ESTIMATING THE IMPACTS OF CIGARETTE TAXES ON YOUTH SMOKING PARTICIPATION, INITIATION, AND PERSISTENCE: EMPIRICAL EVIDENCE FROM CANADA

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SUMMARY
In response to the widespread availability of illegal contraband, the federal and five provincial governments in Canada implemented a 40–60% reduction to cigarette excise taxes in February 1994. We exploit this unique and discrete policy shock by estimating the effects of cigarette taxes on youth smoking with data from the 1992–1996 Waterloo Smoking Prevention Program, 1991 General Social Survey, 1994 Youth Smoking Survey, 1996–1997 and 1998–1999 National population Health Surveys, and the 1999 Canadian Tobacco Use Monitoring Survey. Empirical estimates yield daily and occasional participation elasticities from $-0.10$ to $-0.14$, which is consistent with findings from recent U.S.-based research. A key contribution of this research is in the analysis of lower taxes on a panel of 591 youths from the Waterloo Smoking Prevention Program, who did not smoke in 1993, but 43% of whom confirm smoking participation following the tax reduction. Employing these data reveals elasticities from $-0.2$ to $-0.5$, which suggest that even significant and discrete changes in taxes might have limited impacts on the initiation and persistence of youth smoking.

KEY WORDS: youth smoking participation and initiation; cigarette taxes

1. INTRODUCTION
The United States has witnessed considerable increases in state level excise taxes on cigarettes in recent years (2000–2005) after the implementation of the Master Settlement Agreement (MSA) in 1998.$^1$ A key objective of these hikes was to discourage smoking and particularly, smoking initiation among teens. The underlying motivation, of course, stems from the fact that smoking has been acknowledged to be among the leading preventable causes of death and illness in North America. In this context, numerous studies suggest that most persistent adult smokers begin smoking in their teens.$^2$ A significant amount of research also points to the powerful deterrent impact that higher taxes have on youth smoking participation and initiation.$^3$ Specifically, traditional estimates of youth smoking participation and initiation elasticities are slightly below or above $-1$.$^4$

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$^1$The Master Settlement Agreement (MSA) was signed in 1998 between 46 states and the four major tobacco companies and settled lawsuits brought by the states in order to recover health expenditures as a result of illness from smoking.
$^2$Glied (2002, 2003) provides evidence in this respect as well as an overview of the literature.
$^3$The 2000 U.S. Surgeon General’s Report suggests that increasing the price of tobacco products decreases tobacco use among youth and concludes that raising tobacco taxes is one of the most effective tobacco prevention and control strategies. A 1999 World Bank report states that: ‘The most effective way to deter children from taking up smoking is to increase taxes on tobacco. High prices prevent some children and adolescents from starting and encourage those who already smoke to reduce their consumption.’

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However, there has also been some discussion as to whether cigarette taxes are currently too high in the U.S., and if in fact higher taxes can result in confounded policy. Specifically, higher cigarette taxes that lead to significant inter-state differentials might result in smuggling. Unfortunately, there has been relatively little guidance from the academic literature in this respect. This is because while there exists an abundance of research that has evaluated the impacts of tax increases on teen and youth smoking – on which estimates of participation elasticities are based upon – to the best of our knowledge, there is no study that has succeeded in isolating the impacts of a decrease in taxes on youth initiation and persistence. This is because no jurisdiction in the United States in recent times has actually enacted a significant reduction in excise taxes on cigarettes.

In this regard, Canada provides a rather compelling empirical laboratory. Specifically, the Federal and Provincial governments in Eastern Canada implemented a 40–60% reduction in excise taxes on tobacco in February 1994 in response to concerns on widespread smuggling of contraband tobacco across the Canada–U.S. border. We attempt to contribute to the literature by specifically exploiting these changes to excise taxes and a variety of cross-province surveys to estimate the corresponding impacts on smoking participation among teens. In doing so, we are able to offer a unique time-series profile of youth smoking in Canada.

We also evaluate the effects of taxes on the initiation and persistence of youth smoking. This is facilitated by the availability of a unique longitudinal dataset from the Waterloo Smoking Prevention Project (WSPP), a chronicle of smoking behaviour among a cohort of school children in South-Western Ontario between grades 8 and 12 (1992–1996). Using these data allows us to focus on a panel of 591 teens who did not admit to smoking in 1993 while in grade 9 – but 43% of whom reported smoking participation in the following year, after the tax reduction. Further, all teens in this subsample confirmed smoking participation in 1996 while in grade 12. Intriguingly, excise and sales taxes on cigarettes remained more or less constant between 1994 and 1996.

We are aware of only one other study (DeCicca et al., 2002) that has employed longitudinal data in an effort to evaluate the impacts of taxes on smoking initiation. However, this study, as most others that have focused on estimating the effects of excise taxes on smoking participation, relies on variation in U.S. excise taxes across states and over time from the 1980s and 1990s. One potential problem with this approach is that coefficient estimates of higher taxes might simply reflect unobserved state-specific characteristics. This is because before the tax and price increases that occurred after the signing of the MSA in 1998, the identifying variation from U.S. data originates mostly from cross-state differences with quite limited time-series variation relative to Canadian taxes (see Figure 1). Hence, using the discrete drop in excise taxes witnessed within provinces in Eastern Canada in order to estimate participation and initiation elasticities constitutes a rather interesting policy exercise on this important issue.

4Based on a comprehensive overview of the literature, Chaloupka and Warner (2000) suggest that a 10% increase in cigarette taxes is associated with a 4 and 6% drop in smoking participation among adults. In contrast, many studies suggest youths to be more price responsive than adults. Relevant studies include Lewit et al. (1981), Lewit and Coate (1982), Chaloupka and Grossman (1996), Tauras and Chaloupka (1999), Harris and Chan (1999), and Emery et al. (2001).

5For example, see http://www.taxfoundation.org/news/show/1497.html.

6There is research, which suggests that some jurisdictions actually experienced lower prices despite tax increases (Cigarette Smoking Before and After an Excise Tax Increase and an Antismoking Campaign, Massachusetts, 1990–1996, November 08, 1996/45(44);966–970. Available at http://www.cdc.gov/mmwr/preview/mmwrhtml/00044337.htm). However, a significant amount of studies finds that higher excise taxes do result in an increase in retail prices. See Sloan and Trogdon (2006) for further details.

7DeCicca et al. (2002) find an insignificant correlation between smoking initiation and taxes. Auld (2005) uses contemporary and retrospective data (as opposed to longitudinal) to estimate the effects of taxes on smoking initiation among Canadian youth and obtains an elasticity of −1.3.

8It is also important to note that some studies find either no statistically significant correlation between cigarette taxes and youth smoking or modest participation elasticities. These include Chaloupka and Wechsler (1997), Evans and Huang (1998), Gruber and Zinman (2000), Ross and Chaloupka (2003, 2004), Powell et al. (2005), and Powell and Chaloupka (2005).

9We thank Jon Gruber for kindly providing data on U.S. taxes.
OLS and probit estimates from pooling the WSPP data with the 1991 General Social Survey, 1994 Youth Smoking Survey, 1996–1997 and 1998–1999 National Population Health Surveys and the 1999 Canadian Tobacco Users Monitoring Survey (CTUMS), yields smoking participation and daily smoking elasticities from $-0.10$ to $-0.14$, which are considerably below traditional estimates of the efficacy of cigarette taxes, but quite credible given the significant variation in cigarette taxes and consistent with findings from recent studies (such as Carpenter and Cook, 2008; Sloan and Trogdon, 2004) that employ the pronounced increase in excise taxes witnessed in the U.S. after 2000.\textsuperscript{10}

Finally, our estimates suggest a correlation between lower taxes and a higher probability of smoking initiation and persistence among youths, with implied elasticities between $-0.2$ and $-0.5$ – a finding that is invariant across OLS, Probit, and Instrumental Variables (IV) estimates. In summary, our results suggest that while taxes do impact youth smoking, the effects are somewhat modest. Finally, we evaluate the possibility of amplified tax effects from social interactions. However, coefficient estimates of taxes interacted with the number of classmates who smoke are statistically insignificant.

The remainder of our paper is organized as follows. The following section describes the data. The base empirical specification is explained in Section 3. Estimation results are presented and discussed in Section 4. Finally, the paper concludes with a summary of the main results in Section 5.

2. YOUTH SMOKING AND CIGARETTE TAXES IN CANADA

2.1. Cross-province and time-series surveys

Table I shows mean and standard deviations of smoking participation (1, any smoking in the past month; 0, otherwise) and daily smoking by 15 to 19 year olds across all provinces in Canada, calculated from the: 1991 General Social Survey (GSS); 1994 Youth Smoking Survey (YSS); 1996–1997 and 1998–1999 National Population Health Surveys (NPHS); and 1999 Canadian Tobacco Use Monitoring Surveys (CTUMS).\textsuperscript{11} The time-series variation is indicative of a correlation between lower taxes and

\textsuperscript{10}Carpenter and Cook (2008) find youth smoking participation elasticities between $-0.2$ and $-0.5$. Sloan and Trogdon (2004) do not have results for teens but obtain an elasticity of $-0.27$ for smoking by 18 to 20 year olds. DeCicca et al. (2006) study the 1992 and 2000 waves of the NELS (1992 and 2000) and find that any significant correlation between youth smoking and taxes disappears upon controlling for antismoking sentiments.

\textsuperscript{11}All these surveys contain data that were collected through a combination of in-person and telephone interviews. The National Population Health Surveys are the staple of most health researchers in Canada, and contain a wealth of data on individual specific as well as household health status and associated habits. Prior to the NPHS, the 1985 and 1991 General Social Surveys were typically employed to analyze health policy issues relevant to all provinces. These surveys similarly contain information on a wide array of health habits and choices. The 1994 Youth Smoking Survey (YSS) was conducted by Statistics Canada (with the support of Health Canada) and intended at understanding determinants of smoking behavior of youths. Finally, the Canadian Tobacco Use Monitoring Surveys are conducted for Health Canada by Statistics Canada and are intended to provide data on tobacco use and related issues. The key objective of the survey is to track changes in smoking status, especially for teens and young adults.
increased smoking participation among youth. Specifically, while the 1991 GSS and 1994 YSS yield smoking participation (daily smoking) means of 0.25 (0.18) and 0.23 (0.18), respectively, corresponding means of the 1996–1997 and 1998–1999 NPHS and 1999 CTUMS are significantly higher at 0.31 (0.24), 0.29 (0.22), and 0.29 (0.25), respectively. The increase in teen smoking, after the tax drop in 1994 in Eastern provinces, is striking.

2.2. WSPP

We also construct measures of smoking participation and initiation from the third Waterloo Smoking Prevention Project (WSPP), a longitudinal survey of smoking habits of school children designed and conducted by the Health Behavior Research Group of the Faculty of Health Sciences, University of Waterloo. This survey is based on an initial cohort of grade 6 students identified in 100 schools across 7 school boards in South-Western Ontario in 1990. In order to evaluate the corresponding impacts of taxes with respect to initiation and persistence, we take individuals who reported that they were not smokers in grade 9, but became smokers at some point between grades 10 and 12, and remained so until grade 12.12,13

Table II contains summary statistics of these measures for the entire sample based on responses from 2825 children for each year between 1992 and 1996, or from grades 8 to 12. Unsurprisingly, the number of occasional and daily smokers increased significantly through time, as children grew older. Smoking participation rose from roughly 6% in grade 8 to 31% in grade 11, and then remained relatively stable at 35% in grade 12. In contrast, daily smoking rates increased from 1 to 22% between grades 8 and 11, and then to 27% in grade 12. Smoking participation and daily smoking mean (standard deviations) for the entire sample are 0.349 (0.477) and 0.152 (0.359), respectively.

Table II also consists of statistics for 591 children who were not smokers in grade 9 but became so afterwards.14 What is intriguing is the rather sharp increase in smoking participation within this subsample from grade 9 (0%) to grade 10 (43%) following the 50% cut in excise taxes in Ontario in February 1994. Similarly, the daily smoking rate increased from 0 to 22% between grades 9 and 10. By grade 12, all respondents in this subsample reported smoking participation while 69% confirmed daily smoking.

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12We could have also included youths who did confirm smoking participation at some point after grade 9 (1994), but who quit before grade 12. However, the motivation for defining the subsample in this manner is to focus on individuals who remained persistent smokers during the regime of lower taxes (from 1994 onwards).
13Survey information was collected from questionnaires filled by students, which was conducted during school hours with high levels of participation. Recruitment levels ranged from 65% in 1 board to the maximum 100% in 4 boards. For further details on the survey please refer to Cameron et al. (1999) and Sashegyi et al. (2000).
14Roughly, 18 and 35% of teens report smoking participation in grades 9 and 12, respectively. The 17% difference is equivalent to 480 (0.17 × 2825) individuals. Our sample consists of 591 individuals as the increase in new smokers obviously offsets the number of teens that quit smoking after grade 9.
2.3. Cigarette taxes in Canada

A possible explanation for the steep rise in participation and daily smoking rates among teens observed from the above data could be a result of the 40–60% tax cut imposed by both Federal and most Provincial governments in Eastern Canada in February 1994, in an effort to curb cross border smuggling of contraband tobacco. The federal government reduced its excise tax from $10.36 to $5.36 per carton of 200 cigarettes in February 1994. After the federal tax cut, four eastern provinces (Ontario, Quebec, New Brunswick, Nova Scotia, and Prince Edward Island) dropped their provincial tax rates (levied at the retailer’s end) as well. Table III details average cigarette taxes in 1993, 1994, and 1999 across all provinces. The time-series variation within most Eastern provinces is quite significant. Differences between Eastern and Western provinces are also quite pronounced. Interestingly, these differences persisted (in most cases) until 1999.

3. EMPIRICAL MODEL

3.1. Cross-province and time-series surveys

surveys are limited. Therefore, we rely on a simple approach and estimate the effects of the natural logarithm of excise and sales taxes on cigarettes (in real dollars per 200 cigarettes) on youth smoking participation and daily smoking, controlling for gender and employing province and year effects. In terms of estimation methodology, we use probit (only marginal effects reported) as well as OLS. Summary statistics are contained in Table IV.

3.2. WSPP data

Given the wealth of information available from the WSPP, we are able to employ a more detailed model in order to evaluate the impacts of taxes on youth smoking initiation and persistence.

\[
\text{SMOK}_{ijt} \text{ or DAILY}_{ijt} = \beta_0 + \beta_1 \ln \text{CTAX}_t + \beta_2 \text{ONE}_{ijt} + \beta_3 \text{BOTH}_{ijt} \\
+ \beta_4 \text{SIB}_{ijt} + \beta_5 \text{NSMOK}_{ijt} + \beta_6 \text{GEN}_{ijt} + \beta_7 \text{AGE}_{ijt} \\
+ \beta_8 \text{AGESQ}_{ijt} + \sum \text{YEAR}_t + \sum \text{CSD}_j + u_{ijt}
\]

(1)

where \(i\) represents the individual, \(j\) the census subdivision of residence, \(t\) indexes years, and \(u_{ijt}\) is the error term. The complete dataset consists of information on smoking habits of 2825 children for each year between 1992 and 1996 (grades 8 to 12), specifically yielding 14 125 data points that are pooled with the other cross-province surveys. As noted above, in order to focus on smoking initiation and persistence, we employ a separate panel of 591 youths from 1993 to 1996, resulting in 2364 observations.

SMOK\(_{ijt}\) denotes smoking participation, which is 1 if the individual smoked in the past month, and 0 otherwise. DAILY\(_{ijt}\) is 1 if an individual smokes daily, and 0 otherwise.\(^{15}\) CTAX\(_t\) is the natural logarithm of average annual excise and sales taxes per 200 cigarettes (in real $) in Ontario.\(^{16}\)

A key benefit of employing the WSPP data stems from the rich details that are available for each survey respondent, which allows us to control for a wide array of potential determinants of youth smoking. First, NSMOK\(_{ijt}\) represents smoking participation by one’s classmates and is used to capture

\(^{15}\)SMOK\(_{ijt}\) and DAILY\(_{ijt}\) are derived from responses to: ‘How much do you currently smoke?’.

\(^{16}\)These data were obtained from the Ministry of Finance (Government of Canada) and Statistics Canada.
the effects of peer smoking. We are able to construct this variable because of our ability to match each individual to his/her school. After assigning each individual to his/her respective school, the total number of smokers in each class is defined by the number who admits to smoking participation. We subtracted 1 from this number if the respondent admits to smoking participation, in order to ensure that NSMOK$_{ijt}$ accurately measures the number of ‘other’ smokers in the class (from the perspective of the respondent).

To control for family smoking habits, we construct ONE$_{ijt}$ and BOTH$_{ijt}$, which are dummy variables denoting whether one or both parents are smokers, respectively.\(^\text{17}\) Similarly, SIB$_{ijt}$ refers to smoking by an elder sibling.\(^\text{18}\) GEN$_{ijt}$ is 1 for a male respondent and 0 for a female. AGE$_{ijt}$ and AGESQ$_{ijt}$ refers to the age and age-squared of the individual. Finally, we employ year fixed effects ($\Sigma$YEAR$_{t}$) in order to account for the effects of unobserved events or initiatives that are time-specific.\(^\text{19}\)

The importance of being able to control for smoking by parents, siblings, as well as classmates allows us to disentangle the relative causal impacts of each of these factors on the likelihood of youth smoking. As pointed out by Manski (1995), smoking by classmates might be correlated with family smoking decisions if families with similar tastes and habits self-select themselves into the same neighborhood and their children attend the same school. Hence, an inability to control for family habits will lead to the erroneous conclusion that individual smoking participation is primarily the product of similar peer group behavior.

We attempt to minimize the likelihood of such bias by not only employing controls for parental and sibling behavior but also by constructing fixed effects representing the census subdivision of residence for each family ($\Sigma$CSD$_j$), which is facilitated by access to data on residential addresses.\(^\text{20}\) Adapting this strategy allows us to minimize the possibility of confounded estimates from endogenous sorting by families as well as control for the effects of demographic and socio-economic characteristics that remain fixed for each census sub division through time. Another benefit from the use of such fixed effects is that it allows us to control for the impacts of other unobserved policies or geographic characteristics.\(^\text{21}\)

4. ESTIMATION RESULTS

4.1. Baseline results – smoking participation


\(^{17}\)These variables are constructed from answers to: ‘Does your father (or stepfather or foster father) smoke cigarettes?’ and ‘Does your mother (or stepmother or foster mother) smoke cigarettes?’

\(^{18}\)SIB$_{ijt}$ is constructed from answers to: ‘Do any of your older sisters smoke cigarettes?’ and ‘Do any of your older brothers smoke cigarettes?’.

\(^{19}\)Of course, we are unable to include fixed effects for all years because of our focus on cigarette taxes. So instead of the standard approach of dropping one year fixed effect, we drop two – for 1992 and 1994.

\(^{20}\)According to Statistics Canada, ‘Census subdivision is the general term applying to municipalities (as determined by provincial legislation) or their equivalent (e.g. Indian reserves, Indian settlements and unorganized territories). In Newfoundland, Nova Scotia and British Columbia, the term also describes geographic areas that have been created by Statistics Canada in cooperation with the provinces as equivalents for municipalities for the dissemination of statistical data.’ (http://atlas.gc.ca/site/english/learningresources/glossary/results.html?term=Census%20Subdivision%20(CSD)).

\(^{21}\)Evidence from Carpenter (2009) suggests that local smoking bans, in some instances, does impact smoking participation. The use of these fixed effects also allows us to control for the impacts of transitions from elementary to high school. Specifically, if there is variation in the number of different types of schools across census subdivisions. However, this should not be of significant concern as the supply of both public elementary and high schools in a local area is usually proportionate to population. This is confirmed by our data, which suggests 100 schools at grade 8 and 88 at grade 10.
Columns 1 and 2 focus on smoking participation, while columns 3 and 4 contain results with respect to daily smoking. Columns 1 and 3 consist of estimates without province and year fixed effects while columns 2 and 4 include such two-way fixed effects. Panel A(B) contain OLS (probit) estimates. Before the empirical estimates in Table V are discussed, it is important to acknowledge that widespread smuggling of contraband tobacco occurred during the early 1990s in Eastern provinces in Canada, which might confound estimates of the effects of cigarette taxes. To see this more clearly, assume that most youths smoke smuggled as opposed to nonsmuggled cigarettes. In this case, the costs of cigarettes are not captured through existing cigarette prices or taxes, as purchase costs are reflected through prices of smuggled contraband. Therefore, cigarette prices would not greatly change for youths before and after February 1994, as teens could have simply switched their consumption from smuggled cigarettes to nonsmuggled goods after the sharp reduction in taxes, consequently eliminating any identifying variation available from the discrete drop in cigarette taxes. If this is true, then coefficient estimates of cigarette taxes or prices with respect to smoking may merely reflect a confounded rather than a causal relationship.

However, employing data across provinces and over time enables us to use province and year fixed effects that should mitigate the possibility of coefficient estimates of cigarette taxes being confounded because of the effects of unobserved smuggling. Specifically, we can employ province fixed effects that allows us to identify ‘smuggling’ provinces (Ontario, Quebec, New Brunswick, Nova Scotia, and Prince Edward Island) separately from ‘nonsmuggling’ provinces (Newfoundland, British Columbia, Alberta, and Alberta).

Superscripts a, b, and c refer to 1, 5, and 10% levels of significance. Standard errors of OLS estimates are clustered by province. Probit estimates are in terms of marginal impacts. Standard errors of probit estimates should be interpreted in terms of coefficient estimates, and not the marginal impacts.


<table>
<thead>
<tr>
<th></th>
<th>Smoking participation</th>
<th>Smoking participation</th>
<th>Daily smoking without</th>
<th>Daily smoking with</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>without fixed effects</td>
<td>with fixed effects</td>
<td>fixed effects</td>
<td>fixed effects</td>
</tr>
<tr>
<td><strong>A. OLS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log (cigarette taxes) (CTAX) (_t)</td>
<td>(-0.107)(^a)</td>
<td>(-0.154)</td>
<td>(-0.093)</td>
<td>(-0.135)</td>
</tr>
<tr>
<td>Gender (GEN(_{ij}))</td>
<td>(0.006)</td>
<td>(0.015)</td>
<td>(0.019)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Province and year fixed effects</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Maddala (R^2)</td>
<td>0.012</td>
<td>0.017</td>
<td>0.012</td>
<td>0.02</td>
</tr>
<tr>
<td>Number of observations</td>
<td>32683</td>
<td>32683</td>
<td>32683</td>
<td>32683</td>
</tr>
</tbody>
</table>

| **B. Probit** |                       |                       |                       |                   |
| Log (cigarette taxes) (CTAX) \(_t\) | \(-0.109\)\(^b\) | \(-0.144\) | \(-0.094\) | \(-0.123\) |
| Gender (GEN\(_{ij}\)) | \(0.0003\) | (0.005) | \(0.005\) | \(0.006\) |
| Province and year fixed effects | No | Yes | No | Yes |
| Adjusted \(R^2\) | 0.0122 | 0.0163 | 0.012 | 0.0181 |
| Number of observations | 32683 | 32683 | 32683 | 32683 |

\(\text{Superscripts } a, b, \\
\text{and } c \text{ refer to } 1, 5, \text{ and } 10\% \text{ levels of significance.}
\)

\(^a\)Standard errors of OLS estimates are clustered by province. Probit estimates are in terms of marginal impacts. Standard errors of probit estimates should be interpreted in terms of coefficient estimates, and not the marginal impacts.

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\(^22\)We wish to acknowledge the encouragement of an anonymous referee, who asked us to collect data from a variety of cross-sectional surveys over time and expand the scope of the paper by estimating the effects of taxes on youth smoking participation across all provinces and for a reasonably long time period.

\(^23\)Standard errors of OLS coefficient estimates are clustered by province. Probit coefficient estimates are in terms of marginal impacts that are directly comparable to OLS estimates.

\(^24\)We thank an anonymous referee for pointing this out to us.
Manitoba, and Saskatchewan) and year fixed effects that identify time periods in which smuggling was especially severe.

We obtain extremely similar estimates using OLS (panel A) or probit (panel B). Coefficient estimates of cigarette taxes are larger in magnitude in columns 2 and 4 (employing province and year fixed effects), relative to results in columns 1 and 3. The results suggest (statistically significant) participation and daily smoking elasticities between $-0.10$ and $-0.14$, which are comparable to findings from recent U.S. studies.

Table VI contains OLS and probit estimates of the effects of cigarette taxes with the above pooled data, but after omitting all observations for each ‘smuggling’ province (Ontario, Quebec, New Brunswick, Nova Scotia, and Prince Edward Island) from 1991 to 1994 – the years in which smuggling was the most severe. The objective is to further evaluate the sensitivity of our estimates to the confounding effects of smuggling. We find quite similar results across OLS and probit estimates. Specifically, we obtain tax elasticities of $-0.07$ and $-0.05$ with respect to smoking participation and daily smoking, respectively. Further, coefficient estimates of taxes are statistically significant (at the 1% level) across all columns. Hence, omitting observations for ‘smuggling provinces’ for ‘smuggling years’ results in estimates that are slightly lower in magnitude but still comparable to previous findings. These results offer a lower bound to the potential impacts of cigarette taxes on youth smoking.

As another sensitivity analysis we present estimates in Table VII, which are obtained by pooling the WSPP data with comparable information from the 1994 Youth Smoking Survey (YSS). Doing this allows us to identify the impact of taxes on youth smoking by specifically exploiting: (1) the sharp (50%) decline in cigarette taxes implemented in Ontario in February 1994; as well as the (2) significant difference in taxes that existed between Western and Eastern provinces after February 1994. We are also able to employ more controls because of comparable data across these two surveys. Specifically, we use covariates for individual age and parental smoking.

Coefficient estimates of cigarette taxes from Table VII are quite similar to corresponding estimates contained in Table V. OLS and probit (marginal effects) coefficient estimates of taxes imply daily smoking and smoking participation elasticities of $-0.10$ and $-0.14$, respectively.

4.2. Smoking initiation

The above results suggest that the 1994 reduction in taxes resulted in an increase in smoking participation with elasticities significantly lower than those reported in many other studies. The next issue we attempt to address is the effects of the decline in cigarette taxes on smoking initiation and persistence. The analysis is conducted by exploiting the panel nature of the WSPP, specifically by taking a subsample of the data consisting of individuals who were not smokers in grade 9 but became participants in a subsequent year (either in grades 10, 11, or 12) after the tax reduction in 1994 and continued to smoke in 1995 and 1996. The motivation is to evaluate whether lower taxes are correlated with an increased probability of smoking initiation and persistence. The refined sample consists of the data on smoking patterns of 591 teens over four years, resulting in 2364 observations.

In this respect it is important to emphasize that this is a unique contribution as the 1994 reduction in taxes allows us to evaluate the impacts of lower taxes on smoking initiation under quasi
natural–experimental conditions. To the best of our knowledge, we are unaware of any comparable U.S.-based research.

Table VIII consists of probit (column 1) and OLS (marginal effects) estimates (column 2) with respect to smoking participation, and columns 3 (probit) and 4 (OLS) contain results relevant to daily smoking.\(^{27}\) OLS and probit estimates are quite similar across columns. The results suggest that a change in taxes possesses a statistically significant impact with respect to youth smoking, with participation and daily smoking elasticities of (roughly) \(-0.5\) and \(-0.2\), respectively. These estimates offer some robust evidence that tax reductions are correlated with a modest increase in the likelihood of smoking initiation and participation.

Smoking by classmates (NSMOK\(_{ijt}\)) is insignificantly correlated with an increase in the likelihood of smoking initiation with respect to smoking participation. However, corresponding estimates with

\(^{27}\)Standard errors of OLS coefficient estimates are clustered by school. Probit coefficient estimates are marginal effects that are comparable to OLS coefficients.
respect to daily smoking are statistically significant. On the other hand, smoking by an elder sibling (SIB_{ijt}) is statistically significant across all columns. Specifically, OLS and probit estimates suggest that smoking by an elder sibling is associated with a corresponding 0.02–0.06 increase in the probability of smoking participation and daily smoking. Parental smoking (with the exception of smoking by a single parent with respect to daily smoking) is always statistically insignificant. This should be expected as variation in parental habits over the sample period is quite unlikely.

### 4.3. IV estimates

We have assumed that peer smoking (NSMOK_{ijt}) is an exogenous determinant of youth smoking decisions. A key concern regarding peer behavior is that smoking by classmates might be the result rather than the motivating factor behind an individual’s own smoking behavior. However, the probability of one person’s smoking impacting smoking decisions of all other classmates is quite low. On the other hand, coefficient estimates of the effects of peer smoking might also be biased upwards to the extent that both individual and peer smoking are correlated with unobserved policies or initiatives.

From the perspective of this study, such bias is of concern to the extent that it impacts coefficient estimates of the impacts of taxes. This could occur, for example, through the existence of local rules and regulations, such as smoking by-laws (see Carpenter, 2009) and general ease of access to cigarettes for teens. Arguably, the existence of other unobserved policies aimed at stricter youth smoking access could result in confounded estimates if amendments to cigarette taxes are correlated with the enactment of such policies.

To correct for such measurement error, we employ the total number of classmates as the relevant IV for the number of classmates who smoke. 28 The intuition is that class size determines the number of

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**Table VIII. Probit and OLS Estimates with respect to smoking initiation and persistence (WSPP)**

<table>
<thead>
<tr>
<th></th>
<th>(1) Probit – smoking participation</th>
<th>(2) OLS – smoking participation</th>
<th>(3) Probit – daily smoking</th>
<th>(4) OLS – daily smoking</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log (cigarette taxes) (CTAX_t)</td>
<td>-0.549 (0.688)</td>
<td>-0.4296 (0.027)</td>
<td>-0.2509 (0.872)</td>
<td>-0.195 (0.023)</td>
</tr>
<tr>
<td>Classmates as smokers (NSMOK_{ijt})</td>
<td>0.041 (0.002)</td>
<td>0.0004 (0.0003)</td>
<td>0.107 (0.002)</td>
<td>0.012 (0.004)</td>
</tr>
<tr>
<td>Either father or mother is a smoker (ONE_{ijt})</td>
<td>0.0026 (0.077)</td>
<td>-0.003 (0.016)</td>
<td>0.097 (0.071)</td>
<td>0.0639 (0.018)</td>
</tr>
<tr>
<td>Both father and mother are smokers (BOTH_{ijt})</td>
<td>-0.0047 (0.109)</td>
<td>-0.008 (0.022)</td>
<td>0.0068 (0.098)</td>
<td>0.0239 (0.026)</td>
</tr>
<tr>
<td>One elder sibling is Smoker (SIB_{ijt})</td>
<td>0.0218 (0.077)</td>
<td>0.047 (0.016)</td>
<td>0.055 (0.069)</td>
<td>0.061 (0.019)</td>
</tr>
<tr>
<td>Gender (GEN_ijt)</td>
<td>-0.0499 (0.071)</td>
<td>-0.038 (0.014)</td>
<td>-0.006 (0.064)</td>
<td>0.0003 (0.016)</td>
</tr>
<tr>
<td>Age, age-squared</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Census sub division fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Adjusted $R^2$ (OLS) / Maddala $R^2$ (Probit)</td>
<td>0.4782</td>
<td>0.5336</td>
<td>0.3170</td>
<td>0.3083</td>
</tr>
<tr>
<td>Number of observations</td>
<td>2364</td>
<td>2364</td>
<td>2364</td>
<td>2364</td>
</tr>
</tbody>
</table>

Superscripts a, b, and c refer to 1, 5, and 10% levels of significance.

The data used in these regressions were obtained from the Waterloo Smoking Prevention Program (WSPP) consisting of responses from 591 school children from grades 9 (1993) to 12 (1996) for a total of 2364 observations. Standard errors of OLS estimates have been clustered by school. Probit estimates are in terms of marginal impacts. Standard errors of probit estimates should be interpreted in terms of coefficient estimates, and not the marginal impacts.

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28It is important to emphasize that ‘class’ here implies all students in, for example, grade 7 of a particular school. Usually, each section of a class ranges from 20 to 30 students.
within class smokers and shares no direct association with the error term. Sheer class size should not by itself impact the probability of youth smoking participation. Further, there is no straightforward reason as to why class size should be correlated with the enactment of local antismoking policies. For these reasons, we view class size as a plausible instrument.

Table IX consists of first and second-stage IV regressions with respect to smoking initiation and persistence. We use two methods to conduct the IV analyses. First, first stage regressions estimating the effects of class size in the number of smokers are conducted employing OLS. Second, following Maddala (1983), we use the predicted values of the number of smokers from OLS first stage regressions and rely on a probit specification to estimate its relationship with youth smoking. The second approach employs IVs assuming a linear relationship between the number of smokers and class size as well as number of smokers and youth smoking.

OLS estimates of the effects of class size on the number of class smokers are in column 1. Columns 2, 3, 4, and 5 contain second-stage OLS and probit results. Columns 2 and 4 consist of estimates with respect to smoking participation, while 3 and 5 contain results for daily smoking. Second-stage estimates using a linear model are in columns 2 and 3 while corresponding results from probit models are in columns 4 and 5.

The first important result is that coefficient estimates of class size (from first stage regressions) are positive and statistically significant (at the 1% level). The correct *a priori* sign and statistical significance of the first stage parameter estimate suggests that second-stage regression estimates will be consistent and unbiased.29 Finally, Hausman (1978) tests for specification bias (applied to the linear models) reject at the 5% level, the null hypotheses of classmate smoking to be exogenous with respect to both smoking participation and daily smoking.30

Estimates from second-stage regressions yield results that are consistent with previous OLS results. We obtain cigarette tax elasticities ranging from −0.22 to −0.46 for initiation and persistence. These estimates are strikingly similar to previous OLS and probit results. Coefficient estimates of peer smoking (NSMOK*ij*) are statistically insignificant. Smoking by an elder sibling (SIB*ij*) is statistically significant and shares a positive relationship with smoking participation and initiation. Finally, parental smoking (ONE*ij* and BOTH*ij*) is generally insignificant with respect to smoking initiation and persistence.

We view the IV results with caution as the data does not offer alternate instruments in order to evaluate the sensitivity of our findings. However, what is reassuring is that given our efforts, we still obtain the consistent result that lower taxes are significantly correlated with an increase in the likelihood of smoking participation and initiation.

### 4.4. The effects of social multipliers

We exploit the WSPP data to evaluate the impacts of peers on the efficacy of tax policy. As pointed out by Manski (1995), the effects of social interactions – as measured through peer effects – may be explained by different possibilities. For example, the likelihood of an individual engaging in activities such as drinking and smoking could be motivated by corresponding behavior in the youth’s peer group.31 Therefore, changes in peer group behavior impact individual behavior and result in social multipliers. The implication is that exogenous shocks such as policy changes that impact the probability

---

29As pointed out by Bound et al. (1995) and Staiger and Stock (1997).
30We employ the following form of the Hausman test. Suppose \( Y = X\beta + \epsilon \) and \( Z \) is a set of instruments for \( X \). With some algebraic manipulation, it can be shown that \( \beta_{IV} - \beta_{OLS} = (Z'X)^{-1}Z'\epsilon_{OLS} \), which will be zero if \( Z \) and \( \epsilon_{OLS} \) are uncorrelated. This can be tested by running the regression \( Y = X\beta + Z\beta + \epsilon \) and testing \( \theta = 0 \) with an \( F \) test. The test statistic is distributed with \( r \) and \( n-k \) degrees of freedom, where \( r \) = number of variables in \( Z \), \( n \) = number of observations, and \( k \) = number of explanatory variables.
31Another possibility, which has already been discussed, is that an individual may exhibit similar behavior to a peer group because of similar unobserved “correlated” characteristics, such as parents with similar tastes self-selecting into particular areas.
of smoking will have a social multiplier effect through the peer effect. Consequently, a change in cigarette taxes will have a direct effect on an individual's probability of smoking as well as an indirect impact through the number of peers that smoke. To see this more clearly consider the following model.

Assume that youth \( i \) maximizes the following quasi-linear objective function with respect to \( q_i \):

\[
V(q_i) = b \left( q_i, \sum_{j=1}^{n} q_j(p) \right) - p q_i
\]  

(2)

where \( q_i \) is the number of cigarettes smoked by youth \( i \), and \( q_j \) represents the number of cigarettes smoked by \( i \)'s friend, \( j \). \( p \) is the uniform price. In terms of specific assumptions;

\[
\frac{db}{dq_i} > 0, \quad \frac{db}{dq_j} > 0 \quad \forall j, \quad \frac{d^2b}{dq_i^2} < 0, \quad \frac{d^2b}{dq_j^2} < 0 \quad \forall j
\]

which ensures concavity and hence, a maximum.

With some manipulation, the First Order Condition (FOC) yields \( q_i \) in a reduced form:

\[
q_i = f \left( p, \sum_{j=i+1}^{n} q_j(p) \right)
\]  

(3)

Table IX. Instrumental Variables (IV) estimates

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of classmates</td>
<td>0.36576</td>
<td>-0.4569</td>
<td>-0.2199</td>
<td>-0.468</td>
<td>-0.2374</td>
</tr>
<tr>
<td>(CTAX, ( t ))</td>
<td></td>
<td>(0.0215) ( a )</td>
<td>(0.025) ( a )</td>
<td>(0.779)</td>
<td>(0.882) ( a )</td>
</tr>
<tr>
<td>Classmates as smokers</td>
<td>-0.00013</td>
<td>0.0006</td>
<td>0.0494</td>
<td>0.1190</td>
<td></td>
</tr>
<tr>
<td>(NSMOK, ( j ))</td>
<td></td>
<td>(0.0005)</td>
<td>(0.0005)</td>
<td>(0.0706) ( a )</td>
<td>(0.0143)</td>
</tr>
<tr>
<td>Either father or mother is a smoker (ONE, ( j ))</td>
<td></td>
<td>-0.0021</td>
<td>0.0632</td>
<td>0.0034</td>
<td>0.0971</td>
</tr>
<tr>
<td>Both father and mother are Smokers (BOTH, ( j ))</td>
<td></td>
<td>(0.0157)</td>
<td>(0.018) ( a )</td>
<td>(0.078) ( b )</td>
<td>(0.071) ( a )</td>
</tr>
<tr>
<td>One elder sibling is</td>
<td></td>
<td>0.0487</td>
<td>0.065</td>
<td>0.0141</td>
<td>0.055</td>
</tr>
<tr>
<td>Smoker (SIB, ( j ))</td>
<td></td>
<td>(0.0156) ( a )</td>
<td>(0.018) ( a )</td>
<td>(0.084)</td>
<td>(0.075) ( a )</td>
</tr>
<tr>
<td>Gender (GEN, ( j ))</td>
<td></td>
<td>-0.039</td>
<td>0.0026</td>
<td>-0.0494</td>
<td>-0.007</td>
</tr>
<tr>
<td>Age, age-squared</td>
<td></td>
<td>(0.014)</td>
<td>(0.017)</td>
<td>(0.0707) ( a )</td>
<td>(0.064)</td>
</tr>
<tr>
<td>Census subdivision</td>
<td></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year fixed effects</td>
<td></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Adjusted ( R^2 ) (OLS)/Maddala ( R^2 ) (Probit)</td>
<td>0.7856</td>
<td>0.5337</td>
<td>0.3053</td>
<td>0.4786</td>
<td>0.3150</td>
</tr>
<tr>
<td>Number of observations</td>
<td>2364</td>
<td>2364</td>
<td>2364</td>
<td>2364</td>
<td>2364</td>
</tr>
</tbody>
</table>

Superscripts \( a, b, \) and \( c \) refer to 1, 5, and 10% levels of significance.

\( a \)The data used in these regressions were obtained from the Waterloo Smoking Prevention Program (WSPP) consisting of responses from 591 school children from grades 9 (1993) to 12 (1996) for a total of 2364 observations for smoking initiation and persistence. Probit estimates are in terms of marginal impacts. Standard errors of probit estimates should be interpreted in terms of coefficient estimates, and not the marginal impacts.

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Now we want to model the effect of a price (tax) increase

\[
\frac{dq_i}{dp} = \frac{df}{dp} + \sum_{j=i+1}^{n} \frac{df dq_j}{dq_i dp}
\]  

Multiplying both sides by \( \frac{p}{q_i} \) and with a bit more algebraic manipulation we get

\[
\bar{\epsilon}_p^j = \epsilon_p^j + \sum_{j=i+1}^{n} q^j_i \epsilon_p^j
\]

where \( \epsilon_p^j \) is \( j \)'s aggregate price elasticity; \( \epsilon_p^i \) is \( i \)'s own direct price elasticity; and \( \sum_{j=i+1}^{n} q^j_i \epsilon_p^j \) is \( i \)'s indirect price elasticity, which is composed of \( \epsilon_p^j (j \text{ 's price elasticity}) \) and \( \epsilon_p^j \), which is the elasticity of \( i \)'s cigarette consumption with respect to \( j \)'s cigarette consumption.

Equation (5) has several interesting implications. But we shall restrict ourselves to some rather broad observations that are relevant for our study and from a policy perspective. First, any change in individual \( i \)'s aggregate price elasticity (\( \bar{\epsilon}_p^i \)) is a function of individual \( i \)'s own direct response in cigarette consumption to a price change, as well as a function of how \( i \)'s friends respond to a price change. Specifically, the indirect price elasticity is given by \( \sum_{j=i+1}^{n} q^j_i \epsilon_p^j \), which is a product of not only how an individual \( i \)'s friends respond to price shocks (\( \epsilon_p^j \)) but also how dependent each individual’s smoking habits are on a friend’s cigarette consumption (\( q_j^i \)). Second and perhaps more importantly, \( \epsilon_p^j \) increases with the number of friends that smoke (\( n \)), provided that \( \epsilon_p^j > 0, q_j^i > 0, \forall j \). Hence, the impact of a tax/price increase will be amplified depending on the magnitude of peer effects or social interactions. Further, any other policy initiative, which successfully reduces the number of friends that smoke (\( n \)), will then diminish the aggregate price elasticity (\( \bar{\epsilon}_p^i \)) provided that \( \epsilon_p^j > 0, q_j^i > 0, \forall j \).

Therefore, participation elasticities increase with the number of corresponding peer smokers. On the other hand, it is also quite possible that an increase in the number of peer smokers reduce the marginal impacts of cigarette taxes. This is possible, if what is important to the teen is not the shift in the budget constraint through changes in taxes and hence prices – but the need to ‘fit’ in with a peer group. If smoking is the mechanism for ‘fitting in’, it is quite possible that the number of peer smokers actually results in a reduction in the marginal impact of cigarette taxes. On the other hand, it is also possible that an increase in the number of peer smokers enhances and simplifies access to cigarettes through informal exchange mechanisms such as bartering and ‘gifts’.  

In this case, the marginal impact of taxes will decline with an increase in the number of peer smokers.

To evaluate these competing hypotheses, we interact our peer smoking variable with the natural logarithm of taxes as well as retaining peer smoking and the natural logarithm of cigarette taxes as additional covariates. Empirical estimates are presented in Table X with columns 1 and 2 focusing on the entire sample while columns 3 and 4 consist of estimates of smoking participation and daily smoking but with respect to the panel of 591 individuals. The results, unfortunately, do not yield any conclusive insight. While coefficient estimates of most covariates mirror previous findings, the interaction of cigarette taxes with the number of classmates is statistically insignificant across all columns (with the exception of column 1). Therefore, we cannot make a robust inference on the effects of peer smoking with respect to tax efficacy.

\[32\] The prevalence of cigarettes as gifts in schools has been noted by studies such as Croghan et al. (2003). We are very grateful to an anonymous referee for this insight.

\[33\] Given the similarity between OLS and probit results with respect to previous estimates, we focus on OLS results given the clean interpretation that are yielded by coefficient estimates of interacted covariates.
5. CONCLUSION

What is the impact of lowering tobacco taxes on smoking participation and initiation among teens? We attempt to contribute to the literature by exploiting the rather extraordinary reduction in cigarette taxes witnessed in February 1994 across provinces in Eastern Canada. In doing so, we offer a unique profile of smoking among teens in Canada during the 1990s by pooling a variety of cross-province surveys over time. This allows us to identify the effects of cigarette taxes through cross-province and time-series variation. Perhaps more importantly, this exercise is a significant contribution to policy in Canada, as we are unaware of any other study that has employed such variation in order to assess the efficacy of cigarette taxes with respect to youth smoking. From a more general perspective, the discrete drop in cigarette taxes across some provinces over time, but not others, allows us to evaluate the impacts of lower cigarette taxes under quasi-experimental conditions.

Further, the availability of panel data from the Waterloo Smoking Prevention Project in South-Western Ontario offers a rare opportunity for estimating the impacts of excise taxes on youth smoking initiation and persistence. The use of these data also enables us to control for the potentially confounding impacts of peer, parental, and sibling smoking. Employing these data is important given the general lack of research with respect to cigarette taxes and smoking persistence among teens.

Reported OLS estimates yield participation and daily smoking elasticities between $-0.10$ and $-0.14$, which are lower than traditional estimates of the effects of taxes on youth smoking. Perhaps of more
importance are the range of elasticities with respect to initiation and persistence, suggested by the data. Specifically, we obtain elasticities between $-0.2$ and $-0.5$. In tandem, these findings suggest that while taxes do impact youth smoking, the effects are somewhat modest. Finally, we do not find any evidence of significant multiplier effects from peer smoking.

The small magnitude of participation and daily smoking elasticities should not be surprising. Recent evidence suggest that local school-based policies have been quite effective in deterring smoking initiation and participation. Specifically, Murnaghan et al. (2008) investigate the effects of bans on smoking within school properties implemented by Prince Edward Island with respect to all public schools within the province, between 1999 and 2001. They also attempt to evaluate the effects of ‘Students Working in Tobacco Can Help’ (SWITCH) and ‘Kick the Nic’, which are specific provincial programs intended at reducing teen smoking.\(^{34}\) Their results suggest that smoking bans possess potential as a smoking prevention initiative. These findings should be of interest to economists and may perhaps stimulate more research on the efficacy of school level interventions.

ACKNOWLEDGEMENTS

Sen acknowledges assistance from a SSHRC Standard Research Grant. This study complies with all ethical requirements deemed necessary by the University of Waterloo. No conflicts exist that may bias the results of this study. The usual disclaimer applies.

REFERENCES


\(^{34}\)For further details on SWITCH please refer to http://www.cancer.ca/prince%20edward%20island/prevention/quit%20smoking/pe-partners%20in%20fight%20against%20tobacco.aspx. The website http://www.hc-sc.gc.ca/hl-vs/pubs/tobac-tabac/prtc-reclt-2001/industry-eng.php has a description of ‘Kick the Nic’. Basically, both initiatives have many common objectives, the key one being public forums and education programs designed to educate teens on health risks associated with long-term smoking.


