trivial share of most families’ budgets, and consumers captive to the ups and downs of the market often view high gasoline prices with frustration and even some skepticism. It is well known that taking a hard-line stance against high gasoline prices, along with the oil companies and gasoline retailers behind them, can win political favor among large groups of constituents. The unique salience of gasoline prices makes the political impact of lowering them even greater.

The stated intention of removing gasoline excise taxes was to provide direct relief to consumers by reducing the price of gasoline by the size of the excise tax. However, this occurs only if retailers pass the entire cost savings through its downstream consumers, which may or may not be the case. Given that posted gasoline prices are tax-inclusive and retailers set prices, it depends on whether retailers pass the entire cost savings through to consumers (and maintain previous margins), or pass through only a part of it (and increase their margins), and this in turn depends on competitive conditions. If pass-through is not complete, the tax cut would not have its full desired effect and could reasonably be viewed as inefficient for

¹At the Federal level, the Biden administration released nearly 180 million barrels of crude oil from the Strategic Petroleum Reserve to combat climbing gas prices, with limited effect.

this purpose.² Moreover, excise gasoline taxes constitute a significant portion of revenue for states, and forgoing them can have lasting budgetary effects. For example, in New York a single month of excise tax revenue is typically more than \$153 million dollars. During the six-month period that the state did not collect excise taxes, they stood to lose approximately \$920 million dollars in tax revenue. If pass-through is incomplete, only a portion of foregone tax revenues go to consumers and the other portion provides an unintentional and targeted subsidy to gasoline retailers at a significant cost.

In this work, we exploit a simple natural experiment to assess how the cost-savings from the excise tax relief were passed-through to gasoline prices. To do this, we use daily retail gasoline price data at the city-level for 194 North American cities, and daily regional wholesale price data. In 2022, a total of five states passed excise tax relief policies - Connecticut, Georgia, Maryland, New York, and Florida - each with a different excise tax rate and each suspending their tax for a different duration of time. We are able to estimate pass-through rates in Connecticut, Georgia, Maryland, and New York, but must drop Florida where the assumption of parallel pre-treatment-period trends is clearly violated.³

We estimate pass-through rates using two distinct methodologies. The first is the standard difference-in-differences (DD) methodology, using the above four states as treatment groups and all others (except Florida) as controls. We also use the de Chaisemartin and D’Haultfoeulle (2022) difference-in-differences (CH) estimator which is well-suited for estimating the effect of staggered introduction of states removing their excise taxes on gasoline. This estimator can handle situations where different states remove their excise taxes at different points in time, allowing for a more accurate estimation of the policy’s impact. The estimator avoids comparing later-treated states (like New York) to a control group that includes already-treated states (like Maryland), instead constructing a more appropriate

²We discuss alternative tax relief options later. One key advantage of excise tax relief is that implementation costs are low. A key disadvantage is that it only benefits those who drive a lot.

³We discuss Florida in more detail below and in the appendix.

counterfactual for each state based on the group of “never-treated” states. Moreover, the CH estimator can account for the fact that each state sets a different excise tax rate, meaning that the intensity of treatment varies across states. By incorporating these heterogeneous treatment effects, the estimator provides a more precise and reliable estimate of the average treatment effect, even in the presence of varying treatment timing and intensities. Our models also allow for lagged and potentially asymmetric pass-through of wholesale costs into retail prices, as is commonly done.

There are several interesting results to discuss. First, and most notably, we find that pass-through is not complete. Using the de Chaisemartin and D’Haultfoeuille (2022) methodology of estimating treatment effects, we find that only 73.1% of the excise tax is passed-through to consumers overall. The standard DD methodology yields a similar estimate of 74.5%. About one quarter of all the money spent by state governments on excise tax relief intended to help consumers does not go to consumers. This result is bolstered by placebo policy experiments where do not find a policy effect where one shouldn’t be, and further, this result is not sensitive to potentially overly influential observations. Second, we see that retailers from state to state differ in the degree to which they pass these cost-savings on to consumers. Here, our results using the CH methodology and our results using the DD methodology differ, but in expected ways, and we can see the added value of the more advanced CH methodology. Under the CH methodology, we find that Connecticut has the highest pass-through rate point estimate of 82% and Georgia has the lowest pass-through rate of 62.8%, while Maryland and New York have pass-through rates of 76.3% and 73.3%, respectively, similar to the overall average.

Third and finally, we use our results to comment on the costs and benefits of this policy. All told, even with incomplete pass-through, the policy to reduce excise taxes yielded more than \$1.4 billion dollars in fuel-expenditure savings for consumers across the four states. While large on the aggregate, it translates to only about \$11.70 per month per consumer on

average (and then only if they are drivers of gasoline-burning vehicles). The BLS estimates that the typical household faced an increase of \$6,039 in typical expenditures due to inflation in the period, or about \$503 per month (Meyers et al. (2023)). This means that the ‘inflation relief’ policy only relieved about 2% of the inflation-induced increase in expenditures that a typical household faced.

If we are to think of these fuel-savings as a benefit to consumers, as they are the intended beneficiaries, one would want to weigh these benefits against the costs of the program. It would be near impossible to pin-down the opportunity cost of these tax revenues because needs and circumstances vary by state. However, we can expound on one cost of the program with some detail because it was not the goal of the policy to offer a targeted subsidy exclusively to gasoline retailers. Because pass-through was incomplete and prices did not fall by the amount of the excise tax, gasoline retailers were able to retain a portion of the excise tax for themselves. On the high end, this was approximately \$0.10 per gallon on a average in Maryland, Georgia, and New York, and approximately \$0.05 per gallon in Connecticut at the low end. In total across the four states, gasoline retailers benefited from the policy by more than \$571.5 million dollars. For Maryland, Georgia, and New York, this meant subsidizing gasoline retailers by \$20-\$40 million dollars each month the policy was in place. These tens of millions of additional dollars were spent by each state each month to secure only about \$10 in savings for each driver. We conclude that the excise tax moratorium, while politically popular, is unlikely to an efficient mechanism for inflation relief, and additionally, unlikely to be the source mechanism to target those most in need in hard times.

2 Literature and Background

The incidence of a tax and the distribution of the tax burden between buyers and sellers determines the welfare implications of tax policy reforms. In the case of removing an excise tax on gasoline, the stated intention behind these policies was to provide inflation relief and

the expected outcome was that the full reduction in tax would be passed on to consumers. However, as standard textbook theory shows us, it is the price elasticities of demand and supply together that ultimately determine the amount of tax relief that gets passed through to consumers.

In the standard textbook model, 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 per-unit excise tax. Note that demand in this model responds to the post-tax price, and supply responds to the pre-tax price. Simple algebra shows that the increase in the tax-inclusive price paid by consumers per unit of tax is given by

$$\frac{dp}{d\tau} = \frac{\frac{\partial S}{\partial p}}{\left(\frac{\partial S}{\partial p} - \frac{\partial D}{\partial p}\right)} = \frac{\eta}{\eta - \varepsilon} \quad (1)$$

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

$$\frac{d(p - \tau)}{d\tau} = \frac{\frac{\partial S}{\partial p}}{\left(\frac{\partial S}{\partial p} - \frac{\partial D}{\partial p}\right)} - 1 = \frac{\frac{\partial D}{\partial p}}{\left(\frac{\partial S}{\partial p} - \frac{\partial D}{\partial p}\right)} = \frac{\varepsilon}{\eta - \varepsilon} \quad (2)$$

The terms $\frac{dp}{d\tau}$ and $\frac{d(p - \tau)}{d\tau}$ represent the incidence of the tax to consumers and producers, respectively. Incidence to consumers is greater with more elastic supply and more inelastic demand, and full consumer burden corresponds 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.

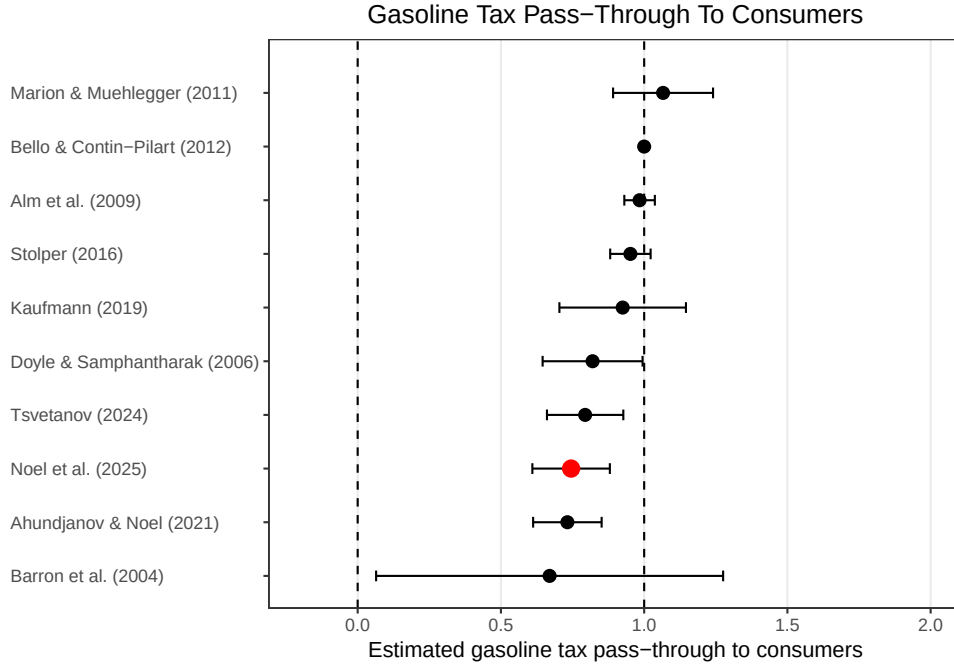
The general consensus in the literature is that demand elasticities for retail gasoline are very low and supply elasticities are generally very high (Brons et al. (2008); Levin et al. (2017); Li et al. (2014); Noel and Roach (2016)). This leads to an expectation that increases in gasoline taxes disproportionately affect consumers, a consensus supported by

most empirical work (Li et al. (2014); Ahundjanov and Noel (2021)). But tax decreases may or may not be fully passed through to consumers (Barron et al. (2004); Doyle and Samphantharak (2008)), especially in the short run and especially with tax decreases known to be temporary in nature, such as the ones we study here (Radchenko and Shapiro (2011)). Thus, whether or not the pass-through of gasoline excise tax cuts is complete or incomplete remains uncertain a priori.

Some studies have found evidence of complete pass-through. Alm et al. (2009) and Marion and Muehlegger (2011) study the pass-through of U.S. state gasoline excise taxes throughout the 1980s and 1990s, concluding that incidence was complete. Bello and Contín-Pilart (2012) identified complete pass-through of regional gasoline taxes in Spain as well. Other studies find incomplete pass-through. Doyle and Samphantharak (2008) study the temporary moratorium of the 5% gasoline tax in Illinois and Indiana during the summer of 2000 and find that 80–100% of tax reinstatement was reflected in retail prices, but only 70% of the tax suspension was passed-through to consumers. Barron et al. (2004) study a reduction in the excise gasoline tax in Connecticut following a significant price spike in 2000, finding that only about two-thirds of the tax reduction reached consumers. Connecticut was one of the several states that passed an inflation relief policy in the 2022 price spike, and we will find that approximately 82% of the excise-tax savings were passed on to consumers in this state. Silvia and Taylor (2014) documented incomplete pass-through of a five-cent excise gasoline tax increase in Washington state. More recently, Ahundjanov and Noel (2021) answer a slightly different question, asking whether or not the *intent* of the tax matters for pass-through to consumers. These authors find that taxes framed as an environmental tax are more easily passed on to consumers with near 100% pass-through, but that the excise tax increase that was not associated with an environmental goal had a lower pass-through rate.

The paper most directly related to ours is Tsvetanov (2024) which estimates price pass-

Figure 1: Estimated Pass-through Rates from Prior Literature



through for the same policy interventions as we do, though our studies differ in several meaningful ways. First, Tsvetanov (2024) limits their treatment and control group to 108 cities along the eastern coast while ours is composed of cities from across the contiguous United States. Tsvetanov (2024) also has a shorter time horizon of data and is only able to include the first few weeks of New York’s policy. In total, their study considers February through June 2022 whereas we use daily data from January 15th 2021 through the end of 2022. The Tsvetanov (2024) paper makes use of a standard two-way fixed effects model which we do as well, though we also use the de Chaisemartin and D’Haultfœuille (2020) treatment effect methodology as it better handles heterogeneous policy implementation dates. Despite these differences, we still come to conclusions that confirm each other’s work. Tsvetanov (2024) finds an average pass-through rate of 79% which is within one standard deviation of our pass-through estimate of 73.1%.

To summarize, Figure 1 displays estimates of the gasoline tax pass-through rate from prior work along with the pass-through rate we estimate in this study (shown in red). This

estimate is the weighted average pass-through rate for policies passed in Connecticut, Georgia, Maryland, and New York. The excise tax rate for these states are \$0.25 in Connecticut, \$0.291 in Georgia, \$0.41 in Maryland, and \$0.353 in New York.⁴ The underlying GasBuddy data which we describe in detail next has information for four cities in Connecticut,⁵ four cities in Georgia,⁶ six cities in New York,⁷ and two cities in Maryland.⁸ The weighted average excise tax is \$0.3215 across all policy-treated cities that had their excise tax removed.

3 Data

Our goal is to examine whether or not gasoline retailers passed through the removal of gasoline excise taxes to consumers, and if so, whether or not this tax savings pass-through was complete or incomplete. Incomplete pass-through of any amount would result in an unintended targeted subsidy to gasoline retailers, and reduce the effectiveness of the program from its stated purpose. The more incomplete, the greater the unintended subsidy and less efficient the program. In our analysis, we rely on the fact that gasoline prices are subject to price swings of the underlying commodity, crude oil, which is affected by global events like the onset of the Ukraine-Russia war or even the announcement of supply cuts from major suppliers like the countries that are part of OPEC. These underlying exogenous cost shocks are common to all U.S. gasoline retailers and provide a source of identifying variation that allows us to assess pass-through in a natural experiment setting while controlling for exogenous variation that is a primary determinant of gasoline prices.

Our data comes from two sources, GasBuddy and the Energy Information Administration (EIA). Data gathered from GasBuddy requires some explanation. GasBuddy is a commercial

⁴In New York, the excise tax is actually a combination of two taxes, both of which were rescinded at the same time. These are the excise tax of \$0.173 and the “pre-paid” sales tax of \$0.18.

⁵Bridgeport, Hartford, New Haven, and Waterbury

⁶Atlanta, Augusta, Macon, and Savannah

⁷Albany, Buffalo, Long Island, New York City, Rochester, and Syracuse

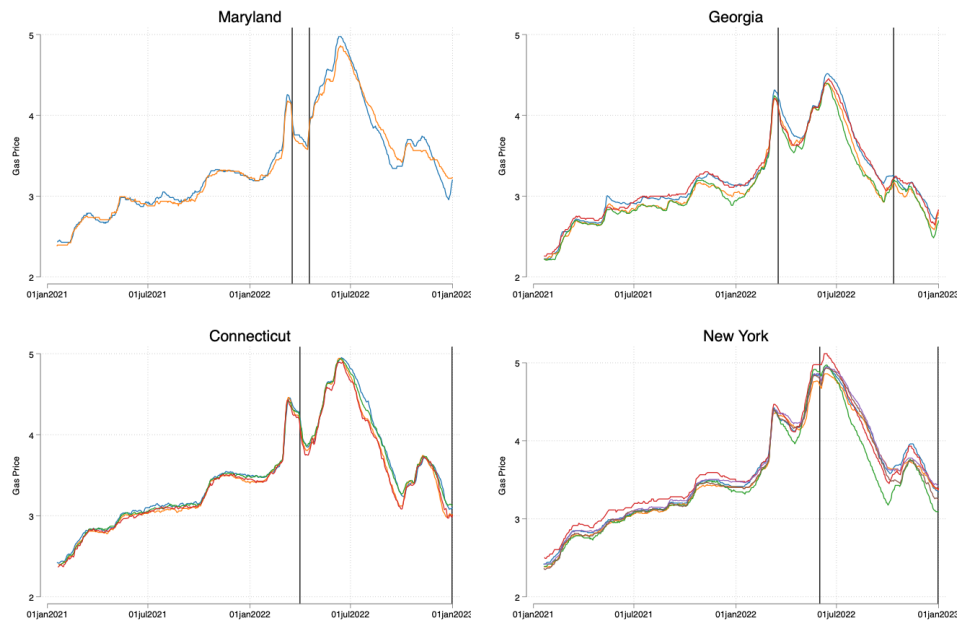
⁸Baltimore and Hagerstown

Figure 2: Price History in States Without Tax Relief



Notes: Figure shows time-series history of average retail gasoline prices and wholesale gasoline prices. Data from states that removed the excise tax are not included in calculating the average retail gasoline price.

Figure 3: Price History for States with Tax Relief



Notes: Figure shows time-series history for all cities with available data within states that passed a tax-relief policy. Each line represents a different city. The first vertical bar on each graph marks the start date of the tax relief and the second marks the end date.

product that allows visitors to its website or smartphone application to search for and view gas prices based on their location. Noel (2018) finds that consumers spend a significant amount of time searching for lower gas prices, and GasBuddy is a key source. To collect their data, GasBuddy incentivizes users to post prices observed at individual stations in their area. There is evidence that gas retailers also participate in the data collection effort so that their prices are up-to-date and accurately displayed to GasBuddy users.⁹ Importantly, the price posted by users of GasBuddy is inclusive of all state and local taxes, as shown on the prominent street-side signboards or on displays at the pump. Some price reports even include a picture of the price from the signboard or pump display. One concern about the underlying data could be that selection effects are driving an artificially low price history. However, we have a few reasons to note that this is not actually impacting the data. First, there is evidence that stations will post their own prices so that they are not “stale” and consumers are not seeing an old price. This adds to the overall stock of stations that the city-based average is constructed. Second, some users will post high price stations in an effort to help others avoid those stations.¹⁰ Finally, we look at counts of establishments by NAICS code and can confirm that there are hundreds of stations for each of the treatment group cities. So, any remaining concern that there is positive or negative selection into being included in the underlying data begins to deteriorate given the large sample size.

GasBuddy aggregates and displays average gas prices by city in historical price charts using the data submitted on individual stations. We collected these price charts for all cities in North America for which GasBuddy displays historical price averages, for the period from January 2021 to January 2023. The data collection process involved saving individual image files from the website and extracting the underlying price data using Web Plot Digitizer.¹¹

⁹Stale gasoline prices fall out of the search results after 24 or 48 hours, depending on user settings.

¹⁰Indeed, this is also the rationale behind stations posting their own price.

¹¹This program requires you to specify fixed points along each axis to set the scale. From there, data is extracted using the color of the data time series in hexadecimal according to a specified pixel resolution. We use a tenth of a pixel resolution in extracting the data.

Longer time horizons (beyond two years) are available on the GasBuddy website, however, we found that as the horizontal axis becomes increasingly compressed in the chart, the accuracy of the extracted prices begins to suffer. We found that the prices extracted from charts with an 18 month or shorter time interval were nearly identical to those extracted from charts with a 2 year time interval. Beyond the 2 year interval charts, we begin to see a divergence between the extracted price and prices more accurately collected on the shorter time interval charts.¹² From these data we have a snapshot in time of how consumer prices changed following the removal of excise gas taxes by state governments in our four treatment states, and how prices were simultaneously changing elsewhere.

Figure 1 plots retail prices and the wholesale price of gasoline for all states that kept their excise tax system in place. Here, we see that gasoline prices generally closely follow wholesale price movements. Figure 2 plots average prices by city in each of the four states that removed their excise taxes with policy start and end dates marked with vertical bars.¹³ The GasBuddy historical price charts have data for four cities in Connecticut,¹⁴ four cities in Georgia,¹⁵ six cities in New York,¹⁶ and two cities in Maryland.¹⁷ We show the beginning and end dates for each state with vertical bars. In Maryland, it is rather easy to see the effect of the policy visually, in part because of the short tenure of the policy. The policy effect is not as plainly apparent in Georgia, though the peak price here appears to be lower than the peak seen in Maryland which followed the run up in wholesale prices which occurred after their excise tax went back into effect. Policy effects in Connecticut and New York are yet

¹²Our web-scraping process is not new in the economics literature and has recently had its legality reaffirmed. In April 2022 the U.S. Ninth Circuit of Appeals ruled (again) in a long-running legal battle initiated by LinkedIn that intended to stop a rival firm from web scraping personal information from users' public profiles. The case was eventually sent to the U.S. Supreme Court and then sent back to the Ninth Circuit to re-review the case. In its second ruling, the Ninth Circuit reaffirmed its original decision and found that scraping data that is publicly accessible on the internet is not a violation under U.S. law. Cite: Tech Crunch April 18, 2022.

¹³The average price in our sample period is \$3.82, with a standard deviation of \$1.10.

¹⁴Bridgeport, Hartford, New Haven, and Waterbury

¹⁵Atlanta, Augusta, Macon, and Savannah

¹⁶Albany, Buffalo, Long Island, New York City, Rochester, and Syracuse

¹⁷Baltimore and Hagerstown

harder to see through time-series figures alone.

Crude price changes and subsequent wholesale price changes are largely responsible for the large peaks and troughs seen in Figures 1 and 2, so we also collect daily wholesale gasoline prices from the Energy Information Administration (EIA) to control for wholesale price changes. We use the Los Angeles Reformulated Blendstock for Oxygenate Blending (RBOB) spot gasoline price as our proxy for wholesale prices for all cities, noting that our results are robust to using the Gulf Coast Free On Board gasoline price or New York Port Free On Board gasoline price instead.¹⁸ All three price series are highly correlated¹⁹ and almost universally within a few cents of each other. We use the RBOB price because this wholesale price is closest to the final product of reformulated gasoline, with oxygenates already blended in. Other proxies for wholesale prices, such as rack prices or dealer-tankwagon prices, are built up from spot prices either directly (via formula pricing) or indirectly, and tend to be highly correlated to both spot prices and to each other. Since we are primarily interested in this study in comparing changes in wholesale and retail prices over time, and not in comparing wholesale or retail price levels across cities, any differential costs in transporting gasoline into individual cities is harmlessly differenced out of our analysis.

4 Methodology

4.1 Difference-in-differences

We model the natural experiment of tax-relief pass-through in two ways, first by using the standard difference-in-differences (DD) methodology that is so commonly used. Cities in Connecticut, Georgia, Maryland, and New York serve as the treatment cities and all other cities (outside of Florida) serve as controls. The treatment period varies by state - Maryland had the shortest policy lasting from March 18th-April 18th 2022; Georgia began their policy

¹⁸Our inclusion of city fixed effects accounts for transportation price differences.

¹⁹Correlation coefficient = 0.97

on the same day but ultimately kept their policy in place through October 12th; Connecticut and New York enacted their policies later (April 1st and June 1st, respectively) but kept their policies in place through the end of our sample (December 31, 2022). The model is a two way fixed effects (TWFE) representation, and in its most basic form is estimated using the equation:

$$\begin{aligned}
 Price_{it} = & \beta_0 + \sum_{n=1}^{186} \left[\beta_n Tax_{it} * Relief_i * \mathbf{1}(i = n) \right] \\
 & + \sum_{d=0}^{14} \pi_{0+d} RBOB_{i,t-d} + \iota_i + \tau \iota_i + \mu_t + \epsilon_{it}
 \end{aligned} \tag{3}$$

where $Price_{it}$ is the average price across all stations in city i on day t . The variable $Relief_i$ is a dichotomous indicator variable equal to one for the four states in our final sample that temporarily rescinded their excise taxes, and Tax_{it} is a dichotomous indicator variable equal to one during just the periods t in which the excise tax was rescinded in a given state i . In total, there are 186 cities in the final sample - 16 treated cities and 170 non-treated cities.²⁰ The interaction term $Tax_{it} * Relief_i$ is our variable of interest and captures the treatment effect of the tax excise removal. Restricting $\beta_1 = \dots = \beta_{186} = \beta$, the estimated interaction coefficient β is equal to the average price decline seen in states that passed a excise tax relief policy relative to those that did not pass such a policy. We also estimate the model on a treatment-state by treatment-state basis, by comparing each treatment state separately to the same set of pure control states (while excluding the other treatment states).

We include a large set of controls in the model. In place of a single main effect for $Relief_i$ as is commonly done, we use a fully flexible and complete set of city indicator variables to control for price level differences across cities. We also include city-specific time trends ($\tau \iota_i$) or, in its place and with very similar results, a complete set of state-month indicator

²⁰There are eight cities in Florida which are dropped.

functions to control for trends and state heterogeneity over time. Even though the treatment periods are relatively long, we nonetheless include a complete set of day-of-the-week variables to protect against possible deterministic patterns over the course the week due to driving patterns or the possible presence of Edgeworth price cycles.²¹ Importantly, we control for wholesale prices using contemporaneous and lagged values of the Los Angeles RBOB spot gasoline price. It is well known that retail gasoline prices respond to wholesale rack and spot prices with a lag, and lagged values of upstream prices capture this effect (Borenstein et al. (1997), Lewis and Noel (2011)). Based on a testing down approach and then rounding d up to an even number of weeks, we use fourteen lags. Our results are not sensitive to other reasonable choices of d .²² The ϵ_{it} is assumed to be an i.i.d. error term, and all results are reported with robust standard errors clustered at the city level.

We also estimate an asymmetric version of the basic model, taking into account that cost increases and decreases may be passed through to downstream prices asymmetrically, rather than symmetrically as it is assumed above. There is a large literature on the asymmetric pass-through of upstream gasoline prices into downstream gasoline prices and it is well known that wholesale cost increases for gasoline tend to be passed through to retail prices more quickly than cost decreases are, for many cities around the world (Borenstein et al. (1997); Verlinda (2008); Noel (2009); Tappata (2009); Radchenko and Shapiro (2011); Marion and Muehlegger (2011); Noel and Roach (2016)). This phenomena is widely known as “rockets and feathers.” While we are not interested here in “rockets and feathers” in its own right, it is potentially important to control for its effect via asymmetric pass-through, especially given that all excise tax removals in the sample are, by definition, cost decreases. Thus, if typical, non-tax-related cost decreases are indeed passed through to retail prices more slowly than

²¹Edgeworth price cycles are cyclical pricing phenomena present in many cities in the U.S. and around the world (Noel (2007), Noel (2018)), and often follow a deterministic weekly or daily pattern (Noel (2019)). There is no evidence of cycles present in any of our treatment cities.

²²All results are statistically indistinguishable when reducing the lag length to a single week, or extending the lag length to a full month.

cost increases, then any delayed pass-through of a typical cost decrease may leak into our estimates of excise tax pass-through rates. Embedding a model of asymmetric pass-through within our framework accounts for this. We estimate the modified equation:

$$Price_{it} = \delta_0 + \sum_{i=1}^I \delta_{1+i}^+ \Delta RBOB_{t-i}^+ + \sum_{i=1}^I \delta_{1+i}^- \Delta RBOB_{t-i}^- + \Gamma x_{it} + v_{it} \quad (4)$$

with

$$\Delta RBOB^+ = \max(0, \Delta RBOB) \quad (5)$$

$$\Delta RBOB^- = \min(0, \Delta RBOB) \quad (6)$$

where Γx_{it} represents all the variables included in Equation 1, including our variables of interest, but excludes $RBOB$. The $RBOB$ variable is now broken into two separate variables, $RBOB$ price increases $RBOB^+$ and $RBOB$ price decreases $RBOB^-$ only, and the coefficients on each are now allowed to differ. We again take $n = 14$, though our results are not sensitive to longer or reasonably shorter time horizons. The v_{it} is assumed to be an i.i.d. error term, and all results are again reported with robust standard errors clustered at the city level.

As written, the estimated coefficient β produced by these models are in dollar-per-gallon units, and represent the average pass-through, in dollars, of the excise tax decreases. These coefficients are particularly interesting in our treatment-state by treatment-state regressions and serve as direct inputs into our policy cost calculations. However, pass-through rates are often easier to compare when expressed as percentage rates (i.e. as percentages of complete pass-through), so we present our results this way as well. To generate pass-through rates, we simply recode the interaction term as a continuous variable instead of a dichotomous one, assigning to it the value of the excise tax relief provided in each state, in dollars per gallon, and then re-estimate. For example, for cities in Maryland between March 18th and April 18th, the interaction term is assigned a value of \$-0.43, since Maryland eliminated its

forty-three cent excise tax during that period. We say that pass-through is complete if prices fall by the amount of the excise tax shortly after the tax is rescinded ($\hat{\beta} = 1$) and incomplete if prices fall by less the amount of the excise tax. We would not expect to see a pass-through rate higher than one without extenuating circumstances.

4.2 Heterogeneity-Robust Difference-in-differences

The recent renaissance of research and advances in DD treatment effect methodology are of particular import to our setting because there was staggered introduction of states removing excise gas taxes which could lead to an issue of negative weighting. Moreover, each state's excise tax rate is different which means an improperly weighted average treatment effect of a larger or smaller excise tax rate change could meaningfully bias results.

The canonical DD setup compares a treatment group to a control group, before and after the treatment timing. However, in our context the treatment gets rolled out at different times across different groups rather than having a single treatment period. This is referred to as a staggered treatment timing setup. de Chaisemartin and D'Haultfoeuille (2020) show that with heterogeneous treatment effects over time, the two-way fixed effects estimator ends up being a weighted average of all possible 2x2 DD estimators comparing early treated units to late treated units. Surprisingly, some of these weights can be negative, leading to counterintuitive results.

The CH methodology proposes estimators that are unbiased and more robust to negative weighting induced by treatment effect heterogeneity over time. The imputation estimator imputes the missing potential outcomes and implements a DD estimator comparing each group to an imputed control group. This stacked regression estimator allows for very flexible treatment effect dynamics by interacting the treatment indicator with exposure time dummies. This avoids the negative weighting issue of two-way fixed effects and provide more credible estimates under certain assumptions about the treatment effects over time. In our

results, we present the treatment effect averaged over the tenure of each tax removal regime as well as by state. We compare these directly with the traditional TWFE estimator. Additionally, we present estimates for event-study models which show the dynamic pathway of excise tax removal. These estimates show that prices were stable relative to the control group prior to each state’s decision to remove their excise tax, and further that each policy took about seven days to level out at the full pass-through amount. These event study results also inform our decision to remove observations from Florida from all analyses. In short, the parallel trends assumption is clearly violated causing treatment effect estimates to be biased. This could be due to the late passage of the policy or the bad timing of the policy which came into effect after prices returned to pre-war levels. We also exclude Florida from the control group prior to the policy passing so that we do not have tainted controls potentially from an intention to treat effect. The time-series of gas prices in Florida as well as event-study estimates are shown in Appendix Figures A1 and A2.

5 Results

5.1 Standard Difference-in-differences Methodology

We begin with the standard difference-in-differences DD model, and TWFE representation, and report results in Tables 1 and 2. Table 1 uses a dichotomous $Tax_{it} * Relief_i$ interaction term, so that its coefficient represents the average change in retail prices, in dollars per gallon, resulting from excise tax relief. Table 2 uses a continuous $Tax_{it} * Relief_i$ interaction term, which takes on values of the respective excise tax reductions in each state, so that its coefficient represents an average excise tax pass-through rate. For clarity, we label these interaction coefficients as ATE (Average Treatment Effect) in the table. In the first three columns of each table, we pool all of the data together and calculate an Overall ATE, i.e. an average ATE across all four treatment states. In the last three columns of each table,

we perform treatment-state-specific regressions with each state-policy indicator separately included to calculate a state-specific ATE for each of the treatment states relative to the policy-free control group.²³ Within each of these column triples, we add additional controls as we move from left to right. Our preferred specifications are the last specifications within each triple (i.e. Specifications (3) and (6) in each table), which include all full set of controls and, importantly, allow for asymmetric cost pass-through. While these models and interpretations are standard, these regressions do not account for negative weighting. Those models are presented next.

We begin with Table 1. Specification (1) of Table 1 estimates the Overall ATE, in dollars per gallon, using only the contemporaneous RBOB variable (restricting $n = 1$) and city fixed effects as controls. Specification (2) adds city-specific time trends and day-of-the-week controls and now allows RBOB prices to be passed through to retail prices with a lag.²⁴ Specification (3) allows RBOB prices to be passed through not only with a lag but potentially asymmetrically as well. In our preferred Specification (3), we estimate an excise tax pass-through of 23.8 cents per gallon on average, statistically significant at better than the 1% level. This estimate represents the average per-dollar savings of consumers across the four treatment states. Specifications (4) through (6) repeat the analysis on a treatment-state by treatment-state basis. In our preferred Specification (6), we estimate that Maryland consumers saved 43.5 cents per gallon on average due to the excise tax removal, Georgia consumers saved 19.6 cents per gallon on average, Connecticut consumers saved 34.8 cents per gallon on average, and New York consumers saved 19.2 cents per gallon on average. All estimates are statistically significant at better than the 1% level. With gas prices averaging around \$4 over the treatment periods, these reductions would seem to be meaningful.

What these estimates do not show, however, is whether consumers received the full benefit

²³Results are nearly identical when we use one treatment state at a time against the control group of states that have not passed a policy.

²⁴We note that the inclusion of a trend variable is what moves point estimates the most. Removing any of the other toggled control variables only adjusts estimates marginally.

of the excise tax removal as was intended with these policies. To see this, Table 2 repeats the analysis of Table 1 but is expressed in terms of excise tax pass-through rates instead of absolute retail price reductions. Note, that asterisks in this table show the test of whether or not pass-through was complete ($\hat{\beta} = 1$) and not the test of whether or not pass-through was statistically different from zero.

The results show that excise tax pass-through, overall, was incomplete. In our preferred overall Specification (3) of Table 2, we estimate that the average excise tax pass-through rate was 74.5%. In other words, only about three quarters of the all the money intended to go to consumers actually went to consumers, while the remaining 25% was retained by gasoline wholesalers and retailers in the form of higher gasoline margins. This represents a substantial and unintended subsidy to gasoline companies that reduced the efficiency of the excise tax relief programs from its stated goals. While there were many programs targeted to assist struggling firms during the pandemic in 2020 and 2021 in particular, the excise tax removal policies of 2022 was not among them. Post-pandemic travel demand was booming in 2022, having already returned to pre-pandemic levels, and was itself a contributor to inflationary pressures and the higher cost of gasoline.²⁵

Specifications (3) through (6) in Table 2 present excise tax pass-through rates on a treatment-state by treatment-state basis. Moving straight to our preferred state-level Specification (6), which includes the full set of controls and allows for lagged and asymmetric cost pass-through, we find significant heterogeneity in the coefficients across states. We estimate a pass-through rate in Maryland of 100.9%, indistinguishable from 100% even with tight standard errors, implying that excise tax pass-through in the state of Maryland was complete. In contrast, we estimate a pass-through rate in Georgia of only 67.5%, statistically significantly different from 100% at better than the 1% level, and implying that only two-thirds of the money intended for Georgia consumers actually went to Georgia consumers. Even lower, we

²⁵The State of the Travel Industry (U.S. Travel, April 2023)

Table 1: Absolute Price Pass-through (TWFE)

	(1)	(2)	(3)	(4)	(5)	(6)
ATE	-0.0234 (0.042)	-0.203*** (0.022)	-0.238*** (0.021)			
Maryland				-0.319*** (0.021)	-0.446*** (0.022)	-0.435*** (0.022)
Georgia				-0.232*** (0.023)	-0.143*** (0.018)	-0.196*** (0.018)
Connecticut				-0.0264 (0.019)	-0.289*** (0.015)	-0.348*** (0.015)
New York				0.134*** (0.029)	-0.184*** (0.023)	-0.192*** (0.023)
RBOB	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
DOW FE	No	Yes	Yes	No	Yes	Yes
City Trend	No	Yes	Yes	No	Yes	Yes
RBOB lags	No	Yes	Yes	No	Yes	Yes
Asym. Costs	No	No	Yes	No	No	Yes
Obs	132456	129852	129666	132456	129852	129666
R ²	0.898	0.949	0.958	0.898	0.949	0.958

Note: Asterisks denote statistical test of the hypothesis that $\beta = 0$ at the traditional significance levels. Robust standard errors clustered by city.

estimate a pass-through rate in New York of only 54.4%, significantly different from 100% at better than the 1% level, and implying that barely half of the money intended for New York consumers went to New York consumers. In these latter two states, our estimates would imply significant efficiency loss.

At the opposite end of the spectrum, we estimate a pass-through rate in our fourth treatment state of Connecticut of 139%, actually overshooting 100% complete excise tax pass-through. This result would seem troubling, because economically we would expect pass-through rates to be bounded between zero and one hundred percent except in unusual circumstances.²⁶ The estimate implies that for every one cent reduction in the excise tax, gasoline prices fell by 1.39 cents. The coefficient varies some depending on the specification, but the overshooting result is fairly robust conditional on maintaining the standard difference-in-differences framework. This troubling result, along with the surprisingly wide range of pass-through estimates we found, suggests a potential issue with the difference-in-differences methodology in our setting, given that the treatment effects and the treatment periods are both heterogeneous. The mechanism by which the TWFE specification can produce such a result is that Connecticut (a state with a relatively small tax cut, treated later) is being compared against a backdrop that includes early-treated states (like Maryland, with a large tax cut) which could lead the model to misattribute price movements and generate a coefficient greater than one. We now pay specific attention to this possible issue.

5.2 The CH Methodology

Goodman-Bacon (2021) and many others have highlighted that differential policy timing and heterogeneous treatment intensity can bias estimated average treatment effects in standard DD-TWFE models because the reported coefficient is a weighted average of all possible

²⁶One possibility would be if the rapidly declining prices triggered a price war among retailers in Connecticut, though we have no evidence or expectation of such. Noel (2009) provides another interesting example.

Table 2: Percent Pass-through (TWFE)

	(1)	(2)	(3)	(4)	(5)	(6)
ATE	0.044*** (0.14)	0.644*** (0.067)	0.745*** (0.069)			
Maryland				0.740*** (0.048)	1.034 (0.050)	1.009 (0.051)
Georgia				0.798*** (0.081)	0.490*** (0.062)	0.675*** (0.061)
Connecticut				0.106*** (0.074)	1.155*** (0.061)	1.390*** (0.061)
New York				-0.379*** (0.082)	0.521*** (0.065)	0.544*** (0.065)
RBOB	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
DOW FE	No	Yes	Yes	No	Yes	Yes
City Trend	No	Yes	Yes	No	Yes	Yes
RBOB lags	No	Yes	Yes	No	Yes	Yes
Asym. Costs	No	No	Yes	No	No	Yes
Obs	132456	129852	129666	132456	129852	129666
R ²	0.897	0.949	0.958	0.898	0.949	0.958

Note: Asterisks denote statistical test of the hypothesis that $\beta = 1$. Failure to reject indicates complete pass-through, rejection of the test indicates incomplete pass-through. Robust standard errors clustered by city.

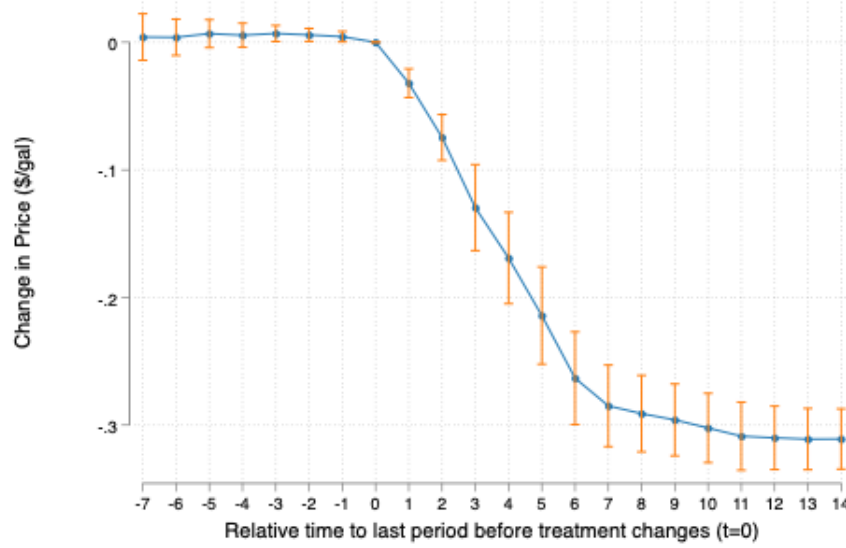
treated-untreated combinations. On the former issue, negative weighting can occur when there are observations from ‘already-treated’ along with those that are ‘not-yet-treated.’ Essentially, there is a tainted control group. On the latter issue, we *should* expect a treatment effect to have more bite when it involves a larger absolute change (e.g. removing an excise tax of \$0.40 cents compared to one that is only \$0.20 cents), but this can be accounted for by comparing to the weighted average of all excise tax changes. Where this stands to confound the estimated coefficient for the average treatment effect, though, is when the removal of a large excise tax has unequal weight over time in the calculation of the weighted average treatment effect.

In our setting, the treatment effect is heterogeneous both in intensity and in time. The intensity of the treatment varies by state simply because each state’s excise tax rate is different, so that the removal of those taxes represented a shock of different magnitudes. The timing of the treatment varies by state simply because each state issued and then rescinded their excise tax policy at different times. Indeed, our setting is a prime example of heterogeneity on both fronts and represents an excellent laboratory for comparing results from a standard DD methodology that does not explicitly take into account heterogeneous effects with the more modern CH Methodology that does. In this section, we examine excise tax pass-through using the de Chaisemartin and D’Haultfoeulle (2020) and de Chaisemartin and D’Haultfoeulle (2022) estimator that calculates dynamic and heterogeneous treatment effects, without concerns over negative weighting.²⁷

We begin by exploring pass-through using the de Chaisemartin and D’Haultfoeulle (2022) event-study design. This allows us to assess issues of pre-trends or anticipation, but also the amount of time it takes for pass-through to occur. Figure 3 displays event study estimates for the average treatment effect across all states that rescinded their excise tax. All

²⁷There are a number of other new difference-in-differences methodologies that we also considered (Wooldridge (2021); Dube et al. (2023), among others) which return statistically similar estimates as the CH methodology used here. We opt to use this methodology because it seamlessly estimates event-study style models as well as calculated an average treatment effect.

Figure 4: Event Study - All States



estimates for the days before the policy start date are near zero and not statistically significant. Following the start of each state’s policy (shown in the figure as ‘day 1’), we see that it takes approximately 7 days for estimates to level out, but after that estimates remain mostly stable.²⁸ Our state-by-state figures are shown in Figure 4 where we again see that the pre-policy estimates are near zero and not statistically significant. Further, we see that any effects due to the policy seem to level-out and stabilize after about a week. After that, daily treatment effect estimates are not statistically different from one another. In the appendix, we show event study estimates for a longer window (28 days) where we see that days 14-28 are not meaningfully different than the day 14 value.

We report our results from the CH Methodology in the top panel of Table 3.²⁹ Within that panel, the top row shows estimated excise tax pass-through in dollars per gallon, and the

²⁸Note, that period 1 refers to the first day of excise tax removal, but that each state’s ‘day 1’ differs. This is similar to the ‘stacked’ design others have used.

²⁹We control for city fixed effects and as before, but this model only includes the contemporaneous value of RBOB to aid in computing demands. Models which include further lags are nearly identical to those that have only the contemporaneous RBOB price as a control. This is expected because the CH methodology notes that, “Estimators with controls are similar to those without controls, except that the first-difference of the outcome is replaced by residuals from regressions of the first-difference of the outcome on the first-differences of the controls and time fixed effects.”

Table 3: Dynamic Treatment Effects

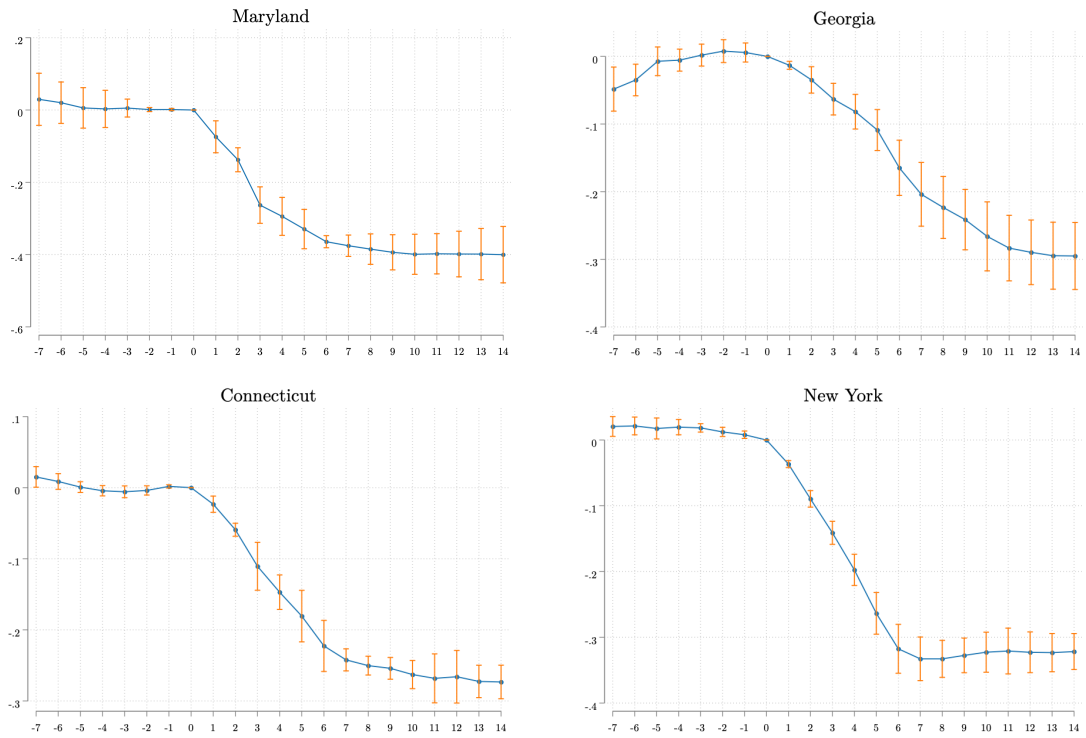
	ATE	Maryland	Georgia	Connecticut	New York
Chaisemartin. & D'Hault. (2020)	-0.235*** (0.025)	-0.329*** (0.025)	-0.183*** (0.018)	-0.205*** (0.010)	-0.259*** (0.012)
Implied Pass-through rate:	73.1%	76.3%	62.8%	82.0%	73.3%
TWFE from Table 2	-0.238*** (0.021)	-0.435*** (0.022)	-0.196*** (0.018)	-0.348*** (0.015)	-0.192*** (0.023)
Implied Pass-through rate:	74.5%	100.9%	67.5%	139.0%	54.4%

bottom row shows the implied excise tax pass-through rates. Specification (1) uses all data and estimates an Overall ATE, while Specifications (2) through (5) are our treatment-state-specific regressions, as before. For reference, we re-display our earlier results from Tables 1 and 2 using the standard DD methodology in the bottom panel of Table 3.

Using the CH methodology, we estimate an overall excise tax pass-through of 23.5 cents per gallon on average, corresponding to a 73.1% pass-through rate. This overall average is very similar to the previous DD estimate of 74.5%, and again shows incomplete pass-through on the aggregate. We reject complete pass-through at better than the 1% level. We again conclude only about three-quarters of the money intended to go to consumers actually went to consumers, with the remaining one-quarter an unintended subsidy to gasoline retailers.

The interesting differences in the results of the DD and CH methodologies comes with the treatment-state-specific regressions. Using the CH methodology, we estimate that Maryland consumers saved 32.9 cents per gallon on average due to the excise tax removal, implying an excise tax pass-through in the state of Maryland of 76.3%. This is statistically significantly less than the 100.9% pass-through rate estimated when using the DD methodology. It implies that pass-through in Maryland was indeed incomplete, but much closer to the overall average, with only about three-quarters of the money intended for Maryland consumers actually going to Maryland consumers. We estimate that Georgia consumers saved 18.3 cents per gallon

Figure 5: Event Study Estimates - State by State



Notes: Figure shows estimated coefficients from event-study analysis. Period is set such that $t = 0$ refers to the day before the excise tax was rescinded in each state. Vertical axis shows change in price per gallon.

on average due to the excise tax removal, implying an excise tax pass-through in Georgia of 62.8%. A little under two-thirds of the money intended for Georgia consumers actually went to Georgia consumers, though we note that this is not statistically different than the overall pass-through rate nor for any of the other state-specific pass-through rates.³⁰ This is similar to our DD estimate from before and again shows incomplete pass-through in Georgia.³¹

We estimate that New York consumers saved 25.9 cents per gallon on average due to the excise tax removal, for an excise tax pass-through of 73.3%, significantly higher than our previous DD estimate of 54.4%. The estimate is now much closer to the average. It continues to show that pass-through was incomplete in New York, however, with less than three-quarters of the money intended for New York consumers actually going to New York consumers. Finally, our estimate for Connecticut is now much more intuitive than it was before. We estimate that Connecticut consumers saved 20.5 cents per gallon on average, implying an excise tax pass-through in Connecticut of 82.0%. This is substantially lower than the previously implausible estimate of 139.0%, and shows that excise tax pass-through in the state of Connecticut was indeed incomplete, statistically significantly so at better than the 1% level. More generally, the estimates across all four states are now closer to each other and closer to the overall average, as we had anticipated *ex ante*. They range from a low of 62.8% in Georgia to a high of 82.0% in Connecticut, with Maryland (76.3%) and New York (73.3%) in the middle and very close to the overall average. While we expected to see some heterogeneity across states, the eighty-four percentage point range produced by the uncorrected DD estimates was suspect and the range of the corrected CH estimates are much more intuitive.

The regional heterogeneity in the point estimate for pass-through rates we see here could

³⁰The 95% confidence interval overlaps with all other implied pass-through rates.

³¹We note that a few of the coefficients before the policy was implemented are statistically significant, though when we test these as a group they cannot be distinguished from zero. Moreover, when we expand to a longer pre-intervention time horizon we find that nearly all pre-intervention coefficients are not statistically significant.

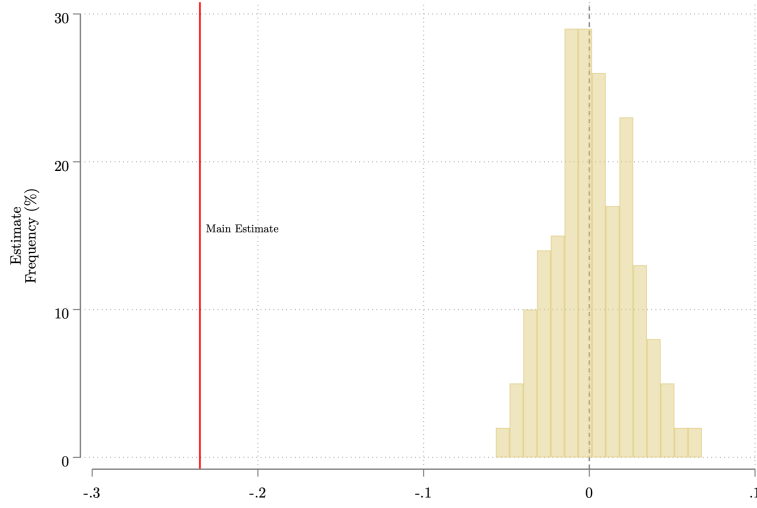
be due to a number of factors, though we note that we cannot statistically distinguish any one state from another. For example, using the lowest and highest pass-through rates, the 95% confidence interval for Georgia’s pass-through rate is 50.7–75.01% compared to Connecticut which includes 74.16–89.84%. Still, when trying to determine reasons why we might see such heterogeneity we arrive at differing mechanisms. On the one hand, there are more gas stations per registered driver in Georgia than in Connecticut which would lead one to believe that firms would be more competitive in Georgia and hence would be more likely to pass-through cost savings – the opposite of what we see in our estimates. On the other hand, Connecticut is part of a major regional population center and may face more external competition, even from stations in nearby states.³² Indeed, Tsvetanov (2024) who estimates a similar pass-through rate as we do here describes how the proximity of New York to Connecticut leads to slightly differing pass-through rate for cities in the interior of New York possibly due to competitive effects, and further that rural areas have a slightly lower pass-through rate as well.

Overall, we conclude that excise tax pass-through was incomplete in each of our treatment states, with only about three-quarters of the tax relief going to consumers. The remaining one-quarter was captured by gasoline wholesalers and retailers, representing a significantly large and unintended subsidy to those wholesalers and retailers. Given that the full value of the excise tax is \$0.431, \$0.291, \$0.353, and \$0.25 in Maryland, Georgia, New York, and Connecticut respectively,³³ we calculate that combined gasoline wholesaler and retailer margins increased approximately \$0.10 per gallon in Maryland, Georgia, and New York, and approximately \$0.05 per gallon in Connecticut, as a direct result of the excise tax holiday. This was an unintended yet well-targeted subsidy to gasoline wholesalers and retailers, in the midst of a post-pandemic travel boom when demand was at pre-pandemic levels, and

³²From Hartford, CT, Providence, RI is only 70 miles away and Boston, MA is 100 miles away. New Haven, CT is similarly close to New York

³³The weighted average of these excise taxes across the 16 cities that saw a policy change is \$0.3215

Figure 6: Distribution of Estimates with Random Treatment Assignment



Notes: Figure shows histogram of all estimated coefficients from placebo policy exercises. Horizontal axis shows the estimated β coefficient, vertical axis shows the binned estimate frequency.

not in the spirit of the policy. This transfer of tax revenues to these businesses represents a meaningful change in profitability, while other sectors of the economy did not receive similar benefits.

5.3 Placebo Policy Experiments

Here, we put our main results through further scrutiny by running two randomized placebo exercises. The treatment effect estimate is defined as the change in price for policy-affected cities relative to the control group of cities for which there was no change in the excise tax rate. The crucial assumption here is that, but for the change in tax rate, we should expect that prices would resemble the control group. Or put differently, there is no larger phenomenon changing prices for both treatment and control groups, nor are there coincident policies that are actually the drivers of change.

One way to check the validity of the DD estimate is to estimate a placebo treatment effect for cities that did not have a change in policy. In essence, we assign treatment status

to a set of cities within our control group and estimate a treatment effect coefficient. If the placebo test is passed, we should expect a DD estimate that is close to zero and not statistically significant. In essence, we shouldn't detect a policy effect because there *isn't* one. Failure of this placebo test - a meaningful effect due to a policy that does not, in fact, exist - would indicate that some other unobserved factor is driving the model to produce a biased result.

For this randomized placebo exercise, we re-estimate the CH specification from before and make use of only the cities that are in the control group. We randomly assign treatment status and assume policy treatment dates that match the varying policy tenures that occurred among states that removed their excise tax.³⁴ So, some control group cities remain control group cities while a randomly selected set of others are now treated as if their policy changed. For example, in one run of the randomization exercise Oklahoma City could be marked as removing their excise tax from March 18th to April 18th even though their tax policy never actually changed.

Using the newly (randomly) assigned treatment status, we estimate the CH specification shown in table 3 and store the treatment coefficient. We repeat this randomized treatment marking and estimation exercise 200 times and display the distribution of point estimates in Figure 6 along with a line marking the point estimate from before. It is plain to see that our estimated coefficient for the effect of the excise tax removal is not a result of chance or model-driven bias. All estimates from the randomized treatment exercise are small in absolute value and clustered around zero. The mean effect size among the randomized placebo treatment estimates is 0.0015 cents, more than 150 orders of magnitude smaller than the non-randomized estimate.³⁵

Next, we test whether some observations are overly influential in determining our main

³⁴We use the same proportion as before with approximately two random cities given Maryland's policy tenure, four given Georgia's, four given Connecticut's, and six given New York's.

³⁵We also compute an average z-statistic across all 200 iterations of 0.063

result. With 194 cities it is not feasible to manually check the influence of all possible combinations of units of observation, so we rely on a method proposed in Broderick et al. (2021). Broderick et al. (2021) show that the sensitivity of treatment effect estimates are due to the signal-to-noise ratio and that many results from papers they surveyed are not robust to dropping even 1% of the observations. For this test, we return to the full data-set and randomly assign an identification number to each city and drop approximately 10% of the sample.³⁶ We run 200 iterations of the random dropping protocol and estimate the CH specification for each sample. Figure 7 plots a histogram of the estimated effect size with randomly dropped subsamples. The figure clearly shows that our estimates are not sensitive to removing observations. We find that the mean effect size across iterations is -0.234 (73% pass-through rate), and that all of the 200 treatment effect coefficient estimates are within 2 cents of the estimate from our preferred specification. Moreover, all of the 200 iterations are statistically significant at at least the 5% level or more.³⁷ We conclude from these randomization tests that our result is not due to model-induced bias, nor is it sensitive to removing cities.

5.4 Quantity Effects and Welfare Calculations

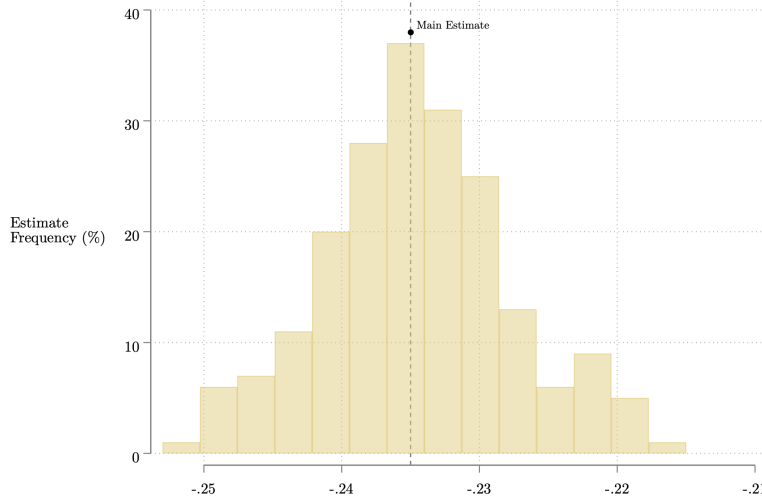
A natural follow-up to any discussion of a price decrease is to assess how the quantity of gasoline demanded has changed in the treatment states. Unfortunately, on this front the data quality are less than ideal and we are only able to estimate a model more akin to a back-of-the-envelope type analysis than a precise causal treatment effect estimate. This is because gasoline sales volumes are only available at a state-by-month resolution.³⁸ Thus, we present these estimates because they are instructive and offer some insights to behavior change (or lack thereof) but argue caution in interpreting any treatment effect as a precise

³⁶The random dropping procedure randomly assigns an identification number to both treated and control units, so observations from both groups may be dropped in a run of the randomization exercise.

³⁷We find a mean z-statistic of -16.3.

³⁸These data come from tax receipts from the Federal Highway Administration.

Figure 7: Distribution of Estimates with Randomly Dropped Subsample



Notes: Figure shows histogram of all estimated coefficients from randomly dropped subsample exercises. Horizontal axis shows the estimated β coefficient, vertical axis shows the frequency of the estimate.

estimate of the change in gasoline consumed due to the excise tax price reduction that was only partially passed-through to consumers.

We use the same model setup as before and define treatment states as those that removed their excise tax.³⁹ Because the data are at a monthly resolution, we show two different interpretations regarding how the indicator variable for the policy being in place is defined. Remaining states serve as our control group.⁴⁰ The dependent variable in this model is the natural logarithm of gasoline sold for each state in each month so that we may interpret the results as an elasticity. We use the TWFE interpretation for specifications (1) and (2) using both treatment indicator variants, and use the CH methodology for specifications (3) and (4) under both variants as well.

Because the data are monthly, we must define the treatment start date with assumptions attached. First, we assign treatment status as the first full month that the excise tax removal

³⁹All models include state and time period fixed effects.

⁴⁰We exclude Florida as before, as well as remove Hawaii, and Alaska.

Table 4: Quantity Effects

	(1)	(2)	(3)	(4)
	TWFE	TWFE	CH	CH
ATE	-0.0892 (0.068)	0.0047 (0.058)	0.0466 (0.069)	-0.0345 (0.107)
Partial Treatment Month Included	N	Y	N	Y

Notes: Dependent variable is the natural logarithm of the total volume of gasoline sold in a state-month. All model variants include state and time period fixed effects.

is in effect in a state. For two of the treatment states, the tax began mid-month meaning for half of the month consumers were exposed to lower prices in only the latter half of that month. For these states, we drop this month from the sample completely. Unfortunately, this means that Maryland must be dropped from the sample in columns (1) and (3) because their policy was in force from March 18th through April 18th. For Georgia, we include April through September as the treatment months because the start and end dates for their policy both occurred in the middle of a month (March 18th - October 12th). Connecticut and New York are assumed to have their treatment status start in April and June, respectively, and go through the rest of the year. This matches the model from before because both states rescinded their excise tax on the first of the month. The next way we define treatment status is to include the partially treated month as counting toward treatment status. These estimates are shown in columns (2) and (4).

Table 4 presents results of the quantity analysis. We do not find a statistically significant change in the amount of gasoline sold at the state-level for any of the model variants, and the point estimates even oscillate between positive and negative values. The point estimates are also near-zero, so this null effect is not simply due to wide standard errors. On the one hand, this is expected given the inelastic nature of gasoline demand in aggregate (Brons et al. (2008); Li et al. (2014); Levin et al. (2017)). On the other hand, we again argue caution in

interpreting these estimates as the causal effect of excise tax reductions on the quantity of gasoline demanded because the data are at a monthly resolution.⁴¹

That said, we can still calculate a back-of-the-envelope accounting of consumer and producer surplus. Given the relatively precise null effects found above, we simply take the observed total volume of gasoline sold in a state during the first full month that the state's policy is in force⁴² and we multiply this by the excise tax rate in the state and the pass-through estimate found using the CH methodology from Table 3. This results in a first month consumer surplus of approximately \$121 million dollars in Maryland, \$83.5 million in Georgia, \$24.5 million in Connecticut, and \$117.8 million in New York.⁴³ Summing over the full time period that each state did not collect excise taxes, approximately \$1.4 Billion dollars were not collected from consumers. However, this large aggregate number masks the incremental impact on a per-consumer basis. Using data on the number of licensed drivers in 2022 from the Federal Highway Administration for each state, we calculate that the first month of each policy resulted in a consumer surplus of \$27.55 per Maryland driver, \$11.34 per Georgia driver, \$9.32 per Connecticut driver, and \$9.75 per New York driver.

The unintended consequence of this policy is that firms were able to retain a portion of the excise tax rate because they did not fully pass-through the reduction to consumers. For the first full month the policy was in place, our pass-through estimates imply that this yielded new producer surplus of approximately \$37.6 million in Maryland, \$49.4 million in Georgia, \$5.4 million in Connecticut, and \$42.9 million in New York. This is revenue that would have otherwise been returned to the state in the form of taxes, or would have been added to consumer surplus if the spirit of the law had been followed. Using data on the number of

⁴¹More elaborate versions of the model that we considered instrumented the average price in a month with the wholesale price of gasoline, though these specifications arrive at near identical conclusions so we opt for the more parsimonious model for brevity.

⁴²For Maryland, we average the total volume sold over March and April because their policy lasted for half of each month.

⁴³Recall, Maryland's excise tax rate is the highest of the four states at 43.1 cents per gallon.

gasoline station establishments for each state in 2022⁴⁴ this results in approximately \$26,793 more profit per gas station in Maryland, \$9,786 per gas station in Georgia, \$5,052 per gas station in Connecticut, and \$9,387 per gas station in New York.

6 Conclusions and Policy Discussion

In the summer of 2022, a handful of states decided to rescind their excise taxes on gasoline to provide “inflation relief” amidst a period of generally higher inflation and high gas prices due to the onset of the Ukraine-Russia war. Gasoline taxes are a significant portion of state tax revenues, though, and they fund much more than road maintenance and repair. Choosing to remove these taxes could lead to lower at-the-pump expenditures for consumers, but the exact amount that consumers are made better off by the policy ultimately depends on the pass-through of the excise tax savings from gasoline wholesalers and retailers who have the statutory incidence of collecting the tax in the first place.

We find that pass-through was incomplete, with only 73% of the excise tax savings passed along on average. Although incomplete, this majority of the incidence accrues to consumers which is expected theoretically given that demand is more inelastic than supply, as is the case in retail gasoline. By state, the point estimate for this pass-through rate is heterogeneous with 82% of tax savings passed-through to retail prices in Connecticut on the high end and only 63% of tax savings passing-through to retail prices in Georgia on the low end. These estimates are all obtained using the de Chaisemartin and D’Haultfoeuille (2022) advanced methodology for difference-in-differences which takes account of the heterogeneity in policy impact and timing.

Our findings enable us to assess the advantages and drawbacks of this particular ‘inflation relief’ policy. On the one hand, overall consumer expenditures did fall by a substantial amount. A back-of-the-envelope accounting of consumer surplus indicates that consumers

⁴⁴U.S. Census Bureau’s Statistics of U.S. Businesses tables, NAICS code 4471

in states that rescinded excise taxes paid \$1.4 Billion dollars less than they otherwise would. This is because they bought essentially the same amount of fuel but paid a lower cost due to lower excise taxes (holding all else equal).⁴⁵

This large change in aggregate consumer expenditures oversells the effectiveness of the policy in providing ‘inflation relief’ to the average driver. To put this in context, consider a driver that travels 1,200 miles per month in a vehicle that gets 25 miles per gallon. This amounts to approximately \$8.78 in inflation relief per month for consumers in Georgia on the low end, and \$15.79 per month for consumers in Maryland. Similar representative drivers in Connecticut and New York would save approximately \$9.84 and \$12.43 per month, respectively. Alternatively, using data on the total number of licensed drivers in each state, we calculate that each driver would save \$27.55 in Maryland, \$11.34 in Georgia, \$9.32 in Connecticut, and \$9.75 in New York per month, assuming these savings accrued evenly to every driver.

It was something, but not a lot. While inflation peaked at 8%, this relief was equivalent to about a 0.28% increase in monthly wages.⁴⁶ We can also compare this average benefit to drivers with reference to a report by the Bureau of Labor Statistics on how expenditures changed for a typical household (Meyers et al. (2023)). In this report, the BLS documents that although incomes rose by approximately 7.5% between 2021 and 2022, consumer expenditures increased by 9% and that this was mostly “attributed to higher prices rather than to an increase in the quantity of goods purchased or a change in consumer preference” (Meyers et al. (2023)). The report specifies that during this time annual expenditures increased by \$6,039 due to inflation-driven expenses, a \$503.25 increase per-month, and also breaks down the change in expenses by category. The category of ‘Food at Home’ for the typical household accounted for \$444 more dollars spent or about \$37 more per month, and gasoline

⁴⁵This is shown in Table 4, but is also confirmed when we compare gasoline sales volumes in each state that rescinded their excise taxes in 2022 versus prior years when the excise tax stayed in force. This is true based on the month (e.g. comparing March 2019 with March 2022) and on per capita terms.

⁴⁶Assuming an annual income of \$50,000.

expenditures accounted for an additional \$972 or \$81 per month. Thus, while our analysis finds that the typical driver in states that removed their excise tax benefited from a \$9-27 reduction in fuel expenses per month, this was only enough to offset a fraction of heightened expenses at the pump, and an even smaller fraction of the overall increase in expenditures the typical household faced.

When we consider the costs side of the policy it is difficult to consider where best these foregone tax revenues would have been spent in their next best use because needs and circumstances vary by state. Regardless, retailers were able to capture some of the cost-savings intended for consumers, so there was an unintentional and well-targeted subsidy provided to gasoline wholesalers and retailers. This was not in the spirit of the policy, nor was it likely the next best-use of foregone tax revenue. However, we note that a tax relief measure should not only be deemed successful if pass through is complete. We find that the policy provided an unintended subsidy to gasoline retailers of approximately \$0.10 cents per gallon of gas sold on average. All told, this diverted tax revenue benefited gasoline suppliers by more than \$571.5 million dollars summed across the four states. By state, each month that the policy was in place retailers received an unintended subsidy of \$37.6 million in Maryland, \$49.4 million in Georgia, \$5.4 million in Connecticut, and \$42.9 million in New York.

The cost-benefit accounting for policymakers to consider is whether or not they believe it is worthwhile to forego tens of millions of dollars in tax revenues monthly to provide an average consumer with approximately ten dollars of savings. We expect it would be administratively less costly to provide any desired inflation relief through an income tax credit (for states with income taxes) or even to provide an advance credit via direct deposit or paper check, where necessary. Having said that, we would also expect that more consumers would balk at a small check in the mail than they would at a seemingly large 20 cent reduction in the price of gasoline, in spite of the fact that that reduction would save little over their

few fill-ups that month. All told, while excise tax rescission policies may have a muted per-driver impact and come at a substantial cost, they may make “political sense” due to the high salience of gasoline prices. While the tax holidays were an economically inefficient tool for inflation relief, their political appeal may stem precisely from their high visibility, a trade-off that is central to understanding this type of policy choice.

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7 Appendix

Florida

Figure A1. Average Prices

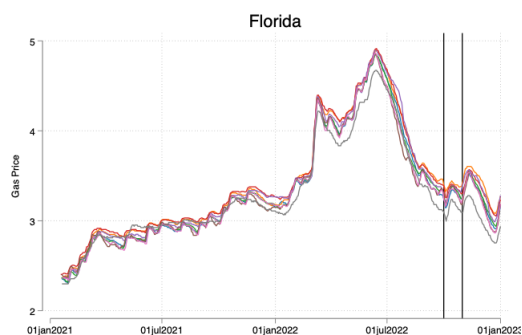
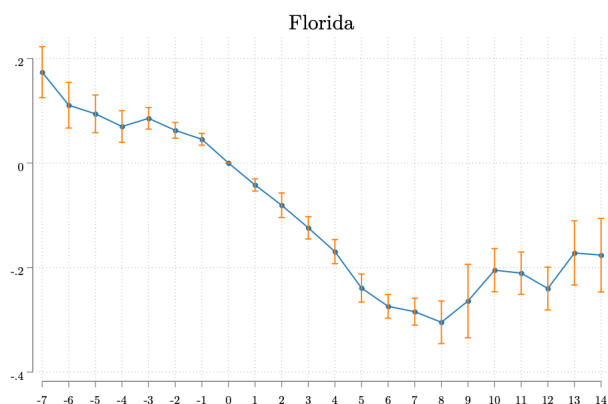


Figure A2. Event Study Estimates



We do not include observations from Florida in our analysis. This is because the parallel trends assumption is clearly violated (Figure A2). This could be due to the late passage of the policy or the bad timing of the policy which came into effect after prices returned to pre-war levels. We also exclude Florida from the ‘control’ group prior to the policy passing so that we do not have tainted controls potentially from an ‘intention to treat’ effect.

Figure A3. Event-Study - All States

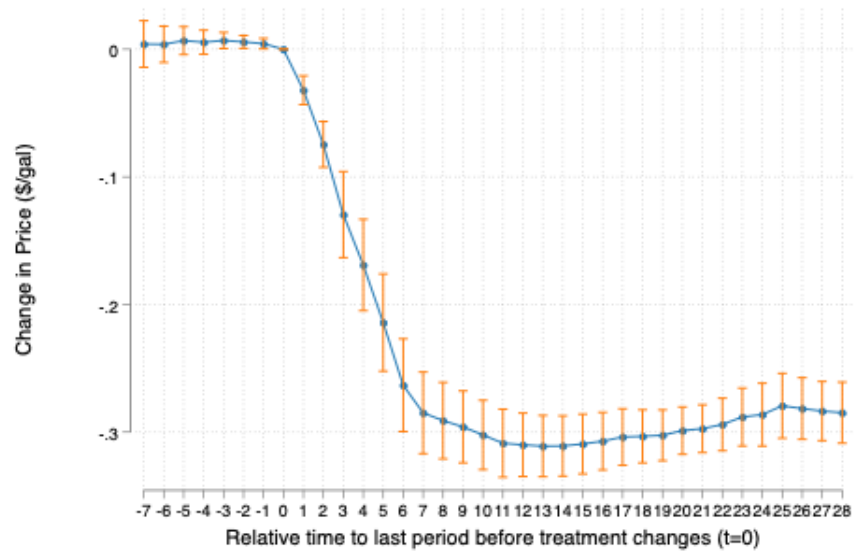


Figure A4. Event Study - State by State

