# Have soda sales tax effects changed over time? Scanner data comparison analyses

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**Abstract**

The scientific evidence on the effect of sugar consumption on obesity has propelled policy makers in several states across the U.S. to propose the imposition of a tax on soft drinks. In this paper, we look at the effect of two tax events: a 5.5% sales tax on soft drinks imposed by the state of Maine in 1991, and a 5% sales tax on soft drinks levied in Ohio in 2003. We investigate this question by using sales data collected by scanner devices in Maine, Massachusetts, New York and Connecticut, as well as Ohio, Illinois, Michigan and Pennsylvania. These samples comprise stores that account for more than 80% of all grocery sales nationwide and include brand-level sales data for the periods of study. We employ a difference-in-difference matching estimator (DIDM) that, in our setting, permits the comparison among treatment and control groups based on brand identity. Results suggest that sales tax had a statistically insignificant impact on the overall consumption of soft drinks. This finding is robust to several alternative specifications, and over time.

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## 1. Introduction

The rate of obesity in the U.S. is increasing dramatically. According to data from the Center for Disease Control and Prevention (CDC), the percentage of obese people in the U.S. has increased from 20% in 2000 to 27.5% in 2010. Americans consume about 25% to 30% more daily calories today than they did 30 years ago[1]. This large increase in calorie intake appears to have been significantly fueled by soda consumption; in 2009, a statement by the American Heart Association indicated that soft drinks and sugar sweetened beverages were the number one contributor of added sugars in Americans' diets. Consistent with this observation, several studies have shown how the consumption of soft drinks has significantly contributed to the increase in obesity, leading to a higher incidence of various diseases such as heart disease, diabetes, stroke, hypertension and cancer. For instance, Libuda and Mathilde (2009) in a review article find that prior research has consistently reported evidence in support of a causal relationship between soft drink consumption and excess weight gain. Similarly, the meta-analysis conducted by Vartanian et al. (2007) shows a clear association of soft drink intake with both increased energy intake and as well as body weight.

In addition to the scientific evidence on the effect of sugar consumption on obesity, it is important to note that soft drinks have a very limited nutritional value. These two facts have propelled policy makers in several states across the U.S. to propose the imposition of a tax on soft drink consumption. Policy interventions that modify the price of a good are supported by economic motivations based on market failures (Marshall 2000; Cawley 2004; Finkelstein, Ruhm, and Kosa 2005; Kim and Kawachi 2006; Powell and Chaloupka, 2009). Specifically, there are negative externalities associated with soft drinks consumption such as the increased health care costs of treating diseases caused by obesity. These can take the form of higher health insurance premiums and higher public health expenditures by the government. Additional social costs may consist of productivity losses (Cawley, 2004). Also, some people may have time-inconsistent preferences that would require public interventions (Powell and Chaloupka, 2009). For instance, children do not take into account the future consequences of their actions, and people, in general, may not appropriately discount the future costs of their behaviors (Komlos, Smith, and Bogin 2004; Smith, Bogin, and Bishai 2005).

Small excise taxes and special sales taxes on soda are already in place in 33 states. The Carbonated Soft Drinks (CSD) industry has succeeded in avoiding a soda tax to be included in the recent national health reform. Soda taxes have been proposed in at least 12 other states, though none of these proposals have yet been approved. The most effective way in which this tax should be imposed is not clear. Different proposals have been discussed, and they differ substantially across states. For example, Mississippi is considering legislation that would tax the syrup used to sweeten soda while the state of New York, in its proposed state budget, recommended a penny-per-ounce tax on sugary beverages. In Washington state, legislators

[1] http://www.cdc.gov/obesity/

approved a two-cent tax on every 12 ounces of soft drinks sold. The overarching political argument is based on the economic rationale that price increases caused by higher taxes will dampen consumption. However, while research has investigated the potential consumption reaction to a tax increase, our assessment is that there is still uncertainty as to what the ultimate impact on consumption will be.

In this paper, we shed light on this issue by looking at the effect of two soft drinks sales taxes: one was imposed by the state of Maine in July 1991 (5.5%), and a more recent one was imposed in Ohio in January 2003 (5%). Initially, we analyzed the effect of the "snack tax" that was in force in Maine from 1991 to 2001 (when it was reduced by 0.5%). The tax in Maine was applied to snack foods, soft drinks, carbonated water, ice cream and pastries. A more recent dataset became available that allowed us to replicate the experiment using the sales tax levied in Ohio in 2003, which applied specifically to soft drinks. However, the definition of soft drinks in Ohio is broad including not only "traditional soda pop beverages" but also "any sweetened nonalcoholic beverage, whether sweetened naturally or artificially, (unless it either contains milk products or a milk substitute or it contains greater than fifty percent (50%) fruit or vegetable juice by volume); many fruit drinks or fruit punches that contain fifty percent (50%) or less juice by volume; bottled tea and coffee drinks"[2].

We first study the effect of this type of tax on CSD volume sales and prices, at the brand level, and then compare the results. We investigate this question by employing sales data collected by supermarket scanner devices in Maine as well as in the states of Massachusetts, New York and Connecticut, during the 1988-1992 period. Then, we compare the outcome from this experiment with the outcome from another case study, for which we utilize scanner data on Ohio, Michigan, Illinois and Pennsylvania, for the period 2001-2006.

The use of a brand-level dataset presents two main advantages. First, we are able to employ a difference-in-difference matching estimator (DIDM) that provides a more powerful identification technique than a difference-in-difference estimator (Todd, 2007). In our setting, the DIDM estimator permits the comparison among treatment and control groups based on brand identity. With the DIDM estimator, the matching mechanism is more transparent as it does not rely on propensity scores (i.e. each brand observed in the treatment group is matched to the same brand in the control group).

A second advantage of a brand-level analysis is that it allows us to study whether the tax imposition causes consumption (or pricing behavior) to vary across brands. This is important since a tax increases the price of different brands by different dollar amounts. By accounting for differences in time-invariant unobservable factors between treated and control cities, we are able to isolate the sole impact of the tax policy on the volume and prices of soft drinks, at the brand level.

[2] Ohio Department of Taxation, available at http://tax.ohio.gov/divisions/communications/information_releases/sales/st200401.pdf

Our main finding is that the tax increase did not alter consumption in Maine, nor in Ohio. While our results are specific to a 5.5% tax increase in Maine, and 5% in Ohio, they may prove to be informative as the current mean sales tax rate (across states) on soft drinks is 5.2%, similar to the levels experienced by Maine and Ohio. Despite the fact that consumer attitude towards consumption of soft drinks might have changed in 12 years, as a consequence of increased awareness regarding the problem of obesity and its possible causes, and to the extent that consumer behavior in Maine and Ohio is similar to that of consumers in other states, our results suggest that the current level of soda sales taxes in the US appears to be too small to actually affect consumption in a sizeable way. If the objective of the tax is to influence behavior through a higher price of unhealthy foods (by inducing consumers to consume less high calorie drinks) our results are disappointing to policy makers. On the other hand, if the objective of the tax is to raise tax revenue and use the additional resources to engage in other strategies to address the obesity problem, then our results suggest that the tax can be successful.

This paper is organized as follows. Section 2 presents a review of related research. Section 3 contains a description of the methodology employed while section 4 describes the data. Section 5 contains the main results and section 6 concludes.

## 2. Literature Review

The effectiveness of a soft drinks tax is still not well-understood. Estimates by Yale University's Rudd Center for Food Policy and Obesity suggest that for every 10% increase in price, consumption decreases by 7.8% (Brownell and Frieden, 2009); this estimate implies an own-price elasticity of demand of -0.78. The authors consider a 100% pass-through rate and compute their estimate based on two specific tax proposals: a 10% sales tax, and a penny-per-ounce tax. Conversely, there is evidence from other studies suggesting that the imposition of a tax would have much milder effects on consumption reduction. For instance, some cross-sectional studies have found minimal to no association among state-level soda taxes and body weight (Fletcher et al., 2010a and 2010b; Powell et al., 2009; Sturm et al., 2010). Fletcher et al. (2010b) provide the first empirical examination of the effectiveness of soft drinks taxation in reducing adult obesity. The authors analyze the ultimate impact of changes in states' taxation rates in the period from 1990 to 2006 on changes in body mass index (BMI) and obesity, by exploiting the fact that approximately half of all states changed their soft drink tax rate in this period. Using an analysis that employed individual-level data, the authors find that soft drink taxes do influence behavior but not enough to lead to large changes in population weight.

Results in Wang (2010) greatly scale down the ones by Brownell and Frieden (2009). As in Brownell and Frieden, Wang analyzes the impact of a 10% sales tax and a penny-per-ounce tax. The methodology consists of specifying a structural dynamic demand model that accounts for storability and heterogeneous tastes for soft drinks, which turn out to be crucial elements for obtaining accurate predictions for the two possible tax policies. The author argues that this model provides more accurate estimates of consumers' price sensitivity and thus allows for a more reliable prediction of the policies' impact. Wang's estimate of the overall price elasticity for soft drinks (-0.33) is less than half of that obtained by Brownell and Frieden. Wang argues that not accounting for intertemporal substitution can lead to an overestimate of the effect of the tax on consumption.

Studies exclusively looking at the effect of an excise tax approach (a fixed fee per ounce) find that such a tax would reduce consumption of sugar sweetened beverages by a range that spans from 10% to 25% (Andreyeva et al., 2011; Institute of Medicine, Washington (DC), 2009; Hahn, 2009; Smith et al., 2010). Regardless of the type of tax being analyzed (excise or ad valorem), inference in most empirical work has relied on an estimate of the own-price elasticity for soft drinks. In turn, estimates of the own-price elasticity for soft drinks differ in the literature since they depend on the methodology used, the type of data available, and whether substitutes (e.g. other beverages) are considered. However, studies typically report that demand for soft drinks (as a product category) is largely price-inelastic. A recent review on demand estimates for food products reports an own-price elasticity for soda and other beverages that ranges between -0.8 and -1 (Andreyeva et al., 2010). Lin et al. (2010) estimated two beverage demand systems using retail purchase data for high-income and low-income households. The authors found that, among high-income households, the demand for CSD is price elastic (mean of -1.29) while among low-income households demand is price inelastic (mean of -0.95). A large variance of price elasticity estimates is illustrated by the results in Zheng and Kaiser (2008) and Dharmasena and Capps (2011) who place the price elasticity estimate for soft drinks at -0.15 and -1.90, respectively.

The counterfactual nature of earlier studies implies that an assumption on the pass-through rate needs to be made; the common practice is to assume that the tax will be fully passed through to the final price (i.e. that the tax-exclusive price after the imposition of the tax will remain unchanged). If firms react to the tax change, for example by reducing their prices to dampen the decrease in consumption, then this assumption would not be appropriate. In addition, most of the previous studies assume that people would respond to the tax the same way they would to a price increase from the soft drink company.[3] However, since a price increase caused by a tax is only reflected at the cash register, and to the extent that consumers are primarily guided by the tag price when making a purchase decision, a price increase through a sales tax is likely (as we find below) to cause a smaller reaction in consumption. Our study does not rely on either assumption. Further, we can directly test, as we will show below, whether the tax is fully passed onto the consumer.

[3] An exception is Fletcher et al. (2010b) who investigate the effect of soft drink taxes on consumption using different soft drink tax events in the U.S. The authors, however, rely on a self-reported survey of soft drinks consumption (rather than actual purchases).

## 3. Method

In this paper, we first investigate the consumption effect of a 5.5% sales tax on soft drinks imposed by the state of Maine in July 1991. We employ sales data collected by scanner devices in Portland, Maine as well as in Boston (Massachusetts), Albany (New York) and Hartford (Connecticut). Subsequently, we consider a more recent similar tax event, a 5% sales tax on soft drinks levied in Ohio on the 1st of January 2003. For the latter experiment we employ scanner data collected in Cleveland (Ohio), Detroit (Michigan), Chicago (Illinois) and Philadelphia (Pennsylvania). The available data therefore limit our comparison to consumption across cities (rather than across entire states). The data, provided by Information Resources Inc. (IRI), come from a sample of supermarkets in the largest metropolitan areas in the U.S. We use two datasets that include brand-level sales information for the periods 1988-1992 and 2001-2006, respectively. More details on characteristics and differences of the two sets of data are provided in the next session.

To the extent that neighboring states serve as a reasonable control for both Maine and Ohio, data in such states allow us to isolate the effect of the tax (on soft drinks consumption) from all other possible factors (trends, seasonality, nationwide changes in companies' policies, etc.). In addition, the brand-level analysis allows us to study whether the tax imposition causes consumption (or pricing behavior) to vary across brands.

We employ a difference-in-difference matching estimator (DIDM), which provides a more powerful identification technique than a difference-in-difference estimator (DID) (Todd, 2007). This difference-in-difference matching (DIDM) estimator is superior to a simple DID estimator because comparison of treated and untreated units is based on their similarity. Conversely, a DIDM estimator is superior to a cross-sectional matching estimator since it accounts for differences in time-invariant unobservables between treated and untreated units (Heckman, Ichimura and Todd, 1997; Heckman, Ichimura, Smith and Todd, 1998). In our setting, the DIDM estimator permits the comparison among treatment and control groups based on brand identity; this means that the matching mechanism is simpler, more transparent and more reliable as it does not rely on propensity scores.

The DIDM estimator tailored for our panel data is given by:

$$\widehat{\alpha}_{DIDM} = \frac{1}{N}\sum_{i=1}^{N}\left\{(\log V_{t'i} - \log V_{ti}) - \frac{1}{\#I_i}\sum_{j\in I_i}(\log V_{t'j} - \log V_{tj})\right\} \quad (1)$$

where i and j denote observations in the treatment and control groups, respectively, while t and t' denote pre- and post-treatment time periods. $I_i$ is the set of units in the control group that are matched to treatment unit i and $\#I_i$ is the number of elements in that set. The variable V denotes the outcome being measured (in our case volume sales or price)[4] and the scalar N is the number of treated units (i.e. brands).

We tailor this estimator to the structure of our data. First, unlike usual matching estimators, we employ all treated units in the analysis rather than only those that would fall into a 'common support' set. Second, instead of relying on propensity scores to match treated and untreated units, we define control units to be those brands in the control cities that match the identity of brand i in the treatment city (i.e. we manually choose the unit j that is matched to unit i). Finally, we study the outcome variable in logarithmic form (i.e. V corresponds to the logarithm of the variable of interest: volume sales or price); we adopt this transformation because the variance of volume sales (across brands) in our dataset is unusually large (see Table 2).

We report results of the estimator both for several control cities (i.e. $\#I_i>1$) as well as for each control city separately (i.e. $\#I_i=1$). In the case of $\#I_i>1$, we consider two control cities (i.e. $\#I_i=2$) as well as all control cities (i.e. $\#I_i=3$) and weight all matches equally. Standard errors are calculated using the formula provided by Abadie and Imbens (2008) for nearest neighbor matching estimators.

As a robustness test, we also report results using the standard DID estimator:

$$\log V_{bmt} = \theta + \beta D_{treatment} + \gamma D_{post} + \alpha_{DID} \left(D_{treatment} * D_{post}\right) + \varepsilon_{bmt} \qquad (2)$$

Where b, m and t denote brand, city and time (quarter), respectively; V denotes the outcome variable (volume sales or price); $D_{treatment}$ is a dummy variable equal to 1 if the observation is in the treatment city and 0 otherwise, and $D_{post}$ is a dummy variable equal to 1 in the post-tax period. Note that the logarithm of the outcome variable allows interpreting $\alpha_{DID}$ as the percentage change in the outcome variable due to the tax.

## 4. Description of the data

We employ two sets of scanner data from IRI Infoscan. Characteristics of the two databases are different, thus we will distinguish between dataset A and dataset B, and proceed with a brief description of the two.

Data from dataset A were collected from a large sample of supermarkets across the U.S. in the period 1988-1992; this sample of supermarkets accounts for 82% of all the grocery sales in the U.S. and includes stores with annual sales of more than 2 million dollars. The dataset includes dozens of brands for 65 metropolitan areas spanning 20 quarters. The database also contains information on the demographics for each metropolitan area, which is identified with

[4] While our primary focus is on quantity, in the empirical analysis we also study whether the imposition of the tax caused sizeable price reactions. This allows us to test whether the tax was, on average, fully passed on to the consumer.

the name of the main city in the area. A potential limitation of the IRI database is the exclusion of convenience stores, bars, restaurants and other retail outlets for soft drinks. This lack of information may be of limited concern as there is evidence suggesting approximately 70% of soft drinks was sold through supermarkets around the time of our study (Higgins et al., 1995).

Dataset B contains store sales data on carbonated beverage sales and pricing spanning 5 years (2001-2006) of weekly data and 47 IRI's metropolitan areas (we refer to a metropolitan area as a "city" henceforth)[5]. Data are available at the store level for each chain. They include only chains and not independent stores, and the observations are drawn from IRI's national sample of stores. For each store in each week, over 250 different carbonate beverage products are offered, comprising a combination of all brands and variety (i.e. regular/diet, packaging, volume, and flavors)(Bronnenberg et al,. 2008).

We used 4 cities for each of our analyses: 1) Portland (ME), Albany (NY), Boston (MA) and Hartford (CT); 2) Cleveland (OH), Chicago (IL), Detroit (MI), Philadelphia (PA). Such cities were chosen on the basis of geographical proximity to the treatment city. Also, the chosen cities showed no major event concerning sales taxes during the period of study. We focus our analyses on 6 quarters: 1) fourth quarter of 1990; first, second and fourth quarters of 1991; and first and second quarters of 1992; 2) second through fourth quarter of 2002; second through fourth quarter of 2003. We exclude earlier and later quarters as the common trend assumption needed for the validity of a DID approach is less likely to hold. We exclude the third quarter of 1991 and the first quarter of 2003 (the quarters in which the each of the two taxes took place) for reasons that will be explained later. We selected brands that are present in all quarters and in all cities in our study; this procedure allows us to have a balanced panel (necessary for matching). In Table 1a and 1b we report the selected brands with the corresponding parent companies, as well as the number of observations. The 24 brands in Table 1a account for the 80% of the total volume sales in the selected city-quarter pairs. The 34 brands in Table 1b account for the 83% of the respective total volume sales in the selected city-quarter pairs.

[5] IRI's metropolitan area definitions are similar to those used by the Bureau of Labor Statistics.

Table 1a. Brands, Parent Companies and Presence in City-Quarter pairs – Dataset A–

| **Brand** | **Company** | **# of City-Quarter pairs where present** |
|---|---|---|
| Canada Dry | Cadbury/Schweppes[6] | 24 |
| Canada Dry Light | Cadbury/Schweppes | 24 |
| Crush | Cadbury/Schweppes | 24 |
| Schweppes | Cadbury/Schweppes | 24 |
| Schweppes Light | Cadbury/Schweppes | 24 |
| Coke | Coca-Cola | 24 |
| Coke Classic | Coca-Cola | 24 |
| Diet Coke | Coca-Cola | 24 |
| Diet Sprite | Coca-Cola | 24 |
| Sprite | Coca-Cola | 24 |
| 7 Up | Hicks & Haas | 24 |
| A & W | Hicks & Haas | 24 |
| A & W Light | Hicks & Haas | 24 |
| Diet 7 Up | Hicks & Haas | 24 |
| Diet Dr Pepper | Hicks & Haas | 24 |
| Dr Pepper | Hicks & Haas | 24 |
| Diet Pepsi | Pepsi Co | 24 |
| Diet Pepsi Free | Pepsi Co | 24 |
| Diet Slice | Pepsi Co | 24 |
| Mountain Dew | Pepsi Co | 24 |
| Pepsi | Pepsi Co | 24 |
| Pepsi Free | Pepsi Co | 24 |
| Slice | Pepsi Co | 24 |
| Diet Rite | Royal Crown | 24 |
| Total #obs | | 576 |

[6] Cadbury Schweppes Americas Beverages became Dr Pepper Snapple Group Inc. on May 5, 2008. http://www.sec.gov/Archives/edgar/data/1418135/000144530511000302/dps2010123110k.htm

Table 1b. Brands, Parent Companies and Presence in City-Quarter pairs – Dataset B–

| **Brand** | **Company** | **# of City-Quarter pairs where present** |
|---|---|---|
| 7up | Cadbury /Schweppes | 24 |
| A & W | Cadbury /Schweppes | 24 |
| Canada Dry | Cadbury /Schweppes | 24 |
| Cherry 7 Up | Cadbury /Schweppes | 24 |
| Diet 7 Up | Cadbury /Schweppes | 24 |
| Diet Cherry 7 Up | Cadbury /Schweppes | 24 |
| Diet Dr Pepper | Cadbury /Schweppes | 24 |
| Diet Rite | Cadbury /Schweppes | 24 |
| Diet Schweppes | Cadbury /Schweppes | 24 |
| Diet Vernors | Cadbury /Schweppes | 24 |
| Dr Pepper | Cadbury /Schweppes | 24 |
| Ibc | Cadbury /Schweppes | 24 |
| RC | Cadbury /Schweppes | 24 |
| Sunkist | Cadbury /Schweppes | 24 |
| Vernors | Cadbury /Schweppes | 24 |
| Welchs | Cadbury /Schweppes | 24 |
| Barqs | Coca-Cola | 24 |
| Caffeine Free Coke Classic | Coca-Cola | 24 |
| Caffeine Free Diet Coke | Coca-Cola | 24 |
| Cherry Coke | Coca-Cola | 24 |
| Coke Classic | Coca-Cola | 24 |
| Diet Barqs | Coca-Cola | 24 |
| Diet Coke | Coca-Cola | 24 |
| Fresca | Coca-Cola | 24 |
| Sprite | Coca-Cola | 24 |
| Caffeine Free Diet Pepsi | Pepsi Co | 24 |
| Caffeine Free Pepsi | Pepsi Co | 24 |
| Diet Mountain Dew | Pepsi Co | 24 |
| Diet Pepsi | Pepsi Co | 24 |
| Diet Sierra Mist | Pepsi Co | 24 |
| Mountain Dew | Pepsi Co | 24 |
| Mug | Pepsi Co | 24 |
| Pepsi | Pepsi Co | 24 |
| Slice | Pepsi Co | 24 |
| Total #obs | | 816 |

As it appears from a comparison between Table 1a and Table 1b, some brands have changed Company ownership over time. For instance, "7 UP" which was initially acquired by Philip Morris in 1978, was sold to Hicks & Haas in 1986. It was then merged with "Dr Pepper" in 1988, and bought by Cadbury Schweppes in 1995.

The IRI database A contains the total volume and the mean price (before taxes) per unit of volume (288 oz), for every brand, in a given city-quarter pair. In dataset A, IRI aggregates information by adding the volume sold for all package sizes of a brand into one observation. In dataset B this aggregation was manually obtained, and the unit of volume is 192 oz. The average price per unit of volume is obtained by aggregating all revenue generated by a brand (regardless of package size) and dividing the resulting aggregate revenue by the aggregate volume sold for that brand[7]. Descriptive statistics for the brands and cities chosen for our study are provided in Table 2a and 2b. These data include information contained in the IRI dataset, as well as data collected from specialized sources (demographics, temperatures). Based on the similarity of demographics, these data suggest that Albany appears to be the most reliable control for Portland as it is the most similar to Portland in terms of size (population), income and temperature. For the same reasons, Detroit is considered the most reliable control for Cleveland.

Table 2a. Summary statistics of demographic and temperature data

| City | Variable Description | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Albany: | Mean Price per brand ($/288 oz) | 4.1 | 0.7 | 1.7 | 7.3 |
| | Volume sold (288 oz) | 71,346 | 129,691 | 2.2 | 1,059,409 |
| | Population[8] | 101,082 | - | - | - |
| | Median income ($) | 31,813 | - | - | - |
| | Annual Minimum Temperature 1990-1992[9] (°F) | 40 | - | -7* | n.a. |
| | Annual Maximum Temperature 1990-1992[9] (°F) | 60 | - | n.a. | 97** |
| Boston: | Mean Price per brand ($/288 oz) | 3.8 | 0.7 | 2.3 | 7.7 |
| | Volume sold (288 oz) | 408,081 | 680,114 | 0.3 | 4,961,379 |
| | Population | 574,283 | - | - | - |
| | Median income ($) | 37,624 | - | - | - |
| | Annual Minimum Temperature 1990-1992[9] (°F) | 45 | - | 3* | n.a. |
| | Annual Maximum Temperature 1990-1992[9] (°F) | 60 | - | n.a. | 99** |
| Hartford: | Mean Price per brand ($/288 oz) | 3.9 | 0.6 | 2.1 | 7.7 |
| | Volume sold (288 oz) | 250,304 | 487,841 | 2.2 | 4,504,493 |
| | Population | 139,739 | - | - | - |
| | Median income ($) | 37,308 | - | - | - |

[7] This procedure effectively yields a weighted average price across package sizes.
[8] As of the 1990 Census.

| City | Variable Description | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| | Annual Minimum Temperature 1990-1992[9] (℉) | 42 | - | -2* | n.a. |
| | Annual Maximum Temperature 1990-1992[9] (℉) | 61 | - | n.a. | 100** |
| Portland: | Mean Price per brand ($/288 oz) | 4.4 | 0.8 | 2.2 | 7.8 |
| | Volume sold (288 oz) | 95,883 | 156,508 | 1.8 | 1,150,649 |
| | Population | 64,358 | - | - | - |
| | Median income ($) | 29,615 | - | - | - |
| | Annual Minimum Temperature 1990-1992[9] (℉) | 39 | - | -14* | n.a. |
| | Annual Maximum Temperature 1990-1992[9] (℉) | 56 | - | n.a. | 96** |

* Record minimum temperature. ** Record maximum temperature.

Table 2b. Summary statistics of demographic and temperature data

| City | Variable Description | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Cleveland: | Mean Price per brand ($/192 oz) | 3.1 | 0.3 | 2.6 | 3.5 |
| | Volume sold (192 oz) | 104,420 | 34,763 | 56,125 | 156,714 |
| | Population[10] | 478,403 | - | - | - |
| | Median income ($) | 25,928 | - | - | - |
| | Annual Minimum Temperature 2002-2004[9] (℉) | 42 | - | -2* | n.a. |
| | Annual Maximum Temperature 2002-2004[9] (℉) | 57 | - | n.a. | 88** |
| Chicago: | Mean Price per brand ($/192 oz) | 2.8 | 0.24 | 2.5 | 3.2 |
| | Volume sold (192 oz) | 328,919 | 81,747 | 158,531 | 414,558 |
| | Population | 2,893,666 | - | - | - |
| | Median income ($) | 38,625 | - | - | - |
| | Annual Minimum Temperature 2002-2004[9] (℉) | 44 | - | -7* | n.a. |
| | Annual Maximum Temperature 2002-2004[9] (℉) | 59 | - | n.a. | 93** |
| Detroit: | Mean Price per brand ($/192 oz) | 3.4 | 0.2 | 3 | 4.1 |
| | Volume sold (192 oz) | 232,984 | 66,877 | 149,579 | 321,747 |
| | Population | 951,270 | - | - | - |
| | Median income ($) | 25,787 | - | - | - |
| | Annual Minimum Temperature 2002-2004[9] (℉) | 41 | - | -2* | n.a. |
| | Annual Maximum Temperature 2002-2004[9] (℉) | 57 | - | n.a. | 91** |
| Philadelphia: | Mean Price per brand ($/192 oz) | 3.1 | 0.1 | 2.9 | 3.5 |
| | Volume sold (192 oz) | 106,466 | 17,647 | 76,771 | 134,432 |
| | Population | 1,517,550 | - | - | - |
| | Median income ($) | 36,669 | - | - | - |
| | Annual Minimum Temperature 2002-2004[9] (℉) | 47 | - | 8* | n.a. |
| | Annual Maximum Temperature 2002-2004[9] (℉) | 62 | - | n.a. | 93** |

* Record minimum temperature. ** Record maximum temperature.

[9] http://www.nesdis.noaa.gov/
[10] As of the 2000 Census.

## 5. Results

In July of 1991, a sales tax of 5.5% on snacks and soda was instituted by the state of Maine. This information was initially obtained from Jacobson and Brownell (2000) and later confirmed (by phone) with staff in the Law and Legislative Reference Library, an office of the Maine Legislature. In our dataset, this date corresponds to the beginning of the third quarter in 1991.

For the second experiment, among the tax events[11] that occurred in the time period covered by dataset B (2001-2006), we selected a 5% sales tax on soft drinks sold in grocery stores and through vending machines, levied in Ohio, on January 1, 2003, which, in our dataset corresponds to the beginning of the first quarter in 2003. The selection of this one event over the others is due to the availability of data for at least one city in the state where the tax was applied, and the availability of data on cities that may represent good controls.

In this section, we first report some descriptive results and later discuss findings from the statistical analyses.

*Descriptive Evidence*

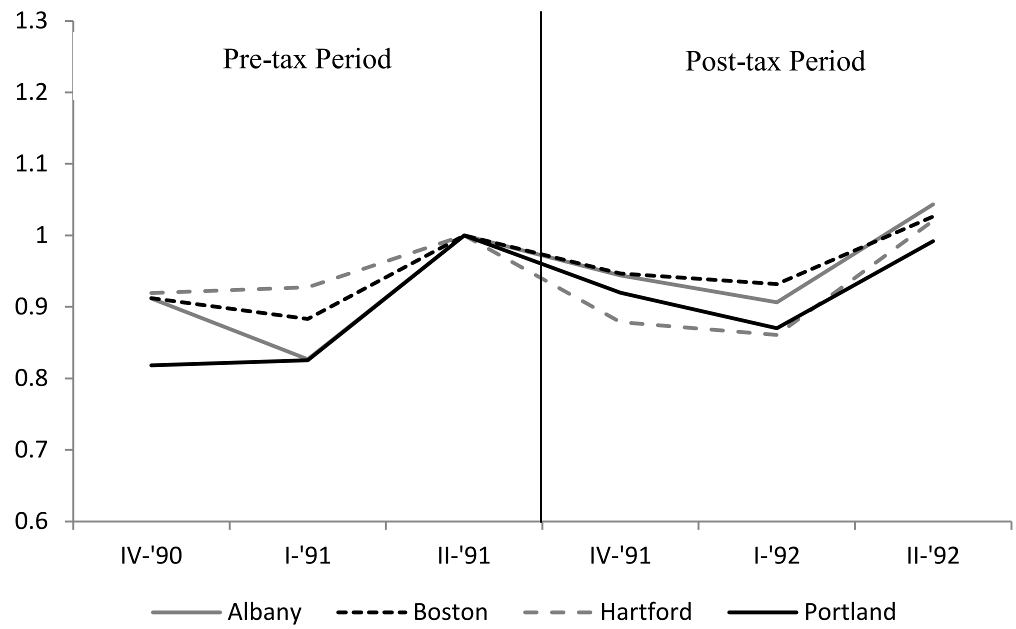
A crucial requirement for the reliability of difference in difference estimators is that the control units should share a common trend with the treatment units. Since this condition is largely difficult to ensure in non-lab environments, one needs to check how plausible this assumption is. We do this by graphically comparing the evolution of the outcome variable of main interest (volume sales) across treatment and control cities. Figures 1a and 1b depict, for each city, the quarterly series of total volume sales. These volume sales are computed using the selected brands reported in Tables 1a and 1b (similar graphs are obtained if all brands are included). The time period spans from the fourth quarter of 1990 to the second quarter of 1992, and from the second quarter of 2002 to the fourth quarter of 2003, respectively. To facilitate comparability, total volume sales are normalized by using volume sales in the fourth quarter of 1990 as the base period, and the fourth quarter of 2002, respectively.

Both Figures exclude the quarter in which the tax was applied. The reason for this is that we observe an unusually large peak in total volume sales for Portland in the third quarter in 1991. This peak only occurs in Portland and we are unsure about its cause. This peak may be a reason to doubt the appropriateness of the control cities as one would expect control cities to mimic volume changes in the treatment city. However, one would be particularly worried about this if such disparity between control and treatment cities is also observed in other quarters. The graph obtained by excluding that specific quarter suggests that the Portland volume sales peak

[11]Source: Bridging the Gap Program, University of Illinois at Chicago Institute for Research and Policy, 2011. Available at: http://www.bridgingthegapresearch.org/research/sodasnack_taxes/

appears to be an isolated event that occurred in the summer of 1991[12] since volume trends seem to be reasonably similar across cities once this quarter is removed from the graph. Due to this seemingly isolated disparity in trends, we exclude the third quarter in 1991 from our analysis. We note that, in any case, this choice will allow us to err on the conservative side when estimating the effect of the tax on consumption (including the spike in volume sales registered in the third quarter of 1991 in the regressions below leads to a positive effect of consumption by the tax increase, an unlikely scenario). For consistency, we dropped the first quarter of 2003 from the analysis of the tax in Ohio.

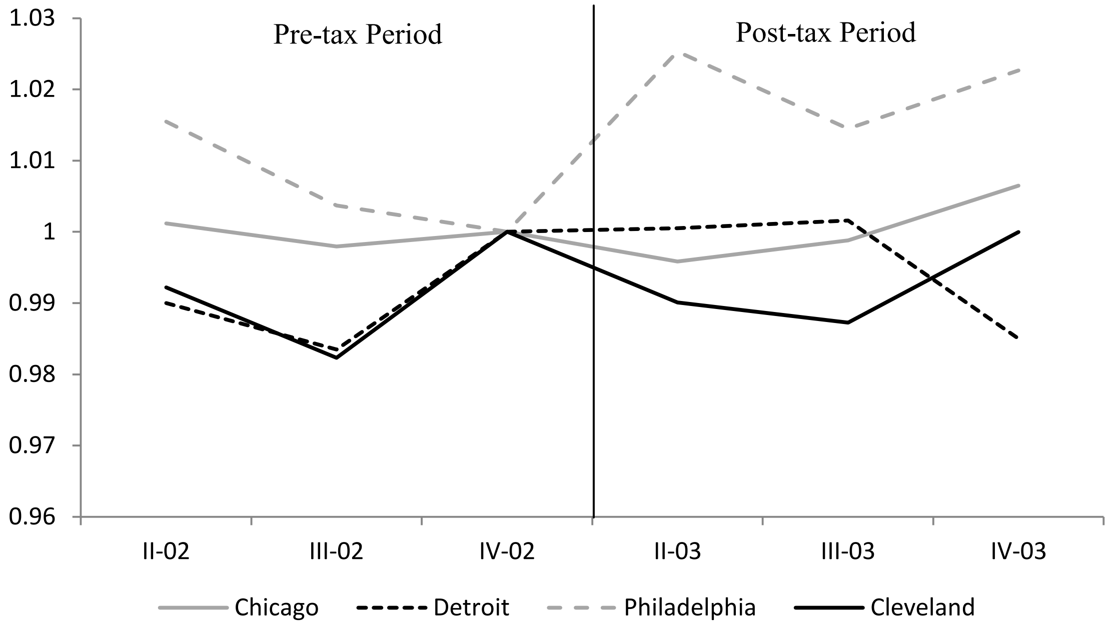
Figure 1a. CSD total volume sales* (y-axis) per city (different lines) and quarter (x-axis), IRI Infoscan Data, Dec 1990 – June 1992 [excluding the III quarter of 1991data]




*Total volume sales have been normalized using the 2nd quarter of 1991 as base period.

[12] We checked whether this event was due to an unusually warm summer in Portland with respect to other cities. Data from NOAA's Satellite and Information Service (http://www.nesdis.noaa.gov/) suggests that this was not the case: the July-September average temperatures in 1990, 1991 and 1992 for our study were, respectively: 62.6°F, 61.6°F, 60.2°F (ME); 66.8°F, 66.5°F, 64.5°F (MA); 64.7°F, 65.1°F, 62.8°F (NY); 68.2°F, 68.1°F, 65.7°F (CT).

Figure 1b. CSD total volume sales* (y-axis) per city (different lines) and quarter (x-axis), IRI Infoscan Data, Apr 2002 – Dec 2003 [excluding the I quarter of 2003 data]




*Total volume sales have been normalized using the 4th quarter of 2002 as base period.

Figure 1a shows reasonably similar volume trends between the treatment city and the control cities in the period prior to the imposition of the soft drinks tax, adding comfort to our methodology. Moreover, and consistent with the demographic information, Albany's volume trend seems to more closely resemble that of Portland. Any significant changes in trends (between Portland and its controls) in the period after the tax increase can be used to roughly infer what the effect of the tax on consumption might have been.

Following the imposition of the tax, all cities show a negative trend in volume sales; further, it appears as if Portland's downward trend (at least when compared with the most reliable control, Albany) might be somewhat more pronounced. While this "graphical" evidence suggests that the tax might have curbed soft drinks consumption in Maine, our overall assessment is that such effect is not substantial.

Similar comments can be made by interpreting the graph in Figure 1b. Clearly, the control city that better represents volume sales trend in Cleveland (treatment city), before the tax was applied, is Detroit. However, also the other cities show a similar trend, despite the level of sales that is visibly higher in Philadelphia and Chicago than in Cleveland and Detroit. From the volume sales trend shown in the graph, Philadelphia does not appear to be a good control city for Cleveland. Unfortunately, we are limited in our choice of controls leaving us to include Philadelphia in the analysis. We will proceed with caution when considering results obtained by the comparison with Philadelphia. The effect of the tax, on the other hand, is not clear from graphical observation, as the level of volume sales in Cleveland is not lower for the quarters after the tax was applied than it is in the quarters before the tax applied.

*Regression Results*

Table 3a and 3b show the DIDM results for volume sales as well as for price. As opposed to the "total volume sales" variable in Figure 1 (which is the sum of volume sales over all brands in a city-quarter pair), "volume sales" in this analysis is measured at the brand level. We define the before period as the three quarters preceding the tax change (i.e. fourth quarter of 1990 through second quarter of 1991; second quarter of 2002 through fourth quarter of 2002) and the after period as the three quarters after the change due to the tax (i.e. fourth quarter of 1991 through second quarter of 1992; second quarter of 2003 through fourth quarter of 2003). Because the matching estimator requires one observation in each the post- and pre-treatment periods, we aggregate quarters by taking the mean of the variable over the quarters considered (for either the before or the after period) and perform the test on the difference of the logs of these mean values (see equation 1)[13]. For robustness purposes, we compute the DIDM estimator for all possible sets of control cities. That is, we consider the case in which we use all 3 control cities in the estimator, as well as cases when we include a pair of cities, or just one city.

The parameter estimates can be (roughly) interpreted as the percentage variation of the variable of interest (in the treatment city) respect to the control city (using the three quarters after the tax was enacted as the after period and the three quarters before the tax enactment as the before period)[14]. We observe from the results that there is no statistically significant change in either price or volume. The price estimates imply that firms did not react in any systematic way as a consequence of the imposition of the tax and that the tax was fully passed through to consumers.

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[13] Results are not sensitive to this method of aggregation. Specifically, our conclusions remain unchanged if we: a) add volume sales across quarters (instead of taking the average); or b) report results on quarter by quarter comparisons. Results of these alternative estimations are shown in the appendix.

[14] Strictly speaking, because we are using the difference of the variable in natural log format, the percentage change in the variable is given by $e^{\hat{\alpha}^{DIDM}} - 1$, where $\hat{\alpha}^{DIDM}$ is the DIDM estimate reported in Table 3. For small enough $\hat{\alpha}^{DIDM}$ (as is the case here) , $\hat{\alpha}^{DIDM}$ is a good approximation of $e^{\hat{\alpha}^{DIDM}} - 1$.

Table 3a: **DIDM** Results for Volume and Price, Portland as treatment city; Albany, Hartford and Boston as control cities

| | Volume change | Price change |
|---|---|---|
| **Control city** | | |
| All control cities | -0.02 (0.04) | 0.00 (0.01) |
| Albany-Boston | -0.04 (0.04) | 0.00 (0.01) |
| Albany-Hartford | 0.00 (0.05) | 0.00 (0.01) |
| Hartford-Boston | -0.02 (0.04) | 0.01 (0.01) |
| Albany | -0.02 (0.05) | -0.01 (0.01) |
| Boston | -0.06 (0.04) | 0.02 (0.01) |
| Hartford | 0.01 (0.06) | 0.00 (0.02) |

Notes: Pre-tax period is fourth quarter of 1990 through second quarter of 1991; post-tax period is fourth quarter of 1991 through second quarter of 1992. The specification uses the mean volume (and mean price) over the pre-tax and the post-tax periods, respectively (alternative specifications are shown in the appendix). The DIDM estimator is applied on the log of these mean values (see equation 1). Standard errors (in parenthesis) correspond to the nearest neighbor estimator provided by Abadie and Imbens (2008).

Table 3b: **DIDM** Results for Volume and Price, Cleveland as treatment city; Chicago, Detroit and Philadelphia as control cities

| | Volume change | Price change |
|---|---|---|
| **Control city** | | |
| All control cities | -0.02 (0.06) | 0.00 (0.01) |
| Chicago-Detroit | 0.00 (0.06) | 0.02 (0.02) |
| Chicago-Philadelphia | 0.00 (0.06) | 0.00 (0.02) |
| Detroit- Philadelphia | -0.06 (0.07) | 0.00 (0.02) |
| Chicago | 0.06 (0.07) | 0.03 (0.02) |
| Detroit | -0.05 (0.08) | 0.02 (0.02) |
| Philadelphia | -0.07 (0.07) | -0.03 (0.02) |

Notes: Pre-tax period is second quarter of 2002 through fourth quarter of 2002; post-tax period is second quarter of 2003 through fourth quarter of 2003. The specification uses the mean volume (and mean price) over the pre-tax and the post-tax periods, respectively (alternative specifications are shown in the appendix). The DIDM estimator is applied on the log of these mean values (see equation 1). Standard errors (in parenthesis) correspond to the nearest neighbor estimator provided by Abadie and Imbens (2008).

Table 4a and 4b present the results for the standard DID regression, which we use as a robustness test for our DIDM estimates. As in the DIDM analysis, we consider two independent variables: the natural logarithm of volume sales and the natural logarithm of price. We make the same comparisons among cities as in the DIDM analysis. The DID regressions confirm the overall DIDM results as they show how the tax application did not yield any statistically significant changes in either price or volume.

Table 4a: **DID** Results for Volume and Price, Portland as treatment city; Albany, Hartford and Boston as control cities

| | Volume change | Price change |
|---|---|---|
| **Control city (#obs)** | | |
| All control cities (576) | -0.04 (0.03) | 0.00 (0.01) |
| Albany-Boston (432) | -0.07 (0.05) | 0.00 (0.01) |
| Albany-Hartford (432) | -0.02 (0.04) | -0.01 (0.01) |
| Hartford-Boston (432) | -0.03 (0.04) | 0.01 (0.01) |
| Albany (288) | -0.07 (0.09) | -0.01 (0.01) |
| Boston (288) | -0.07 (0.04) | 0.01 (0.01) |
| Hartford (288) | 0.02 (0.06) | 0.00 (0.02) |

Notes: Pre-tax period is fourth quarter of 1990 through second quarter of 1991; post-tax period is fourth quarter of 1991 through second quarter of 1992. The DID estimator is applied on the log of volume and price, respectively; the reported coefficient corresponds to $\hat{\alpha}_{DID}$ in equation (2). Standard errors (in parenthesis) are clustered at the brand level[15].

Table 4b: **DID** Results for Volume and Price, Cleveland as treatment city; Chicago, Detroit and Philadelphia as control cities

| | Volume change | Price change |
|---|---|---|
| **Control city (#obs)** | | |
| All control cities (816) | -0.02 (0.07) | 0.00 (0.02) |
| Chicago-Detroit (612) | -0.01 (0.07) | 0.02 (0.02) |
| Chicago-Philadelphia (612) | 0.00 (0.07) | 0.00 (0.02) |
| Detroit- Philadelphia (612) | -0.07 (0.07) | -0.01 (0.02) |
| Chicago (408) | 0.05 (0.07) | 0.03 (0.02) |
| Detroit (408) | -0.07 (0.08) | 0.01 (0.02) |
| Philadelphia (408) | -0.06 (0.07) | -0.03 (0.02) |

[15] Clustering at the company level does not alter our results. In all cases, significance levels as reported in Table 4 remain unchanged. We choose to report brand-level clustering because clustered errors are valid only for a sufficiently large number of clusters, ideally more than 20-25 (Cameron, Gelbach and Miller, 2008).

Notes: Pre-tax period is second quarter of 2002 through fourth quarter of 2002; post-tax period is second quarter of 2003 through fourth quarter of 2003. The DID estimator is applied on the log of volume and price, respectively; the reported coefficient corresponds to $\hat{\alpha}_{DID}$ in equation (2). Standard errors (in parenthesis) are clustered at the brand level[16].

To further understand whether there are particular patterns in price and volume changes at the brand level, we visually inspect price and volume changes for each brand in our dataset. In the following Figures (2a/b and 3a/b) we plot the change in price and the change in volume for each brand–city pair. In the Figures, brands appear on the horizontal axis. Cities are depicted by markers. The Figures not only highlight the importance of using a control in measuring the desired effect, but they are also consistent with the econometric results in that there is not a clear pattern suggesting a sizable effect of the tax increase in either volume or prices at the brand-level.

Figure 2a. Change in Volume sales by Brand and City (log (Mean Volume sales IV'91-II'92/Mean Volume sales IV'90-II'91))

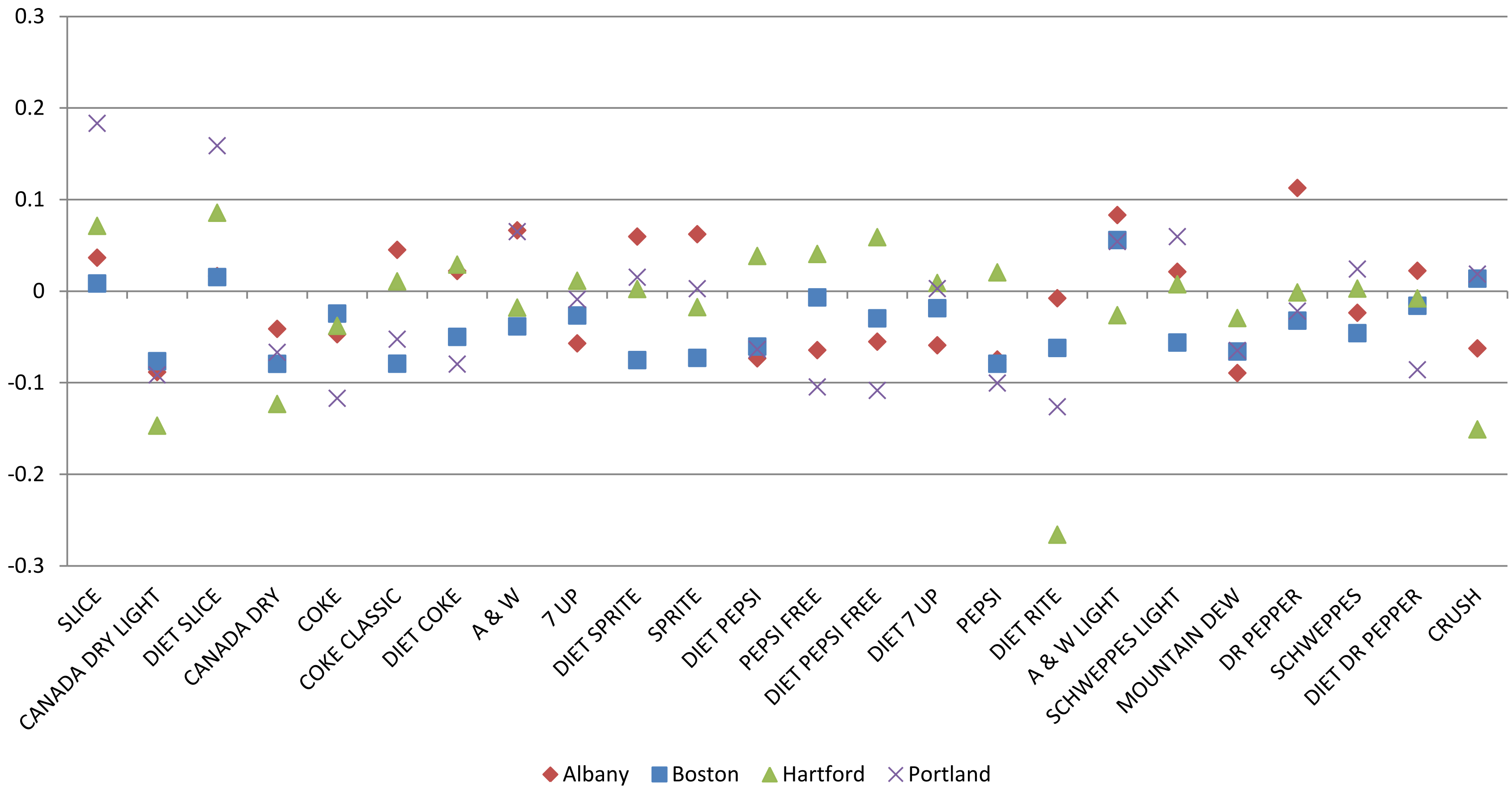


Figure 2b. Change in Volume sales by Brand and City (log (Mean Volume sales II'02-IV'02/Mean Volume sales II'03-IV'03))

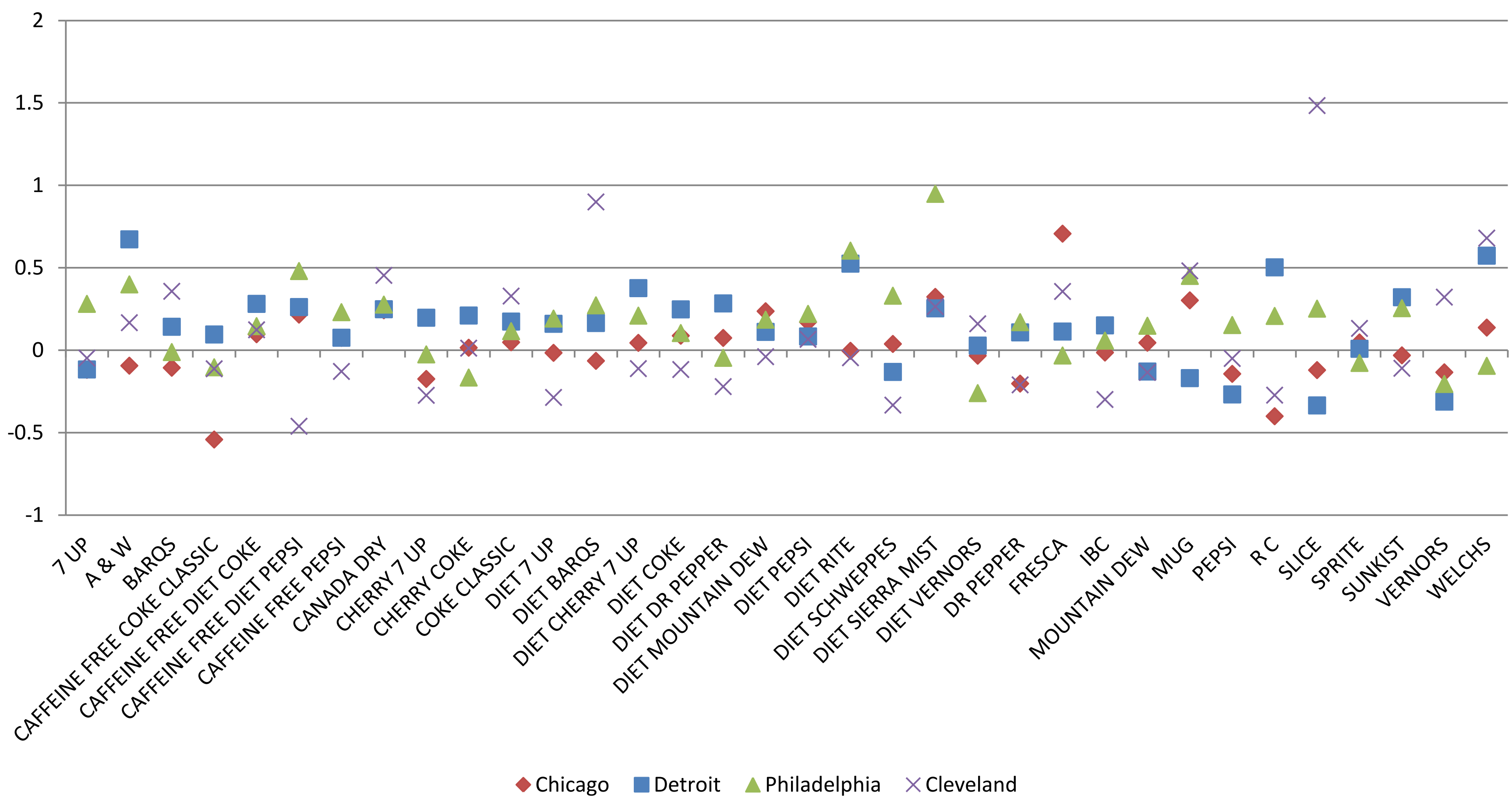


Figure 3a. Change in Price by Brand and City (log (Mean Price IV'91-II'92/Mean Price IV'90-II'91))

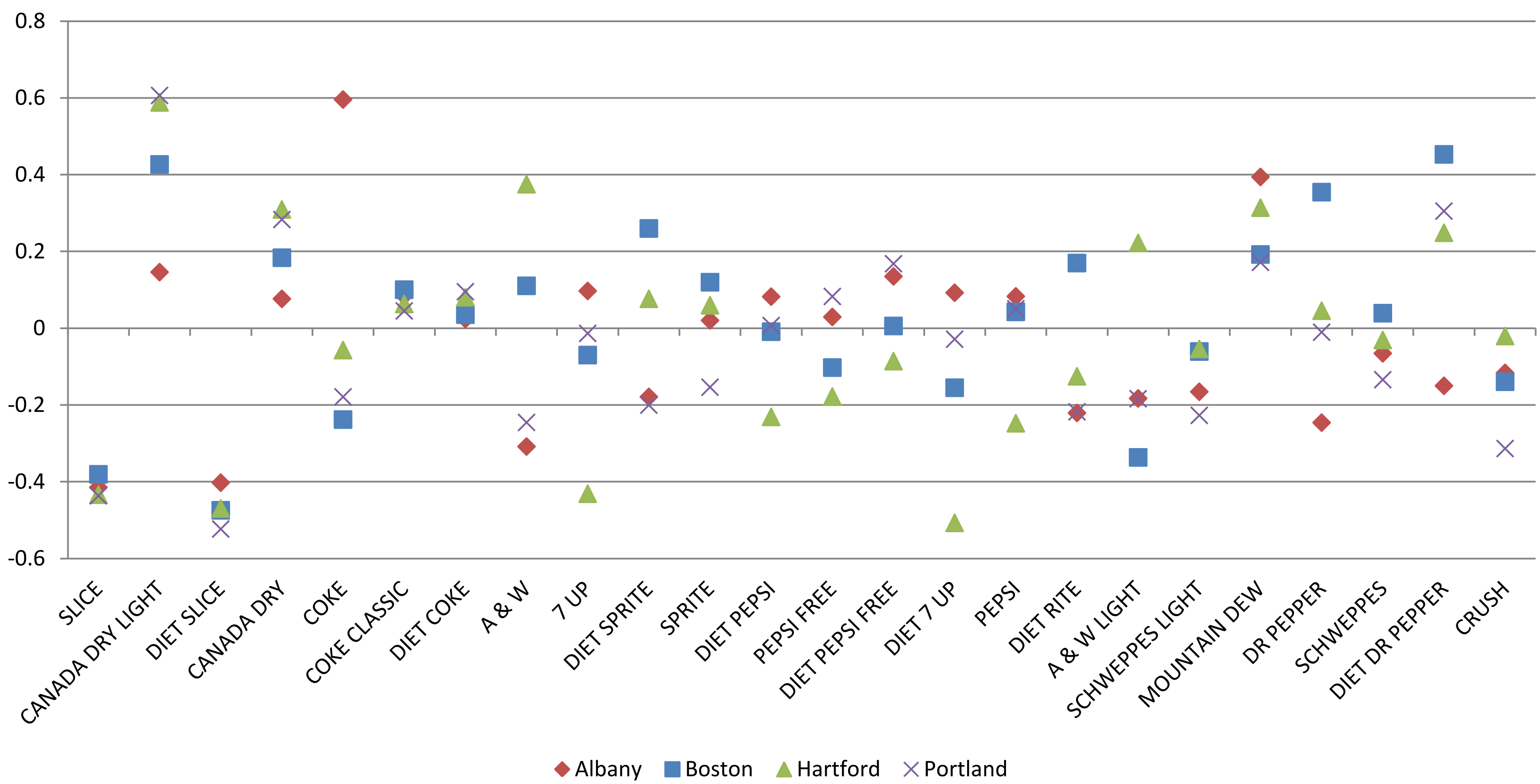
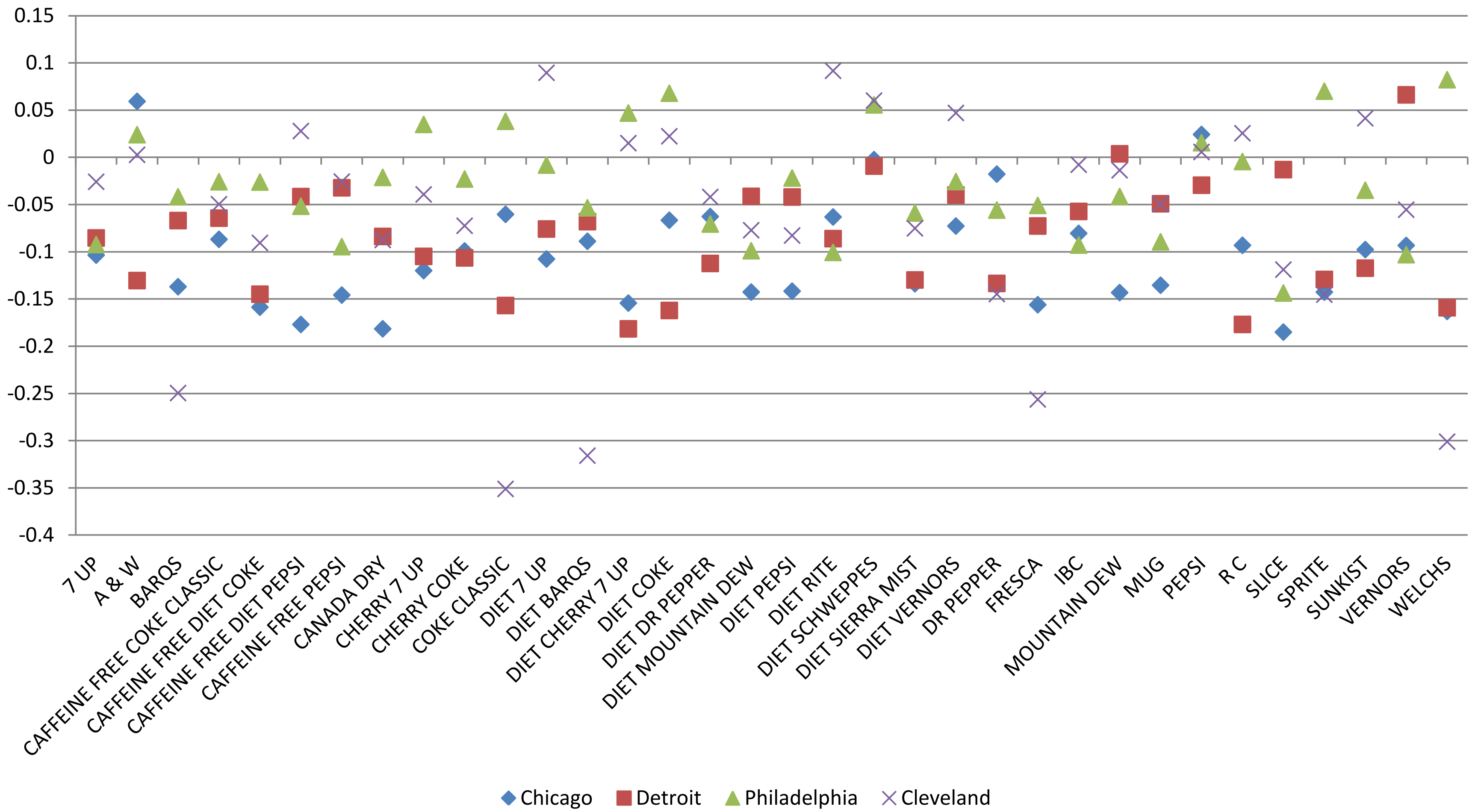


Figure 3b. Change in Price by Brand and City (log (Mean Price II’02-IV’02/Mean Price II’03-IV’03))



## 6. Conclusion

In this paper we show the results of DIDM and standard DID estimations, with the aim of uncovering the effect that the imposition of a soft drinks sales tax might have had on brand-level consumption and prices. Results suggest that the 5.5% sales tax that Maine applied to soft drinks in July of 1991 did not cause a generalized impact on volume sales at either the aggregate or the disaggregate (brand) level. Subsequently, using a more recent dataset (2001-2006) available to us we identified a similar tax event, which occurred during the time covered by this dataset, and we replicated the analysis. This allowed us to verify whether the efficacy of this type of tax has changed with time and with possible greater awareness of people about the correlation between obesity and soda consumption. We also found that in the second case the application of a sales tax on soft drinks did not affect the consumption in a sensitive way. We find that our results are robust to several alternative specifications (see the Appendix).

While this outcome is consistent with the generalized conclusion in the literature that demand for soft drinks is inelastic, it casts some doubt about whether one should use price elasticities to form counterfactuals for how consumers might react to tax increases. Specifically, we find that such counterfactuals might be optimistic as they predict an actual reduction in consumption. The fact that the tax is not displayed on the shelf (where many consumers may base their purchasing decisions) may help explain why a tax does not cause a reduction in consumption in our data. One caveat of our study regarding Maine is that the tax was also applied to other high-calorie foods (snacks and pastries), so there is not much room for a possible substitution effect away from soda and towards other sources of sugar. This could partly explain the insignificant impact on soft drinks consumption in our experiment. However, that caveat has been eliminated in the second part of the study, given that no tax was applied on candies, pudding/gelatin, sugar and sugar substitutes or snacks in Ohio in 2003. Those items are considered food by Ohio Legislation and sales tax exempted[17]. Still, our results raise interesting questions about the role of substitute categories when a commodity is taxed. For example, if the impact on soft drink consumption in our study had been statistically significant and the tax had been applied only on soft drinks, a reduction in consumption could have reflected a switch towards higher consumption of other sugary products (and not the reduction in sugar intake intended by policy makers).

While we only look at an isolated instance of a tax increase, our results may have broader implications as the tax applied in Maine and Ohio are very close to the mean sales tax applied to soft drinks (currently in practice in 33 states), which is 5.2% (Brownell et al., 2009). Also, because our data for price excludes the tax, and results from our sample do not reflect a statistically significant change in prices as a result of the policy, we can directly test whether firms reacted in their pricing decisions. Our results suggest that the price increase due to the tax was entirely passed through to the consumer. This finding may be informative for future

[17] http://tax.ohio.gov/divisions/communications/information_releases/sales/st200401.pdf

researchers in suggesting a likely pass-through rate for a tax increase when one needs to be assumed for counterfactual purposes.

Our results show the taxes in Maine and Ohio did not significantly decrease consumption. Therefore, these taxes have the effects of raising tax revenues for the states. While this added tax revenue should, in principle, be reinvested in programs and campaigns to promote a healthier consumption of food, in most of the cases the revenue from the "snack-taxes" has become part of the general treasury, as occurred in Maine (Jacobson and Brownell, 2000). Sales taxes like the ones we study have been demonstrated to be regressive in previous studies (Wang, 2010; Lin and Smith, 2010; Chouinard et al., 2006). In particular, it has been found that soda taxes generate a welfare loss not homogenously distributed across households of different income levels, with poorer consumers being more affected by such taxes (Wang, 2010). The nature of our data does not allow us to separate the effects between different types of consumers; in this sense, we find that the "average" effect of the tax on consumption is null. To the extent heterogeneous effects of taxation exist, there will be households that indeed reduce their consumption when a tax is applied while others might either be insensitive. Specifically, there might be individuals that show strong soda consumption habits, probably due to a component of addiction caused by either caffeine or high glucose content, or their combined effect (West, 2001; Keast and Riddell, 2007). Because the objective of the policy is to curb consumption for consumers who are less likely to give up consumption of soda, discussion of any proposal of special "soda taxes" should be accompanied by a systematic agenda of redistributive interventions on health.

The analysis presented in this paper is part of a larger research agenda that intends to fully exploit the broader and richer scanner panel data spanning the 2001-2006 period, when several soda taxes were implemented. The richness of the data (weekly supermarket sales at the zip code level for all consumer packaged goods in 65 metropolitan areas in the U.S.) will allow us to answer, among other things, two questions we were not able to address here: the effect of the tax across different types of consumers, and the substitution of consumption towards sugary goods that are not affected by the tax.

Finally, we note one methodological point. While our matching mechanism is simple and intuitive, we are not aware of other studies that have applied this approach. We think that this could be a particularly useful technique in work that investigates the effect of a policy (or other environment changes) that is homogenously applied to a differentiated commodity.

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## Appendix

Table A1a: **DIDM** Results for Volume and Price, Portland as treatment city and Albany, Hartford and Boston as control cities [sum of volume instead of mean]

| | Volume change | Price change |
|---|---|---|
| **Control city** | | |
| All control cities | -0.02 (0.04) | 0.00 (0.01) |
| Albany-Boston | -0.04 (0.04) | 0.00 (0.01) |
| Albany-Hartford | 0.00 (0.04) | -0.01 (0.01) |
| Hartford-Boston | -0.02 (0.05) | 0.00 (0.01) |
| Albany | -0.02 (0.05) | -0.01 (0.01) |
| Boston | -0.06 (0.04) | 0.01 (0.01) |
| Hartford | -0.01 (0.06) | 0.00 (0.02) |

Notes: Pre-tax period is fourth quarter of 1990 through second quarter of 1991; post-tax period is fourth quarter of 1991 through second quarter of 1992. The specification uses the **sum** of volume (or price) over the pre-tax and the post-tax periods, respectively. The DIDM estimator is applied on the log of these mean values (see equation 1). Abadie-Imbens standard errors in parentheses.

Table A1a: **DIDM** Results for Volume and Price, Cleveland as treatment city and Chicago, Detroit and Philadelphia as control cities [sum of volume instead of mean]

| | Volume change | Price change |
|---|---|---|
| **Control city** | | |
| All control cities | -0.01 (0.08) | 0.01 (0.03) |
| Chicago-Detroit | 0.05 (0.08) | 0.07 (0.04) |
| Chicago-Philadelphia | -0.02 (0.08) | -0.02 (0.04) |
| Detroit- Philadelphia | -0.05 (0.08) | 0.00 (0.04) |
| Chicago | 0.08 (0.09) | 0.05 (0.04) |
| Detroit | -0.02 (0.10) | 0.09** (0.04) |
| Philadelphia | -0.13 (0.09) | -0.09** (0.04) |

Significance level: **=5%. Abadie-Imbens standard errors in parentheses.
Notes: Pre-tax period is second quarter of 2002 through fourth quarter of 2002; post-tax period is second quarter of 2003 through second quarter of 2003. The specification uses the **sum** of volume (or price) over the pre-tax and the post-tax periods, respectively. The DIDM estimator is applied on the log of these mean values (see equation 1). Abadie-Imbens standard errors in parentheses.

Table A2a: **DIDM** results for Volume and Price change, Portland as treatment city and Albany-Hartford-Boston as control [quarter by quarter comparisons instead of aggregate comparisons]

| | **Volume change** | | **Price change** | |
|---|---|---|---|---|
| **Date** | **Control cities** | | | |
| | Albany | Hartford - Boston - Albany | Albany | Hartford - Boston - Albany |
| II'91 v. IV'91 | 0.02 (0.05) | 0.02 (0.04) | -0.03 (0.02) | 0.01 (0.01) |
| IV'90 v. IV'91 | -0.01 (0.17) | 0.07 (0.09) | -0.03 (0.02) | -0.02 (0.02) |
| I'91 v. I'92 | -0.07 (0.13) | -0.10 (0.08) | -0.07** (0.03) | -0.01 (0.02) |

Significance level: **=5%. Abadie-Imbens standard errors in parentheses.

Table A2b: **DIDM** results for Volume and Price change, Cleveland as treatment city and Chicago-Detroit-Philadelphia as control [quarter by quarter comparisons instead of aggregate comparisons]

| | **Volume change** | | **Price change** | |
|---|---|---|---|---|
| **Date** | **Control cities** | | | |
| | Detroit | Chicago - Detroit - Philadelphia | Detroit | Chicago - Detroit - Philadelphia |
| IV'02 v. II'03 | -0.02 (0.11) | 0.01 (0.08) | 0.03 (0.03) | 0.01 (0.03) |
| II'02 v. II'03 | -0.09 (0.08) | -0.12 (0.07) | 0.08 (0.05) | 0.03 (0.03) |
| III'02 v. III'03 | -0.09 (0.12) | -0.12 (0.09) | 0.08 (0.05) | 0.03 (0.02) |

Abadie-Imbens standard errors in parentheses.

Table A3a: **DID** results for Price and Volume change, Portland as treatment city and Albany-Hartford-Boston as control [quarter by quarter comparisons instead of aggregate comparisons]

| | **Volume change** | | **Price change** | |
|---|---|---|---|---|
| **Date** | **Control cities** | | | |
| | Albany (96) | Hartford - Boston - Albany (192) | Albany (96) | Hartford - Boston - Albany (192) |
| II'91 v. IV'91 | 0.02 (0.06) | 0.02 (0.04) | 0.03 (0.02) | 0.02 (0.02) |
| IV'90 v. IV'91 | -0.01 (0.17) | 0.07 (0.06) | -0.03 (0.03) | -0.02 (0.02) |
| I'91 v. I'92 | -0.06 (0.06) | -0.06 (0.06) | -0.07** (0.03) | -0.01 (0.02) |

Significance level: **=5%. Standard errors (in parentheses) are clustered at the brand level.

Table A3b: **DID** results for Price and Volume change, Cleveland as treatment city and Chicago-Detroit-Philadelphia as control [quarter by quarter comparisons instead of aggregate comparisons]

| | **Volume change** | | **Price change** | |
|---|---|---|---|---|
| **Date** | **Control cities** | | | |
| | Detroit | Chicago - Detroit - Philadelphia | Detroit | Chicago - Detroit - Philadelphia |
| IV’02 v. II’03 | -0.01 (0.03) | 0.00 (0.06) | 0.03 (0.03) | 0.02 (0.03) |
| II’02 v. II’03 | -0.08 (0.07) | -0.10 (0.08) | 0.08 (0.06) | 0.03 (0.02) |
| III’02 v. III’03 | -0.08 (0.11) | -0.11 (0.10) | 0.08 (0.05) | 0.03 (0.02) |

Standard errors (in parentheses) are clustered at the brand level.