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RETAIL TOBACCO DISPLAY BANS

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Abstract

Bans on retail tobacco displays, of the type proposed by New York's Mayor Bloomberg in March 2013, have been operative in several economies since 2001. Despite an enormous number of studies in public health journals using attitudinal data, we can find no population-based econometric studies of the type normally used in Economics. This paper attempts to fill that gap by using data from the annual Canadian Tobacco Use Monitoring Surveys. These data afford an ideal opportunity to study events of this type given that each of Canada's provinces implemented display bans at various points between 2003 and 2009. We use difference-in-differences methods to study three behaviors following the introduction of bans: participation in smoking, the intensity of smoking and quit intentions. A critical element of the study concerns the treatment of contraband tobacco. Our estimates provide little support for the hypothesis that behaviors changed significantly following the bans, although there is evidence that the ban reduced smoking intensity among youth.

JEL Classification: H2, I12, I18

Key words: cigarettes, display ban, smoking participation, intensity, quit intention

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1 Introduction

In March of 2013, New York's Mayor Bloomberg proposed that cigarette vendors be prevented from displaying their products openly in retail outlets. Such displays are most frequently located on shelves behind the cashier. In Mr. Bloomberg's view, and in the view of many health advocates, they serve to 'normalize' cigarettes and also act as advertising. Their presence may furthermore prompt unplanned purchases and hence increase tobacco consumption. Since cigarettes are an accepted carcinogen, responsible for many thousands of deaths in New York each year, the proposal might contribute to the long-term objective of reducing mortality and morbidity.

Should a retail display ban (RDB) progress to legislation, it would make New York the first city in the US to adopt it. Broadly, vendors would be required to put cigarettes and other tobacco products out of customer view - in covered shelving, or camouflaged by a blind of some type. In addition to possibly reducing the purchase of cigarettes by adults, this proposal would afford children and teens greater shelter from a toxic product.

The New York Times reported on March of 2013 that the Village of Haverstraw, NY, passed a similar ban in 2012, but rescinded the law because it had insufficient resources to fight a legal challenge presented by convenience stores and tobacco manufacturers.

Tobacco retail displays are a unique marketing mechanism. Universally these displays form a 'power wall' at the point of payment. Tobacco sales carry a high margin for vendors, and manufacturers have feared losing their right to display their products in this way (Harper, 2005). These retail displays are distinguished from other point-of-sale marketing techniques by their scale, their ubiquity and permanence. Numerous studies examine the impact on sales of product- and brand-specific promotions (for example, Gedenk and Neslin, 1998). The tobacco power walls however form a marketing mechanism both for individual brands and tobacco as a whole.

The purpose of this paper is to investigate the efficiency of such a deterrent measure, using data from Canada's provinces. A precisely similar measure was implemented at different points in time in each of Canada's Provinces in the most recent decade. The annual Canadian Tobacco Use Monitoring Surveys (CTUMS) contain a wealth of information. They contain the responses to monthly interviews with twenty thousand individuals each year on their smoking habits, and are available from 1999 to the present time. Since the interview month is reported in the data, and given that the month and year during which the retail display bans were introduced vary by province, these data provide an ideal opportunity to examine whether bans of the type proposed for New York may be effective either at the extensive margin - in reducing smoking participation, or at the intensive margin - in reducing the number of cigarettes smoked per person per week, or indeed whether they induce people to adopt an intention to quit.

Measures of this type have been operative for a number of years. Both England and Wales adopted bans for large retail outlets in 2012, and corner stores will be required to adhere to the ban by 2015. Scotland introduced such a measure for large stores in 2013; smaller stores will become subject to the ban in 2015. Singapore, at the time of writing, is preparing for a public discussion on the introduction of the measure. Several states in Australia have adopted bans (Ash, 2012). Ireland joined the club in 2009 (Cancer Research, 2009); Norway joined in 2012; Iceland was an early adopter in 2001. In sum, display bans are operative in numerous jurisdictions, and the key question for policy makers is whether such bans are effective in changing behavior, and if they are, to what degree.

The existing research on display bans contains few, if any, population-based econometric studies. This is partly because the measures are relatively new in most economies, and partly because the survey data necessary for the analysis is sparse. In particular, only one of the above economies (Australia) has a federal structure that permits bans to be implemented in a decentralized (and hence asynchronous) manner, which improves the possibility of being able to find some signals in the data. Most studies that have addressed the potential effectiveness of these bans have asked individuals if they approve of the measures, or if they think the measures should be helpful in quitting, or if individuals exposed to marketing make respondents more susceptible to smoking. The very large samples that are available through the CTUMS makes it possible to investigate if the bans have actually deterred smoking or have reduced the intensity of smoking.

Being able to measure the impact of specific policy measures on actual smoking behavior, rather than beliefs about the effectiveness of bans, has assumed greater importance in view of a recent ruling in the District of Columbia Court of Appeals in 2012: A federal judge ruled in favor of tobacco manufacturers in their objection to the Food and Drug Administration's proposals to require graphic health warnings (GHWs) on cigarette and tobacco packaging in general. The Court was not satisfied with the results from 'attitudinal' studies, and ruled that the FDA had provided "not a shred of evidence" that the introduction of GHWs would reduce the number of American smokers (Huang, Chaloupka and Fong, 2013). These authors also provide a timeline for this ruling. At the end of the day, the Court was not satisfied with distal attitudinal evidence, and its ruling was based on the lack of solid proximate evidence on the impact of that measure on smoking prevalence. In the absence of such evidence the Court ruled that the First Amendment rights of the tobacco manufacturers would be violated by the GHWs proposed by the FDA under a section of the 2009 Family Smoking Prevention and Tobacco Control Act. This precedent set by the Court suggests that if RDBs are to be put in place in the US, solid evidentiary findings on their impact on actual behaviors in other economies could be critical.

The paper proceeds as follows. In the next section, we review the relevant literature. Section 3 describes the timeline of RDB adoption across provinces in Canada, the empirical methodology that can exploit such variation in adoption dates, and the implication of the presence in the market of contraband tobacco for our estimation. Section 4 describes the data source. Section 5 presents the main results and robustness checks. Section 6 offers a discussion and concludes.

2 Existing literature and approaches

The results that emerge from many of the public health studies on RDBs (also termed point-of-purchase (PoP) bans) are suggestive rather than conclusive. For example, McNeill *et al* (2010) found that vendor compliance to PoP bans was high in Ireland, that support for the law grew post implementation, and that interviewees thought it would be easier to quit as a result of the display ban law being enacted. But they found no evidence of significant short-term prevalence changes among youth or adults. Yet the article concludes positively: "there were encouraging signs that the law helped to denormalize smoking". Using the same data, Quinn *et al* (2011) found that no statistically significant change in sales during the twelve months following the introduction of the display ban. The interesting conclusion drawn by the authors is that a change in behavior *should not* be observable in the near term: "The removal of point of sale displays is aimed at reducing the pernicious effects of tobacco advertising on children and is therefore likely to have an impact on sales over a much more protracted time period."[1] Scheffels and Ravik (2011) used Norwegian data. Focus group interviews were carried out before and after the ban among smokers and non-smokers. Behavior changes are not recorded, and the article concludes "Consumers believed that the ban *could* contribute to preventing smoking initiation among young people and to some extent also support cessation efforts." Hoek *et al* (2009) interviewed 20 individuals in depth following the implementation of the ban in New Zealand. They write: "participants strongly supported banning tobacco retail displays primarily because they *thought* this would reduce youth initiation, promote greater consistency with smoke-free promotions and assist those attempting to quit." Brown *et al* (2011) followed several thousand smokers in Canada between 2006 and 2009. They found strong support among smokers for the ban, and concluded that "the implementation of tobacco control measures, such as the removal of tobacco displays, *appear to sustain support* among smokers, those most likely to oppose such measures."

[1] Such a strong conclusion might suggest that any significant positive near-term impact recovered through econometric analysis might spurious. We do not share this perspective.

Clattenberg *et al* (2012) focus upon unplanned purchases of cigarettes, given that these may be triggered by displays. They interviewed 301 Vermont smokers immediately after their tobacco purchases, and found that 11% of cigarette purchases were unplanned, with 31% of total buyers agreeing that point-of-sale-advertising made quitting more difficult. Consequently the authors conclude that "Reducing unplanned purchases prompted by tobacco point of sale advertising *could* improve the likelihood of successful cessation among smokers." Wakefield *et al* (2008) report a similar finding. These authors do not state that there is no guarantee that if a smoker forgets to buy cigarettes as a result of no PoP promotion, he or she may still purchase cigarettes at another retail outlet before exhausting his or her stock. Hence, a finding such as this provides little guidance as to whether actual prevalence or number of cigarettes smoked decline in response to PoP bans.

A second characteristic of this literature is that the statistical work suffers from several inference-related issues. One is endogeneity. Several studies (e.g. MacFadyen, Hastings and MacIntosh (2001) find that teens who possess promotional materials from cigarette manufacturers are more likely to smoke than students who do not. But the possession of such materials could equally well be an outcome rather than a cause of smoking. A second is measurement. For example, Sargent *et al* (2002) correlated susceptibility to smoking on the part of middle-school students with the number of exposures to cigarette messages in movies. The number of messages is the product of movies watched times messages per movie. But the causation could either be attributable to the intensity of tobacco messages per movie or to the number of movies watched. A third problem relates to unobservables. For example Henriksen, Feighery *et al* (2004) find that students who are more exposed to promotion and messaging in corner stores are more susceptible to smoking than students who are less exposed. Again this exposure measure might indeed capture the impact of the amount of promotion, but equally may reflect individual type - teens who tend to 'hang out' in corner stores may be a personality type more likely to smoke. A widely cited review of this literature (Paynter and Edward, 2009) presents a favorable perspective on RDBs, but it does not venture into the specification of models or the possibility that the conclusions offered in the articles reviewed may be consistent with more than one hypothesis.

In contrast to this literature there exist a number of econometric studies that focus on particular subgroups of the general population. For example, Harris *et al* (2014), Adams *et al* (2012) or Abrevaya (2006) focus upon the impact of tobacco use and tobacco control measures on birth outcomes and behavior during pregnancy. However, we can find no econometric studies on the effectiveness of RDBs whose focal point is the general population. There appears to be no large data base available apart from the CTUMS that enables treatment and control groups to be formed with actual behaviors as the outcomes.

3 Empirical Methodology

3.1 Cigarette Display Bans in Canada

Table 1 shows the dates when the RDB legislation was introduced in Canada's provinces. Substantial variation characterizes the timing of the law. The ban first came into effect in Saskatchewan in October 2002 and remained operative until March 2004 when it was challenged successfully by the tobacco manufacturers. The ban was reinstated in Saskatchewan by a higher-court order in January 2005.[2] After Saskatchewan, all of Canada's provinces implemented the ban: Manitoba (October 2005); PEI (June 2006); Nova Scotia (December 2006); Ontario and Quebec simultaneously (May 2008); Alberta and British Columbia (July 2008); New Brunswick (January 2009) and Newfoundland (January 2010). Compliance to the laws would seem to be universal. In contrast to regulations such as the prohibition of sales to minors, where verification is more difficult, verification of the banning of retail displays is immediate and simple. Cohen *et al* (2011) indicate that their survey of retail outlets yielded a 99.8% compliance rate.

[2]In the regression analysis, our policy variable is switched on and off to match these sub periods.

3.2 Difference-in-Differences Design

To estimate the causal effect of the display bans, we employ the difference-in-differences (DD) methodology that exploits the variation in the timing of the law coming into effect across Canadian provinces, as noted in Table 1. The DD method is often used to evaluate the treatment effect of a medical or policy intervention on a subset of groups.[3] It explicitly accounts for variations in treatment across groups and over time by calculating the change over time for each group (the first difference) and then subtracting the resulting change in the control group from the treatment group (the second difference). The control group in our study is composed of those years and months of data in provinces prior to the ban implementation; the treatment group is the complement of this.

As long as the treatment and control groups are affected by time-varying confounding variables in a similar way, the DD estimates will reflect the effect of the legislation. Formally, we estimate a DD regression of the following form, for individual i in year t in province p:

$$Y_{ipt} = \alpha + \beta(DisplayBan)_{pt} + X'_{ipt}\phi + \eta_p \ + \chi_t \ + \xi_{ipt} \tag{1}$$

Y indicates outcome variables; $DisplayBan$ is the difference-in-differences indicator variable of interest, equal to one if the individual is in a province and a time period when the law is effective, and zero otherwise; β represents the difference-in-differences estimate of the effect of the legislation; The interpretation of the policy coefficient is the change in the outcomes following the policy, compared to the pre-policy period and to the provinces with no bans in place. X is a vector of control variables that include individual characteristics (education, gender, marital status, etc.). The time effects χ include monthly dummies (there are 11 month dummies which control for seasonal effects) and yearly dummies (which control for year specific unobserved factors and shocks). The province-specific fixed effects η control for

[3] Classic studies on the difference-in-differences methodology include Rubin, 1974; Meyer, 1995; Athey and Imbens, 2006, among others. For recent studies that use the difference-in-differences method in the health domain, see for example, Currie et al. (2009), Cutler et al. (2010) and Carpenter et al. (2011).

time-invariant differences across provinces (such as smoking cultures, levels of spending on healthcare, etc.).

The DD technique assumes that the trends in the smoking outcomes for both the treated and control groups would be the same in the absence of the legislation. We will examine and explicitly control for any differences in the trends before the legislation by including province-specific linear and quadratic time trends.

3.3 The Problem of Contraband Product

While the CTUMS that we use in this paper form a detailed month-to-month description of smoking behaviors across the economy, the behaviors reported are subject to the influence of illegal tobacco products.[4] Illegal product accounts for almost one quarter of the total market in many developed economies. This is the estimate of West *et al.* (2008) for the UK for example. Even in the US, where tobacco taxes have been low historically, there are concerns about the possible growth in supply from Indian reservations in the face of recent dramatic increases in state taxes. At the time of writing, a pack of cigarettes costs around $10 in New York City and this has given rise to legal disputes over the treatment of reservation sales (Tobacco Free Kids, 2009).

Although the presence of illegal products in the Canadian market does not distinguish it

[4] Illegal tobacco is a world-wide phenomenon. Many developed economies have seen an enormous tax-induced wedge open up between unit production cost and the legal retail price. For example, in Canada where most of the illicit product comes through First Nations reserves, zip-lock bags of 200 cigarettes sell for between $10 and $20 to the end user, and still yield substantial profits to the vendors (Royal Canadian Mounted Police, 2011). This translates into a price per pack to the smoker of between $1 and $2, compared to a retail price in the legal market in the neighborhood of $10.

Evidently, at such low prices, cigarettes are cheap to produce and distribute, and organized gangs see this product as being as profitable as drugs such as cocaine, heroin or marijuana, but traffic in cigarettes (and marijuana) carry lower penalties than the 'harder' substances (Easton, 2004).

Internationally, certain economies appear to turn a blind eye to the existence of production facilities set up domestically that are geared to supply an illegal overseas market. These cigarettes are referred to as 'illicit whites'. An example of this is a brand called "Jin Ling"; it is produced in a Russian tax-free zone (Kaliningrad). Other examples are "Raquel" from Cyprus and "Richman" from the United Arab Emirates (Joossens and Raw, 2011). A variant of this behavior involves established legal producers in the West exporting product, duty free, knowing that it will be imported illegally elsewhere. Joossens and Raw report that the magnitude of this particular form of contraband has declined as a result of investigations of, and lawsuits against, manufacturers.

from most other tobacco markets in the modern era, its presence has significant consequences for data analysis. First, the price of the cigarette aggregate, as registered by an official price index, may vastly overstate the price that consumers are paying at times when the illegal product accounts for a large share of the market. Second, individuals who actually purchase the illegal product are less likely to report their behaviors truthfully in surveys. The consequence of the first problem is that the official price series is biased upwards for those time periods when illegal sales are significant. This errors-in-variables problem may result in biased coefficient estimates (Wooldridge, p. 318, 2009). The consequence of the second problem is that total sales of cigarettes – as measured by consumer responses - will be biased downwards by more than the normal degree.[5]

The extent to which these two problems affect the coefficient estimates on the display bans also depends upon the correlation between the timing of peak illegal sales and the behavior of the key variable in our analysis – the implementation dates of the display bans. Figure 1 describes the pattern of illegal sales we have estimated for the Canadian market using a fairly standard method described in Physicians for a Smoke-Free Canada (2011). In essence we compute an estimate of the total market by assuming there is no illegal product in the early years and then use surveys to predict total sales, legal and illegal, in future years by inflating survey responses. From this total we subtract legally-reported sales to yield the illegal market share as a residual.

A critical aspect of Figure 1 is that it indicates illegal sales were at their peak at the time display bans were introduced in Quebec and Ontario (see table 1 above) – markets which together account for a substantial share of the total market. Since artificially higher prices and the illegal share are at their maximum at a time when the retail display bans were introduced in these markets, we cannot be sure that our models will correctly attribute causation.

[5] Smokers 'normally' under-report their consumption; but the magnitude of this under-reporting can be estimated by inflating survey responses to represent the population, and by comparing the result with legal shipments data.

Two solutions to this collinearity *cum* errors-in-variables combination suggest themselves: one is to use data up to the beginning of the contraband problem (roughly 2005); the second is to exclude those provinces where contraband was most serious. Quebec's Ministère de la Sécurité Publique (2011) proposes that virtually the complete contraband problem for all years is confined to Quebec and Ontario. Accordingly, our primary strategy is to estimate the DD models for eight of Canada's ten provinces using data for all thirteen years of the survey. We also estimate the models up to and including the year 2005 for all provinces. The latter is a weaker approach because the identification of the ban effects hinge essentially on two provinces – Saskatchewan (which introduced the ban in 2002, then shelved it and re-imposed it in 2005 following a successful court ruling), and Manitoba, which implemented the ban midway through 2005.

4 Data and Descriptive Analysis

The Canadian Tobacco Use Monitoring Surveys interview people aged 15 and above and provide the most comprehensive data on tobacco use by Canadians. Our analysis employs all CTUMS cycles available, from 1999 to 2011 for eight of the ten Canadian provinces. These cross sectional surveys have a number of nice features that make them well suited for this analysis. First, the CTUMS are available for the years before and after the ban, thus facilitating the application of the DD methods. Second, they report the month associated with each interview, so we can identify the period before and after the policy at the monthly level. Third, since its lowest admissible age is 15, we can study the effect of display bans on both youth and adult groups separately. This is valuable given that part of the public health push for display bans is premised on the idea that, in addition to reducing unplanned purchases, young smokers and young potential smokers grow up seeing that smoking is a 'normal' activity. Fourth, the CTUMS have rich information on smoking behaviors – not only on participation, but also on intensity and quit intentions. This allows us to study a variety of potential policy impacts.

Our analysis focuses on three outcome variables: (i) an indicator of whether a person currently smokes; (ii) the number of cigarettes smoked weekly by a smoker; and (iii) whether the person intends to quit in the next 1 and 6 months.

Table 2 displays the characteristics of the CTUMS data used in our analyses. 20% of our sample consists of current smokers. Among those who smoke, the reported average number of cigarettes consumed per week is 69, about 10 per day. The average age is 44, with youngest age being 15 and oldest respondents being 95. 61% of the sample has secondary education or lower or is still in school, while 17% has a college degree and the remaining 22% a university degree. The survey deliberately oversamples youth, but we use weighted observations in all of our estimations. About 1/4 of the sample are single. Household size averages 3 persons. 89% of the sample speaks English and 2% uses French while the rest use other languages. 95% of the sample can speak either French or English. Males account for 50% of the sample.

The trends in participation and number of cigarettes smoked per smoker, for each province, are presented in Figures 2 and 3. The vertical lines denote the times at which RDBs were introduced. These unconditional graphics do not immediately suggest breaks in behavior.

5 Regression Results

5.1 Baseline Estimates

5.1.1 Effect on smoking participation

The effect of the RDB on smoking prevalence is reported in table 3. Column 1 displays estimates from the base line regression using the strategy described above – the sample for eight provinces for the period 1999-2011. All regressions include month and province fixed effect variables, though we do not report the associated coefficients. The policy coefficient estimate is interpreted as the change (in percentage points) in the probability of smoking following the policy in the adopting province, relative to the period before the display ban adoption and relative to the provinces where the ban had not yet become effective. The coefficient estimates indicate that the ban has no detectable impact on smoking participation: the numerical value is small (circa 0.001) and not statistically significant.[6] Other covariates have expected signs: the age variable has an inverted U-shape; compared with the omitted education category of less-than-secondary schooling, people of higher education are less likely to smoke; married people or those living in common law are less likely to smoke, while those who are separated are the opposite; those who speak either English or French are more likely to be smokers than those who use other languages; men are more likely to smoke than women and those who live in larger households are less likely to smoke.

As the DD regressions assume the trends in outcomes across provinces are similar, in the next 2 columns we include the province-specific linear time trends in the regressions (in column 2) and quadratic trends (column 3) to control for the potential differences in time trends across provinces. The trends are almost invariably negative, and where the quadratic term is included it is generally positive, indicating that the rate of participation decline slows down. Surprisingly, the price variable does not have a significant impact in this regression. This may be because prices truly are not significant in influencing participation, or because they have a strong common trend during the period 2000-2005.[7] To test the degree of collinearity between province-level prices and time trends we regressed the real cigarette price in each province against time and time squared.[8] As anticipated, the coefficient on the linear term was positive; it was also significant in every case. The coefficient on the quadratic

[6] Recent econometric literature has drawn attention to the issue of underestimated standard errors in estimation that involves clustered data (see, Moulton, 1990; Bertrand et al. 2004). Among recommended remedies to this problem, Donald and Lang (2007) proposed to use t-distribution with $G-L$ degrees of freedom rather than the standard normal distribution for inference, where L is the number of repressors that are invariant within clusters. In particular, for the case of few G clusters such as ours (i.e. G=8 provinces), Cameron and Miller (2010) suggested using t-distribution with at least $G-1$ degrees of freedom. Monte Carlo simulations by Cameron, Gelbach, and Miller (2008) suggest that this technique works reasonably well. We will follow this approach and use t-distribution with $G-L$ degrees of freedom for inference.

[7] We say common trend because cigarette taxation is a shared federal-provincial jurisdiction, and changes in federal excise tax rates usually trigger a responsive increase in provincial taxes.

[8] These results are available upon request.

term was negative and significant in every case. This pattern corresponded to expectations given that virtually all of the price increase in every province took place between 2000 and 2005. The R-squared varied between 0.85 and 0.95. Ultimately we are not concerned with the precise value of the price elasticity provided that the treatment of the price variable does not contaminate the coefficient on the display ban.

5.1.2 Effect on smoking intensity

The second set of regressions examines the impact of the ban on smoking intensity measured by the log of the number of cigarettes smoked per week. For this analysis we use the subsample of smokers only. Therefore, the policy coefficient is the percentage change in the number of cigarettes smoked per week as the result of introducing the RDB. We also estimate these regressions using OLS, which means that this analysis is equivalent to a second-stage estimation of a traditional two-stage smoking model. It is worth mentioning that because the ban did not change the composition of who smokes, as shown in the previous section, there is no compositional bias for the estimates from these intensity regressions.

The results are reported in Table 4. The coefficient on the policy dummy from the baseline regression reported in column 1 is negative but small (-0.04) and statistically insignificant. All other covariates carry signs and statistical significance levels that are consistent with their values in the participation equation.

The second and third columns of this table furnish estimates from regressions that include province-specific time trends. The display ban effects become larger in magnitude (from -0.04 to -0.05 and -0.08) but are still not statistically significant. In contrast to the result in the participation regression above, the price coefficients become larger and statistically significant, indicating that higher prices lead to lower smoking intensity.

5.1.3 Effect on quit intentions

Table 5 contains estimates associated with a 6-month ahead quit intention variable. The CTUMS ask interviewees two questions on quit intentions: if smokers intend to quit within the coming month, and if they intend to quit in the coming six months. In these regressions our samples are composed of all smokers, again estimated without time trends and then with linear and quadratic terms included. Once again we can detect no evidence of the ban having an impact on quit intentions. While the coefficient is negative in all of the six regressions, in no case is it significant.[9]

5.2 Robustness Tests

5.2.1 Choice of estimator

To test if the DD approach might yield results contrary to a qualitative choice estimator we reran the participation regression using a probit model, in conjunction with a dummy variable for the display ban. The results failed to reject the null hypothesis of no ban impact. The coefficient values (reported in the upper panel of table 6) were very small and close to those returned from the DD estimates and not significant (except for the regression with a quadratic trend included).

5.2.2 Using quit attempts instead of quit intentions

We estimated the models using actual quit attempts instead of quit intentions as the outcome. Quit attempts represent a stronger form of quitting efforts than quit intentions and are an indicator of smoking cessation or relapse among those who have recently tried to quit. As quit attempts may be related to unplanned purchases, a mechanism through which a display ban might affect smoking behavior, they may capture better the effect of the display on smoking behavior. As quit attempts are count variables with many zero values, we modelled this

[9] The 'six' regressions include the 1-month ahead quit intention as well as the 6-month ahead. The former are available from the authors upon request.

outcome using the zero inflated negative binomial estimator. The results indicate that the ban did not lead to an increase in the number of quit attempts, nor increase the probability of having any 24-hour quit attempt.[10]

5.2.3 Excluding smuggling period

We estimated the models up to and including the year 2005. As there was little smuggling before this year, this supports including all ten provinces in the regressions. A disadvantage of this exercise is that the identification of the ban effects hinges essentially on two provinces – Saskatchewan (which introduced the ban in 2003, then shelved it and re-imposed it in 2005 following a successful court ruling), and Manitoba, which had the ban in place for half of 2005. The results are reported in the second panel (for participation outcome), the third panel (for intensity outcome) and the final panel (for quit intention outcome) in table 6. The display ban coefficients are small and not statistically significant.

5.2.4 Effects on different age groups and smoking statuses

Although the effect of the ban on smoking participation for the whole population appears insignificant, it might still impact some specific age groups. Media and policy discussions often suggest that youth would be less likely to smoke in the absence of tobacco displays. To explore this, we re-estimated the regressions for several specific age groups, focussing upon youth (15-24), prime age (25-64) and older (65+) smokers.

The top panel of table 7 displays estimates from the participation regression. In all three specifications, the display ban coefficients are small and statistically insignificant for each age group. The middle panel reports estimates for the intensity regressions. The display ban is negative in all three age groups, and we finally encounter a negative and significant coefficient - for the youngest age group 15-24. However, this effect is not invariant to the

[10] These results are available upon request. The output of a zero inflated negative binomial regression has 2 sets of coefficients: the first set of coefficients is from the equation predicting counts for the "positive outcome" group. The second set of coefficients is from the equation that predicts membership in "zero outcome" group. These can be interpreted as logit coefficients.

inclusion of province-specific time trends. Estimates for the quit intention regression are reported in the bottom panel of table 7. Again, the display ban has no significant effect on quit intentions for any age group. The display ban coefficients are either positive (though small and statistically insignificant) or have the wrong sign.

Since the impact of RDBs on youth may depend upon whether young smokers obtain their tobacco legally or not, we reestimated the above set of equations for those individuals above the legal age limit. Those aged 16, for example, may obtain their tobacco from friends and thus not be subject to the impact of point-of-sale marketing. When the age of 18 is used as a cut-off we found no impact of the RDBs on participation, but found a positive impact on intensity in most, though not all, of the regressions. Also, since the legal age of smoking is 19 in some provinces, we used the corresponding subset of provinces to reestimate the equations. Ontario has a legal age of 19, though has large illegal sales in some years. So we explored the impact both including and excluding Ontario. When Ontario was excluded the RDBs were found, as before, to impact intensity but not participation. When Ontario was included the intensity effect disappeared. Some significant impacts on intentions were present. In summary, the bans may have impacted the intensity of legal youth smoking, though not the participation rates.

5.2.5 Occasional and daily smokers

As occasional and daily smokers might behave differently in response to the ban, we re-estimate the DD regressions for these two separate smoker groups. Intensity outcomes are reported in the upper panel of table 8, and indicate that bans have no effect on either of these two groups of smokers. Neither does the display ban impact smoker quit intentions: as shown in the lower panel of table 8, the coefficients on the display ban are small and statistically insignificant.

6 Concluding Remarks

Our objective in exploring the impact of retail tobacco display bans has been to see if a population-based econometric study might shed light on actual behaviors following the introduction of such bans. They have been introduced in several jurisdictions and a large literature in the public health field supports such bans. Many of these studies are 'attitudinal': they are based upon data describing how smokers and nonsmokers feel about the bans. For example, do they 'believe the bans are good', or that the bans should 'help smokers quit', or 'denormalize smoking for minors'? Some of these studies also lend themselves to being interpreted in more than one way because of problems associated with the specification of variables and the models. Other econometric studies that do find significant impacts (both small andlarge) aim at particular subpopulations, such as pregnant women.

Consequently, the current study may be the first of its type, and while the data base used is ideal for confronting the question at hand, we find no systematic support for a significant impact of the bans on participation or quit intentions, and just limited support (among youth) for a reduction in intensity. Our findings were invariant to a number of robustness checks. In particular, province specific time trends were included in most of our regressions in order to capture the impact of changes in culture, and the impacts of whatever other economic and societal changes took place during this period (for example the implementation of public-place smoking bans). Time trends frequently deprive the coefficients of interest of significance, if the two are correlated. This is not the case in our analysis. In virtually all regressions, whether a time trend was included or not, the RDB coefficient was largely unaffected. Hence the RDB impacts are not being swept up by the time trend.

That said, the hypotheses we have tested warrant some amplification. Our study focused on investigating an immediate significant break in behaviors, not a lagged break at say three months, or six months, or some other number of months. Such testing for an immediate break possesses several merits. First, in the absence of reasonable priors or theory on how long lags should be, one could test numerous lag structures and perhaps one such structure might enable us to reject the null for some behaviors; but the statistical meaning of such an approach is not obvious. Second, the theory suggests that the break should be immediate for a large class of smokers. A repeated claim in the public health literature is that display bans trigger the memory of potential buyers – and the absence of the trigger means that many unplanned purchases that would have been concluded in the presence of the display are no longer made. We emphasized in the introduction that it is incorrect to infer that smoking would necessarily decline in the absence of the display trigger - an individual who is not prompted to purchase cigarettes by the absence of a RDB may still remember to purchase his or her cigarettes before exhausting their stock. Third, a majority of smokers at any given time intend to quit. In view of this it is reasonable to think of policy measures as 'tipping' event. That is, they help individuals who have in a particular sense already decided to quit, to make the transition. And it is reasonable to expect a new policy measure in that context to have an impact right away, or not at all. Having said that, just as our results suggest no participation impact, it is possible that the bans may still contribute to a longer term denormalization of tobacco use which could in turn impact participation. Measuring such an impact remains a challenge.

Finally, the literature in this area is very short on evaluating and trading off the value of reductions in participation and intensity. Quitting has always been the main objective of public health advocates. However, the appropriateness of focussing on participation to the virtual exclusion of reduction supposes a particular form for the damage reduction function: that it is concave. A strongly concave damage function in the number of cigarettes smoked implies that participation becomes overwhelmingly important, whereas reducing intensity at most levels of consumption has small value: technically, going to zero cigarettes from a small number has substantial value. A convex damage function implies the opposite - reducing intensity is all important, particularly for heavy smokers, whereas reducing consumption from a low intensity to zero has modest value. Hence, being able to trade off findings of the type we have supplied in this paper begs the nature of the damage function in the first instance, and the impact of these measures on heavy and low intensity smokers on the other. Much remains to be established in this area.

Table 1. Timeline of display bans in Canada

Province	Display ban in effect
Saskatchewan	Oct 2002 - Mar 2004
	19 Jan 2005*
Manitoba	15 Aug 2005
PEI	1 Jun 2006
Nova Scotia	1 Dec 2006
Ontario	31 May 2008
Quebec	31 May 2008
Alberta	1 Jul 2008
British Columbia	1 Jul 2008
New Brunswick	1 Jan 2009
NFL	1 Jan 2010

* http://www.mhp.gov.on.ca/en/smoke-free/factsheets/Tobacco_PointOfSale-041505.pdf

Table 2. Summary statistics

Variable	Mean	Std. Dev.	Min	Max	Obs
Current smoker status	0.196	0.397	0	1	218,479
Weekly quantity of cigs	68.931	68.837	0	630	57,631
Log weekly quantity	4.165	1.101	0	6.44	43,293
Age	43.888	18.025	15	95	218,479
Less than secondary	0.207	0.405	0	1	215,445
Secondary	0.402	0.490	0	1	215,445
College	0.167	0.373	0	1	215,445
University	0.224	0.417	0	1	215,445
Married/common law	0.622	0.485	0	1	216,778
Widowed/separated	0.106	0.307	0	1	216,778
Single	0.264	0.441	0	1	216,778
Male	0.496	0.500	0	1	218,479
Hhsize	2.868	1.255	1	9	218,479
French	0.023	0.149	0	1	216,932
English	0.890	0.312	0	1	217,391

Notes: CTUMS 1999-2011; Sample contains 8 provinces (excluding Quebec and Ontario)

Table 3. Effect of display ban on smoking participation

	(1)	(2)	(3)
	Baseline	(1) + Linear trend	(2) + Quadratic trend
Display ban	0.0011	0.0007	0.0016
	(0.0036)	(0.0033)	(0.0037)
Price (in log)	-0.0436	0.0197	0.0220
	(0.0323)	(0.0144)	(0.0213)
Age	0.0130***	0.0130***	0.0130***
	(0.0010)	(0.0010)	(0.0010)
Age squared	-0.0002***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)
Secondary school	-0.0514***	-0.0514***	-0.0514***
	(0.0053)	(0.0053)	(0.0053)
College	-0.0960***	-0.0960***	-0.0960***
	(0.0102)	(0.0102)	(0.0102)
University	-0.1813***	-0.1815***	-0.1816***
	(0.0149)	(0.0148)	(0.0149)
Married/common law	-0.0528***	-0.0527***	-0.0527***
	(0.0028)	(0.0027)	(0.0027)
Widowed/separated	0.0443***	0.0443***	0.0443***
	(0.0104)	(0.0103)	(0.0103)
French	0.0234*	0.0228*	0.0231*
	(0.0108)	(0.0106)	(0.0105)
English	0.0686***	0.0683***	0.0685***
	(0.0064)	(0.0063)	(0.0063)
Male	0.0350***	0.0350***	0.0350***
	(0.0016)	(0.0016)	(0.0016)
Hhsize	-0.0148***	-0.0150***	-0.0150***
	(0.0022)	(0.0021)	(0.0021)
Constant	0.2743*	0.0466	0.0578
	(0.1293)	(0.0584)	(0.0808)
Observations	213,580	213,580	213,580
R-squared	0.062	0.063	0.063

Notes: Each column shows the results from a DD regression estimated by OLS. Data from 8 provinces (excluding Ontario and Quebec) for the period 1999-2011. All models include province, month, and year fixed effects. Standard errors throughout are clustered at the province level and estimates are weighted. Inference uses t-distribution with 5 degrees of freedom. Significance levels are: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4. Effect of display ban on smoking intensity

	(1)	(2)	(3)
	Baseline	(1) + Linear trend	(2) + Quadratic trend
Display ban	-0.0440	-0.0506	-0.0833
	(0.0452)	(0.0503)	(0.0562)
Price (in log)	-0.2607	-0.4402**	-0.4563**
	(0.1558)	(0.1428)	(0.1605)
Age	0.0735***	0.0735***	0.0736***
	(0.0037)	(0.0037)	(0.0037)
Age squared	-0.0007***	-0.0007***	-0.0007***
	(0.0000)	(0.0000)	(0.0000)
Secondary school	-0.1536***	-0.1535***	-0.1535***
	(0.0134)	(0.0133)	(0.0132)
College	-0.2823***	-0.2819***	-0.2815***
	(0.0352)	(0.0352)	(0.0351)
University	-0.5947***	-0.5947***	-0.5948***
	(0.0312)	(0.0313)	(0.0309)
Married/common law	0.0427*	0.0423*	0.0421*
	(0.0204)	(0.0203)	(0.0204)
Widowed/separated	0.0927***	0.0925***	0.0925***
	(0.0224)	(0.0226)	(0.0225)
French	0.3225***	0.3241***	0.3246***
	(0.0598)	(0.0601)	(0.0604)
English	0.4766***	0.4764***	0.4772***
	(0.0385)	(0.0389)	(0.0390)
Male	0.2334***	0.2332***	0.2334***
	(0.0111)	(0.0111)	(0.0109)
Hhsize	-0.0506***	-0.0504***	-0.0505***
	(0.0092)	(0.0093)	(0.0093)
Constant	3.3580***	4.1269***	4.2374***
	(0.6047)	(0.5826)	(0.6533)
Observations	42,487	42,487	42,487
R-squared	0.1163	0.1165	0.1168

Notes: Each column shows the results from a DD regression estimated by OLS. Data from 8 provinces (excluding Ontario and Quebec) for the period 1999-2011. All models include province, month, and year fixed effects. Standard errors throughout are clustered at the province level and estimates are weighted. Inference uses t-distribution with 5 degrees of freedom. Significance levels are: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Effect of display ban on intention to quit

	(1)	(2)	(3)
	Baseline	(1) + Linear trend	(2) + Quadratic trend
Display ban	0.0049	0.0040	-0.0010
	(0.0218)	(0.0225)	(0.0234)
Price (in log)	-0.0561	0.0237	-0.0530
	(0.0481)	(0.0948)	(0.0967)
Age	0.0013	0.0013	0.0013
	(0.0011)	(0.0011)	(0.0011)
Age squared	-0.00005***	-0.00005***	-0.00005***
	(0.0000)	(0.0000)	(0.0000)
Secondary school	0.0218*	0.0220*	0.0220*
	(0.0097)	(0.0096)	(0.0096)
College	0.0686***	0.0685***	0.0684***
	(0.0154)	(0.0155)	(0.0154)
University	0.0492**	0.0493**	0.0494**
	(0.0204)	(0.0204)	(0.0203)
Married/common law	0.0493***	0.0499***	0.0500***
	(0.0101)	(0.0103)	(0.0104)
Widowed/separated	0.0553***	0.0559***	0.0557***
	(0.0146)	(0.0149)	(0.0148)
French	0.0761**	0.0756**	0.0764**
	(0.0312)	(0.0309)	(0.0308)
English	0.0915*	0.0913*	0.0915*
	(0.0428)	(0.0427)	(0.0427)
Male	-0.0118	-0.0118	-0.0117
	(0.0086)	(0.0086)	(0.0086)
Hhsize	0.0090**	0.0088**	0.0088**
	(0.0031)	(0.0032)	(0.0032)
Constant	0.7283**	0.4562	0.6466
	(0.2172)	(0.3964)	(0.3939)
Observations	40,965	40,965	40,965
R-squared	0.021	0.022	0.022

Notes: Each column shows the results from a DD regression estimated by OLS. Data from 8 provinces (excluding Ontario and Quebec) for the period 1999-2011. All models include province, month, and year fixed effects. Standard errors throughout are clustered at the province level and estimates are weighted. Inference uses t-distribution with 5 degrees of freedom. Significance levels are: *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Robustness checks

	(1)	(2)	(3)
	Baseline	(1) + Linear trend	(2) + Quadratic trend
	Probit regression for participation		
Display ban	0.0096	-0.0081	0.0049**
	(0.0067)	(0.0053)	(0.0014)
Cig. Price (in log)	-0.0729***	-0.0889***	-0.0868***
	(0.0098)	(0.0124)	(0.0153)
Observations	213,580	213,580	213,580
	Excluding data from smuggling period		
Participation outcome			
Display ban	0.0125	0.0217	0.0227
	(0.0118)	(0.0127)	(0.0145)
Cig. Price (in log)	-0.0832*	-0.0987*	-0.0924*
	(0.0403)	(0.0455)	(0.0446)
Observations	146,180	146,180	146,180
R-squared	0.060	0.060	0.060
Intensity outcome			
Display ban	0.0375	0.0145	0.0257
	(0.0470)	(0.0716)	(0.0728)
Cig. Price (in log)	0.109	-0.0101	0.0731
	(0.0819)	(0.0895)	(0.100)
Observations	33,020	33,020	33,020
R-squared	0.120	0.120	0.120
Intention to quit outcome			
Display ban	0.00828	-0.00216	0.00842
	(0.0107)	(0.00701)	(0.0164)
Cig. Price (in log)	-0.188	-0.132	-0.125
	(0.109)	(0.108)	(0.132)
Observations	29,071	29,071	29,071
R-squared	0.027	0.028	0.029

Notes: All models include province, month, and year fixed effects. Standard errors throughout are clustered at the province level and estimates are weighted. Inference uses t-distribution with 5 degrees of freedom. Significance levels are: *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Effect of display ban by age group

	Baseline	Linear trend	Quadratic trend	N
Participation outcome				
Age 15-24	0.00686	0.00602	0.00478	101,303
	(0.00968)	(0.00948)	(0.0102)	
Age 18-24	0.00658	0.00466	0.00323	65,526
	(0.0113)	(0.0112)	(0.0128)	
Age 25-64	0.00160	0.00171	0.00621	88,368
	(0.00332)	(0.00388)	(0.00535)	
Age 65+	-0.00454	-0.00450	-0.0126	23,909
	(0.0103)	(0.00932)	(0.00719)	
Intensity outcome				
Age 15-24	-0.0784*	-0.0743*	-0.0964**	21,029
	(0.0339)	(0.0370)	(0.0290)	
Age 18-24	-0.102**	-0.0926*	-0.112**	16,894
	(0.0366)	(0.0394)	(0.0295)	
Age 25-64	-0.0309	-0.0383	-0.0725	19,480
	(0.0449)	(0.0493)	(0.0521)	
Age 65+	-0.110	-0.132	-0.149	1,978
	(0.139)	(0.151)	(0.191)	
6 month quit intention outcome				
Age 15-24	0.0207	0.0233	0.0360	20,667
	(0.0219)	(0.0231)	(0.0209)	
Age 18-24	0.0208	0.0241	0.0405*	16,443
	(0.0180)	(0.0189)	(0.0171)	
Age 25-64	0.00663	0.00344	-0.000809	18,312
	(0.0274)	(0.0286)	(0.0346)	
Age 65+	-0.0796	-0.0702	-0.111*	1,986
	(0.0478)	(0.0422)	(0.0492)	

Notes: Each column shows the results from a DD regression estimated by OLS. Data from 8 provinces (excluding Ontario and Quebec) for the period 1999-2011. All models include province, month, and year fixed effects. Standard errors throughout are clustered at the province level and estimates are weighted. Inference uses t-distribution with 5 degrees of freedom. Significance levels are: *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Effect of display ban, daily smokers versus occasional smokers

		(1)	(2)	(3)
		Baseline	(1) + Linear trend	(2) + Quadratic trend
Intensity outcome				
Daily smokers	Display ban	-0.0251	-0.0298	-0.0282
N=34,415		(0.0267)	(0.0286)	(0.0387)
	Price	-0.122	-0.145*	-0.0674
		(0.0649)	(0.0690)	(0.0940)
Occasional smokers	Display ban	-0.0359	-0.0414	-0.0571
N=7,262		(0.0518)	(0.0542)	(0.0683)
	Price	0.0307	0.156	-0.0202
		(0.158)	(0.203)	(0.310)
Quit intention				
Daily smokers	Display ban	0.00307	0.00228	-0.000393
N=31,202		(0.0257)	(0.0266)	(0.0279)
	Price	0.0107	0.0717	0.0299
		(0.0516)	(0.105)	(0.111)
Occasional smokers	Display ban	0.00317	0.00318	-0.0132
N=9,176		(0.0214)	(0.0199)	(0.0173)
	Price	-0.361**	-0.215*	-0.402**
		(0.104)	(0.109)	(0.128)

Notes: Each column shows the results from a DD regression estimated by OLS. Data from 8 provinces (excluding Ontario and Quebec) for the period 1999-2011. All models include province, month, and year fixed effects. Standard errors throughout are clustered at the province level and estimates are weighted. Inference uses t-distribution with 5 degrees of freedom. Significance levels are: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

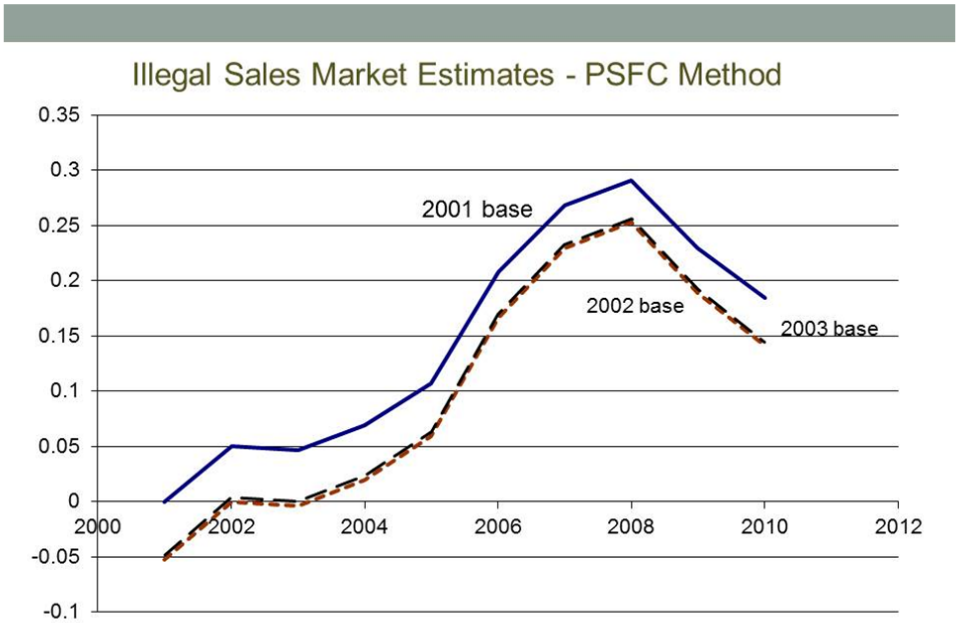

Figure 1: Trend in Illegal Sales Market Estimates

The vertical axis is the estimated percentage of the Canadian market for cigarettes accounted for by illegal cigarettes. Calculations by the authors follow the method used by Physicians for a Smoke Free Canada (PSFC), 2011. The trends are obtained by assuming that there was essentially no illegal product in the market in 2001, or 2002 or 2003. This assumption matches police reports, in this period of relatively low legal prices. For each of these base years the authors establish a factor of proportionality between the quantity of cigarettes smoked economy wide on the basis of responses to the CTUMS on the one hand, and actual reported legal sales on he other. When such a factor of proportionately is applied to sales in the mid and later years of the decade a substantial gap is observed between reported legal sales and predicted total consumption. The difference is attributed to contraband product. This method is considered superior to measures based on police seizures of illegal product, on account of the variability of the latter and also on account of the fact that seizures are recognized to represent just a tiny fraction of total contraband.

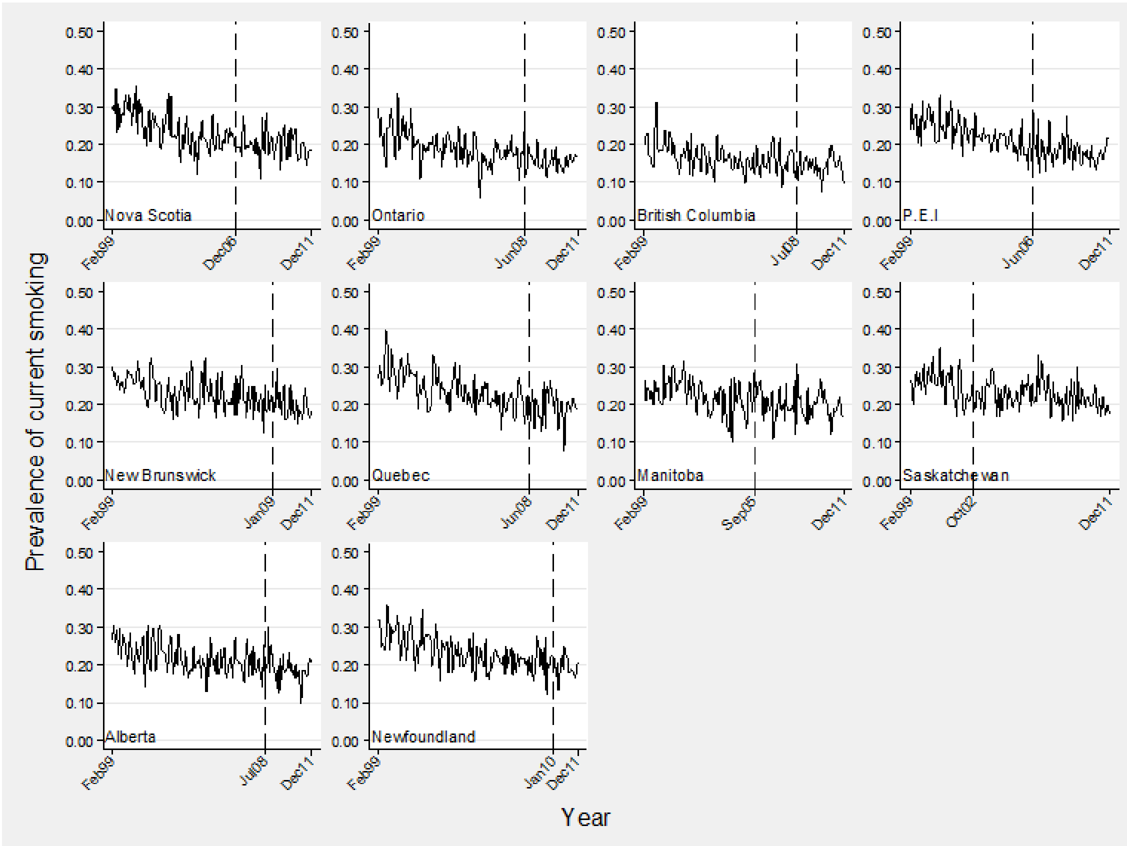

Figure 2. Monthly prevalence of smoking

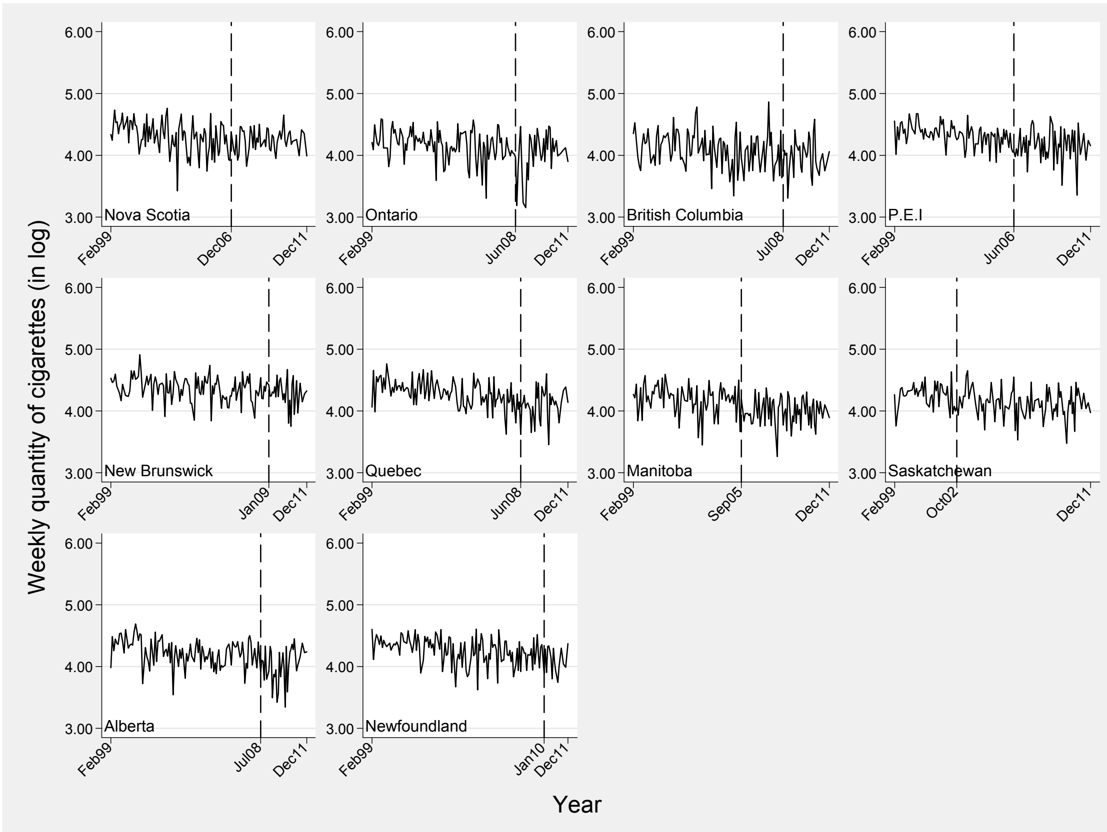

Figure 3: Weekly quantity of cigarettes (in log)

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