$2.00 Gas! Studying the Effects of Gas Tax Moratorium

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Abstract

Despite the considerable attention paid to the theory of tax incidence, there are surprisingly few estimates of the pass-through rate of sales taxes on retail prices. This paper estimates the effect of a suspension and subsequent reinstatement of the gasoline sales tax in Illinois and Indiana on retail prices. Earlier laws set the timing of the reinstatements, providing plausibly exogenous changes in the tax rates. Using a unique dataset of daily gasoline prices at the station level, retail gas prices are found to drop by 3% following the elimination of the 5% sales tax, and increase by 4% following the reinstatements, compared to neighboring states. Some evidence also suggests that the tax reinstatements are associated with higher prices up to an hour into neighboring states, which provides some evidence on the size of the geographic market for gasoline. Effects across different competitive environments are considered as well.

This work is preliminary and comments are welcome.

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1. Introduction

Gasoline prices are particularly visible, and when they spike there are often calls to reduce or eliminate gas taxes (Berryman, 2005). State and federal taxes add forty cents to the average gallon of gasoline in the US, resulting in over $8 billion in tax receipts each year (EIA, 2005). For a tax moratorium to reduce prices it is necessary to estimate the pass-through rate—the proportion of the tax paid by consumers—at least in the short run.

The theory of tax incidence has been well studied and suggests that sales and excise taxes should be fully passed onto consumers in competitive markets with constant marginal costs.[1] Less than 'full shifting' is expected in markets with increasing marginal costs, while the pass-through rate may be less than, or greater than, one-hundred percent in markets that are less competitive. In addition, tax increases in one state may lead to higher prices across the border as stations there face greater demand.

Despite this attention paid to tax incidence, surprisingly little empirical work has estimated the effect of sales taxes on prices (Poterba, 1996; Besley and Rosen, 1999; Kenkel, 2005). This paper studies a moratorium on gasoline sales taxes in Illinois and Indiana during the summer and fall of 2000 to estimate the effect of tax changes on retail prices, along with the effect on competition along the state borders. Using a unique data set of daily prices at the gas-station level, linked with census and geographic data, prices are compared with neighboring states before and after the tax changes.

The tax moratorium offers four main advantages. First, the reinstatement of the gasoline tax in these states is an arguably exogenous change in the tax rate. In Indiana the moratorium ended after 120 days as a result of a 1981 law that allowed the tax

[1] Kotlikoff and Summers (1987) and Fullerton and Metcalf (2002) offer a detailed reviews.

suspension. In Illinois, the tax repeal was set to expire at the potentially arbitrary time of the end of the year. While the dates of the reinstatements were known and could have increased demand (and prices) just prior to the tax increases, the lack of a pre-existing trend in price differences across states at the time of the reinstatements suggests that the end of the tax suspensions provide plausibly exogenous changes in the tax rate. Second, the repeal and subsequent reinstatements allow estimates of the pass-through rate for both decreases and increases in the tax. Similar estimates of the effect of the tax on retail prices would suggest that the price changes are tied to the tax rate policies. The comparisons also provide a test for asymmetry in the response to changes in stations' marginal costs, as the downward price adjustment may be stickier than the upward adjustment (Borenstein, Cameron, and Gilbert, 1997). Third, while the short-run nature of the policy change does not inform long-run effects, it does provide a way to study the extent of the geographic market for gasoline holding station locations fixed. Fourth, gasoline is a homogeneous product where quality differences across space are minimized when studying the pass-through rate of sales taxes across retailers. The key differentiation in the market would appear to be location, a subject we consider in detail below.

The results suggest that retail gas prices drop by 3% following the elimination of the 5% sales tax, and increase by 4% following its reinstatement. The tax increase in Indiana is associated with higher prices up to an hour away from its border, though the evidence is mixed for the Illinois reinstatement. Cities and neighborhoods with different numbers of competitors were examined, and the results are fairly robust across market types. Results are also similar across a number of robustness and specification checks.

As a whole, the results suggest that pass-through rates are between sixty and eighty percent for retail gasoline in the short run.

The structure of the paper is as follows. Section two briefly provides background on the reasons for the tax repeals and how they were conducted, as well as a review of the related literature to place the current results in context. Section three describes the data and presents mean comparisons of ZIP code characteristics across the comparison groups. Section four presents the empirical model and results. A number of specification and robustness tests are also described. Section five concludes.

2. Background and Related Literature

In the spring of 2000, gasoline prices rose sharply in the Midwest. Figure 1 shows the average daily price of regular unleaded gasoline in Chicago, the rest of Illinois, Indiana, Kentucky, and the other states that are neighbors to Illinois and Indiana (Wisconsin, Iowa, Missouri, Ohio and Michigan). Average prices spiked to $2.11 per gallon in Chicago, and $1.78 in Indiana on June 19, a very high, and politically unattractive, price at the time (though quite common in 2005). The spike has been attributed to short supplies of refined gasoline in anticipation of a change in the reformulation required of refineries, along with unexpectedly high demand (FTC, 2001). Much was written about the increase in gasoline prices at the time, as evidenced by at least four reports investigating the cause of the price spikes including an investigation by the Federal Trade Commission (Conrad, Howard, and Noggle, 2000; Shore, 2000; Martin, 2001; FTC, 2001).

In response, on June 20, 2000, the governor of Indiana used the power granted under a 1981 statute to declare an energy emergency, allowing him to suspend the five percent sales tax on gasoline for 60 days, with a possible extension for another 60 days. He announced that the suspension would begin on July 1st, with the sixty day timeframe. He later extended the suspension three times: on August 22nd changing the end date to September 15th, on September 13th extending the suspension until September 30th, and on September 28th allowing the suspension to run its 120 day course.[2] The excise tax that was tied to highway funding was not allowed to be suspended without legislative approval.

In Illinois, the legislature responded by passing a bill on June 28th suspending its five percent sales tax on gasoline, set to begin on July 1st as well. The moratorium in Illinois was set to end six months later on January 1, 2001. Of the neighboring states, only Michigan had a sales tax to consider suspending, and while there was talk of a suspension it did not occur.

The effect of the repeal on gasoline taxes was questioned at the time. While the Illinois law required gasoline retailers to post a notice that the gasoline tax had been suspended, the governor of Illinois, George Ryan, noted: "There is no guarantee in a free-market economy that prices will go down, but I believe that the political and public pressure applied by the roll back of the sales tax will help force prices down." (Ryan, 2000). A subsequent appraisal showed that prices had fallen following the tax

[2] This was an election year in Indiana, and the governor was criticized for continuing the sales tax suspension in August and September when prices had already fallen (see Figure 1). On October 25th, the governor made known that the sales tax would be ending. State press releases noted that the sales tax would be reinstated at midnight on October 25th, but the clear jump in prices occurred at the 120 day mark from the time of the tax suspension. Price differences between Indiana and neighboring states did not change on September 1st, 15th, or 30th. The timing of the effect is discussed further below.

suspension, though the investigation did not attempt to disentangle the effect of the tax suspension with the falling wholesale prices at the time (Conrad, Howard, and Noggle, 2000).

Related Literature

Poterba (1996) reviews the early empirical literature on sales tax incidence and argues that there has been relatively little empirical work on the incidence of sales taxes.[3] Further, the evidence has been mixed with regard to incomplete, full, and over shifting of taxes on retail prices. Poterba (1996) analyzes city-specific clothing price indices for eight cities during 1947-77 and fourteen cities during 1925-39. Retail prices rose by approximately the amount of the sales tax for the post-war period but only by two-thirds during the Depression years. Besley and Rosen (1999) use quarterly data for 12 commodities (such as bananas, bread, milk and Big Macs) in 155 cities during 1982-90. Their model includes city fixed effects to test the effect of changes in tax rates on retail prices. While the pass-through rates for some commodities are estimated to be close to 100%, their results also suggest that several commodities have what has been labeled overshifting: pass-through rates of well over 100%. Kenkel (2005) recently studies the effect of tax hikes on the price of alcoholic beverages in Alaska. His interesting results suggest that alcohol taxes are more than fully passed through to beverage prices.

In the case of gasoline prices, the prediction of the pass-through rate is ambiguous, as the market for retail gasoline is usually characterized as imperfectly competitive due to the spatially-differentiated nature of the market (Verlinda, 2004). Other factors that determine local market demand elasticity such as demography,

[3] Early studies include Brown (1939), Due (1942) and Bishop (1968).

household income, and means of commuting are also likely to affect the pass-through rate.

In terms of the empirical literature on gasoline taxation, Chouinard and Perloff (2002) used a monthly panel of the 48 US contiguous states and the District of Columbia between 1989 and 1997 to estimate a reduced-form model of gasoline prices. Among other results, they find that tax variations and market power (measured by mergers) contribute substantially to geographic price differentials. Using a model with state fixed effects, they also find that 50% of federal tax incidence is passed on to consumers, 75% of state *ad valorem* taxes like those studied here are passed on to consumers, and nearly all of the state excise taxes are passed on to consumers. Meanwhile, wholesale prices appear unaffected by changes in state tax rates. In another paper, Chouinard and Perloff (2004) also find that state specific taxes fall almost entirely on consumers, especially in states that use relatively little gasoline and have lower elasticity of supply. While the timing of the tax changes were not explored, the results are consistent with less than or full shifting of taxes in gasoline markets.

Alm, Sennoga, and Skidmore (2005) also consider a panel of monthly gasoline prices for the fifty U.S. states and estimate the effect of changes in excise taxes on retail prices using a model with state fixed effects. They find that the excise tax is fully passed on to consumers within the first month of a tax change, with no effect on retail prices of the one-month lag in the tax level. This is consistent with the results presented below that comparisons just before and after the tax changes likely capture the full (if short-run) effects on retail prices. The paper also notes the potential impact of spatial competition by comparing the results for urban versus rural states. They find that the pass-through

rates are similar, though somewhat smaller in rural states where stations are thought to be more widespread. In addition, they test whether prices respond differently to excise tax increases and decreases and do not find an asymmetry in the response. Like the Chouinard and Perloff papers, the timing and potential endogeneity of the excise tax changes were not considered, and aggregate data precluded the study of the spatial effects of the tax changes at the station level.

Our analytical framework follows the Chouinard and Perloff reduced form specification. The demand function for gasoline is given by $Q = D(p, X)$, where p denotes the gas price and X represents exogenous demand shifters. On the supply side, the reduced-form marginal cost function is $MC(t, W)$, where t is the tax parameter and W represents cost shifters. Profit maximization implies that the marginal cost of a firm equals its marginal revenue. Therefore, we have $p = f(t, X, W, Z)$, where Z consists of the variables that capture the market power of the firms, which in turn results in price being different from firms' marginal revenue.

In the model, the effect of a tax increase on price also depends on demand conditions, cost structure and market power of the firms. In a perfectly competitive market, we would expect that the tax would be completely passed through to the customer when the firms face constant marginal cost, while the pass-through rate is less than one when they have increasing marginal cost. However, as pointed out by Katz and Rosen (1985) and Stern (1987), the 'full tax shifting' case cannot be generalized as the upper bound when the market is not perfectly competitive. In that case, the pass-through rate of the tax could be either below or above 100%, depending on the elasticity of demand. For example, in the case of isoelastic demand, the pass-through rate in a market

with an oligopoly is more than 100%, and will be still greater in monopolistic competition if the elasticity of demand is less than one.

The gas tax repeal studied here may impact competition across state borders as well. A decrease in the state tax rate may lower prices for that state's drivers, as well as drivers from neighboring states if stations across the border are part of the same market.

Previous evidence suggests that consumers should not be willing to travel very far to save on gasoline. Manuszak and Moul (2005) use local tax differences to study the static trade-off between price and traveling. Using data from the Chicagoland area (Chicago, Cook County, Will County and Northwest Indiana) in July 2001, they find that a typical Chicagoland consumer must save $0.55 per fill-up in order to travel to a station an additional mile away. This suggests that traveling for cheaper gas prices is not cost effective.

Nevertheless, the market size is likely reflected by commuting patterns. Commuters, and interstate commercial drivers, do not require additional travel to find lower prices. These drivers may delay their purchase to find the station with cheaper prices along their routes and may extend the size of the market for gasoline.[4] The analysis below considers the effect of the tax changes on competition across state borders, which will also provide some evidence on the appropriateness of using neighboring states as comparison groups.

There are other studies that look at cross-border competition in other industries. For example, Coats (1995) studies the effects of state cigarette taxes in the US and finds that about fourth-fifths of the sales response to state cigarette taxes is due to cross-border

[4] The US Census metropolitan statistical areas are defined by commuting patterns for largely this reason.

sales. Sumner (1981) and Ashenfelter and Sullivan (1987) also use cross-state differentials in cigarette excise tax to study the extent of market power in the industry. Further, the effects of a state's spending policies on other states have been considered as well (see, for example, Case, Hines, and Rosen, 1993).

The tax changes studied here may affect prices across time as well as across space. For example, consumers may delay their purchase of gasoline just prior to the tax suspension, or increase their purchase of gasoline just prior to the tax reinstatements. This may lead to lower prices just prior to the suspension and higher prices prior to the reinstatement. Such changes in prices are explored below.

One advantage of the temporary nature of the suspension is that it allows a study of the extent of the geographic market for gasoline taking the location of the stations as given. In equilibrium, differences in taxes across jurisdictions simultaneously determine not only the gas price in each station, but also dictate the location of the gas stations across geographic areas. The temporary tax changes are not likely to change the location decisions of the station owners, or the decision for new stations to enter the market. As a result, the effect of the tax changes should be reflected mainly in the gas prices, as they change the static trade-off between price and traveling costs across borders for gasoline.

4. Data Description

The analysis uses a unique dataset of daily gasoline prices at the station level. These prices are collected by Wright Express Financial Services corporation, a leading provider of payment processing and financial services to commercial and government car, van and truck fleets in the United States. Their Oil Price Information Service (OPIS)

provides wholesale and retail gasoline prices for up to 120,000 gasoline stations each day. One advantage of the data is that measurement error should be minimized, as the prices are recorded electronically from their clients' charge cards.[5] Prices for regular unleaded are collected, along with wholesale prices from the nearest refinery that produces the formulation of gasoline used by the station.

Figure 1 showed that the price spike was particularly severe in Chicago, and noticeably less so in Kentucky. For greater comparability, the main analysis will exclude the Chicago-Naperville-Joliet, IL, IN, WI Metropolitan Statistical Area, as well as Kentucky, though the results are similar when these areas are included as discussed below. Stations in Illinois, Indiana, and five neighboring states: Michigan, Ohio, Missouri, Iowa, and Wisconsin are compared, where roughly 6,000 stations are surveyed each day. The data has some missing days, however, especially on weekends and holidays. For example, there are no observations during July 1- July 4, and few observations right at January 1. Instead, the main analysis will focus on prices just before and after the tax changes as allowed by the data, using two days before and after to increase the number of stations observed. For the July tax repeal, June 27 and 28 are compared to July 5 and July 6; October 26 and 27 are compared to October 31 and November 1 for the Indiana reinstatement; and relatively fewer observations for December 29 and 30 are compared to January 2 and 3 for the Illinois reinstatement. Specifications that consider longer time frames and flexible treatments for time variation are estimated as well.

The OPIS data also include the street address of the station. US Census of Population data at the ZIP code level in 2000 are used as controls for neighborhood

[5] Further information on the methodology is available at http://opisnet.com/methodology.asp

characteristics such as the age, race, and educational composition, median household income, population, and commuting behavior. The US Gazetteer ZIP Code file from the US Census was collected, which includes the area of the ZIP code and its latitude and longitude. In addition, the 2000 US Census ZIP Code Business Patterns database records the number of gasoline stations in each ZIP code, which will be used as a control on the local competitive environment. Maps were used to identify those ZIP codes with an interstate highway as a measure of access to gasoline stations that are effectively closer in terms of travel times.

The address data also allows an estimate of the distance from the station to the state border. First, ZIP codes that comprise the state borders were identified using ZIP code maps. Then, for stations in Illinois/Indiana, the minimum driving distance in minutes from each ZIP code to a neighboring state ZIP code was calculated using software from Mapquest™. This data source has the advantage of calculating distances as consumers would travel from one area to the next, as opposed to distances calculated along a straight line. For stations in the neighboring states, the distance from each station to the nearest Illinois or Indiana ZIP code is also calculated. This allows a comparison of stations near the Illinois/Indiana border, as well as those farther from the border, to test the effect of the Illinois/Indian tax changes on stations in neighboring states.

One potential limitation of the pricing data is that it may oversample stations used by consumers who travel extensively. The coverage appears fairly complete in the Midwest, however. When comparing stations in the pricing survey and those in the Census data, the median ZIP code had three quarters of the stations surveyed. Further,

the surveyed stations include many unbranded gasoline stations, and over a third of the stations surveyed are in ZIP codes with no interstate.[6]

Table 1 describes the variables used as controls and demonstrates that the comparison groups are similar. The sample considered is for the July tax repeals. The differences are statistically significant given the large number of observations, but the neighborhoods are fairly similar. For example, the Illinois/Indiana ZIP codes have median household incomes are $42,000 versus $43,000 in the neighboring states. The commuting patterns were computed for all workers in each ZIP code, and stations in both comparison groups are located in ZIP cods with an average of 82% of workers who drive to work alone and commute times in minutes that are nearly identical. The population is somewhat smaller in the Illinois/Indiana ZIP codes (19,000 versus 21,000). Another difference is in terms of average gas prices: retail prices were ten cents cheaper in Indiana/Illinois, partly because prices had declined already prior to the moratorium. Wholesale prices, meanwhile, were four cents cheaper per gallon. One reason for the similarity in the ZIP codes is the exclusion of Chicagoland. When stations in the Chicago MSA were included, the Illinois/Indiana ZIP codes tended to have much higher gasoline prices, were more urban, higher income, younger, and more likely to have residents using public transportation.

5. Empirical Model and Results

[6] To explore the types of ZIP codes that have better coverage in the pricing sample, the ZIP code count of the number of stations in the sample was regressed on the observable characteristics in Table 1. The main result is that more populous ZIP codes are associated with more stations surveyed, even after controlling for the number of stations in the Census data.

All results in the paper are presented with respect to three time periods: the summer of 2000 when stations in Illinois/Indiana are compared to neighboring states; the fall of 2000 when Indiana is compared to its neighboring states including Illinois; and the winter of 2000/2001 when Illinois is compared to its neighboring states including Indiana.

The main results are in Figure 2, which reports the results of a local linear regression of the difference in average log retail prices between Illinois/Indiana and neighboring states against time. These models were separately estimated before and after the tax changes, and the size of the discontinuity at the time of the tax change is a difference-in-difference estimate of the effect of the tax change on retail prices.[7]

Figure 2A reports the results for the summer of 2000. One feature is the downward trend in the difference in gas prices prior to the moratorium. Recall that there was a spike in the spring of 2000, and the legislation appears to be dealing with a problem that was already ameliorating—a chief criticism of the tax changes (Martin, 2001). This pre-reform trend in price differences suggests that the difference-in-difference estimate may reflect other changes in the market, and may overstate the effect of the tax change on retail prices—the price differences may have declined without the tax change. The difference does appear to level off prior to the tax change, however, and the difference-in-difference estimate implied by the local linear regressions is a reduction

[7] Results shown have a bandwidth of 7 days. Results are slightly smaller with a bandwidth of 14 days, with difference in difference estimates of -1.8%, 4.1%, and 2.0%. OLS estimates with flexible time frames are discussed below.

in retail prices of 2.7 log points, or approximately 2.7%, following the suspension of the 5% sales tax.[8]

Figures 2B and 2C compare the differences in retail prices against time when the gas taxes are reinstated. The pre-reform differences in prices are much flatter than the July comparison when the reform was reacting to market conditions, suggesting that the sunset provisions provide plausibly exogenous changes in the tax rate. The effect appears to fade somewhat thirty days after the reform at the end of November, but this is due to a temporary increase in retail prices in Illinois not experienced by Indiana or its other neighboring states. Meanwhile, a slight decline in relative prices is found in the last week of December (-0.4%) that continues in the first week of January (-0.7%).

While the end dates were not chosen to reflect concerns about market conditions, the announced end date may affect purchasing habits prior to the tax increase. If demand increased just prior to the tax increase, the price would be expected to rise as well, though this is not observed in the data. This suggests that the comparison of prices before and after the reform provide a useful estimate of the effect of the tax change on retail prices.

The local linear regression estimates suggest that Indiana's retail price increased by 4% relative to neighboring states at the time of the 5% tax increase, while Illinois's retail price rose by 2.8% at the time of its reinstatement.

These raw comparisons do not take into account differences between Illinois/Indiana and neighboring states, including potential differences in wholesale prices

[8] The distinct jump in prices suggests that the effects happened right away, as opposed to consumers increasing demand just before the tax increases, or stations reacting slowly over time. In terms of the stations' responses, an S-shape in the jump would be expected if some stations responded immediately while others waited (Caballero et al., 1995). Responses by day are considered in more detail below.

changes. To test the effect of the tax changes on retail prices controlling for these factors, the following model is estimated for station s selling brand b at time t:

$$\begin{aligned}\ln(\text{Retail Price}_{sbt}) = \gamma_0 &+ \gamma_1 \mathbf{1}(\text{Illinois or Indiana})_s + \gamma_2 \text{Post Reform}_t \\ &+ \gamma_3 \mathbf{1}(\text{Illinois or Indiana})_s * \text{Post Reform}_t \\ &+ \gamma_4 \ln(\text{Wholesale Price})_{st} + \gamma_5 X_s + \delta_b + \varepsilon_{sbt}\end{aligned}$$

where $\mathbf{1}(\text{Illinois or Indiana})_s$ is an indicator that the station is in Illinois or Indiana for the July time period, Indiana for the October comparison, and Illinois for the January comparison; Post Reform$_t$ is an indicator that the gas price is observed after the tax change; and X_s is a vector of the station's ZIP code characteristics described in Table 1.

Brand fixed effects are also included, as decisions at the brand level may affect the reaction of prices to the change in taxes.[9] One issue is that brands primarily in one state will be nearly collinear with the indicator for Illinois or Indiana. As a result, the indicators for major brands that make up at least five percent of the station observations in both the treatment and comparison groups are included. These major-brand stations account for 60% of the observations. Results are nearly the same when brand fixed effects for all stations are included while restricting the data to those brands observed in both comparison groups, but including the indicators for only the major brands allows the use of all the stations.

Table 2 reports the results of models with and without controls. Again, the estimates are broken into three time periods: just before and after July 1, October 31, and January 1. Column (1) includes no controls and retail prices are estimated to fall by roughly 3.5% in July, and increase by 3.9% in October and 2.7% in January. These estimates imply pass-through rates of 70%, 78%, and 54%, respectively.

[9] Hastings (2004) studied the conversion of independent gasoline stations into branded gasoline stations and found evidence of consumer brand loyalty which can affect the price response to the tax changes.

Column (2) includes the wholesale price which serves to control for differential changes in the costs of the stations at the time of the reform. (The potential effect of the tax reform on wholesale prices is explored below.) The estimates decrease to 2.9% in July, remain about the same in October at 4.0%, and increase in January to 3.6%.

The neighborhood characteristics may differ across the comparison groups as well, which may affect the response to shocks at the time of the reform. When the control variables listed in Table 1 and brand indicators are included, the results are essentially unchanged as shown in Column (3). This confirms the conclusion of Table 1 that the Illinois and Indiana neighborhoods are similar to those in the neighboring states, especially with the exclusion of Chicagoland, as well as the ability of the difference-in-difference estimate to control for time invariant differences across the comparison groups.

The results are similar to the response of retail prices to wholesale price changes. When data from all of the states considered here from April 1, 2000 to March 31, 2001 were considered, a 5% increase in a station's wholesale price is associated with a 3% increase in its retail price. This estimate was stable to the inclusion of control variables and station fixed effects.

The point estimates are also consistent with an asymmetry in response to marginal costs increases versus decreases (Borenstein, Cameron, and Gilbert, 1997). While the equality of response to the repeal and subsequent reinstatements of the tax cannot be rejected, the point estimates do suggest that the fall in prices is somewhat smaller than the increase in response to tax changes.

Of the control variables, the signs of the associations are generally as expected (see Appendix Table A1 for complete results). For example, log wholesale prices are positively associated with log retail prices. Income is positively associated with prices, possibly reflecting demand and cost of land. The fraction of the ZIP code workers who carpool is positively associated with gasoline prices, likely reflecting the decision to carpool both in high gas price areas and around congested cities again with high land costs. Last, the fraction of workers with long commutes is negatively associated with retail prices in July and January, possibly as they increase the number of competitors to include gasoline stations along the major commuting routes.

Wholesale Prices

The previous discussion has restricted attention to the retail gasoline market and controls for wholesale prices in an attempt to compare stations with similar marginal costs. If wholesale prices were affected by the tax reforms then the difference-in-difference estimates would be affected as well. For example, in July when wholesale prices and taxes are falling at the same time, the estimates would be smaller in absolute value as the wholesale price control variable soaked up some of the effect of the reform. Table 2 showed that the estimate did decrease in July when the wholesale price control was introduced, though the estimates were stable in October and increased in January.

Column (4) of Table 2 further investigates the effect of the tax reforms on wholesale prices. The model is similar to those above, but with log wholesale price as the dependent variable. The difference-in-difference estimates reveal small decreases in wholesale prices at the time of the tax changes, including a 1.4% drop in January at the time of the Illinois reinstatement compared to its neighboring states. The lack of a

significant effect of sales tax changes on wholesale prices suggests that they can serve as controls for differential costs across the comparison groups. The result is also consistent with the prior estimates that the tax incidence does not seem to pass on to wholesalers in the market for gasoline (Chouinard and Perloff, 2004).

Competition across Borders

One question that arises when using neighboring states as a comparison group is whether or not they were affected by the reforms in Illinois and Indiana. The effect of the tax change on border competition can also yield insights into cross-border tax incidence and the extent of the geographic market for gasoline. If the border stations, consumers along the border, and drivers commuting across the border respond to the tax change, we would expect smaller difference-in-difference estimates at the border. The stations in the treated states would be under less pressure from the cross-border competitors to pass along the tax savings, and the stations just across the border would be under more pressure to match any price declines.

To test the effect of the tax change at different distances from the border, stations were categorized by their distance in five minute intervals from the Illinois/Indiana border (0-5 minutes, 5-10 minutes, and so on). For stations in Illinois or Indiana, this is the traveling time to the nearest border (including Kentucky), while for the neighboring states this is the distance to the nearest tax reform state. For example, the distance recorded for stations in Michigan is the distance to Indiana, while the distance recorded for a station in Indiana is the minimum distance to any neighboring state. Note that, for the July comparison, Illinois and Indiana are treated as one treated region, so the Illinois-Indiana border is omitted from the calculation. Some cells had few observations, so the

analysis was restricted to cells with at least 10 stations in each of the comparison groups. This results in few observations far from the border, especially in January.

In each of these cells, the average before-after price difference was calculated separately for Illinois/Indiana stations and the set of stations in neighboring states. The difference between these price changes was then calculated for each five-minute interval from the Illinois/Indiana border.

Figure 3 presents a local linear regression of these price changes against distance from the Illinois/Indiana border.[10] The horizontal axes in each of the panels are the five-minute intervals from the border. One regression shows the difference in average log retail prices before and after the reform in Illinois/Indiana; a second shows the before-after difference in the neighboring states; and the third is the difference between these two, representing the difference-in-difference estimate comparing stations that are a similar distance from the Illinois/Indiana state borders.

In July, the raw data provide little evidence of an effect on border competition, with larger difference-in-difference estimates (in absolute value) closer to the border. Not controlling for other observable characteristics, the price drops roughly 14 percent in the Illinois/Indiana region after the reform regardless of distance to the borders. In the neighboring states, the prices drop roughly 10 percent seventy minutes and more from the border, though less of a decline is seen closer to the tax reform states. This leads to a difference-in-difference estimate that varies from -7% at the border to -5% farther from the border.

For the reinstatements, the raw comparisons do reveal smaller difference-in-difference estimates closer to the border. In October, the price increase in Indiana is not

[10] The estimates use a bandwidth of twenty minutes representing four five-minute cells.

related to distance to the border, although the neighboring states have smaller price declines near the border compared to stations farther from the treated states. The estimated difference-in-difference therefore increases from 3.7% at the border to an estimated 6.9% ninety minutes from the border, dropping to 5.3% one hundred twenty-five minutes from the border. The large decline at the very end of Figure 2B is partly due to few observations that far from the border in Indiana.

In January, the raw data are again somewhat mixed: smaller difference-in-difference estimates are found near the border, but the estimates also decline for when comparing stations far from the border. While the estimates in July and October had cells with generally over one hundred stations, the January comparison employs cells with generally less than fifty stations, especially farther from the border where cells are typically close to twenty stations. The decline in the effect far from the border in January, therefore, should be treated with some caution.

One way around the lack of data is to collapse the five-minute intervals into thirty-minute intervals. In addition, the unconditional comparisons in Figure 3 do not take account of potential differences in observable characteristics, such as wholesale prices or the commuting behavior of residents across space that may affect the results. Table 3 reports estimates for a model that considers four regions defined by thirty minute intervals. Stations over an hour and a half from the border, for example, are stations within an oval-shaped region in the center of the treated states. For the untreated states, this is an indicator that the stations are simply far from the treated states.

The models include the same specification as before, but now include the three-way interaction between each of the three distance categories, **1**(Illinois or Indiana), and

Post Reform. The indicators for each distance category and their two-way interactions with **1**(Illinois or Indiana), and Post Reform are also included. The excluded category in Table 3 is less than thirty minutes from the border, and the first row reports the difference-in-difference estimate for this group: -3.5% in July, 3.2% in October, and 2.8% in January. Although the preliminary look at the raw data discussed above suggests smaller estimates for stations farther from the border in July comparison, this is not the case when the models with full controls are considered. The effect is slightly larger at thirty to sixty minutes away where summing the coefficient on IL/IN*Post and IL/IN*Post*1(>=30minutes and <60minutes) reveals a larger difference-in-difference estimate of -4.2%. The effect drops slightly at sixty to ninety minutes away from the border (-3.6%), and is smaller still at stations even farther away from the border (-2.4%). None of these differences across distance categories are statistically significant, however.

In October, the effect found in the raw data is present when controlling for observable characteristics: a difference-in-difference estimate of 3.2% at the border, remaining flat at 3.3% between thirty minutes and one hour from the border, but increasing to 4.3% over an hour away from the border. These differences are statistically significantly different and represent pass-through estimates increasing from 64% to 86%.

In January, the shape of the effect found in the raw data is again evident in the model with controls, with smaller treatment effects at the border, large treatment effects sixty to ninety minutes from the border, and smaller effects for stations far from the border. This decline in the effect far from the border was unexpected and may reflect the fact that the stations very far from the borders in the comparison states and the stations in

the very middle of Illinois are less comparable. Again, the January results should be taken with some caution given the relatively smaller sample sizes.

Another way to consider border competition is to consider whether the difference-in-difference estimates are smaller when stations directly across the border from one another are compared. Table 4 considers such stations by considering Metropolitan Statistical Areas (MSAs) that overlap state borders, and all stations along the treated state borders. MSAs are defined by commuting patterns, which may provide a reasonable market definition for gasoline consumers.

Column (1) considers Chicagoland, which, up to now, has been excluded from the analysis due to the lack of comparability among ZIP code characteristics and the size of the price spike that led to the tax repeal. When stations within the Chicago-Naperville-Joliet, IL-IN-WI, Metropolitan Statistical Area are considered for July, the model compares stations in Chicago and Gary, IN, with stations just across the Illinois/Wisconsin state border. Only three percent of the stations are in the comparison group, but the estimate suggests a 2.7% decline in prices in the treated stations relative to the Wisconsin stations—similar to the estimate found in the larger sample. In the October and January comparisons, there are more stations in the comparison groups: 86% in October and 14% in January. The estimated effect is much smaller in October (0%) when stations largely in Gary, IN, are compared to Chicago and Wisconsin stations within Chicagoland. The estimate is somewhat smaller in January (2.7%) when Chicago stations are compared to the Wisconsin and Indiana stations. The smaller effects found when the tax was reinstated may reflect differences in the comparison groups, but are also consistent with smaller treatment effects at the border.

The other border MSAs in July are St. Louis (MO-IL), Cincinnati (OH-IN), South Bend (IN-MI), Jackson (MO-IL), Davenport (IA-IL), and Burlington (IA-IL).[11] For stations in these overlapping MSAs, a similar model to those presented in Table 2 was estimated with MSA fixed effects. These models use within-MSA variation in the tax regimes and retail prices to estimate the effect of the tax reforms on retail prices. Table 4 shows that the difference-in-difference estimate is larger in July (-3.6%), slightly larger in October (4.2%), but smaller in January (1.5%). These estimates provide mixed evidence that the difference-in-difference estimates are indeed smaller near the border.

A third way to consider the border competition is to compare stations that are just across the border from one another. To estimate these effects, stations in Illinois/Indiana that are not in a border ZIP code were excluded, as well as stations outside Illinois/Indiana that are more than thirty minutes from the border. To compare the estimates to the main results, Chicagoland is also excluded. Then, an indicator for the "treated-state ZIP code" was created, which is the ZIP code for a station in Illinois/Indiana, and the nearest Illinois/Indiana ZIP code for stations outside of Illinois/Indiana. Table 4 presents estimates of models similar to those presented in Table 2, but with treated-state ZIP code fixed effects. The estimates are again larger in July (-5.6%), but smaller in October (3.1%) and January (1.3%). While the evidence is again somewhat mixed in terms of the larger effects for July and smaller effects for October and January, the reinstatements suggest smaller difference-in-difference estimates for stations that compete along the border compared to stations farther from the border.

[11] In order to compare the results with previous estimates in Table 2, we do not include the MSAs along the Kentucky border in this analysis.

Overall, the results on distance suggest smaller difference-in-difference results when analyzed close to the border, especially for the reinstatements. This is consistent with the effect of the tax extending across state borders. The October results appear to be more stable given the relatively larger sample size in October, and the lack of a major driving holiday that may affect the results. These estimates suggest at 3.2% increase at the border and a 4.3% increase farther from the border. The effect of the tax on retail prices for stations continues to suggest effects on retail prices of between 3% and 4% following the suspension and reinstatement of the 5% sales tax.

Competitive Environment

Another way the pass-through rates can differ is when the market conditions differ. The temporary nature of the moratorium implies that it should not affect the structure of the market. To the extent entry barriers are low, each market may be competitive, whereas zoning regulations may result in high barriers in some locations. One caveat is that the number of stations likely reflects population density as well, so it is important to control for population and area while investigating any relationship between number of stations and the price response to the tax changes.

Table 5 considers how the difference-in-difference estimates vary across different types MSAs and ZIP codes defined by the number of gasoline stations. The comparison is how geographic areas with many gasoline stations in the treated states respond to the tax change relative to areas with similar numbers of gasoline stations in the neighboring states to control for common trends that may differ by market structure. MSAs were chosen as they reflect commuting patterns and may reflect the geographic market, while

ZIP codes may reveal differences if markets are much smaller. Results were similar when only the treated states were considered.

The results suggest little relationship between the number of stations and the response to the tax change, with a smaller effect found for MSAs with more stations in July and January, but a larger effect found for October. Panel A reports the coefficient on the interaction between the natural logarithm of the number of gasoline stations reported to the Census bureau, an indicator for Illinois/Indiana, and an indicator for Post reform. The models also include the main effects as well as two-way interactions between these variables. The first three columns consider the MSA level and the data are restricted to stations within MSAs, where the average MSA has 544 stations. The estimates suggest that a doubling of the number of stations decreases the size of the price decline in July by 0.5 percentage points, an increase in the effect of the tax increase in November (by 0.4 percentage points), and an imprecisely estimated but small decrease in January (by 0.1 percentage points). At the ZIP code level, similar estimates are found in July and October, though the effect is found to be larger in January with an increase in stations (by 0.7 percentage points).

To explore the idea that the effect may not be log-linear in the number of stations, panel B breaks the samples into quartiles based on the number of stations. Estimates are reported for the interaction between IL/IN and Post reform, representing the difference-in-difference estimate for the excluded quartile: areas with the fewest numbers of stations. The three-way interactions of IL/IN, Post Reform, and the top three quartiles are reported to measure the difference in the estimated effect of the tax reform as the number of stations increase. In July, the average number of stations in each quartile was

36, 113, 441, and 1307 for the MSA comparison and 2, 7, 12, and 20 for the ZIP code categories.

The results largely mirror the results in Panel A. In July, the number of stations in the MSA is not related to the price response until the top quartile, when the estimated effect becomes much smaller. It should be noted, however, that only 5% of the stations in the top quartile are in the treated states in July. The October reinstatement is associated with an increasing effect with the number of stations, while the January reinstatement is associated with a slight rise and fall of the effect with the number of stations. At the ZIP code level, no relationship with the number of stations is found in July or October, while the January reinstatement suggests a rise in the estimated effect in the third quartile.

The mixed results, coupled with the size of the estimated effects, suggest that the earlier results are fairly robust to the type of market considered. If markets with many stations are thought to be competitive, then the robustness of the results across markets with different numbers of stations is consistent with gasoline markets requiring only a few stations to be as competitive as those with many more stations.

Another way market sizes can differ is access to an interstate highway, as ease of travel may increase the number of competitors. Stations far from an interstate may face less elastic consumers who cannot quickly get to other stations or delay purchase, and supply may be less elastic as well given the potentially narrower market. The result on pass-through is therefore ambiguous, and the empirical results can provide some insight into which effect may dominate. When ZIP codes with an Interstate highway were compared to ZIP codes without an interstate, the results are mixed, however. The effect

of the tax change on prices is found to be 1 percentage point larger in July for the stations far from an interstate, no difference in October, and 1 percentage point smaller in January, though all three differences were not statistically significantly different.[12]

Last, differences in demand may impact the price response to the tax changes. To test for these differences, ZIP codes were broken into two groups according to the median household income. The average income level in the bottom half of ZIP codes is roughly $34,000, while the average ZIP code in the top half has an income level of $51,000. Higher income consumers may be less elastic, and the pass through may be larger as a result. The results provide mild evidence that the pass-through increases with income level, with the effect of the reform on prices relatively flat with respect to income in July, the effects were found to be 0.5 percentage points higher in the wealthier ZIP codes in October and January. Taken together, the results appear similar across different types of neighborhoods.

Specification Checks

The previous results suggest that the effect of the tax changes on retail prices are fairly robust across increases and decreases in the tax rate, across different types of ZIP codes, and across space with smaller effects at the border. Table 6 provides additional tests that suggest the main results are robust to the choice of sample and estimators.

Spatial Autocorrelation

So far the standard errors have been clustered at the state level to provide conservative estimates. Another approach would be to directly model the spatial autocorrelation in the data. Using the latitude and longitude of each station's ZIP code, it

[12] One caveat in these results is that the interstate runs through these ZIP codes, but there may not be an exit within the ZIP code.

is possible to describe the stations according to a distance grid. The standard errors can then be estimated using a two-dimensional spatial autocorrelation structure set out in Conley (1999).[13] Column (1) reports the results when a model of retail prices that controls for wholesale prices (as in the second column of Table 2) is estimated and the standard errors are corrected for spatial autocorrelation.[14] The results show that the standard error estimates are fairly robust to the estimation method, with estimates that are slightly smaller than the clustered standard errors for July and slightly larger in January. Meanwhile, in October the estimates reveal some instability in the estimation as the standard errors become unrealistically small.

Expanded Timeframe

One issue with the above analysis is that it focused on the days just before and just after the reform, similar to an event study.[15] This was justified in part by the lack of pre-existing trends in the price differences shown in Figure 2. Another way to test the robustness of this approach is to examine a longer timeframe. Column (2) of Table 6 reports the results of models that include data from one month before and one month after the tax changes as in Figure 2. The models include the full controls, as well as quadratic trends allowed to vary across comparison groups before and after the tax changes. The time trend is centered at the reform date so that the coefficient on the interaction between the Illinois/Indiana indicator and the post-reform indicator provides the difference-in-

[13] The correction calls for cutoffs after which the information is no longer incorporated into the correction. The estimates presented here used two times the standard deviation of latitude/longitude degrees in the data.

[14] When the full set of controls is included, the estimated standard errors become much smaller. The more parsimonious specification here provides estimates that are closer to the conservative ones presented throughout the paper.

[15] The estimates here used prices observed two days before and after the tax reforms to increase the number of stations observed, especially for the January comparison where many stations were not observed during the holiday season. When one day before and after the reforms were considered, the estimates were smaller in July (-2.0%), similar in October (3.7%), and slightly larger in January (4.3%).

difference estimate at the time of the reform. The result in July is fairly similar, a 3.3% decline at the time of the tax change. A smaller increase is found in October (3.2%), while the change is slightly larger in January (4.2%). The results hinge on the way time trends are controlled and there is a risk of overfitting that can contaminate the comparison. When linear trends are used instead of linear and quadratic, the coefficient estimates are -1.2% in July, 4.4% in October, and 0.8% in January. The main results are between these linear and quadratic results, and, given the lack of pre-existing trends in the October and January retail price differences, it appears that the short-window results presented earlier are fairly robust. Meanwhile, the July results that appear most sensitive to the pre-existing trends in Figure 2 are actually similar when quadratic time trends and the full set of controls are included.

Another way to be more flexible regarding timeframes is to consider data ten days before and ten days after the reforms, with indicators for each day to trace out the effect over time. Table 2A in the appendix displays the results for a model that includes indicators for each day (excluding the day before the tax change). The model includes a Post Reform indicator which can be interpreted as the change in price after the reform, while the post-reform daily indicators represent the difference from the first day after the reform. The estimates at the time of the reform are smaller in July (-2.2%), similar in October (3.7%), and larger in January (4.6%). The differences appear stable before and after the reforms, especially within one week of the reforms. No increase in price is seen just prior to the reinstatements, which may have been expected given the potential for an increase in demand. A drop in the effect is seen after day nine of the tax reinstatement in Indiana, though this appears to be a temporary drop when later data are considered.

Meanwhile, the effect in January is found to fade somewhat over the ten days after the reform, though again this is temporary as suggested by Figure 2.

The last column in Table 6 reports the results including Chicago and Kentucky in the analysis. Not surprisingly given the Chicago MSA results in Table 4, the results are somewhat smaller with the larger sample. The results are qualitatively similar, however, with estimates of -2.5%, 3.2%, and 3.3%. While the pre-existing trends and mean comparisons reflect a better comparison when these two areas are excluded, the results are largely robust when they are included.

Over the many estimates, the October results appear to be more stable as they do not have a major driving holiday at the same time as the reform. One way to consider whether holidays always have a differential effect in Illinois/Indiana compared to the neighboring states is to consider a difference-in-difference estimate just before and after Memorial Day when no tax policy change was in effect. When this was estimated no effect was found (a coefficient of 0.003). Another way to consider the effect of the July 4th and January 1st holidays is to consider whether other states see differential price changes across state borders. To test this idea, models were estimated similar to those in Table 2, but for gas stations in Pennsylvania and New York. To mimic those results that exclude Chicago, New York City was excluded from the comparison. Cross-border differences in retail prices are close to zero when these two "untreated" states are compared with estimates of -0.6%, 0.1%, and -0.2% for July 1, October 31, and January 1, respectively.[16]

[16] The lack of difference between NY and PA remains when NYC is included, with estimates of -0.8%, 0.1%, and -0.1% in July, October, and January, respectively.

Other dates that may show a difference are September 1st, September 15th, and September 30th: the dates the Indiana reform was set to expire prior to the Governor's extensions. No difference is found for these dates, with difference-in-difference estimates of 0.2%, -0.9%, and 0.3%, respectively. Last, the Indiana press announced that the end of the suspension would be October 25th, but Figure 2 and Table A2 show that there was no change in retail prices at that time. The jump in price occurs at the 120 day mark allowed for by law, suggesting that this is when the tax change went into effect.

A final specification check considers the comparisons of Illinois and Indiana separately for the July suspensions. Retailers in the two states may have reacted differently to the reform due to other regulations in place, such as the Illinois mandate that stations alert consumers to the sales tax repeal. When Illinois versus Wisconsin, Iowa, and Missouri is considered, the difference-in-difference estimate is -3.1% for July and 3.6% for January. Similarly, for the Indiana versus Michigan and Ohio comparison, the difference-in-difference estimates are -2.7% and 4.5%. These specification checks suggest that the results are not very sensitive to the treatment of time, the influence of holidays, and the choice of comparison states.

6. Conclusion

When gasoline prices spike, governments are under some pressure to respond to the volatility by cutting taxes. Illinois estimates that the state lost $157 million in tax revenue (Noggle, 2005), while Indiana estimates a loss of $46 million (Nass, 2000). One question is how much of a reduction in retail prices did the tax suspension buy? Further, despite a great deal of attention paid to the incidence of taxation, surprisingly few

empirical studies of the pass-through rates of sales taxes have been conducted. Using a unique dataset of gasoline station prices, and a plausibly exogenous change in tax rates, it is possible to estimate the effect of a tax change on gasoline prices, at least in the short run.

The estimates here suggest that the suspension of the 5% sales tax led to decreases in retail prices of 3% compared to neighboring states. When the tax was reinstated, retail prices rose by roughly 4%. The reinstatement estimates are particularly compelling given that the timing of the reinstatement was not based on market conditions, but rather a 1981 law in the case of Indiana, and the end of the calendar year in the case of Illinois.

The results also suggest that the difference-in-difference estimates are smaller when stations across the border from one another are considered. These estimated differences increase up to approximately one hour from the state border, suggesting that competitive pressures may extend that far into neighboring states. In particular, when the October reinstatement is considered, which has relatively more stations observed and no major holiday to affect the results, the retail prices are found to increase by 3% at the border and 4% for stations more than an hour from the border. These results also suggest that stations in the neighboring states, particularly those farther from the state border, provide a useful comparison group to test the effect of the tax changes on retail prices.

Little association was found between the number of gasoline stations in the city or ZIP code and the effect of the tax change on prices. If areas with many gasoline stations are thought to be competitive, then this result is consistent with gasoline markets requiring only a few gas stations to be competitive.

Meanwhile, the differences in prices across comparison groups are found to be stable prior to the tax reinstatements, consistent with the timing of the reinstatements providing a plausibly exogenous change in the tax rate. The effects are fairly robust across different types of ZIP codes, time periods, and comparison states, and suggest pass through rates of between 60 and 80%.

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Figure 1: Retail Gasolline Prices in Midwest: April 2000-April 2001

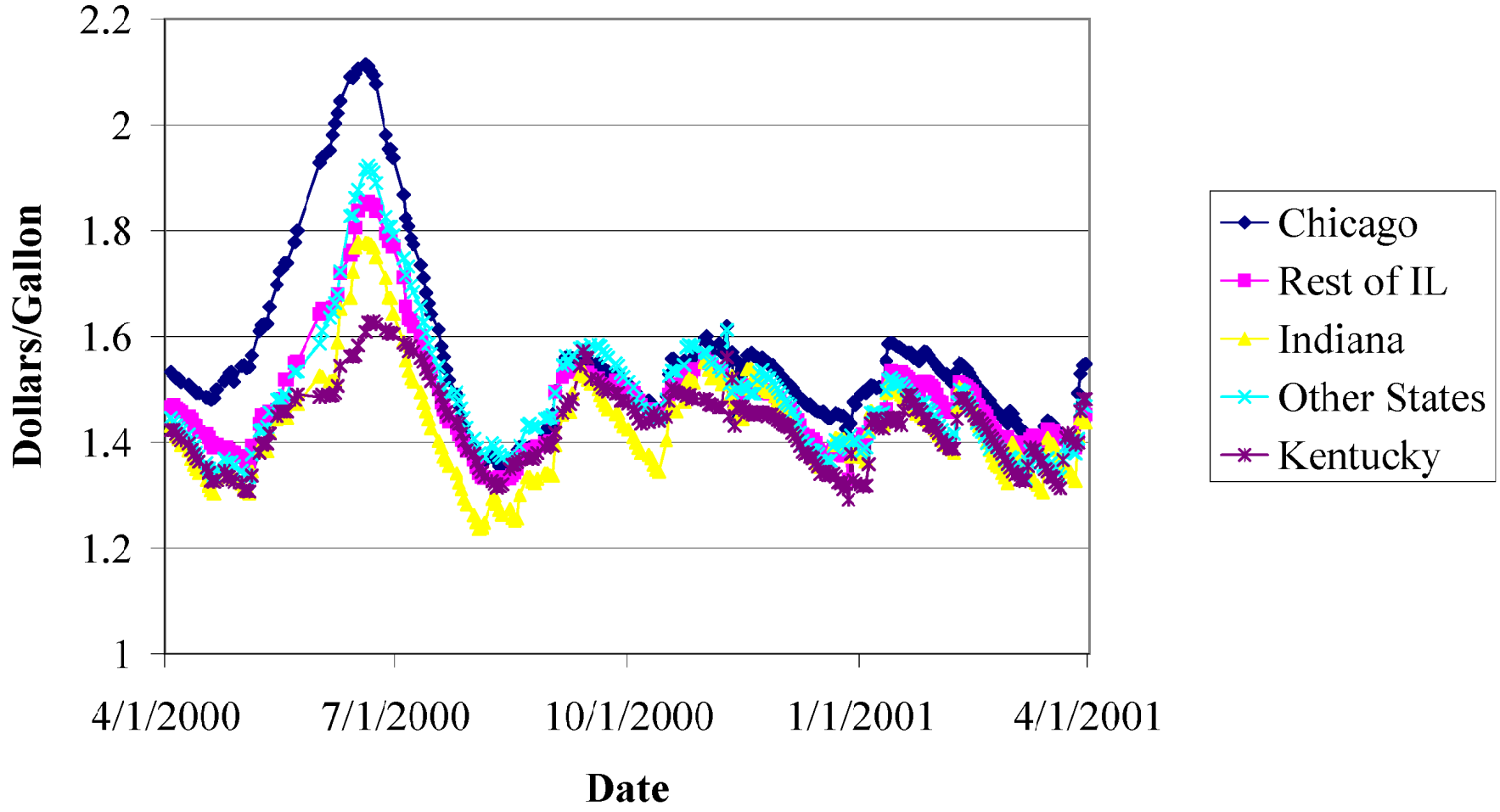


Figure 2A: Summer 2000 Difference in Log Gas Prices
IL/IN vs. Neighboring States: MI, OH, MO, IA, WI

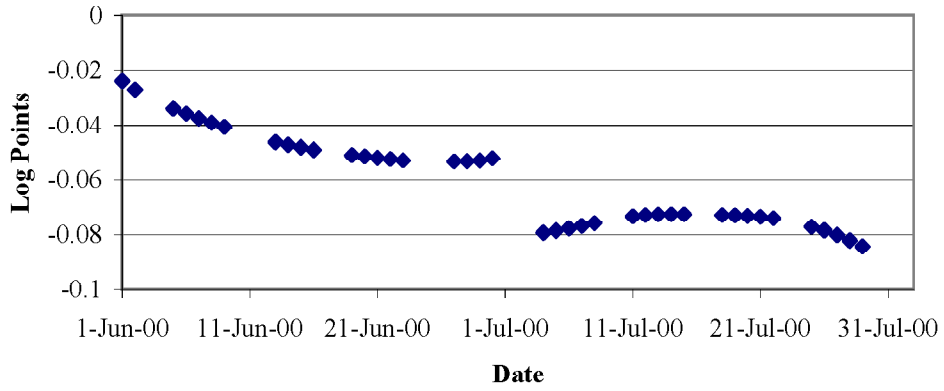


Figure 2B: Fall 2000 Difference in Log Gas Prices
IN vs. Neighboring States: MI, OH, IL

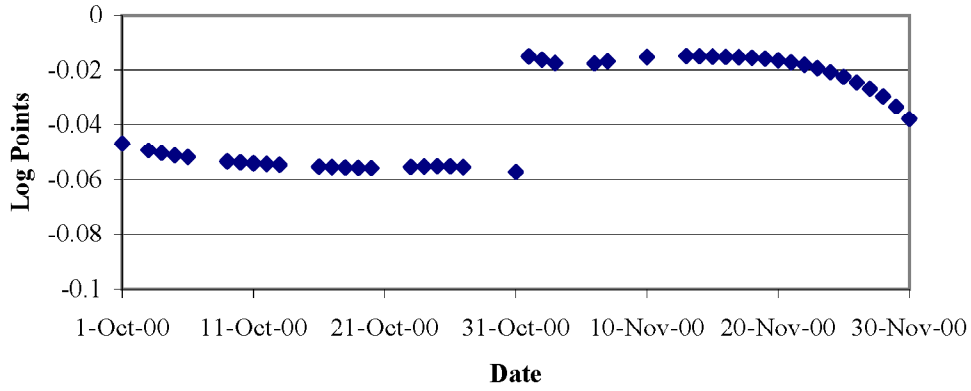


Figure 2C: Winter 2000/2001 Difference in Log Gas Prices
IL vs. Neighboring States: MO, IA, WI, IN

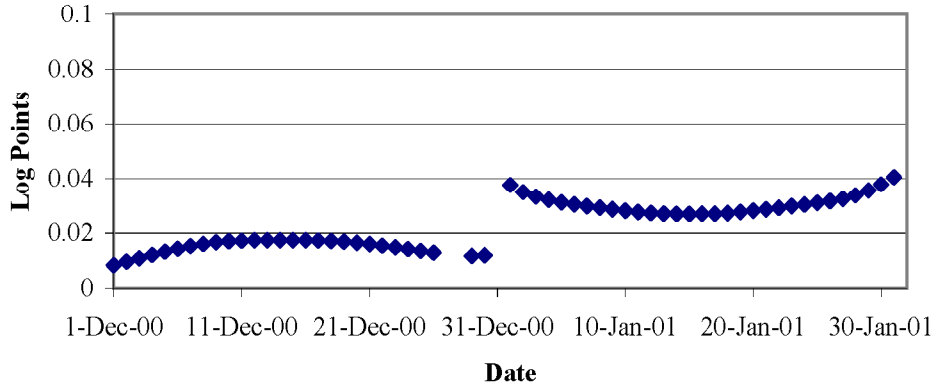


Figure 3A: Change in Retail Price: July

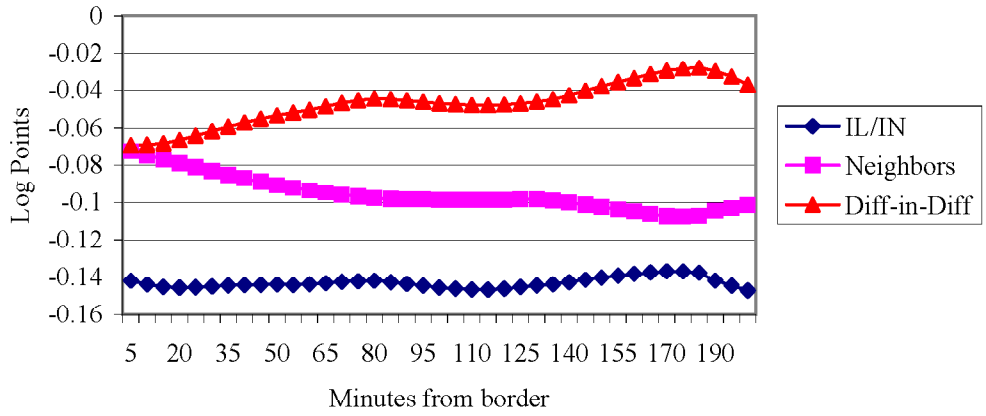


Figure 3B: Change in Retail Price: October

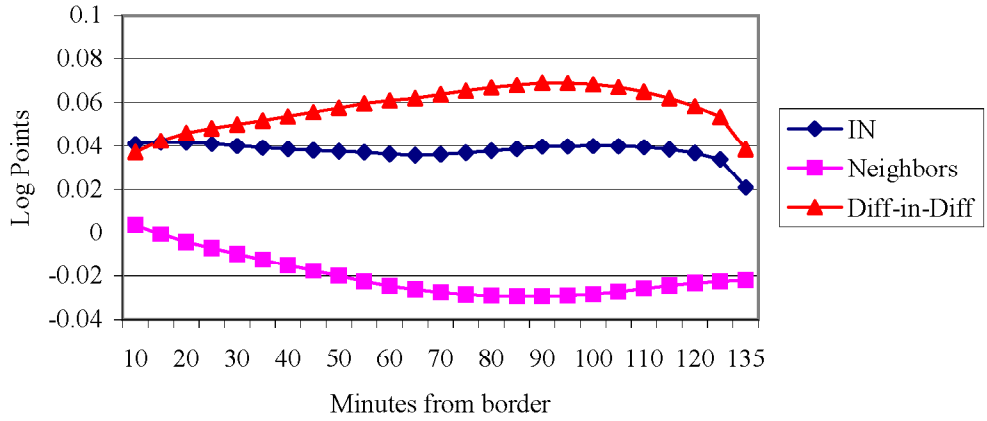


Figure 3C: Change in Retail Price: January

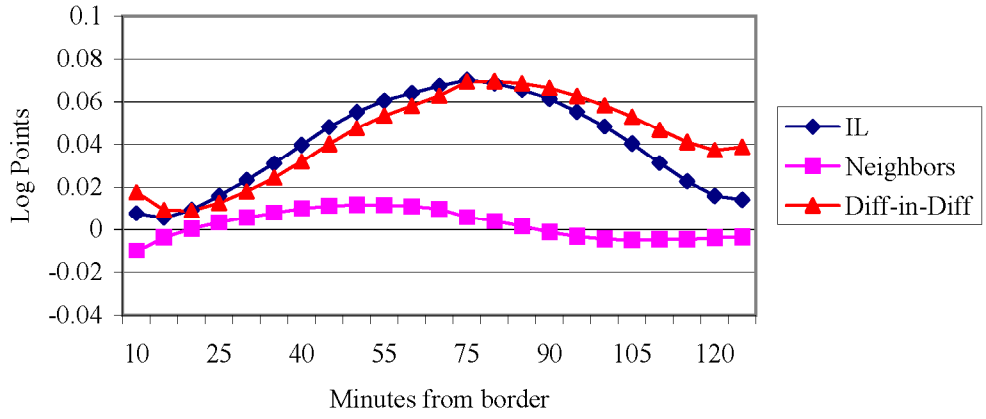


Table 1: Selected Summary Statistics: July 2000

		Illinois & Indiana		Neighboring States	
Variable		Mean	Std. Dev.	Mean	Std. Dev.
Prices	retail price	1.67	0.12	1.78	0.14
	log(retail price)	0.51	0.07	0.57	0.08
	wholesale price	1.08	0.06	1.12	0.08
	log(wholesale price)	0.07	0.05	0.11	0.07
ZIP Code	population	19221	13312	21121	15005
Characteristics	area (square miles)	73.78	61.08	69.80	76.16
	gas stations	9.76	7.04	9.75	7.21
	income	41515	9776	43027	12431
Race	white	0.89	0.12	0.87	0.18
	black	0.05	0.10	0.07	0.16
	Hispanic	0.03	0.03	0.02	0.04
Age	age 0-18	0.27	0.03	0.26	0.04
	age 19-34	0.21	0.06	0.21	0.06
	age 35-64	0.38	0.04	0.39	0.04
	age 65+	0.14	0.04	0.14	0.04
Education	less than high school	0.18	0.07	0.16	0.07
	high school	0.37	0.08	0.35	0.09
	some college	0.27	0.04	0.28	0.05
	college	0.19	0.11	0.21	0.12
Commuting	drive alone	0.82	0.05	0.82	0.07
	car pool	0.11	0.03	0.10	0.04
	public transport	0.01	0.01	0.01	0.03
	other transport	0.03	0.03	0.03	0.03
	work at home	0.03	0.01	0.03	0.02
	0-10 minutes	0.23	0.09	0.22	0.10
	10-20 minutes	0.33	0.10	0.32	0.10
	20-30 minutes	0.19	0.08	0.20	0.07
	30-45 minutes	0.15	0.07	0.16	0.07
	45-60 minutes	0.05	0.03	0.05	0.04
	60+ minutes	0.05	0.02	0.05	0.03

Neighboring states: MI, OH, MO, IA, WI. Prices observed June 27, June 28, July 5, & July 6
5945 Illinois observations; 23488 Neighboring State observations.

Table 2: Regression Results

A: July Tax Repeal

Dependent Variable:	Log(Retail Price)			Log(Wholesale Price)
	(1)	(2)	(3)	(4)
Illinois or Indiana	-0.048	-0.013	-0.014	-0.035
	(0.038)	(0.025)	(0.021)	(0.017)
Post July 1	-0.052	0.029	0.025	-0.088
	(0.007)	(0.013)	(0.015)	(0.006)
(IL or IN)*Post July 1	-0.035	-0.029	-0.029	-0.007
	(0.007)	(0.008)	(0.008)	(0.006)
Observations	29675	29675	29433	29433
R-Squared	0.23	0.60	0.64	0.57
Mean of Dep. Var.	0.560	0.560	0.560	0.560

B: October Tax Reinstatement

Dependent Variable:	Log(Retail Price)			Log(Wholesale Price)
	(1)	(2)	(3)	(4)
Indiana	-0.056	-0.052	-0.053	-0.015
	(0.009)	(0.007)	(0.007)	(0.001)
Post Oct. 31	-0.014	-0.008	-0.009	-0.021
	(0.006)	(0.006)	(0.006)	(0.001)
IN*Post Oct. 31	0.039	0.040	0.040	-0.005
	(0.006)	(0.007)	(0.006)	(0.002)
Observations	22092	22092	21884	21884
R-Squared	0.16	0.18	0.26	0.41
Mean of Dep. Var.	0.457	0.457	0.456	0.456

C: January Tax Reinstatement

Dependent Variable:	Log(Retail Price)			Log(Wholesale Price)
	(1)	(2)	(3)	(4)
Illinois	0.019	-0.001	-0.005	0.029
	(0.035)	(0.024)	(0.021)	(0.007)
Post Jan. 1	-0.000	-0.038	-0.020	0.051
	(0.004)	(0.004)	(0.004)	(0.002)
IL*Post Jan. 1	0.027	0.036	0.037	-0.014
	(0.004)	(0.004)	(0.004)	(0.003)
Observations	7090	7090	7071	7071
R-Squared	0.04	0.24	0.39	0.41
Mean of Dep. Var.	0.303	0.303	0.303	0.303
Controls:				
Wholesale Price	No	Yes	Yes	-
ZIP Codes Characteristics	No	No	Yes	Yes

Panel A: Prices observed June 27, June 28, July 5, July 6; Panel B: Prices observed Oct. 26, Oct. 27, Oct. 31, Nov. 1
Panel C: Prices observed Dec. 29, Dec. 30, Jan. 2, Jan. 3. Standard errors are reported, clustered at the state level.

Table 3: Distance to Border

Dependent Variable:	Log(Retail Price)		
	(1)	(2)	(3)
Time Period	July Repeal	Oct. Reinstatement	Jan. Reinstatement
IL/IN*Post	-0.035	0.032	0.028
	(0.012)	(0.009)	(0.006)
IL/IN*Post*1(>=30min & <60min)	-0.007	0.001	0.009
	(0.006)	(0.003)	(0.004)
IL/IN*Post*1(>=60min & <90min)	-0.001	0.012	0.053
	(0.007)	(0.002)	(0.009)
IL/IN*Post*1(>=90min)	0.011	0.011	-0.012
	(0.010)	(0.003)	(0.004)
Observations	29433	21884	7071
R-Squared	0.66	0.28	0.42
Mean of Dep. Var.	0.560	0.456	0.303

Column(1): Neighboring states: MI, OH, MO, IA, WI & Prices observed June 27, June 28, July 5, July 6
Column(2): Neighboring states: MI, OH, IL & Prices observed Oct. 26, Oct. 27, Oct. 31, Nov. 1
Column(3): Neighboring states: MO, IA, WI, IN & Prices observed Dec. 29, Dec. 30, Jan. 2, Jan. 3.
All models include full controls. Standard errors are reported, clustered at the state level.

Table 4: Border Fixed Effects

A: July Tax Repeal (IL/IN Border)

Dependent Variable:	Log(Retail Price)		
	(1)	(2)	(3)
	Chicago MSA	Other Border MSAs	<30 Minutes
Illinois or Indiana	0.066	0.028	0.009
	(0.001)	(0.010)	(0.016)
Post July 1	-0.027	-0.028	-0.023
	(0.001)	(0.006)	(0.009)
(IL or IN)*Post July 1	-0.027	-0.036	-0.056
	(0.010)	(0.008)	(0.013)
Observations	4037	2900	1858
R-Squared	0.73	0.79	0.80
Mean of Dep. Var.	0.640	0.496	0.506

B: October Tax Reinstatement (IN Border)

Dependent Variable:	Log(Retail Price)		
	(1)	(2)	(3)
	Chicago MSA	Other Border MSAs	<30 Minutes
Indiana	-0.050	-0.049	-0.043
	(0.007)	(0.005)	(0.005)
Post Oct. 31	0.023	-0.013	-0.007
	(0.001)	(0.000)	(0.007)
IN*Post Oct. 31	0.001	0.042	0.031
	(0.001)	(0.000)	(0.008)
Observations	4040	1251	1047
R-Squared	0.65	0.45	0.61
Mean of Dep. Var.	0.453	0.460	0.449

C: January Tax Reinstatement (IL Border)

Dependent Variable:	Log(Retail Price)		
	(1)	(2)	(3)
	Chicago MSA	Other Border MSAs	<30 Minutes
Illinois	0.047	0.037	0.023
	(0.019)	(0.008)	(0.010)
Post Jan. 1	0.001	0.019	0.012
	(0.002)	(0.001)	(0.003)
IL*Post Jan. 1	0.027	0.015	0.013
	(0.003)	(0.003)	(0.003)
Observations	2449	1098	692
R-Squared	0.75	0.54	0.63
Mean of Dep. Var.	0.394	0.257	0.267
MSA Fixed Effects	No	Yes	No
IL/IN Border ZIP Fixed Effects	No	No	Yes

Panel A: Neighboring states: MI, OH, MO, IA, WI & Prices observed 6/27, 6/28, 7/5, 7/6
Panel B: Neighboring states: MI, OH, IL & Prices observed 10/26, 10/27, 10/31, 11/1
Panel C: Neighboring states: MO, IA, WI, IN & Prices observed 12/29, 12/30, 1/2, 1/3
Column (3): Stations closest to KY excluded.
All models include full controls. Standard errors are reported, clustered at the state level.

Table 5: Number of Gas Stations in the same MSA or ZIP Code

	MSA Level			ZIP code Level		
Dependent Variable:	Log(Retail Price)					
A. Interaction with Number of Stations						
	(1)	(2)	(3)	(4)	(5)	(6)
	July Repeal	Oct. Reinstatement	Jan. Reinstatement	July Repeal	Oct. Reinstatement	Jan. Reinstatement
IL/IN*Post*log(Number of Stations)	0.0050	0.0038	-0.0010	.0044	.0031	.0072
	(0.0020)	(0.0020)	(0.0031)	(0.0028)	(0.0020)	(0.0017)
Observations	25324	19797	6665	29765	21884	7071
R-Squared	0.66	0.26	0.47	0.62	0.28	0.41
Mean of Dep. Var.	0.558	0.455	0.302	0.560	0.456	0.303
B. Interaction with Number of Station Quartiles						
	(1)	(2)	(3)	(1)	(2)	(3)
	July Repeal	Oct. Reinstatement	Jan. Reinstatement	July Repeal	Oct. Reinstatement	Jan. Reinstatement
IL/IN*Post	-0.032	0.031	0.044	-0.031	0.039	0.039
	(0.007)	(0.009)	(0.003)	(0.007)	(0.007)	(0.004)
IL/IN*Post*Number of Stations 2nd Quartile	-0.005	0.010	0.015	0.002	-0.002	-0.003
	(0.005)	(0.006)	(0.007)	(0.002)	(0.003)	(0.006)
IL/IN*Post*Number of Stations 3nd Quartile	0.003	0.020	0.004	-0.001	0.000	0.017
	(0.006)	(0.009)	(0.014)	(0.003)	(0.003)	(0.002)
IL/IN*Post*Number of Stations Top Quartile	0.029	0.032	-0.019	0.003	0.005	-0.006
	(0.009)	(0.007)	(0.006)	(0.002)	(0.005)	(0.006)
Observations	25324	19797	6665	29433	21884	7071
R-Squared	0.66	0.27	0.50	0.64	0.28	0.41
Mean of Dep. Var.	0.558	0.455	0.302	0.560	0.456	0.303

Column(1): Neighboring states: MI, OH, MO, IA, WI & Prices observed June 27, June 28, July 5, July 6
Column(2): Neighboring states: MI, OH, IL & Prices observed Oct. 26, Oct. 27, Oct. 31, Nov. 1
Column(3): Neighboring states: MO, IA, WI, IN & Prices observed Dec. 29, Dec. 30, Jan. 2, Jan. 3.
All models include full controls. Standard errors are reported, clustered at the state level.

Table 6: Specification Checks

A: July Tax Repeal

Dependent Variable:	Log(Retail Price)		
	(1) S.E. Corrected for Spatial Autocorrelation	(2) Timeperiod: +/- 1 Month	(3) Include Chicago & KY
Illinois or Indiana	-0.013	-0.026	-0.002
	(0.014)	(0.028)	(0.018)
Post July 1	0.029	-0.005	0.025
	(0.010)	(0.013)	(0.011)
(IL or IN)*Post July 1	-0.029	-0.033	-0.025
	(0.006)	(0.009)	(0.010)
Observations	29675	351112	36689
R-Squared	0.60	0.75	0.65
Mean of Dep. Var.	0.560	0.497	0.561

B: October Tax Reinstatement

Dependent Variable:	Log(Retail Price)		
	(1) S.E. Corrected for Spatial Autocorrelation	(2) Timeperiod: +/- 1 Month	(3) Include Chicago & KY
Indiana	-0.052	-0.047	-0.042
	(0.0002)	(0.029)	(0.014)
Post Oct. 31	-0.008	-0.098	-0.001
	(0.00005)	(0.032)	(0.009)
IN*Post Oct. 31	0.040	0.032	0.032
	(0.0002)	(0.033)	(0.010)
Observations	21890	240473	29468
R-Squared	0.18	0.39	0.23
Mean of Dep. Var.	0.457	0.412	0.448

C: January Tax Reinstatement

Dependent Variable:	Log(Retail Price)		
	(1) S.E. Corrected for Spatial Autocorrelation	(2) Timeperiod: +/- 1 Month	(3) Include Chicago
Illinois	-0.001	-0.016	0.011
	(0.017)	(0.019)	(0.021)
Post Jan. 1	-0.038	-0.015	-0.020
	(0.007)	(0.009)	(0.004)
IL*Post Jan. 1	0.036	0.042	0.033
	(0.006)	(0.008)	(0.003)
Observations	7090	153951	11086
R-Squared	0.24	0.45	0.55
Mean of Dep. Var.	0.303	0.344	0.319

All Models include full controls, except Column(1), which includes log(wholesale price) only.

Neighboring states: Columns (1) & (2) as in previous tables; Column (3), includes KY for July and November.

Column (2): Data observed one month before and one month after tax change and models include quadratic trend terms allowed to vary by comparison group before and after the policy change.

Standard errors are reported, clustered at the state level, except Column (1).

Table A1: Full Regression Results

	Dependent Variable:	Log(Retail Price)		
		(1)	(2)	(3)
	Time Period	July Repeal	Oct. Reinstatement	Jan. Reinstatement
Location & Time	Illinois or Indiana	-0.014	-0.053**	-0.005
		(0.020)	(0.007)	(0.021)
	Post Reform	0.025	-0.009	-0.020**
		(0.015)	(0.006)	(0.004)
	(IL or IN)*Post Reform	-0.029**	0.040**	0.037**
		(0.008)	(0.006)	(0.004)
Price	log(wholesale price)	0.891**	0.265	0.623**
		(0.142)	(0.098)	(0.099)
ZIP Code Characteristics	population	-2.33e-07	-9.71e-08	2.26e-08
		(1.99e-07)	(2.15e-07)	(2.44e-07)
	area (square miles)	2.77e-05	6.65e-05	1.04e-05
		(2.49e-05)	(2.29e-05)	(5.17e-05)
	gas stations	-0.0002	-0.0002	-0.001
		(0.001)	(0.0001)	(0.0004)
	income	8.96e-07*	0.000	6.92e-07
		(3.14e-07)	(0.000)	(4.04e-07)
Race excluded: white	black	0.020	-0.004	-0.006
		(0.025)	(0.010)	(0.030)
	Hispanic	0.088**	-0.036*	0.066
		(0.023)	(0.006)	(0.081)
Age excluded: age 0-18	age 19-34	0.057	0.003	-0.005
		(0.041)	(0.022)	(0.095)
	age 35-64	0.124	0.041	0.100
		(0.056)	(0.042)	(0.060)
	age 65+	0.088	-0.050	-0.116
		(0.045)	(0.031)	(0.116)
Education excluded: HS drop out	high school	0.057	-0.004	0.386*
		(0.145)	(0.064)	(0.109)
	some college	0.116	0.009	0.191
		(0.068)	(0.032)	(0.162)
	college	-0.002	0.046	0.208*
		(0.070)	(0.038)	(0.061)
Commuting excluded: drive alone, work at home and 0-10 minutes	car pool	0.087*	0.114**	0.154
		(0.028)	(0.016)	(0.062)
	public transport	0.251	0.046	0.305
		(0.130)	(0.112)	(0.212)
	other transport	0.168	0.070	0.249
		(0.071)	(0.052)	(0.103)
	10-20 minutes	-0.092	-0.010	-0.124
		(0.041)	(0.031)	(0.044)
	20-30 minutes	-0.088*	0.003	-0.082
		(0.030)	(0.035)	(0.046)
	30-45 minutes	-0.103	0.012	-0.226*
		(0.066)	(0.019)	(0.072)
	45-60 minutes	-0.141	0.005	-0.281
		(0.064)	(0.051)	(0.113)
	60+ minutes	-0.057	0.047	-0.020
		(0.077)	(0.051)	(0.081)
	Observations	29433	21884	7071
	R-Squared	0.64	0.26	0.39
	Mean of Dep. Var.	0.560	0.456	0.303

Column (1): Prices observed June 27, June 28, July 5, July 6; Column (2): Prices observed Oct. 27, Oct. 31, Nov. 1, Nov. 2; Column (3): Prices observed Dec. 29, Dec. 30, Jan. 2, Jan. 3. Standard errors are reported, clustered at the state level.
Models also include indicators for major brands. *=significant at 5%, **=significant at 1%.

Table A2: Expaned Timeframe

A: July Tax Repeal

Dependent Variable:	Log(Retail Price)		
	(1)	(2)	(3)
Time Period	July Repeal	Nov. Re-Instatement	Jan. Re-Instatement
IL/IN*1(t-10)	0.009	-0.000	0.025
	(0.031)	(0.016)	(0.010)
IL/IN*1(t-9)	0.012	0.002	0.020
	(0.032)	(0.008)	(0.013)
IL/IN*1(t-8)	0.013	0.002	0.005
	(0.030)	(0.009)	(0.009)
IL/IN*1(t-7)	0.003	-0.000	-0.000
	(0.025)	(0.009)	(0.008)
IL/IN*1(t-6)	0.002	0.000	0.003
	(0.025)	(0.010)	(0.007)
IL/IN*1(t-5)	0.006	0.003	0.006
	(0.020)	(0.010)	(0.007)
IL/IN*1(t-4)	0.002	-0.003	0.004
	(0.016)	(0.002)	(0.007)
IL/IN*1(t-3)	0.003	0.001	0.007
	(0.012)	(0.003)	(0.006)
IL/IN*1(t-2)	0.007	-0.002	0.009
	(0.008)	(0.002)	(0.006)
IL/IN*Post	-0.022**	0.037**	0.046**
	(0.004)	(0.005)	(0.007)
IL/IN*1(t+2)	-0.008	0.006	-0.001
	(0.003)	(0.002)	(0.006)
IL/IN*1(t+3)	-0.003	0.006	-0.016
	(0.005)	(0.003)	(0.012)
IL/IN*1(t+4)	-0.005	0.001	-0.016
	(0.005)	(0.006)	(0.018)
IL/IN*1(t+5)	0.007	0.003	-0.020
	(0.007)	(0.007)	(0.014)
IL/IN*1(t+6)	0.002	-0.005	-0.019
	(0.008)	(0.012)	(0.012)
IL/IN*1(t+7)	0.003	-0.000	-0.022*
	(0.009)	(0.014)	(0.008)
IL/IN*1(t+8)	0.006	0.010*	-0.030**
	(0.009)	(0.003)	(0.002)
IL/IN*1(t+9)	0.005	-0.032**	-0.025**
	(0.012)	(0.003)	(0.002)
IL/IN*1(t+10)	0.005	-0.033**	-0.032*
	(0.011)	(0.002)	(0.010)
Observations	168942	113678	47688
R-Squared	0.75	0.37	0.46
Mean of Dep. Var.	0.544	0.432	0.320

Omitted category is the day before the reform and the post reform coefficients represent the difference from the day after the reform.
Column(1): Neighboring states: MI, OH, MO, IA, WI
Prices observed 6/14,6/15,6/16,6/19,6/20,6/21,6/22,6/23,6/27,6/28,7/5,7/6,7/7,7/8,7/11,7/12,7/13,7/14,7/15,7/18
Column(2): Neighboring states: MI, OH, IL & Prices observed Oct. 27, Oct. 31, Nov. 1, Nov. 2
Prices observed 10/16,10/17,10/18,10/19,10/20,10/23,10/24,10/25,10/26,10/27,10/31,11/1,11/2,11/3,11/6,11/7,11/10,11/13,11/14,11/15
Column(3): Neighboring states: MO, IA, WI, IN & Prices observed Dec. 29, Dec. 30, Jan. 2, Jan. 3.
Prices observed 12/19,12/20,12/21,12/22,12/23,12/24,12/25,12/26,12/29,12/30,1/2,1/3,1/4,1/5,1/6,1/7,1/8,1/9,1/10,1/11
All models include full controls including main effects of the time indicators. Standard errors are reported, clustered at the state level.
*=significant at 5%, **=significant at 1%.