USING LITTERED PACK DATA TO ESTIMATE CIGARETTE TAX AVOIDANCE IN NYC

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Using data on tax stamps obtained from random samples of littered packs of cigarettes, collected once before and three times after a June 2008 New York State tax increase, we find that baseline New York City tax avoidance is high relative to national estimates, and that rates of avoidance are particularly high in neighborhoods with high levels of poverty and in relatively close proximity to a Native American reservation. The share of littered packs with no tax stamp increased from 15 to 24 percent after the tax increase. We find that in addition to the large increase in avoidance, cigarette consumption declined.

Keywords: tax avoidance, interjurisdictional differentials, excise taxation, health and smoking

JEL Codes: H26, H73, H27, I18

I. INTRODUCTION

In recent years, state and local governments have enacted large increases in cigarette taxes, with the dual aims of increasing tax revenue and discouraging smoking. The effect of tax increases depends on the behavioral response of smokers. There is considerable evidence that many smokers are able to avoid state and local cigarette taxes by purchasing cigarettes from lower tax jurisdictions or from untaxed sources such as the Internet or Native American reservations. These tax avoidance opportunities may be enhanced by organized smuggling of cigarettes from low tax states.

In June 2008 New York State (NYS) raised its cigarette tax rate from $1.50 to $2.75, bringing the combined New York City (NYC) and NYS rate to $4.25 per pack, the highest in the United States. The increase raised the minimum retail price of a typical pack of cigarettes in NYC from $6.82 to more than $8.00 per pack.¹ In 2009, per pack

¹ NYS regulates the minimum price at which cigarettes can be sold at retail. This is explained in greater detail in footnote 29.
Cigarette taxes in the adjoining states of New Jersey, Connecticut, and Pennsylvania were respectively $2.70, $3.00, and $1.35, while the average rate for all U.S. states was $1.34.

Cigarettes are among the most heavily taxed of all commodities in the United States. Despite the fact that states tend to cluster in their cigarette tax rates — due to tax mimicking (Chernick, 2011) — very large differentials occur in places where local governments levy additional cigarette taxes. It is useful to focus on behavioral responses in high tax jurisdictions, because they provide information about maximum potential rates. If the principal behavioral effect of a tax change is to increase avoidance, then the health benefits from reduced smoking may be attenuated, and there will be little gain in tax revenues, as some critics charge.²

A methodological difficulty with studying the behavioral impact of tax changes is that we do not usually have direct high quality measures of tax avoidance and evasion. Previous literature on cigarette tax avoidance has generally relied either on econometrically inferred measures of avoidance or self-reports of avoidance rather than direct observation of behavior. Measures of tax avoidance, like measures of other economic phenomena, are most useful for predicting behavioral responses to policy changes when they are based on direct observations of behavior. In this paper, we draw on an innovative method of data collection to obtain direct estimates of avoidance behavior. The method, first used to study cigarette taxation in Chicago (Merriman, 2010), involves the collection of a random sample of littered cigarette packs from a representative sample of sub-areas. By examining the tax stamps on the collected packs, we can determine whether NYC and/or NYS cigarette taxes have been paid. Packs without a city or state stamp are considered to have avoided the tax, and the results can be used to estimate tax avoidance in the population.

The behavioral effects of a particular policy change may be difficult to identify, because other factors, such as economic conditions, may change at the same time as the policy. A particular strength of our experimental design, a significant improvement over Merriman’s (2010) method, is that because we knew about the tax increase well before it took effect, we were able to collect data immediately before and just after the tax increase. This sequence limits the potential influence of unrelated factors. Using data collected 15 months after the tax increase, we also estimate longer run effects.

We believe that our work makes three distinct contributions to the literature on tax avoidance. First, we extend the methodology of Merriman (2010) to a different city and therefore gain insight about the generalizability of those findings.³ Secondly, we improve upon the methodology of Merriman (2010) by employing a before-and-after structure that both helps us learn about the impact of a tax increase and mitigates potential bias if litterers are in some way unrepresentative of the general population of

² For example, Fleenor (2008, p. 1) argues that, “If history is any guide, most cigarettes sold will actually be trucked up from Virginia or shipped in from China …” See also Newman (2008).

³ In particular we wanted to know if there would be a sufficient density of littered packs in NYC to be sure that we would get useable data and we also wondered if littered pack collections would obtain results that were reasonably consistent over time and across space.
smokers. Merriman (2010) measured the relationship between proximity to a low tax source of cigarettes and tax avoidance. In addition to proximity we measure the change in avoidance after a tax increase. Finally, we display data from several rounds (after the tax increase) and therefore provide information about the stability of this measure of tax avoidance over time.

We find that the share of littered packs that had an appropriate NYC tax stamp fell from 55 percent prior to the tax increase to 49 percent immediately after the tax increase. In subsequent rounds of data collection three months and one year and three months after the tax increase the share with an appropriate stamp was essentially unchanged. In addition to the tax we find some empirical evidence that the level of avoidance is influenced by the share of residents in poverty in a Census tract and the distance to low tax sources of cigarettes. We find a relatively small elasticity of taxable sales so that tax revenue increased substantially after the tax increase. Our point estimate suggests that the tax increase caused cigarette consumption to fall from about 22.1 million to between 20.5 and 19.8 million packs per month, and we are able to reject the null hypothesis of no change in cigarette consumption at a 70 percent or greater level of confidence.

The paper has five additional sections. The next section provides a brief literature review. The empirical analysis is contained in Section III and is divided into three subsections: general methodology, data collection, and results. In Section IV we decompose the response to the tax increases into changes in consumption versus changes in avoidance, and in Section V we estimate revenues losses from cigarette tax avoidance in NYC. Section VI concludes.

II. LITERATURE REVIEW ON CIGARETTE TAX AVOIDANCE

Despite a relatively small share of consumer expenditures on cigarettes there has been a relatively large literature on tobacco tax avoidance, both because of its potential importance to public health and public finance and because it may serve as a prominent case to help us better understand more general principles of tax avoidance.\(^4\) We offer only a brief summary of the most relevant research methodologies and empirical results about cigarette tax avoidance. Merriman (2009) contains additional details.

A major problem in studying tax avoidance is that it is inherently difficult to observe. Early literature on cigarette demand functions (Baltagi and Levin, 1986; Chaloupka, 1991; Becker, Grossman and Murphy, 1994; Saba et al., 1995; Thursby and Thursby, 2000) econometrically estimated tax avoidance based on the residual correlation between tax-paid sales or survey-reported consumption and measures of access to low tax (or price) sources of cigarettes after controlling for other factors that might affect the demand

\(^4\) The 2009 Consumer Expenditure Survey from the U.S. Department of Labor reported that only 0.8 percent of consumer expenditures went to “tobacco products and smoking supplies.” See http://www.bls.gov/cex/#tables. Farrelly, Nonnemaker, and Watson (2012), however, report that low income smokers in NY spent almost one-quarter of their annual household income on cigarettes.
for cigarettes. These studies may provide unsatisfying analyses of tax avoidance since they do not rely on direct observation of the key behaviors.

Several papers do attempt to directly estimate tax avoidance. In a novel paper, Galbraith and Kaiserman (1997) note that during the particular historical period they studied, Canadian cigarette exports to the United States were sometimes (illegally) diverted back to Canada and sold tax free. They use data on exports of Canadian blend cigarettes to the United States as a direct (though inferred) measure of untaxed Canadian consumption. Stehr (2005) and Goolsbee, Lovenheim, and Slemrod (2010) measure tax avoidance as the difference between tax-paid sales (as reported in administrative data) and consumption as reported in survey data. These studies then analyze the sensitivity of inferred tax avoidance to some measure of access to low or no tax cigarettes (geographic proximity and internet connectivity, respectively). However, survey-reported cigarette consumption apparently greatly under-represents actual cigarette consumption. Stehr (2005, p. 280) finds that, “reported consumption as a percentage of tax-paid sales averaged 57.1% from 1985 to 2001. Using the correction for tax avoidance, this figure drops to 52.1%.” The under-reporting of cigarette consumption greatly complicates our ability to obtain a straightforward measure of tax avoidance by comparing reported consumption with administrative data measuring sales.

Hyland et al. (2005) and Chiou and Muehlegger (2008) use responses to surveys in which smokers were directly asked about the location of their purchases when the location of residence was known. These studies develop measures of avoidance by comparing the tax rate at respondents’ residences with the tax regime at their purchase locations. Both studies find significant tax avoidance and Chiou and Muehlegger’s (2008) simulations suggest that cross-border shopping may increase significantly in response to cigarette tax increases. However, since smokers greatly understate even the degree to which they consume cigarettes one might be skeptical of the information they provide about the degree to which they avoid or evade cigarette taxation.

Recent studies find quite a bit of variance in the amount of tax avoidance and its responsiveness to tax changes and proximity to lower-priced cigarettes. Stehr (2005) finds that between 59 and 85 percent of the taxable sales elasticity in the United States is due to changes in the locality of purchase. He estimates that almost 13 percent of cigarettes in 2001 were purchased without paying the home state tax. Goolsbee, Lovenheim, and Slemrod (2010) find that the price elasticity of taxable sales of cigarettes has almost doubled in recent years because avoidance has been facilitated through purchases on the Internet.6

5 Lovenheim (2008) uses a similar methodology but his dependent variable measures survey-reported-consumption. Other papers measure tax-paid sales.

6 See Ribisl, Kim, and Williams (2007) for more detailed information about internet cigarette purchases.
Lovenheim (2008) finds a zero elasticity with respect to home state price, suggesting the very strong conclusion that state tax increases have no impact on consumption. His estimates imply that about 20 percent of NYS’s cigarettes evade the tax and that the percentage of smokers consuming smuggled cigarettes declines sharply with distance to a lower price border.\(^7\) Chiou and Muehlegger (2008) find that consumers are willing to travel three miles to save $1 on a pack of cigarettes and estimate the rate of casual smuggling (cross border shopping) at only 4 percent.

The Lovenheim (2008) and Chiou and Muehlegger (2008) results are national in scope. Because car travel is the dominant mode of travel in the United States, their estimates of the relationship between tax avoidance and distance are based implicitly on national rates of automobile ownership and cost of travel. Estimates of casual avoidance by distance are likely to be different in NYC, since rates of automobile ownership are relatively low, and the cost of travel to the closest state cigarette tax border is greater because of the presence of tolls for bridges and tunnels between NYS and New Jersey.\(^8\)

Hyland et al. (2005) conducted telephone interviews with more than 900 randomly selected smokers in upstate New York counties that are close to Native American Reservations. About two-thirds of respondents said that they regularly purchased cigarettes from Native American reservations where state taxes may be evaded.

Recently Lakhdar (2008) has reported on a study of tobacco tax avoidance in France using various methodologies. One methodology was to extract cigarette packs from a waste collection plant in the Paris suburb of Nanterre in November 2005 and December 2006. About 19 percent of the packs were of foreign (usually lower priced) origin in 2005 and about 16 percent were of foreign origin in 2006. This level of tax avoidance was generally consistent with the estimates found using other methods. In the study that is most similar to ours, Merriman (2010) collected a random sample of littered cigarette packs from 135 areas in and around the city of Chicago. The study identified the share of packs for which the city, county, and state taxes were paid. It found that only about one-quarter of littered cigarettes packs had paid the city cigarette tax, which is more than $3 per pack higher than taxes in the neighboring state of Indiana. The study also found that the probability of tax avoidance rose significantly with proximity to the Indiana border.

In summary, recent studies find an implied level of cigarette tax avoidance ranging from about 4 percent at the national level (Chiou and Muehlegger, 2008) to as high as 75 percent at a local level (Merriman, 2010). Study of specific local areas such as NYC can contribute to a better understanding of the reasons that avoidance levels vary across time and over space.

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\(^7\) Because Lovenheim’s estimates imply both that consumption is unchanged with tax increases and that consumption declines as distance to the border increases, they imply that tax increases result in increased consumption among those close to the border.

\(^8\) There is an extensive public transportation network of buses and trains linking NJ and NYC. However, public transportation is slow, inflexible, and expensive compared to auto travel in uncongested areas, and is thus a poor substitute for auto travel for the purpose of cross-border shopping.
III. EMPIRICAL ANALYSIS

A. Background and Methodological Considerations

On April 11, 2008, the NYS legislature approved a cigarette tax increase of $1.25 per pack, to be implemented on June 3, 2008 (State of New York, Department of Public Health, 2008). Within a few days of the law’s passage, we drew up plans to conduct a before-and-after study of its effect using the littered pack method. We believed that this data collection strategy had the potential to make an important improvement to the methodology employed in Merriman (2010) by mitigating potential bias arising from the fact that littered packs may misrepresent the degree of tax avoidance among the population of all cigarettes consumed because individuals who litter may be more (or less) likely to avoid local cigarette taxes. So long as the relative bias in the littered pack sample is the same before and after the tax increase our methodology yields an unbiased estimate of the impact of the tax increase on avoidance.

In the United States cigarette packs are enclosed in cellophane wrapping at the conclusion of the manufacturing process and then shipped to regional wholesalers’ warehouses. When the location of retail sales is established, a tax stamp is affixed to the outside of the cellophane in all but three US states. Since the tax stamps are on the cellophane, if the cellophane has been separated from the pack it is not possible to determine whether the tax was paid. We used the packs with cellophane to study tax stamps but also collected and coded packs without cellophane to determine whether there was a systematic difference in the distribution of brands with and without cellophane.

Our data collectors gathered littered cigarette packs in a random sample of Census tracts in NYC. Collectors were instructed to pick up all packs. The Census tracts completely cover NYC so that, with appropriate weighting, the sampled locations provide a representative sample. Within a Census tract, we weighted each littered pack equally and calculated the share of packs that avoided taxation.

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9 Merriman (2010) reports on several procedures he used to infer the representativeness of the littered packs he collected. He finds for example that the brand distribution of the tax-paid packs in his litter sample is broadly similar to the brand distribution of tax-paid packs in scanner data. Merriman also compared littered packs to packs appropriately discarded in public garbage cans in three areas of the city and found no consistent differences between the litter samples and the garbage samples. Procedural and health obstacles prevented us from performing an analogous check in NYC. Williams, Curnow, and Streker’s (1997) study of littering in Australia found little evidence of consistent demographic or economic differences between litterers and non-litterers.

10 Every state except North and South Carolina and North Dakota require tax stamps. Many localities also require tax stamps. The process of transporting unstamped cigarettes is tightly regulated in the United States.

11 Note that our method weights each pack equally. In contrast, surveys of smokers generally weight each respondent equally. Chiou and Muehlegger (2008) find that heavier smokers are more likely to avoid taxes so that equal weighting of survey respondents will understate avoidance. By weighting each pack equally our method has the potential to remedy this potential defect of survey studies. If heavier smokers are equally likely to litter, but more likely to avoid the tax, then the littered pack method will appropriately show more avoidance than the survey method.
B. Data Collection Strategy

We designed a study with a sample of 30 of NYC’s 2,216 Census tracts. We first did a pre-test in four randomly selected Census tracts to determine the amount of time data collectors would need to spend in each tract to collect a sufficient number of littered packs. In the pre-test we collected an average of seven packs with cellophane from each tract. We then proceeded to the full study.

In the full study data collectors were given precise maps and were instructed to walk the periphery of each Census tract, picking up all littered packs. The data collectors were told to stop the data collection after they had found nine packs with cellophane or after 45 minutes, whichever came first. Two collectors were assigned to each Census tract. Each Census tract was surveyed (walked) once in each round of data collection. No collections were done on days with rain or on subsequent days if the ground was still wet. In subsequent rounds collectors were instructed to walk exactly the same route as in the first round. Collected packs were returned to the research office and coded there.

The relatively arbitrary decisions to collect a maximum of nine packs per track and to collect data in 30 Census tracts were made in light of time and budget constraints — we had a short period to collect data and a relatively small budget to fund data collection. Despite the relatively small sample sizes we can conduct surprisingly informative tests about population parameters and changes in the population parameters as we demonstrate later in the paper.

The mean population per Census tract in NYC in the 2005 data used to select sample tracts was 3,698 and mean employment was 1,695. We randomly selected sample Census tracts using the probability proportional to size method so that the probability each Census tract was in the sample was proportional to its weighted residential population and employment. Residential population was assigned a weight of three, and employment a weight of one. This weighting rule was based on the assumption that, with a five-day work week, the typical smoker smokes roughly three times as much in the Census tract of residence as in the Census tract of employment. Under this assumption our sample of littered packs should be representative of packs smoked by NYC residents and workers.\footnote{This weighting scheme neglects tourists and other visitors to the city because we lack data on their locations and frequency and duration of visits. It seems reasonable to believe that tourists and non-employee business travelers are most heavily concentrated in Manhattan and that our weighting scheme might thus underweight Manhattan. Merriman (2010) discusses the effect of tourists and other visitors on estimates of tax avoidance. Also, see footnote 15.}

Figure 1 shows a map of NYC, with sample tracts indicated. The largest number of selected tracts in any of the five boroughs was in Queens, with 11 tracts. There were eight tracts in Brooklyn, five in Manhattan, and three each in the Bronx and Staten Island. Figure 2 shows a map of the region surrounding NYC and shows the tax borders for New Jersey, NYS and Connecticut. The map also indicates the location of the Poospatuck Indian Reservation on Long Island which is allegedly the source of many of the untaxed cigarette packs that find their way to NYC (Caruso, 2008).
Figure 1
Map of NYC Census Tracts

<table>
<thead>
<tr>
<th>Borough</th>
<th>Miles to Poospatuck</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bronx</td>
<td>54.1</td>
</tr>
<tr>
<td>Manhattan</td>
<td>58.7</td>
</tr>
<tr>
<td>Queens</td>
<td>51.9</td>
</tr>
<tr>
<td>Brooklyn</td>
<td>59.2</td>
</tr>
<tr>
<td>Staten Island</td>
<td>70.6</td>
</tr>
</tbody>
</table>

Legends:
- Surveyed Tract
- New York City

Figure 2
Map of the Study Area Showing Tax Boundaries and Poospatuck Indian Reservation
Data were collected in four rounds. The first round was done between May 15 and May 30 of 2008, just before the state tax increase on June 3. The second round, done between June 16 and July 3, was designed to measure the immediate effect of the tax increase. To deal with potential problems of hoarding prior to a tax increase, and to allow more time for behavioral adjustments to the tax increase, a third round was implemented between September 8 and October 13 of 2008. Finally, a fourth round of data was collected 15 months after the tax increase, from September to October of 2009.

The three months of elapsed time from the date of the tax increase to the third round of data collection is short enough that it is reasonable to assume that the many other factors that may affect the purchasing and consumption behavior of smokers are essentially unchanged. Hence, the before-and-after comparison provides a relatively clean measure of the effect of increased cigarette taxes. The fourth round tests whether the immediate adjustments persist over time as other factors change.

As shown in Table 1, 223 packs with cellophane were collected in the first round, 262 packs were collected in both the second and third rounds, and 269 packs in the fourth round. The average number of packs with cellophane collected per Census tract was 7.4 packs in the first round, and 8.8 packs in subsequent rounds. In 58 percent of survey attempts, collectors were able to reach the goal of nine packs with cellophane within the 45 minute limit.

C. Results

1. Basic Tables

The basic results for the four rounds of data collection are shown in Table 2. We present statistics for five categories of packs: (1) no tax paid; (2) NYC tax stamps; (3) only NYS tax paid; (4) other state tax paid; (5) foreign tax paid or unknown. In general, we assume that the tax stamp found on a littered pack reveals the location of its purchase. A pack with a NYC stamp has paid both the NYS tax and the NYC tax. Packs with only a NYS stamp have not paid the NYC tax. Packs with no tax stamp may have been purchased on a Native American Reservation or in one of the three low

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13 One possible issue regarding our before and after comparison method is hoarding behavior by vendors or consumers. Despite the fact that vendors are assessed a “floor tax,” equal to the amount of the tax increase on old stamps that they have in inventory at the time of the tax increase, they may have incentive to hoard, because they are allowed to delay payment for several months, with no interest due. Cigarette stamps are identified both by their color and the name of the jurisdiction. When the tax rate is changed, the color of the stamp is changed. We found that the color of the stamp was largely unchanged in the collection round immediately following the tax increase, indicating that many packs with older stamps were being littered after the tax increase. Presence of an “old” stamp after the tax increase does not necessarily imply tax avoidance since retailers are required to charge (and pay) the new tax on any packs sold after the tax increase regardless of the color of the stamp. The predominance of older stamps in our second (June 16 to July 3) round of collection is consistent with both the hypothesis that consumers or vendors hoarded cigarette packs prior to the tax increase and the hypothesis that it takes time for the new stamps to be introduced into the cigarette distribution system. By our third data collection in September of 2008 we found almost none of the old stamps, which suggests that any stockpiling (by retailers or consumers) was irrelevant to the third round of collection.
tax states (North Carolina, South Carolina, and North Dakota) that do not require tax stamps. The table gives our point estimate for the proportion with each type of stamp in each round, the standard error on the point estimate, and tests of equality of proportions between the various rounds.\(^\text{14}\)

In round one (May 2008), 15 percent of packs had no tax paid, 55 percent had NYC tax stamps, 9 percent had NYS stamps, and 14 percent had stamps from other states.\(^\text{15}\) In round two (June 2008), the proportion with no tax paid increased to 24 percent, while the proportion with NYC stamps went down to 49 percent. The proportion with NYS only stamps was basically unchanged. Results from round three (September–October 2008) were very similar to round two as 24 percent of packs had no stamp and 48 percent had a NYC stamp. The percentage with a NYS stamp was basically unchanged, going

\[
\begin{array}{cccc}
\text{Round} & \text{With Cellophane} & \text{Without Cellophane} & \text{Total} \\
1 & 223 & 169 & 392 \\
2 & 262 & 156 & 418 \\
3 & 262 & 162 & 424 \\
4 & 269 & 159 & 428 \\
\end{array}
\]

\(^{14}\) We treat the proportions of each type of stamp as a random variable about which we have sample data. Our point estimates of the proportions weight each Census tract (rather than each pack) equally since our tract sampling procedures are designed to produce a representative sample of cigarettes smoked in NYC. Since tracts were weighted in the sample selection algorithm, the unweighted mean of the tract means is an unbiased estimated of the population mean. For more detail on statistical issues, see Merriman (2009). Because data on propensity to smoke by Census tract are not available our procedure implicitly assumes smoking propensity is constant across tracts. Alternatively we could weight by number of littered packs found (i.e., use raw percentages) which would implicitly assume that the density of littered packs varied with smoking propensity. We note that this procedure results in only minor changes in Table 2.

\(^{15}\) Some readers may wonder whether stamps from NYS and out-of-state stamps are likely to result from “incidental” littering by those who regularly commute into and out of the area for work. Only a small percentage of the NYC potential smoker population regularly commutes to or from NYC for work. Population census data show that about 76 percent of the 3.7 million people who work in NYC also live there. According to the same data, about 88 percent of the 2.9 million employed people who live in NYC also work in NYC. In the absence of tax avoidance (purposeful action to lower tax burdens), people who work in NYC but live elsewhere would purchase a significant share of their cigarettes in NYC, as would those who live in NYC but work elsewhere. The combined impact of these factors, using methods illustrated in Merriman (2010), would predict little incidental littering in the absence of tax avoidance. If commuters greatly changed their behavior to avoid the higher taxes in response to the tax increase, we would expect to find increased out-of-state stamps in the post-tax-increase litter collections.
## Table 2
Basic Findings Rounds 1, 2, 3, and 4 Share of Packs in Each Tax Payment Category

<table>
<thead>
<tr>
<th>Round</th>
<th>(1) Round 1</th>
<th>(2) Round 2</th>
<th>(3) Round 3</th>
<th>(4) Round 4</th>
<th>Test of Equality</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Round 1 versus 2</td>
</tr>
<tr>
<td>Period</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>05/15/08–05/30/08</td>
<td>0.15 (0.04)</td>
<td>0.24 (0.03)</td>
<td>0.24 (0.03)</td>
<td>0.24 (0.04)</td>
<td>0.052</td>
</tr>
<tr>
<td>06/16/08–07/03/08</td>
<td>0.55 (0.04)</td>
<td>0.49 (0.04)</td>
<td>0.48 (0.03)</td>
<td>0.48 (0.04)</td>
<td>0.2308</td>
</tr>
<tr>
<td>09/08/08–10/13/08</td>
<td>0.09 (0.02)</td>
<td>0.08 (0.02)</td>
<td>0.08 (0.02)</td>
<td>0.08 (0.02)</td>
<td>0.711</td>
</tr>
<tr>
<td>09/11/09–10/07/09</td>
<td>0.14 (0.02)</td>
<td>0.14 (0.03)</td>
<td>0.17 (0.03)</td>
<td>0.14 (0.02)</td>
<td>0.948</td>
</tr>
<tr>
<td></td>
<td>0.07 (0.02)</td>
<td>0.05 (0.01)</td>
<td>0.04 (0.01)</td>
<td>0.07 (0.01)</td>
<td>0.335</td>
</tr>
<tr>
<td></td>
<td>223</td>
<td>262</td>
<td>262</td>
<td>269</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. Calculations appropriately weight packs to give each Census tract equal weight.

1 The presence of a NYC stamp implies that the NYS as well as the NYC cigarette tax has been paid.

2 P value for test of hypothesis that mean in a given round equals mean in round 1.
from nine to 8 percent in the two rounds after the increase. In the fourth round (September–October 2009), 24 percent of the packs we found again had no tax stamp. The total number of packs with a NYC stamp was very similar to rounds two and three. There was little change in the proportion of NYS, other state, or foreign tax stamps in round four.

The similarity in the proportions across all four rounds of data collection supports the overall validity of the littered pack method. The direction of change in the pattern of stamps is consistent with the hypothesis that tax avoidance increased when the tax increased. Regarding timing, the similarity of results in the three post-increase rounds of data collection suggests that much of the adjustment to the tax increase was immediate, and led to a permanent increase in avoidance. Despite the relatively small number of Census tracts sampled, the hypothesis that the proportion of packs without tax stamps was constant before and after the tax increase has little support (p-value of almost 0.05).

It is notable that almost all of the increase in tax avoidance comes from an increase in packs without stamps, rather than an increase in packs with stamps from other states. Such purchases could be done via the Internet, through direct purchases by consumers at Native American reservations, or by illegal purchases from smugglers who transport cigarettes from reservations to the streets of NYC. Surveys, anthropological evidence on illegal street purchases, and court cases brought by NYC all suggest that purchases from reservations are an important avoidance method (Shelley et al., 2007; City of New York, 2009).

At the time of our study, the street cost of smuggled cigarettes from Virginia or North Carolina was likely to be higher than the street cost of untaxed cigarettes from nearby Native American reservations because of the tax in those states and the greater distance the illegal packs must be transported. As discussed by Merriman (2002), the supply cost of smuggled cigarettes is likely to be increasing in the amount of smuggled cigarettes. The supply cost will depend on the cost at the source and the distance from source to destination. This is because transport costs are likely to depend on distance, both directly because of the cost of shipping and because the probability of detection is likely to increase with distance traveled.

The expected higher cost of long-distance smuggled cigarettes is supported by our littered pack data, which show untaxed packs to be more important as a source of tax avoidance than cigarettes from low-tax distant states. After the tax increase, we found that untaxed cigarettes went up as a share of littered packs, while long-distance, low-tax cigarette shares remained unchanged. This result is consistent with administrative data showing a large increase in the number of packs imported onto nearby reservations from which they are likely to be resold without the state tax being paid. Anecdotal evidence suggests that most untaxed cigarettes from nearby reservations are brought into NYC by smugglers. The profits from taxed avoidance are divided between the wholesaler (the reservation), the retailer (the smuggler), and the consumer. However, we lack data on the change in the price of smuggled cigarettes after the tax increase, and hence are unable to determine the shares of the tax savings that went to the various participants. The shares of littered packs with a NYC stamp, or no tax stamp at all, are broken out by Census tract before and after the tax increase in Figures 3 and 4. We might expect
Figure 3
Share NYC Tax Paid Before and After Tax Increase by Tract

Notes: “Before” is percentage in round 1. “After” is mean percentage in rounds 2 to 4. Tract numbers correspond to map in Figure 1.
Figure 4
Share No Tax Stamp Before and After Tax Increase by Tract

Notes: “Before” is percentage in round 1. “After” is mean percentage in rounds 2 to 4. Tract numbers correspond to map in Figure 1.
that the share of packs with a NYC stamp would go down after the tax increase. If geographic proximity to unstamped cigarettes is a key determinant of post-tax increase tax avoidance, we might expect to see larger declines in NYC stamps in Queens than in Brooklyn. Brooklyn tracts 5, 8 and 10 (Figure 1) do show increases or no decreases in NYC tax stamps after the tax increase. Brooklyn tracts 4 and 6 however, show declines in NYC tax stamps. Tracts in Queens, which is much closer to the Poospatuck reservation tracts that already had low shares of packs with NYC stamps, do not show declines (tracts 27 and 21). Queens tracts 25 and 26 do, however, show large declines in NYC stamps after the tax increase. The story is clearly complicated and, at the tract level, location alone does not fully predict either the share of packs with NYC stamps or the change in the share after the tax increase. Figure 4 is similar to Figure 3 but shows the share of packs with no tax stamp by tract before and after the tax increase. Once again, locational proximity by itself cannot fully explain the observed patterns with some tracts that are relatively remote from Poospatuck (e.g., tract 10 in Brooklyn) having relatively high shares of untaxed stamps.

As shown in Table 2, in rounds 1 and 2, 14 percent of packs had stamps from other states. The percentage increased to 17 percent in round 3, but dropped back to 14 percent in round 4. Thus, for every three to four packs with a NYC stamp, we found one with a stamp from another state. While this represents a significant proportion of cigarettes smoked in NYC, there was no significant change in the proportion from other states after the tax increase.

Table 3 shows the distribution by state of packs with stamps from states other than New York. This table is based on small and un-weighted numbers so we caution readers that changes between rounds and differences across states should be interpreted carefully. Column 1 of the table shows the relevant state tax rate. The most important source is Virginia, with 40 percent or more of the other-state stamps in the post-increase rounds. Other important sources are New Jersey and Florida. Virginia and Florida had the fourth and fifth lowest cigarette tax rates in the country, respectively. While the NJ rate of $2.58 per pack was only 17 cents less than the NYS rate, the difference between the NJ rate and the NYC rate was $1.67. Thus, our data indicate that about 15 percent of littered packs in NYC were purchased in other states. Almost half of the out-of-state purchases were from distant but very low-tax states, suggesting that tax differentials play an important role in consumption choices.

---

16 Another potential source of information about the impact of the tax increase on tax avoidance is the annual NYC Community Health Survey (See “Survey Data on the Health of New Yorkers,” The New York City Department of Health and Mental Hygiene, http://www.nyc.gov/html/doh/html/survey/survey.shtml). Our analysis of this data shows a jump in non-taxed sales of a similar order of magnitude to that found in the littered pack data after the tax increase. See Merriman and Chernick (2011) for details.

17 See “State Sales, Gasoline, Cigarette, and Alcohol Tax Rates by State, 2000–2010.” Tax Foundation, http://www.taxfoundation.org/publications/show/245.html. The most popular brands were Marlboro (47 percent), Newport (27 percent), and Parliament (10 percent); roughly 2 percent of packs (1.7 percent of packs with cellophane) were Native American brands. We found only 2 packs with Native American brands in round 1 but 7, 8, and 16 packs respectively in rounds 2, 3, and 4.
Table 3
Percentage of Packs with Non-NYS Stamps by State and Round of Data Collection

<table>
<thead>
<tr>
<th>Issuing State</th>
<th>State Tax Rate</th>
<th>Pre-tax</th>
<th>Post-tax Round 2</th>
<th>Post-tax Round 3</th>
<th>Post-tax Round 4</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>01/01/2008</td>
<td>05/15/08–05/30/08</td>
<td>06/16/08–07/03/08</td>
<td>09/08/08–10/13/08</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(N=36)</td>
<td>(N=32)</td>
<td>(N=43)</td>
<td>(N=40)</td>
</tr>
<tr>
<td>California</td>
<td>0.87</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>2.5</td>
</tr>
<tr>
<td>Connecticut</td>
<td>2.00</td>
<td>0</td>
<td>0</td>
<td>2.3</td>
<td>2.5</td>
</tr>
<tr>
<td>Delaware</td>
<td>1.15</td>
<td>2.8</td>
<td>9.4</td>
<td>4.6</td>
<td>2.5</td>
</tr>
<tr>
<td>Florida¹</td>
<td>0.34</td>
<td>19.4</td>
<td>25</td>
<td>9.3</td>
<td>5</td>
</tr>
<tr>
<td>Georgia</td>
<td>0.37</td>
<td>0</td>
<td>0</td>
<td>2.3</td>
<td>2.5</td>
</tr>
<tr>
<td>Illinois</td>
<td>0.98</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>2.5</td>
</tr>
<tr>
<td>Massachusetts</td>
<td>1.51</td>
<td>0</td>
<td>3.1</td>
<td>0</td>
<td>2.5</td>
</tr>
<tr>
<td>Missouri</td>
<td>0.17</td>
<td>2.8</td>
<td>3.1</td>
<td>2.3</td>
<td>0</td>
</tr>
<tr>
<td>New Hampshire</td>
<td>1.08</td>
<td>0</td>
<td>0</td>
<td>4.6</td>
<td>0</td>
</tr>
<tr>
<td>New Jersey</td>
<td>2.58</td>
<td>22.2</td>
<td>9.4</td>
<td>14</td>
<td>30</td>
</tr>
<tr>
<td>Pennsylvania</td>
<td>1.35</td>
<td>8.3</td>
<td>6.3</td>
<td>7</td>
<td>2.5</td>
</tr>
<tr>
<td>Virginia</td>
<td>0.30</td>
<td>36.1</td>
<td>40.6</td>
<td>40</td>
<td>47.5</td>
</tr>
<tr>
<td>Washington, DC</td>
<td>1.00</td>
<td>2.8</td>
<td>3.1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Don’t know</td>
<td>5.6</td>
<td>0</td>
<td>14</td>
<td>0</td>
<td>0</td>
</tr>
</tbody>
</table>

¹ Florida’s cigarette tax increased to $1.34 in 2009.
Our estimated rates of avoidance in NYC are lower than those found in Chicago (Merriman, 2010) where about three-quarters of littered packs lacked a local tax stamp. A number of factors might explain this difference. In Chicago, a significant portion of the cigarette tax can be avoided by leaving the city, the county, and the state. The $2 per pack county tax can be avoided at many locations in three separate directions (north, west, or south). In Chicago, city, county, and some state taxes can be avoided by traveling to Indiana which results in a saving of $3.36 per pack. In NYC cross-border shopping will not save consumers as much money since the tax differential with the closest neighboring state, New Jersey, is lower than the differential with neighboring areas in Chicago. Travel to Pennsylvania is quite lengthy and may require car travel. Traveling to Poospatuck will save consumers about $4.25 per pack but the reservation is only a single location that is probably unfamiliar location to most individuals. Thus, individual tax avoidance in NYC is probably more difficult than in Chicago. Large scale tax avoidance by smugglers may be discouraged by potential exposure to civil and criminal penalties.

2. Regression Analysis: Geographic Variation in Avoidance Patterns

To investigate whether patterns of tax avoidance differed systematically by population characteristics and location of the sample Census tracts, we aggregated avoidance data to the tract-round level and merged this data with demographic variables from the 2005–2010 American Community Survey (ACS) 5-year estimates, and as-the-crow-flies measures of distance from the Census tract to the nearest borders with NYS, New Jersey, and the Poospatuck Native American Reservation (located in Suffolk County of NYS). We expect that our dependent variable — the share of packs without a tax stamp — will fall with distance from Poospatuck but will rise with distance to the NJ and NYS borders since packs with these stamps are potential substitutes for unstamped packs. ACS data included the percentage of each tract’s population below the poverty line, median income, and a set of racial and ethnic variables. With 30 tracts in the sample and four rounds of data collection, there are 120 observations.

In Table 4 we report estimates from a set of ordinary least squares regressions in which the dependent variable is the proportion of packs without a tax stamp and the independent variables include the percentage of individuals in the tract in poverty, a dummy variable indicating whether the data was collected before the tax increase (round one) or after the tax increase (rounds two, three and four), and various indicators of the Census tract’s location. All regressions are unweighted and standard errors are estimated clustering at the tract level and bootstrapped.

---

18 ACS five-year estimates use five years of survey data to create estimates of selected economic and demographic characteristics of tracts (U.S. Census Bureau, 2011).
19 In earlier versions of this paper we did similar analyses with U.S. Census data and also looked at the percentage of households in rental units. None of the substantive results were substantially different with these data.
20 If stringency of tax enforcement differed across locations it might be important to include such measures in our specification. However, NYC and NYS tax authorities did not indicate there was any differential in tax enforcement resources across NYC, while an official with the Department of Finance said they had no information on this issue.
Table 4
Regressions of Share No Tax Paid on Census Tract Characteristics
Observations: 120

<table>
<thead>
<tr>
<th></th>
<th>(1) Model 1</th>
<th>(2) Model 2</th>
<th>(3) Model 3</th>
<th>(4) Model 4</th>
<th>(5) Model 5</th>
<th>(6) Model 6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Round 1 dummy</td>
<td>–0.088**</td>
<td>–0.133*</td>
<td>–0.088**</td>
<td>–0.088**</td>
<td>–0.133*</td>
<td>–1.969</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td>(0.071)</td>
<td>(0.042)</td>
<td>(0.041)</td>
<td>(0.075)</td>
<td>(1.677)</td>
</tr>
<tr>
<td>Percent poverty</td>
<td>0.004***</td>
<td>0.006</td>
<td>0.003*</td>
<td>0.005***</td>
<td>0.007</td>
<td>0.010</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.005)</td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.005)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Percent poverty *</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post tax increase dummy</td>
<td>–0.003</td>
<td>–0.003</td>
<td>–0.006</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.007)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bronx</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.224***</td>
<td>0.557***</td>
<td>0.557***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.139)</td>
<td>(0.148)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Brooklyn</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.173***</td>
<td>0.290***</td>
<td>0.290***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.060)</td>
<td>(0.062)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Queens</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.149***</td>
<td>0.280**</td>
<td>0.280**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.111)</td>
<td>(0.115)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Manhattan</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.170***</td>
<td>0.480***</td>
<td>0.480***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.111)</td>
<td>(0.117)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to NJ border (miles)</td>
<td>0.005</td>
<td>0.029***</td>
<td>0.055***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.009)</td>
<td>(0.017)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to NYS border (miles)</td>
<td>0.008</td>
<td>0.051***</td>
<td>0.035</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>-------------------------------</td>
<td>-------</td>
<td>-----------</td>
<td>-------</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.016)</td>
<td>(0.035)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to Poospatuck (miles)</td>
<td>−0.011</td>
<td>−0.025*</td>
<td>−0.002</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.014)</td>
<td>(0.028)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to NJ border (miles) *</td>
<td>−0.011</td>
<td>−0.025*</td>
<td>−0.002</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post tax increase dummy</td>
<td>0.022</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to NYS border (miles) *</td>
<td>0.022</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post tax increase dummy</td>
<td>0.039</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to Poospatuck (miles) *</td>
<td>−0.031</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post tax increase dummy</td>
<td>(0.034)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.166***</td>
<td>0.178***</td>
<td>0.038</td>
<td>0.686</td>
<td>0.695</td>
<td>1.154</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.029)</td>
<td>(0.033)</td>
<td>(0.554)</td>
<td>(0.731)</td>
<td>(0.861)</td>
</tr>
<tr>
<td>Adjusted R-squared</td>
<td>0.083</td>
<td>0.079</td>
<td>0.115</td>
<td>0.091</td>
<td>0.164</td>
<td>0.165</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Dependent variable is share of littered packs with no stamp. Data on Census tract means are pooled over four rounds of data collection. Standard errors are block bootstrapped at the Census tract level using 50 replications. Staten Island is the omitted borough dummy in models 3, 5, and 6.
In the first column of Table 4, the coefficient on the round one dummy variable is statistically significant and indicates that, on average, the share of packs that avoided the tax increased about 9 percentage points after the tax increase. The poverty rate has a statistically and quantitatively significant effect on avoidance. A coefficient of 0.004 (specification 1) on the poverty rate implies that a one standard deviation (13.1 percent) increase above the mean poverty rate (17.8 percent) would lead to a 5 percentage point increase in no tax paid, from 22 to 27 percent. In regressions that are not reported here, we also included as independent variables (in place of the poverty rate) the ratio of employment to population, median family income, and the share of non-white households. Other measures of economic circumstances also indicted that areas with higher economic deprivation had increased tax avoidance.\textsuperscript{21}

Model 2 adds a term that interacts the poverty rate with a dummy variable equaling one for round 1 and zero for rounds 2, 3, and 4 occurring after the tax increase. The coefficient is positive and has a large magnitude, but we do not estimate it precisely enough to reject the hypothesis that higher poverty tracts had the same increase in tax avoidance as those with low poverty.

Model 3 adds dummy variables for the boroughs of NYC. Despite the fact that Census tract poverty rates are correlated within a borough we find an independent effect of borough. The results indicate that avoidance rates in the Bronx are highest followed by Brooklyn, Manhattan and Queens. Staten Island, the (omitted) reference borough has the lowest avoidance rates. Model 4 replaces borough dummies with as-the-crow-flies distance variables to the New Jersey and New York State borders and to the Poospatuck Native American reservation. Model 5 includes all of the variables used in any of the first four specifications. Model 6 includes distances to tax borders interacted with a round one dummy.

In model 4 the coefficients on the distance variables are statistically insignificant but have the signs we might expect if consumers went to Poospatuck to purchase unstamped cigarettes and if crossing the city border (to NYS) or the state border (to NJ) were a substitute for going to Poospatuck to buy unstamped cigarettes. Somewhat surprisingly, in models 5 and 6 the point estimates of all the coefficients except some of the variables interacted with the round 1 are statistically significant. In both specification 5 and 6 all of the distance variables have the theoretically expected signs and are statistically significant. In model 5 the point estimate of our coefficient on distance to Poospatuck implies that the share of packs without a tax stamp falls 2.5 percent for each mile of distance to Poospatuck (a minimum distance of 46.7 miles and a maximum distance of 72.7 miles). These results are even stronger than those of Merriman (2010), who found that a one-mile increase in distance to the lower-tax state border increases the probability of a local stamp by about 1 percent. Although our results have a rather large confidence interval, it seems plausible that distance could be even more important in NYC — where cigarettes bought on a Native American reservation can avoid all state

\textsuperscript{21} Substantive conclusions about the other independent variables and interaction terms are essentially unchanged when we substitute a different measure of economic conditions for the poverty rate.
and local taxes — than in Chicago, where travel makes it possible only to avoid some state or local taxes.

Model 6 interacts the distance to tax border variables with a dummy variable that equals one after the tax increase (rounds 2, 3, and 4) and zero in round 1. The coefficients on these interacted variables allow us to investigate whether distance to lower tax sources of cigarettes became a more important determinant of tax avoidance after the tax increase than it was before the increase. We might expect the avoidance gradient to become flatter after the tax increase so that the sign on interacted distance to NJ or NYS should be negative while the sign on interacted distance to Poospatuck should be positive. The coefficients on both interacted distance to NYS (positive sign) and interacted distance to Poospatuck (negative sign) are statistically insignificant but have counter-intuitive signs. The statistical insignificance of the coefficients is perhaps not too surprising; with only 120 observations our regressions have limited power. The interacted coefficient on distance to NJ is more than twice as large as its standard error and has a negative sign, in accordance with intuition. Take together these results might suggest that smokers’ propensity to evade taxes is responsive to distance but that the tax evasion gradient changes little when the size of the price difference increases without a change in sign.

The empirical results also suggest that tax avoidance may be higher in poorer NYC neighborhoods. Merriman (2010) found that higher poverty rates were associated with less rather than more tax avoidance in Chicago. We believe that this difference between the NYC and Chicago might be attributable to differences in the costs and mechanisms of avoidance in the two locations. The incentive to avoid cigarette taxes depends on the potential savings from obtaining untaxed or lower tax cigarettes, relative to income. All other things equal, the lower the income level, the greater the relative economic burden of taxes at any level of cigarette consumption. Therefore, one might expect that poor smokers would have a greater incentive to avoid taxes. This income-related pattern of incentives will be reinforced if avoidance costs are lower for poor smokers, but offset if avoidance costs vary inversely with income.

Because the poor are less likely to own a car, the marginal cost of traveling to border states to avoid cigarette taxes — the main source of avoidance in Chicago — is likely to be greater relative to income for poor smokers. By contrast, in NYC, untaxed or lower taxed cigarettes are more likely to be sold illegally by vendors who bring them to poor neighborhoods. This difference is related to the proximity of Native American reservations in NYC and the greater density of population in NYC than Chicago. Proximity makes it cheaper for bootleggers to obtain untaxed cigarettes, while the higher density

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22 Readers are cautioned that the insignificance of some distance variables in specification 6 does not cast doubt on the tax evasion gradients we found in specification 5. To determine the impact of distance in specification 6, we must calculate the joint impact of the distance variable and the distance variable interacted with the relevant post tax increase dummy.

23 Kurti (2011) presents evidence of substantial tax avoidance in one of the poorest areas of NYC, the South Bronx, also using the littered pack method of data collection.
in NYC makes it more profitable to sell illegal cigarettes at street level because a supplier of untaxed cigarettes is more likely to have sufficient demand to cover his or her fixed costs at any particular location.24

IV. DECOMPOSING CHANGES IN TAXED SALES INTO CHANGES IN CONSUMPTION AND AVOIDANCE

A crucial question in evaluating the social benefits and costs of cigarette tax increases is the effect of such increases on cigarette consumption. In this section we combine the littered pack data with data on the sales of NYC tax stamps to estimate the effect of the June 2008 tax increase on cigarette consumption. We calculate confidence intervals around our point estimates.25

The total number of packs consumed in NYC (C = Consumption), which is unobserved, is by definition

\[
C \equiv \left( \frac{1}{\text{TAXED}_{\text{NYC}} / C} \right)^{\text{TAXED}_{\text{NYC}}},
\]

where \( \text{TAXED}_{\text{NYC}} \) is the number of packs on which NYC (and NYS) taxes were paid. Our litter data (reported in Table 2) provide consistent and unbiased estimates of the ratio \( \left( \text{TAXED}_{\text{NYC}} / C \right) \). We use this data to estimate \( Z = \left( \text{TAXED}_{\text{NYC}} / C \right)^{-1} \) and its variance as \( V(Z) = \sigma_Z^2 \).26 We use observed cigarette tax stamp sales to measure \( \text{TAXED}_{\text{NYC}} \) in each period. Using this framework in Appendix 1 we derive standard errors for our estimates of consumption, the change in consumption, avoidance, the change in avoidance, and the price elasticities of consumption and avoidance.

Although (1) is conceptually straightforward, aligning the timing of data collection, and hence our estimate of \( Z \), with taxed retail sales presents practical difficulties. NYS collects and reports monthly data on sales of cigarette tax stamps to cigarette wholesalers. The wholesalers affix the stamps to cigarette packs and transfer the stamped packs to retailers. Because there is a strong financial incentive for wholesalers to minimize

24 Discussions with attorneys in the NYC legal department suggest that they believe that the latter method (i.e. larger scale bootlegging) is the main way in which cigarettes from Poospatuck reach NYC. See also City of New York (2009). Smokers in NYC or Chicago could theoretically also obtain low or no tax cigarettes through the internet. In practice, however this does not seem to be a popular option. As discussed above NYS has made vigorous efforts to make internet cigarette sales inconvenient. Illinois has done less enforcement but has demanded payment from internet cigarette buyers on occasion (Jones, 2005). Also, at the time Merriman’s (2010) study was done taxes were so low in Indiana ($0.95) relative to Chicago ($4.05) that the incentive to purchase packs on the internet and wait for delivery was small.


26 We estimate \( E(Z) \) and \( \sigma_Z \) using our littered data sample and the Stata command “ratio” with data weighted by Census tract.
the time between their stamp purchases and transfer to retailers, and because cigarettes lose freshness rapidly, the lag between wholesalers’ tax stamp purchases and retail sale, consumption, and subsequent littering by smokers is normally brief. However, there is compelling empirical and anecdotal evidence that, immediately prior to a tax increase, wholesalers stockpile tax stamps and that, immediately after a tax increase, tax stamp sales fall. This pattern is illustrated for the four most recent tax increases in NYC in Figure 5. In each case we see a jump in tax stamp sales in the period immediately prior to the tax increase and a sharp decline immediately after the tax increase.

Table 5 presents monthly data on sales of NYC tax stamps from January 2007 through March 2010. As can be seen in Figure 5 there is a clearly discernible surge in sales in April and May of 2008, immediately before the tax increase, and a clear downturn in sales immediately after the tax increase (June 2008). By July 2008 it seems that sales of tax stamps were relatively unaffected by the pre-tax surge.

![Figure 5](image_url)

Vertical lines indicate tax increases
NYC (dotted line) 7/2/2002

27 As discussed in footnote 13 wholesalers cannot evade the tax increase by stockpiling but still may have an incentive to stockpile since they are normally given an interest-free grace period of several months to pay for the tax increase.
In order to measure retail sales of legally stamped cigarette packs for the months corresponding to each of our data collections, we use average monthly sales in the two months prior to the data collections in September 2008 (round 3) and September 2009 (round 4). We believe these measures of stamp sales are relatively “uncontaminated” by stockpiling related to the 2008 tax increase. However, because of stamp stockpiling there seems to be no uncontaminated measures of legal NYC retail sales near the period of our round 1 and round 2 data collections in May and June of 2008. We estimate May 2008 retail sales using the average value of NYC tax stamp sales in March and April of 2007.

We do not attempt to estimate June 2008 legal retail sales because we have no clear methodology of disentangling the effects of wholesale stamp stockpiling and behavioral changes in response to the tax increase.

Some of our calculations are summarized in Table 6. As shown in row 1 of Table 6, $TAXED_{NYC}$ fell from 12.2 million stamps per month prior to the tax increase to about

---

**Table 5**

<table>
<thead>
<tr>
<th>Month</th>
<th>2007</th>
<th>2008</th>
<th>2009</th>
<th>2010</th>
</tr>
</thead>
<tbody>
<tr>
<td>January</td>
<td>10.7</td>
<td>10.4</td>
<td>8.1</td>
<td>7.3</td>
</tr>
<tr>
<td>February</td>
<td>10.5</td>
<td>11.5</td>
<td>8.8</td>
<td>8.0</td>
</tr>
<tr>
<td>March</td>
<td>9.9</td>
<td>9.9</td>
<td>8.1</td>
<td>9.3</td>
</tr>
<tr>
<td>April</td>
<td>14.5</td>
<td>16.6</td>
<td>11.2</td>
<td></td>
</tr>
<tr>
<td>May</td>
<td>13.2</td>
<td>16.9</td>
<td>9.5</td>
<td></td>
</tr>
<tr>
<td>June</td>
<td>13.5</td>
<td>7.9</td>
<td>9.7</td>
<td></td>
</tr>
<tr>
<td>July</td>
<td>13.7</td>
<td>10.1</td>
<td>10.3</td>
<td></td>
</tr>
<tr>
<td>August</td>
<td>13.7</td>
<td>9.4</td>
<td>10.7</td>
<td></td>
</tr>
<tr>
<td>September</td>
<td>11.5</td>
<td>10.3</td>
<td>8.7</td>
<td></td>
</tr>
<tr>
<td>October</td>
<td>14.1</td>
<td>10.9</td>
<td>9.8</td>
<td></td>
</tr>
<tr>
<td>November</td>
<td>12.7</td>
<td>9.4</td>
<td>9.0</td>
<td></td>
</tr>
<tr>
<td>December</td>
<td>13.8</td>
<td>11.9</td>
<td>10.5</td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>151.7</td>
<td>135.1</td>
<td>114.3</td>
<td></td>
</tr>
</tbody>
</table>

Source: Office of Tax Policy Analysis of the NYS Department of Taxation

---

28 We regressed monthly wholesale sales of NYC tax stamps from January 1999 to March 2010 on a series of monthly dummies, a set of four dummy variables representing the four state and local taxes increases in the period, and a monthly time trend. Each of the tax increase dummies was set equal to zero prior to the relevant tax increase and one for all months after the tax increase. The coefficient on the monthly time trend was −0.014 (with a standard error of 0.013) suggesting that, after controlling for seasonal effects and tax changes, there is no time trend in wholesale sales of NYC excise tax stamps.
### Table 6
Taxed Sales, Estimated Consumption and Avoidance in NYC Cigarette Market before and after June 3, 2008 Tax increase

<table>
<thead>
<tr>
<th></th>
<th>Before Tax Increase</th>
<th>After Tax Increase</th>
<th>Change</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Round 1</td>
<td>Round 3</td>
<td>Round 4</td>
</tr>
<tr>
<td></td>
<td>05/15/08–05/30/08</td>
<td>09/08/08–10/13/08</td>
<td>09/11/09–10/07/09</td>
</tr>
<tr>
<td>Taxed sales (observed) ($millions per month)²</td>
<td>12.17</td>
<td>9.79</td>
<td>9.53</td>
</tr>
<tr>
<td>Point estimate of ratio consumption to taxed sales³</td>
<td>1.82</td>
<td>2.09</td>
<td>2.08</td>
</tr>
<tr>
<td>Standard deviation of ratio consumption to taxed sales³</td>
<td>0.13</td>
<td>0.15</td>
<td>0.16</td>
</tr>
<tr>
<td>Implied Consumption (millions of packs per month)⁴</td>
<td>22.14</td>
<td>20.48</td>
<td>19.84</td>
</tr>
<tr>
<td>Standard deviation of implied consumption (millions of packs per month)</td>
<td>1.57</td>
<td>1.46</td>
<td>1.52</td>
</tr>
<tr>
<td>Implied Avoidance (millions of packs per month)⁵</td>
<td>9.98</td>
<td>10.70</td>
<td>10.32</td>
</tr>
<tr>
<td>Standard deviation of avoidance (millions of packs per month)</td>
<td>1.57</td>
<td>1.46</td>
<td>1.52</td>
</tr>
</tbody>
</table>

**Notes:**
1. Price per pack from Minimum Wholesale and Retail Cigarette Prices New York State Department of Taxation and Finance Publication 509 various dates.
2. Taxed sales (Retail NYC tax stamp sales) were obtained from the Office of Tax Policy Analysis of the NYS Department of Taxation.
3. We omit round 2 data since, as explained in the text we are unable to derive reliable data on taxed sales for that round.
4. Round 1 taxed sales are assumed to be average NYC tax stamp sales in March and April 2007. Round 3 taxed sales are assumed to be average sales in July and August 2008. Round 4 sales are assumed to be average sales in January to December 2009.
5. The ratio of consumption to taxed sales and the standard deviation of this ratio are estimated using the littered pack sample in Table 2.
6. Implied consumption × taxed sales × (consumption/taxed sales).
7. Implied avoidance is consumption minus taxed sales.
9.8 million shortly after the tax increase (the time period corresponding to round 3) and to about 9.5 million after one year and three months (the time period corresponding to round 4), declines of 20 and 22 percent, respectively. Sales with a NYC tax stamp fell from 55 percent in round 1 to 48 percent in rounds 3 and 4 (Table 2). The net effect of these two changes implies a decline in total consumption. The point estimates imply a decrease in consumption of 1.66 million (7.5 percent) between rounds 1 and 3 and a decrease in consumption of 2.3 million (10.4 percent) between rounds 1 and 4.

The standard errors on the change in consumption are rather wide, however, and the estimates of the ratio of consumption to taxed sales (Z) are sufficiently imprecise that the highest level of confidence at which we can reject the hypothesis that consumption is the same for the first and third rounds is 70 percent, and 77 percent for the first and fourth rounds.

Avoidance is calculated as estimated consumption minus taxable sales. As shown in Table 6, we estimate that monthly avoidance went from about 9.98 million to 10.70 million packs from round 1 to round 3, but fell to 10.32 million in round 4. As is the case for consumption, our estimates are imprecise and the highest level of confidence at which we can reject the hypothesis of no increase in avoidance between rounds one and three is 59, and the highest level of confidence at which we can reject the hypothesis of no increase in avoidance between rounds one and four is 54 percent.

In order to compare our results to others in the literature, it is helpful to express our findings as implied price elasticities of taxed sales, consumption, and avoidance. These elasticities can be calculated based on the data displayed in Table 6 combined with data on retail cigarette prices. The NYS Department of Taxation and Finance regulates minimum retail cigarette prices. The minimum price of a typical cigarette pack sold in NYC increased from $6.82 at the time round 1 data were collected to approximately $8.21 at the time round 3 data were collected. Price rose to $8.63 at the time round 4 data were collected because of a 38 cents per pack increase in the federal cigarette excise tax that took effect on April 1, 2009.

The implied price elasticity of taxable sales is therefore –0.96 from round 1 to round 3, while the elasticity from round 1 to round 4 is –0.82.
The first elasticity estimate is substantially smaller than the most recent estimates of the price elasticity of taxable sales (Goolsbee, Lovenheim, and Slemrod, 2010; Chernick, 2011). The round 1 to round 4 calculation is even smaller than the round 1 to 3 estimate. This is probably due to the fact that the price increase between round 3 and round 4 was due to an increase in the federal cigarette tax and hence more difficult to avoid than a state or city-specific increase.

The point estimates of the price elasticities of consumption are 0.37 (with a standard deviation = 0.52) (round 1 to round 3) and 0.39 (standard deviation = 0.44) (round 1 to round 4), which are reasonably similar to recent national estimates of the price elasticity of cigarette consumption. The point estimates of the avoidance elasticity — the percentage change in avoidance as a result of a 1 percent change in price — is 0.35 (standard deviation = 0.95) in the short run and is substantially lower than the estimates of Lovenheim (2008). The long run avoidance elasticity estimate of 0.13 (standard deviation = 0.75) is even lower but this may be explained by the fact that prices increased due to a difficult-to-avoid increase in federal cigarette taxes.

V. REVENUE LOSS FROM TAX AVOIDANCE IN NYC

The most direct way to calculate revenue losses from cigarette tax avoidance is to use survey estimates of total smoking among NYC residents, and compare the implied tax revenue to actual revenue collections. Using this method, the NYC Independent Budget Office (2007) estimates that the total loss to the city and the state from cigarette tax avoidance is about $66 million per year. However, if cigarette consumption is underreported in the NYC survey, the IBO method will understate revenue losses.30

Even if consumption is reported accurately, the difference between consumption and taxable sales gives an upper bound for the losses from tax avoidance, since consumption is likely to be reduced if avoidance becomes more difficult. We provide two estimates of revenue loss from tax avoidance. The first estimate assumes no behavioral response to the elimination of tax avoidance, while the second incorporates an estimated consumption response. We calculate the revenue losses for a measure of tax avoidance based only on packs with no stamp. We assume that NYS is unable to collect any taxes on packs purchased from other states.

Actual revenue before and after the tax increase is computed using the average monthly stamp sales amounts from Table 5. The first approach is simply to multiply consumption levels, as estimated in the previous section of this paper, by the share of littered packs without a tax stamp, and apply the relevant tax rate. This approach thus

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30 Stehr (2005) notes that national cigarette consumption, as reported in the Current Population Survey supplement, is only a little more than half of taxable sales. Therefore, it is notable that, in contrast to the national pattern, reported consumption in NYC exceeds taxable sales. Thus even if consumption is somewhat underreported on the NYC survey, the results still imply substantial amounts of tax avoidance.
assumes no consumption response to eliminating untaxed cigarettes. Before the June 2008 tax increase, this approach implies that revenue and foregone revenue are

\[
\text{Revenue}_{\text{NYC+NYS}} = 12.2 \text{ million} \times 3.00 = 36.6 \text{ million/month} \approx 439.2 \text{ million/year},
\]

\[
\text{Foregone Revenue}_{\text{NYC+NYS}} = \text{Total Consumption} \times \text{Share Unstamped} \times \text{Tax Rate} \\
= 22.14 \text{ million} \times 0.15 \times 3.00 = 9.96 \text{ million/month} \approx 119.5 \text{ million/year}
\]

Note that our estimate of revenue losses is almost double that computed by the IBO.

After June 2008 tax increase, this approach implies that revenue and foregone revenue are

\[
\text{Revenue}_{\text{NYC+NYS}} = 9.8 \text{ million} \times 4.25 = 41.7 \text{ million/month} \approx 500 \text{ million/year}
\]

\[
\text{Foregone Revenue}_{\text{NYC+NYS}} = 20.5 \text{ million} \times 0.24 \times 4.25 = 20.9 \text{ million/month} \approx 251 \text{ million/year}
\]

Hence, revenue lost to tax avoidance is estimated to increase by $131 (=251–119.5) million/year. This amount is more than twice as high as the $61 (=500–439) million increase in tax revenue.

Our second approach is to assume a consumption response to eliminating untaxed cigarettes. If increased enforcement made it difficult or impossible to purchase unstamped cigarettes, the price increase on previously unstamped cigarettes could curtail consumption among those who avoided the tax. If all cigarettes in NYC were purchased at the legal price, the price for previously unstamped cigarettes would rise about 64 percent. Assuming a price elasticity of demand of 0.45 (typical of those estimated in the literature), we would expect a 29 percent decline in demand for (previously) untaxed cigarettes after the tax increase. Assuming that untaxed packs were 5.94 million/month (29 percent of $20.5 million), consumption among this group of smokers

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31 These calculations assume that the 12.2 million stamps per month sold in March and April 2008 were typical of monthly sales before the tax increase and that the 9.8 million stamps per month sold in July and August 2008 were typical of monthly sales after the tax increase. While these assumptions may not be strictly true because they do not take account of seasonality in the sales of tax stamps, they provide reasonable approximations of average monthly sales over the year and are consistent with the consumption analysis in the previous section.

32 Assuming the tax increase was fully forward shifted, the price of a pack of cigarettes with a NYC stamp would have increased by about 20 percent, from $6.82 to $8.21. Under our counterfactual of no unstamped cigarette sales, assume that the retail price of previously unstamped packs would rise from the $5 average street level retail price (Shelley et al., 2007) to the legal price $6.82 before the tax increase and $8.21 after the tax increase. This would represent an increase of 36 percent before the tax increase and 64 percent after the tax increase.
would drop to 4.2 million packs per month. Hence, the revenue that would be realized by total elimination of untaxed cigarettes would equal

\[
\text{Foregone rev (with consumption response)} = 4.2 \text{ million} \times $4.25 = $17.9 \text{ million/month} \\
\approx $214 \text{ million/year.}
\]

Thus, taking account of the potential consumption response to elimination of untaxed cigarettes reduces the revenue gain from eliminating consumption of untaxed cigarettes by about 15 percent ($214 million as compared to $251 million).

VI. CONCLUSION

We use data from littered packs collected in the streets of NYC to analyze the response to a $1.25 cents per pack increase in the NYS cigarette tax in June 2008. This increase brought the combined NYC NYS tax to $4.25, the highest in the nation. We randomly selected 30 Census tracts, and collected data on tax stamps on a total of 1,016 packs in four rounds — once before, once immediately after the tax increase, once three months later, and once a year and three months later.

In the first round, 55 percent of packs had NYC tax stamps, 9 percent had NYS stamps, 15 percent had no tax paid, and 14 percent had stamps from other states. Results from the second, third, and fourth rounds of data collection were remarkably similar and quite different from the first round. Data collection more than one year after the increase indicates that the change in behavior was long lasting, with patterns of avoidance almost identical to the third round.

As expected, avoidance rates went up after the tax increase. However, the source of this increase was entirely from an increase in packs with no tax stamp, whose share went from 15 to 24 percent, while the share with NYC stamps went down to 49 percent in round 2, and 48 percent in rounds 3 and 4. Our samples are large enough to make us confident that these changes are not the result of random statistical variation. The proportion with other types of stamps was basically unchanged after the tax increase. For every three to four packs with a NYC stamp, we found one with a stamp from another state in all rounds of data collection. Many of the out-of-state stamps came from low tax states such as Florida and Virginia.

Our estimated rates of avoidance are much higher than estimated national rates but lower than those found by Merriman (2010). Merriman found that proximity to lower tax sources of cigarettes (from other states) was associated with increased avoidance in Chicago. We find abundant empirical evidence, consistent with conventional wisdom, that packs without state tax stamps, which could have been sold from a Native American reservation, frequently make their way into NYC. We find some statistical evidence to support the hypothesis that proximity to the nearest Native American reservation is associated with more avoidance. We find that avoidance rates were significantly higher in poorer Census tracts, with a one standard deviation increase in poverty rates implying a 5 percentage point increase in avoidance rates.
Combining the littered pack data with data on taxable sales, we estimate that three months after the tax increase, 68 percent of the decline in sales was due to a decline in consumption. By 15 months after the tax increase, we estimate that about 88 percent of the drop in taxed sales was due to a decline in consumption. However, our estimates are imprecise and cannot rule out with a high degree of confidence the possibility of no decline in consumption.

Our results are at odds with Lovenheim’s (2008) national estimates. Lovenheim finds that tax increases do not diminish consumption and thus suggests that the decline in taxable sales following a tax increase is due to avoidance. Our estimate of the change in avoidance in NYC was much smaller than recent national estimates. We speculate that, because avoidance rates were already so high in NYC relative to the rest of the country, the opportunities for increased avoidance/evasion in response to a tax increase are more limited than in other states.

Compared to other U.S. cities, NYC is admittedly something of an extreme case because it has very high tax rates and a source of untaxed cigarettes in close proximity. Economics as a field has demonstrated the ability to learn a lot from polar cases (e.g., pure competition or pure monopoly), and we think that a lot can be learned from NYC’s experience. Even with the highest rates of cigarette taxation in the nation before June 2008, the cigarette tax increase resulted in an increase in revenue and a decline in consumption. However, our results also suggest that untaxed smoking results in a substantial revenue loss to NYC and NYS. After the June 2008 tax increase, annual losses to the city and the state from untaxed cigarettes in NYC are between $214 million and $251 million per year. These amounts, which range from 43 to 50 percent of revenue collected, exceed prior estimates of revenue losses by a substantial amount, and suggest that there may be a considerable fiscal return to increased enforcement efforts.

Using family expenditure data, Gruber, Sen, and Stabile (2003) find that the elasticity of demand for cigarettes is substantially greater for relatively low income smokers. Though our results are only for one city, and differ from the results of a similar study in Chicago, they suggest at least the possibility that the higher elasticity among the poor is reflective of a differentially greater tax avoidance response, as opposed to a greater reduction in consumption. The health effects of higher cigarette taxation for low-income populations are quite different under the two alternative interpretations. Given the importance of this question and the contrast between our results and Merriam’s (2010) results from Chicago, extending the littered pack methodology to other cities would be a useful area for future research.

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REFERENCES


Using Littered Pack Data to Estimate Cigarette Tax Avoidance in NYC


APPENDIX: STATISTICAL BASIS FOR THE CALCULATION OF SOME STANDARD DEVIATIONS

We derive the standard deviations displayed in Table 6 and the standard deviations for the elasticities discussed in the text as follows. To estimate the standard error of consumption (C) note that

\[ V(C) = V(Z \times \text{TAXED}_{NYC}) = (\text{TAXED}_{NYC})^2 V(Z). \]

Because the quantity \( \text{TAXED}_{NYC} \) comes from administrative data we treat it as a known parameter, i.e., we assume \( V(\text{TAXED}_{NYC}) = 0 \). Also
\[ V(\Delta C) = V(C_{t+1} - C') - V(C') + V(C') - 2 \text{Cov}(C_{t+1}, C'). \]

Since our estimates of consumption are derived from independent random samples of littered packs at a point in time, the covariance between the estimates is zero,\(^{33}\) so the variance of the change is simply the sum of the variances.

Similarly, avoidance (A) is

\[ A = Taxed_{NTC} \equiv \left( Z * Taxed_{NTC} \right) - Taxed_{NTC} = Taxed_{NTC} (Z - 1) \]

and the variance of our estimate of avoidance will be

\[ V(A) = V(Taxed_{NTC} (Z - 1)) = \left( Taxed_{NTC} \right)^2 V(Z). \]

The price elasticity of consumption is \( \varepsilon = \left( \frac{\Delta C}{C} \right) \left( \frac{P}{\Delta P} \right) \). We treat the price and the change in price as known (non-random) variables and calculate the variance in our elasticity estimate using the delta method (Patterson, 2010).

\[
\text{Var}\left( \frac{\Delta C}{C} \right) = \left( \frac{\mu_C^2}{\mu_C} \right) \sigma_C^2 + \left( \frac{1}{\mu_C^2} \right) \sigma_{C}^2 - 2 \left( \frac{\mu_C}{\mu_C^2} \right) \text{Cov}(C, \Delta C).
\]

Since \( \text{Cov}(C, \Delta C) = \text{Cov}(C_{t+1} - C', C_{t+1} - C') \approx \sigma_C^2 + \text{Cov}(C_{t+1}, C_{t+1}) = \sigma_C^2, \) in our application

\[
\text{Var}\left( \frac{\Delta C}{C} \right) = \left( \frac{\mu_C^2}{\mu_C} \right) \sigma_C^2 + \left( \frac{1}{\mu_C^2} \right) \sigma_C^2 - 2 \left( \frac{\mu_C}{\mu_C^2} \right) \sigma_C^2.
\]

The price elasticity of avoidance is \( \varepsilon = \left( \frac{\Delta C}{C} \right) \left( \frac{P}{\Delta P} \right) \) and calculation of its variance are identical.

---

\(^{33}\)Technically, \( \text{Cov}(C_{t+1}, C') = \text{Cov}(Z_{t+1} * \text{TAXED}_{\text{NTC},Z}, Z' * \text{TAXED}'_{\text{NTC}}) - \left( \text{TAXED}_{\text{NTC}} * \text{TAXED}'_{\text{NTC}} \right) \text{Cov}(Z_{t+1}, Z') \).

The covariance of Z between different rounds is zero for reasons stated in the text. Empirically the covariance of Z between different rounds is also very close to zero.