The Impacts of Restrictions on the Display of Tobacco Products

A Supplemental Report by Europe Economics

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1 INTRODUCTION

1.1 Europe Economics is a private sector consultancy, based in London, which specialises in the application of economics and econometrics to problems arising predominantly in the fields of public policy, regulation and competition. Our clients include government departments, regulators and competition authorities, companies large and small, professional and trade associations, charities, law firms and public affairs firms. The expert credentials of the Project Director for this report, Dr Andrew Lilico, are set out in his curriculum vitae which appears in Appendix 3. In particular, Dr Lilico is an expert in microeconomic analysis and regulatory impact assessment.

1.2 The Department of Health in the UK published on 31st May 2008 its “Consultation on the future of tobacco control” (hereafter “the FTC Document”). In this context, Europe Economics produced for JT International and Gallaher Limited (both members of the Japan Tobacco Group and referred to below as “JTI”) on 2nd September 2008 a report that provides expert economic analysis of the issues raised by aspects of the FTC Document. Europe Economics’ 2008 Report (hereafter “the 2008 Report”) can be found at http://www.jti.com/cr_home/industry_regulation.[1]

1.3 This Supplemental Report was requested by JTI to consider how data only recently made available (see paragraph 1.5 for a description and source of this data) might affect the views expressed in the 2008 Report. Unless defined in this document, the definitions used have the same meaning as in the 2008 Report.

1.4 Specifically, Dr Lilico was asked to consider how new smoking prevalence and consumption data available from the same sources as considered in the 2008 Report might affect the views expressed in that report concerning the impact of restrictions on the display of tobacco products (sometimes referred to hereafter as “display bans”).

1.5 The following data of relevance to these views has recently become available:

(a) Additional data was made available on 13th August 2009 from Health Canada[2] for 2008 on how many people smoke cigarettes and how many cigarettes are smoked per day in Canadian provinces. Specifically, this includes additional data for those provinces in which a small amount of data following the introduction and enforcement of display bans was available at the time of the 2008 Report. It also includes data from further provinces that have subsequently introduced such bans or that, although they had already introduced such bans before September 2008, either no data were available for the period following the introduction and enforcement of such restrictions or the period of such data was so short that it was not suitable for statistical analysis.

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[1] A correction to Figure 3.1 on page 31 of the 2008 Report was made on 24 February 2009.
[2] Link: http://www.hc-sc.gc.ca/hc-ps/tobac-tabac/research-recherche/stat/index-eng.php

(b) An additional year's smoking prevalence data from Statistics Iceland[3] was made available on 21st September 2009, as well as additional data from ESPAD[4] regarding youth smoking prevalence in Iceland.

(c) An additional year's data on the numbers of cigarette brands and manufacturers active in Iceland (from ÁTVR, the Icelandic State Alcohol and Tobacco Store) and Thailand (from AC Nielsen retail audit data).

1.6 In Section 2 below, we consider the Canadian data. The main body of that Section sets up a series of models based on a panel — that is to say, a dataset consisting of figures for each Canadian province considered across a series of years. We use the data to consider the extent to which statistical correlation exists between the introduction of restrictions on display and the number of cigarette smokers and average number of cigarettes smoked in the overall population and amongst 15-19 year olds. An Appendix reports our statistical tests with much more formality and technicality than the main body of the text, and we direct the interested or technically-minded reader there for more detail.

1.7 Section 3 turns to consider the data from Iceland on smoking prevalence and average number of cigarettes smoked. We first update the analysis we did in September 2008, based on the recently released Statistics Iceland data, then move on to consider whether anything can be inferred from the limited ESPAD dataset.

1.8 Section 4 updates our previous analysis on competition and innovation for Iceland and Thailand.

1.9 We note that we remain of the same views as those expressed in our previous report as regards the economic role and nature of cigarette packs and the ways in which branding affects the nature of a product (as set out in Section 2 of that report), and the role of brand imagery (including its significance for switching at point-of-sale) as set out in Section 3 of that report.

1.10 The term "Relevant Date" is used (as it was in the 2008 Report) to refer to the date at which restrictions on display have come into effect or begun to be enforced. (This issue is discussed in detail at paragraphs 4.28-4.31 of the 2008 Report.)

Summary of Report's findings

1.11 The findings from the 2008 Report remain unchanged, in particular as regards:

(a) "The impact upon innovation of the display bans introduced recently in Iceland and Thailand is as yet unclear." (Finding 9)

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[3] Link: http://www.statice.is/Statistics/Health,-social-affairs-and-justi/Lifestyle-and-health

[4] Link: http://www.espad.org/keyresult-generator

(b) "A display ban would materially impair new innovation." (Finding 8(a))

(c) "Evidence from Iceland and Thailand suggests that…a display ban, as well as having negative competition effects itself, may also be bringing to realisation a number of the negative competition effects of other measures." (Finding 11)

(d) "Data from Iceland [and] Thailand…suggest that, at least so far, display bans have had no measurable impact upon prevalence of smoking, either among the young or among the population as a whole." (Finding 13(a) and (b))

1.12 The following finding from the 2008 Report is modified:

> "Data from...relevant Canadian provinces suggest that, at least so far, display bans have had no measurable impact upon prevalence of smoking, either among the young or among the population as a whole."

This modification is necessary as, having considered the recently released 2008 Canadian data using more detailed and powerful statistical tests than were available to us in September 2008 (given the limited data which existed at that point), we have found that, although the presence of the display ban has no statistical correlation with the extent of smoking prevalence for the general population in Canada, the display ban is strongly and materially correlated with increased prevalence amongst 15-19 year olds. This statistical correlation is discussed further at paragraphs 2.46 to 2.47 and 2.49(c) below and in Appendix 2 to this report.

1.13 Overall, our conclusion remains that there is, as yet, no credible statistical evidence that the introduction of display bans has been associated with reduced smoking prevalence, and in particular, no evidence of such an effect in respect of those aged 15-19.

2 CANADIAN DATA ON PREVALENCE AND AVERAGE NUMBER OF CIGARETTES SMOKED

2.1 In the 2008 Report, we considered (at paragraphs 6.7 to 6.23) the relationships between the display bans in place in Manitoba and Saskatchewan and smoking prevalence and the average number of cigarettes smoked in those provinces. Having had, at that time, the opportunity to review data up to and including 2007, our view was as follows:

(a) Commenting on the annual Canadian smoking prevalence and consumption data to 2007 (paragraph 6.9 in the 2008 Report),

> "while we acknowledge that the number of data points after the Relevant Date of the policy is limited, it seems difficult to conclude that the display ban has done anything to change smoking behaviour amongst the young in Manitoba, which seems relatively fixed and constant over time."

(b) Commenting on the monthly Canadian smoking prevalence and consumption data to 2007 (paragraph 6.14 in the 2008 Report),

> "Display bans have not yet affected the percentage [of] daily smokers in Manitoba and Saskatchewan. Of course, the time period is clearly still relatively short, and it remains possible in principle that an effect might be visible over a longer timescale."

(c) Commenting on the Canadian smoking prevalence and consumption data to 2007 in general (paragraph 6.17 in the 2008 Report),

> "The analysis...suggests that the claim that the display bans have impacted upon tobacco consumption in Canada remains "very speculative" as of 2008. We cannot yet conclude from these data alone that the regulatory restrictions in these Canadian provinces have resulted in reduced prevalence or consumption."

2.2 With an additional year of data and with data recently made available on additional provinces that have introduced display bans, we have been able to update our previous analysis.

Putative impact of display bans in respect of prevalence and average number of cigarettes smoked

2.3 Insofar as restrictions on display might have impacts upon smoking prevalence and/or the average numbers of cigarettes smoked, we consider them as belonging to two broad classes:

(a) Impacts on levels; and

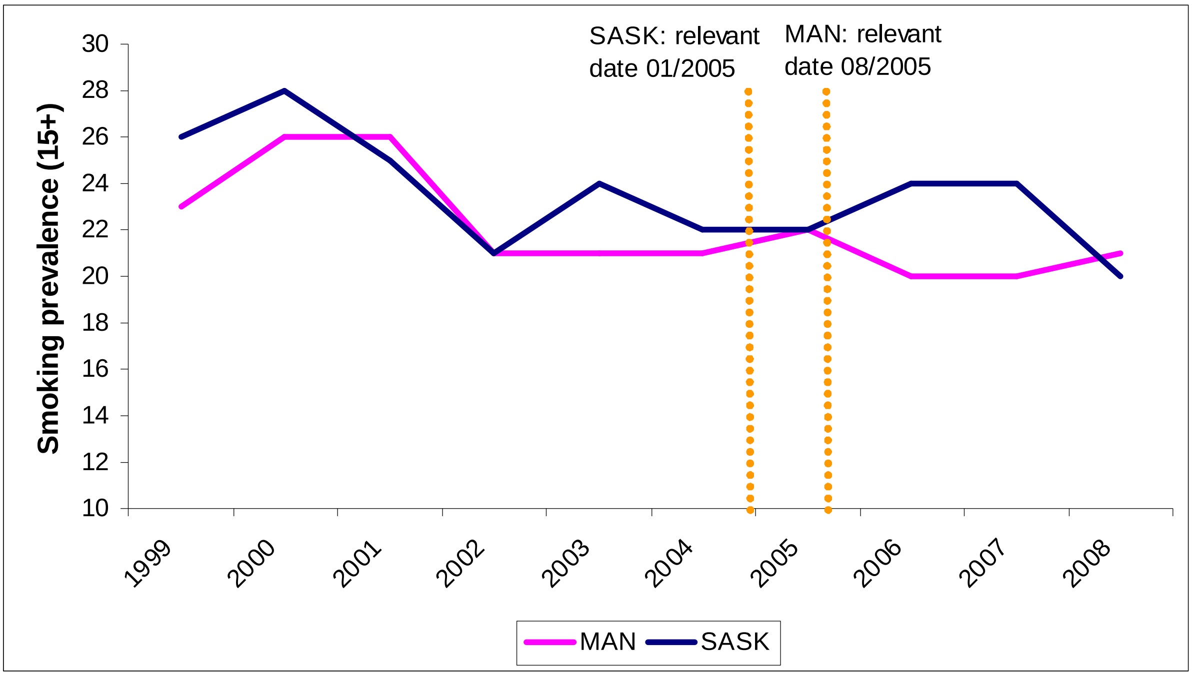
(b) Impacts on trends.

2.4 For example, if one were to introduce a measure that had an impact upon the average length of ties worn by businessmen, one form of impact would be making such ties shorter or longer – we call this an impact on levels. Another form of impact would be changing the trend of tie length – for example, if ties were getting steadily longer until the measure were introduced, but then started getting shorter after introduction of the measure, there would have been an impact on the trend.

Time Series Data on Manitoba and Saskatchewan

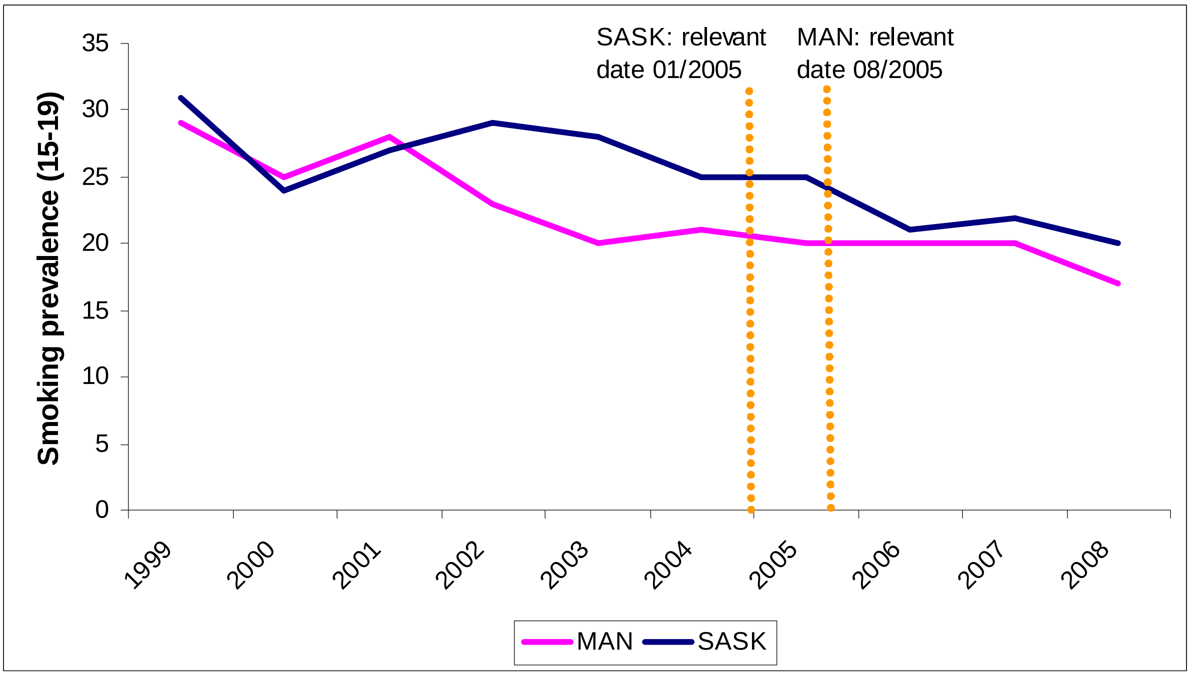
2.5 In Figure 2.1 to Figure 2.4 below we extend the annual data series illustrated in Figures 6.3-6.6 of our 2008 Report for Manitoba (MAN) and Saskatchewan (SASK).

Figure 2.1: Cigarette smoking prevalence (aged 15+)



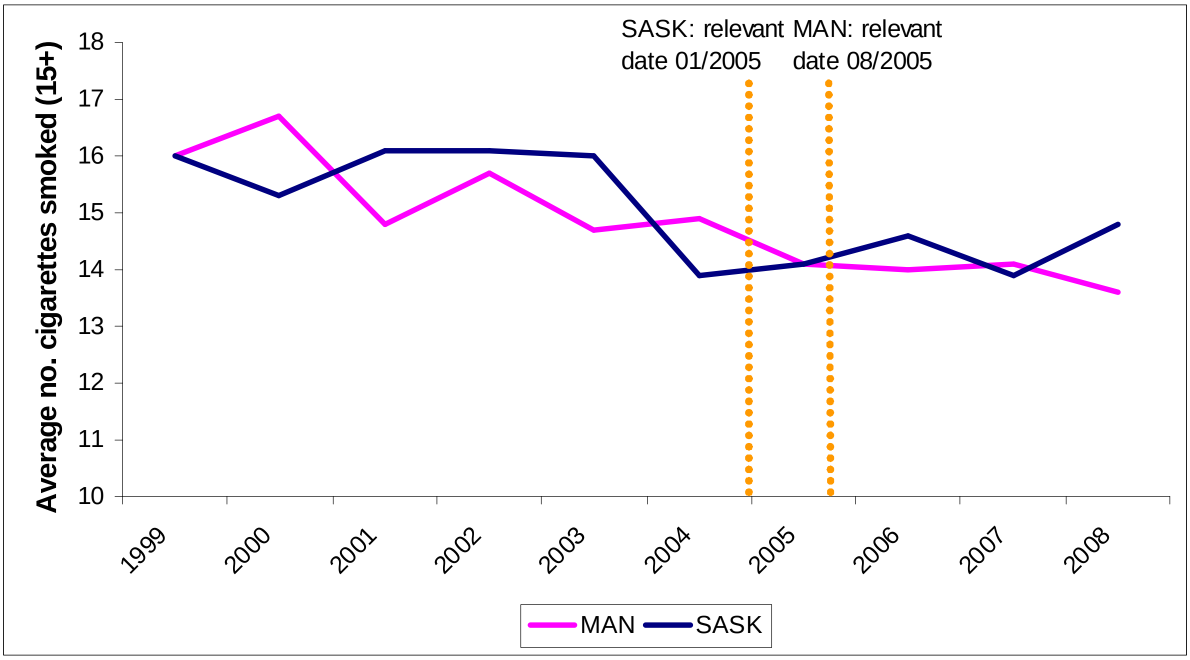
Source: Canadian Tobacco Use Monitoring Surveys, 1999-2008 (as available from Health Canada)

Figure 2.2: Cigarette smoking prevalence (aged 15-19)



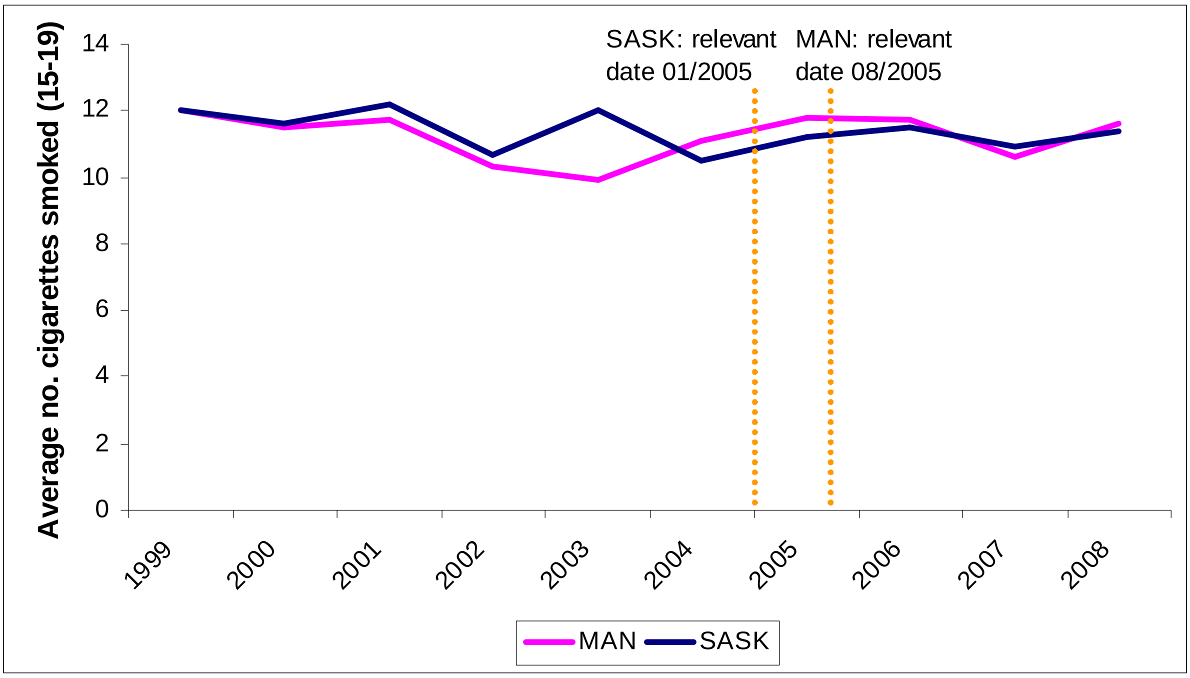
Source: Canadian Tobacco Use Monitoring Surveys, 1999-2008 (as available from Health Canada)

Figure 2.3: Average number of cigarettes smoked (per person per day for those aged 15+)



Source: Canadian Tobacco Use Monitoring Surveys, 1999-2008 (as available from Health Canada)

Figure 2.4: Average number of cigarettes smoked (per person per day by those aged 15-19)



Source: Canadian Tobacco Use Monitoring Surveys, 1999-2008 (as available from Health Canada)

2.6 Visually, it is not clear that the display ban has had any material impacts. We can say no more than was said at the time of the 2008 Report, namely:

(a) Paragraph 6.8 in the 2008 Report,

> "The prevalence trend for the general population and for 15-19 year olds in Manitoba […] seems to be falling."[5]

(b) Paragraph 6.9 in the 2008 Report,

> "The number of cigarettes smoked (per person per day) for 15-19 year olds in Manitoba in […] seems as trend-less as the prevalence series for this age group. Therefore, while we acknowledge that the number of data points after the Relevant Date of the policy is limited, it seems difficult to conclude that the display ban has done anything to change smoking behaviour amongst the young in Manitoba, which seems relatively fixed and constant over time. There may be more of a suggestion of a downward trend in the series for the general population on cigarettes smoked (per person per day) but again it does not seem to be a strong one."

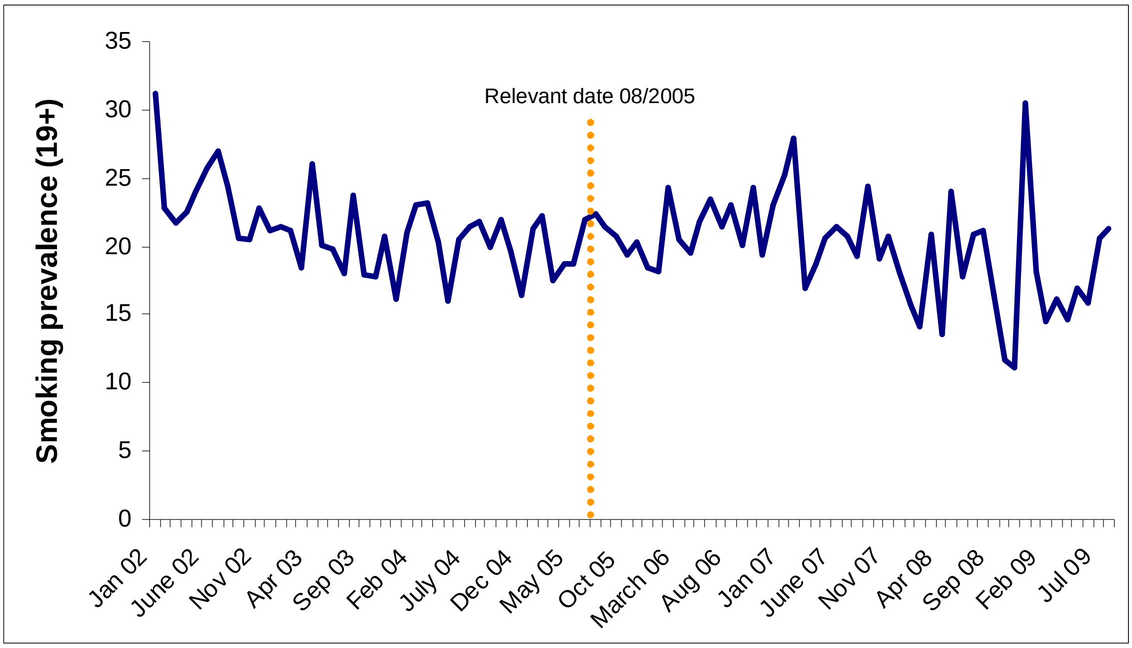
[5] Given that the 2008 data shows an increase for the general population, that aspect of this statement is now less secure.

(c) Paragraph 6.11 in the 2008 Report,

"Upon visual inspection there appears to be a general downward trend in the prevalence series, for Saskatchewan."

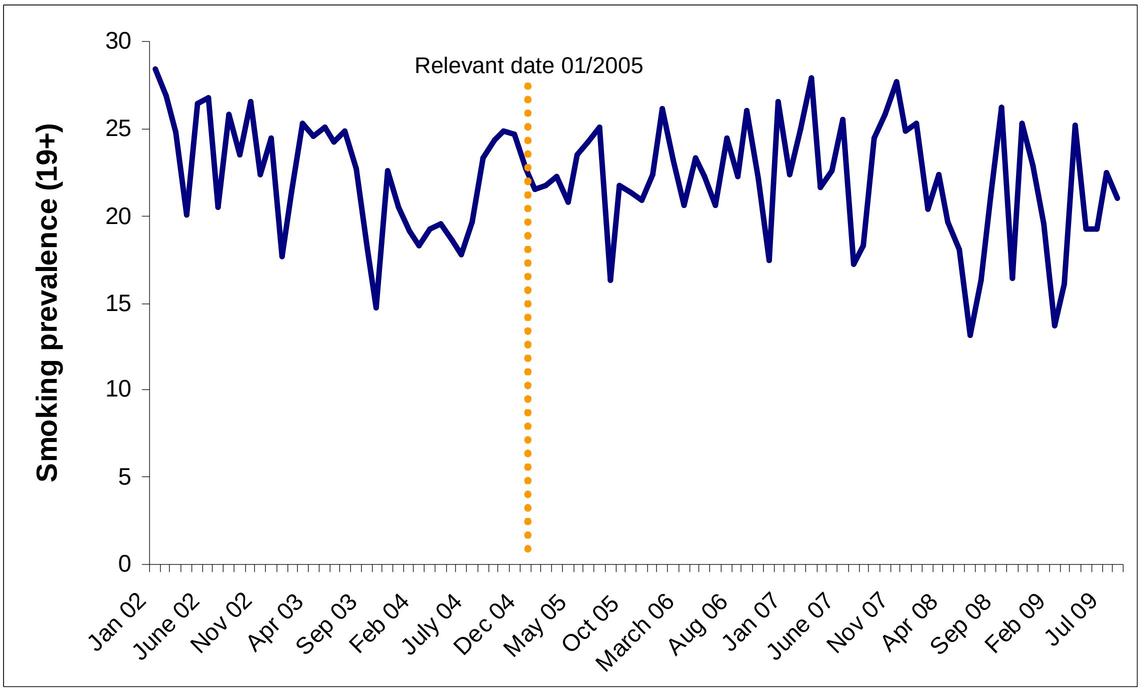
2.7 In Figure 2.5 and Figure 2.6 below we have also updated the monthly data series illustrated in Figures 6.8-6.9 of the 2008 Report for Manitoba and Saskatchewan.

Figure 2.5: Percentage of people that are daily smokers in Manitoba (sample weighted according to actual population distribution)



Source: data for the period of January 2002 to February 2007 were collected through an Omnibus Tracking Study conducted by Maritz Research Company. The data from February 20, 2007 onwards were collected through a tracking study performed by Research Strategy Group.

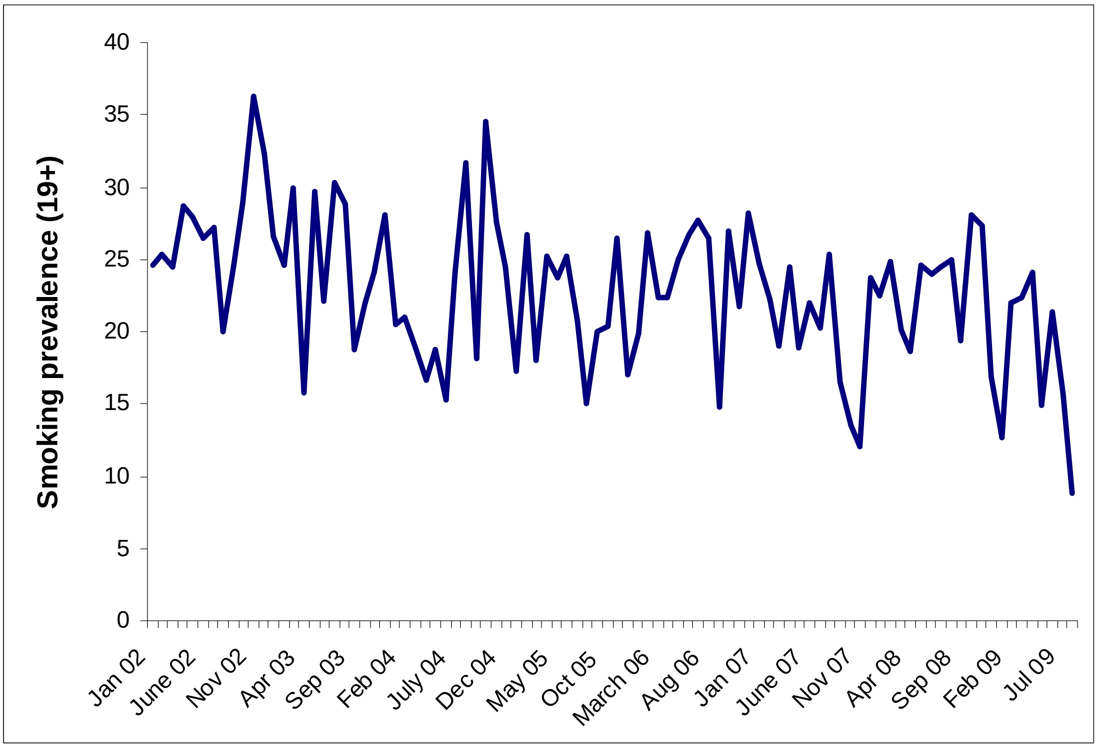
Figure 2.6: Percentage of people that are daily smokers in Saskatchewan (sample weighted according to actual population distribution)



Source: Data for the period of January 2002 to February 2007 was collected through an Omnibus Tracking Study conducted by Maritz Research Company. The data from February 20, 2007 onwards were collected through a tracking study performed by Research Strategy Group.

2.8 The statistical advantage which this monthly data has over annual data is that there are sufficient data points after the display bans were enforced to perform "Chow" tests. (A "Chow" test examines statistically whether the trend in the data before the relevant dates differs from that afterwards.) The "Chow" tests described in the 2008 Report were conducted using monthly prevalence data for British Columbia (which appear at Figure 6.7 of the 2008 Report) as a statistical control. In this Supplemental Report, we have replaced the British Columbia monthly series with that of Newfoundland and Labrador (see Figure 2.7 below). (British Columbia no longer constitutes a useful control because a substantial amount of the updated prevalence data for British Columbia covers a period subsequent to the enforcement of a display ban in that jurisdiction.)

Figure 2.7: Percentage of people that are daily smokers in Newfoundland and Labrador (sample weighted according to actual population distribution)



Source: Data for the period of January 2002 to February 2007 was collected through an Omnibus Tracking Study conducted by Maritz Research Company. The data from February 20, 2007 onwards were collected through a tracking study performed by Research Strategy Group.

2.9 The results of the "Chow" tests performed using Newfoundland and Labrador as a control are reported in Appendix 2, and confirm that the enforcement of the bans is not statistically associated with a change in the trend of prevalence for Manitoba and Saskatchewan[6] — in other words, statistically speaking, display bans have not yet been correlated with a change in the percentage of daily smokers in Manitoba and Saskatchewan. Of course, the time period remains relatively short, and thus it remains possible in principle that an effect might become visible over a longer timescale.

The Option of More Powerful Tests

2.10 Although the "Chow" test on monthly data on percentages smoking daily in Saskatchewan, Manitoba and British Columbia was in our view the best statistical investigation available as at September 2008, additional data that has become available

[6] We note that a statistical break is found for the Saskatchewan series, but the period after the structural break has no associated trend model. Thus, there is no statistical basis for associating this structural break with a change in trend.

recently allow us to conduct more detailed and powerful statistical tests that were not possible for the 2008 Report.

2.11 In particular, the Canadian Tobacco Use Monitoring Surveys ("CTUMS") dataset from Health Canada provides annual smoking prevalence data from 1999 to 2008 for the general population (15+) and for an age group of particular interest, those 15-19 years old.[7] This data is available for all ten Canadian provinces, though not for the three Canadian territories: Northwest Territories, Yukon, and Nunavut.

2.12 In addition to the display bans effective on the dates set out in paragraph 4.30 of the 2008 Report for Manitoba, Saskatchewan and British Columbia, display bans have also been enforced in the following Canadian provinces on the following dates:

Province	Abbreviation	Enforcement Date
Prince Edward Island	PEI	31 May 2006
Nova Scotia	NS	31 March 2007
Ontario	ONT	31 May 2008
Québec	QUE	31 May 2008
Alberta	ALB	1 July 2008
New Brunswick	NB	1 January 2009

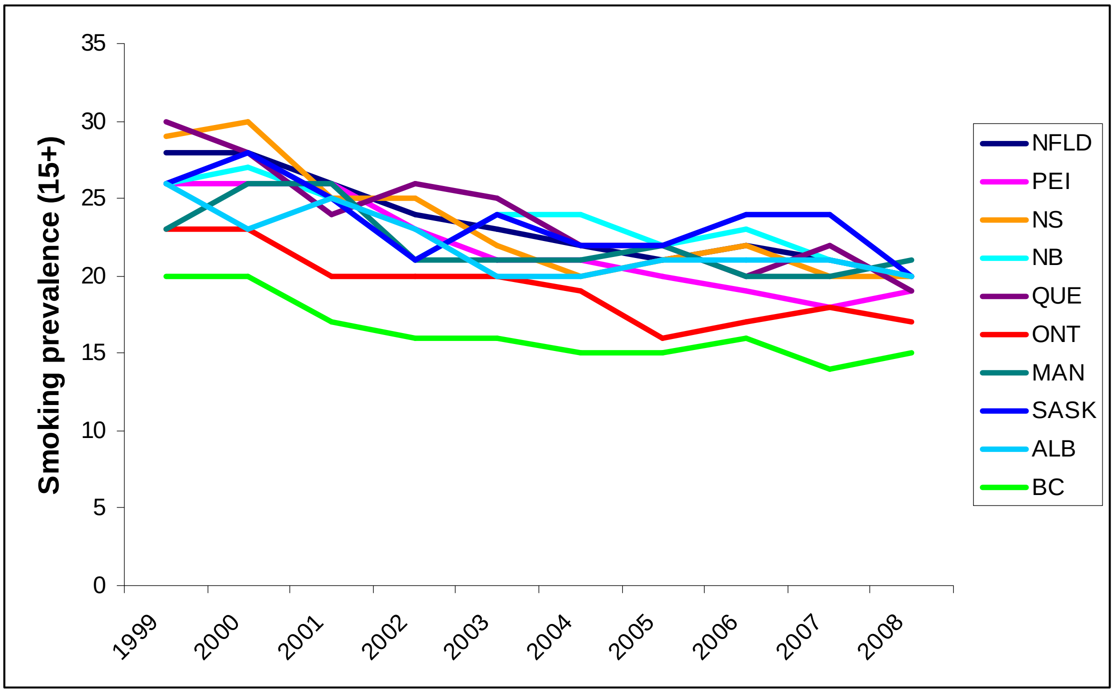
2.13 Newfoundland and Labrador ("NFLD") is expected to enforce a display ban from 1 January 2010.

2.14 At the date of the 2008 Report, the display bans for PEI, NS, ONT, QUE and ALB had not been enforced for a sufficient period of time to consider whether, either visually or statistically, the ban had had any material impact on existing trends in prevalence and/or consumption.

2.15 Below we illustrate this newly available data for smoking prevalence and cigarette consumption for all ten Canadian provinces for the general population (Figure 2.8 and Figure 2.10 below), and for those aged 15-19 years old (Figure 2.9 and Figure 2.11 below).

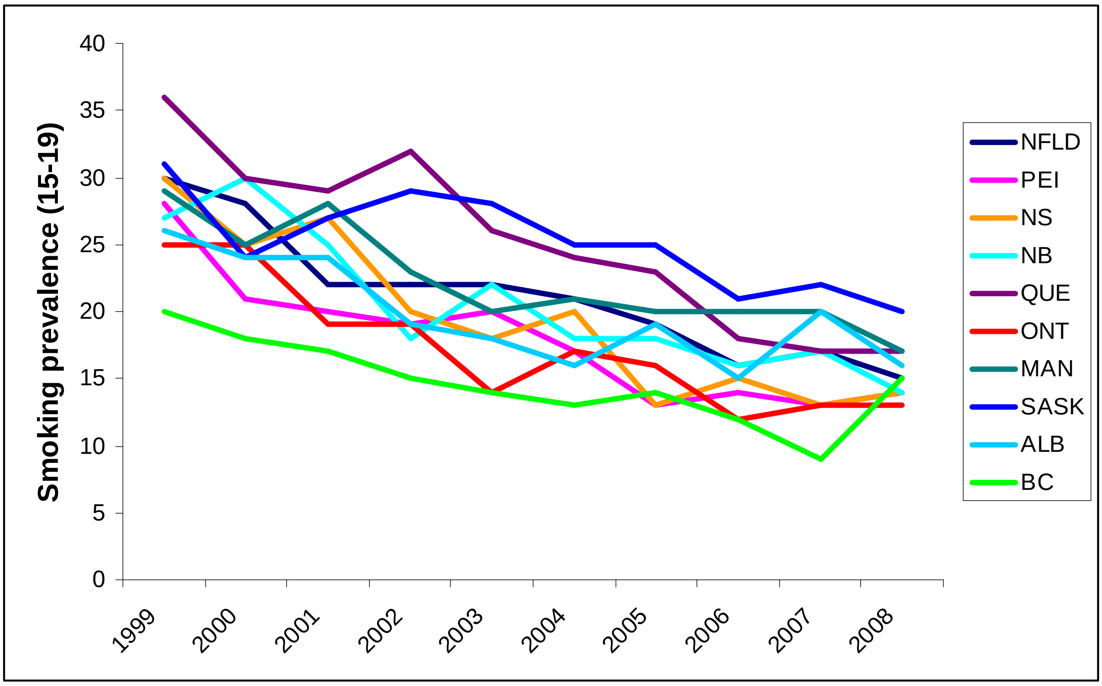
[7] Source: http://www.hc-sc.gc.ca/hc-ps/tobac-tabac/research-recherche/stat/index-eng.php

Figure 2.8: Cigarette smoking prevalence (15+)



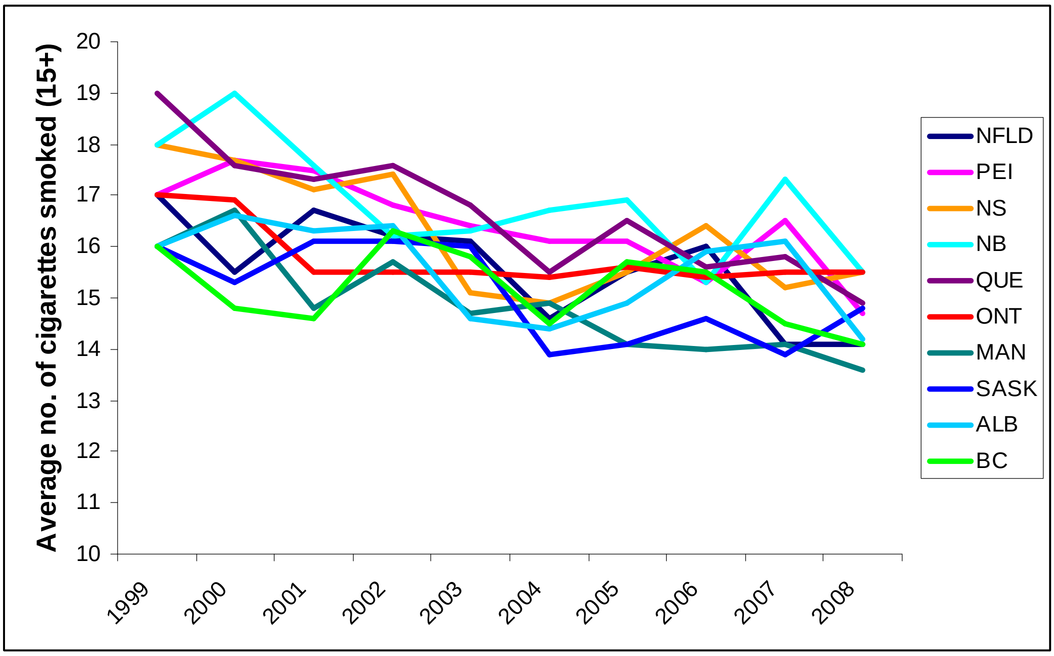
Source: Canadian Tobacco Use Monitoring Surveys, 1999-2008 (as available from Health Canada)

Figure 2.9: Cigarette smoking prevalence (15-19)



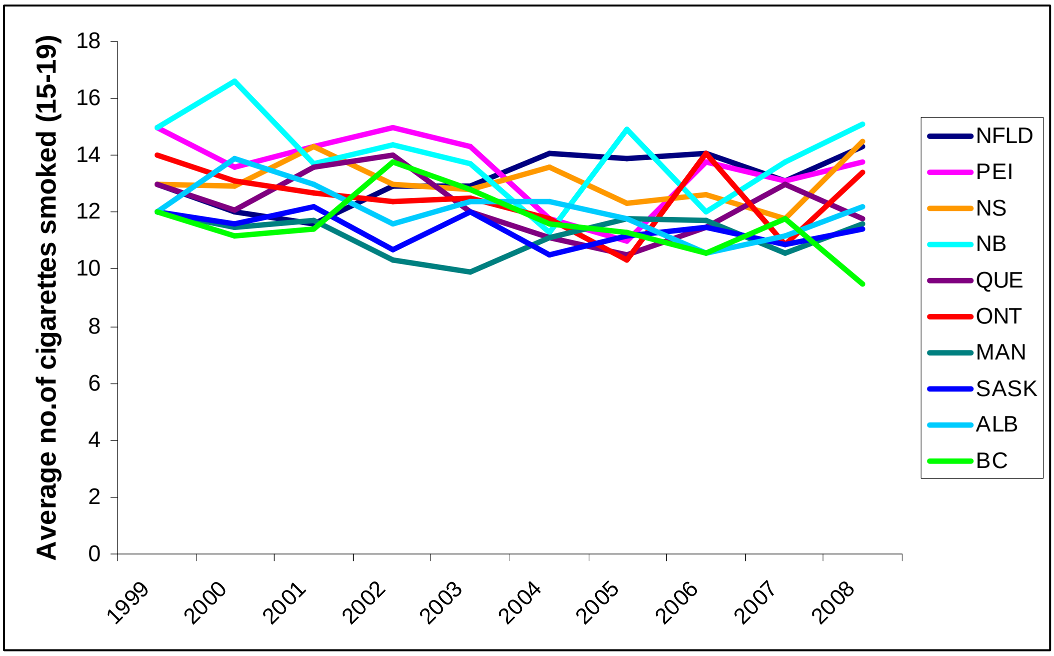
Source: Canadian Tobacco Use Monitoring Surveys, 1999-2008 (as available from Health Canada)

Figure 2.10: Average number of cigarettes smoked (per person per day by those aged 15+)



Source: Canadian Tobacco Use Monitoring Surveys, 1999-2008 (as available from Health Canada)

Figure 2.11: Average number of cigarettes smoked (per person per day by those aged 15-19)



Source: Canadian Tobacco Use Monitoring Surveys, 1999-2008 (as available from Health Canada)

2.16 A simple visual inspection of the graphs above does not allow a robust assessment of whether there is a statistical impact of display bans for two reasons. First, the figures are too volatile for a visual observation to assess robustly any potential structural change in the series. Second, the majority of provinces have had a display ban enforced too recently.

2.17 However, because we now have available a number of years for which several provinces had display restrictions whilst other provinces did not, and years in which the same province did not have such restrictions and later years in which display was restricted, we are able to employ data on all provinces for all years from 1999 to 2008. Such a dataset, in which cross-sectional statistics are available across a number of years, is known as a "panel". A brief explanation of a panel model is provided below.

Description of a panel model

2.18 In general, the impact of the display ban on smoking prevalence and consumption can be inferred by applying two different methodologies. The first methodology exploits the *cross-sectional* dimension of the data and infers the impact of the display ban from a comparison of the provinces where the display ban is enforced with those where it is not enforced (see Figure 2.12 below by reference to the data for smoking prevalence amongst those aged 15-19 on a percentage basis).

Figure 2.12: Cross-sectional dimension based on a sample of 10 data points

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
NFLD	30.0	28.0	22.0	22.0	22.0	21.0	19.0	16.0	17.0	15.0
PEI	28.0	21.0	20.0	19.0	20.0	17.0	13.0	14.0	13.0	14.0
NS	30.0	25.0	27.0	20.0	18.0	20.0	13.0	15.0	13.0	14.0
NB	27.0	30.0	25.0	18.0	22.0	18.0	18.0	16.0	17.0	14.0
QUE	36.0	30.0	29.0	32.0	26.0	24.0	23.0	18.0	17.0	17.0
ONT	25.0	25.0	19.0	19.0	14.0	17.0	16.0	12.0	13.0	13.0
MAN	29.0	25.0	28.0	23.0	20.0	21.0	20.0	20.0	20.0	17.0
SASK	31.0	24.0	27.0	29.0	28.0	25.0	25.0	21.0	22.0	20.0
ALB	26.0	24.0	24.0	19.0	18.0	16.0	19.0	15.0	20.0	16.0
BC	20.0	18.0	17.0	15.0	14.0	13.0	14.0	12.0	9.0	15.0

Source: Canadian Tobacco Use Monitoring Survey (CTUMS) 1999-2008 (as availalbe from Health Canada)

Ban in effect

Ban not in effect

2.19 This methodology requires data on many provinces with the display ban and many without. The size of our sample (only ten data points) is inadequate for this purpose.

2.20 An alternative methodology is that of exploiting the *time-series* dimension of the data and inferring the impact of the display ban by a comparison of the smoking prevalence of a province before and after its enforcement (see Figure 2.13 below).

Figure 2.13: Time-series dimension based on a sample of 10 data points

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
NFLD	30.0	28.0	22.0	22.0	22.0	21.0	19.0	16.0	17.0	15.0
PEI	28.0	21.0	20.0	19.0	20.0	17.0	13.0	14.0	13.0	14.0
NS	30.0	25.0	27.0	20.0	18.0	20.0	13.0	15.0	13.0	14.0
NB	27.0	30.0	25.0	18.0	22.0	18.0	18.0	16.0	17.0	14.0
QUE	36.0	30.0	29.0	32.0	26.0	24.0	23.0	18.0	17.0	17.0
ONT	25.0	25.0	19.0	19.0	14.0	17.0	16.0	12.0	13.0	13.0
MAN	29.0	25.0	28.0	23.0	20.0	21.0	20.0	20.0	20.0	17.0
SASK	31.0	24.0	27.0	29.0	28.0	25.0	25.0	21.0	22.0	20.0
ALB	26.0	24.0	24.0	19.0	18.0	16.0	19.0	15.0	20.0	16.0
BC	20.0	18.0	17.0	15.0	14.0	13.0	14.0	12.0	9.0	15.0

Source: Canadian Tobacco Use Monitoring Survey (CTUMS) 1999-2008 (as availalbe from Health Canada)

Ban in effect

Ban not in effect

2.21 A time-series comparison requires many years of data before and after the display ban is enforced, and therefore cannot be applied to the data as this would be based on a series (highlighted in blue above) with only ten data points. The monthly data used in our Chow tests have twelve datapoints each year, greatly magnifying the size of the sample, and so our monthly data allows some time series analysis.

2.22 However, by combining both the *cross-sectional* and *time-series* dimensions of our annual dataset (see Figure 2.14 below), we:

(a) greatly increase the statistical power of the tests that can be performed using it;

(b) significantly reduce the number of datapoints required for meaningful analysis; and

(c) at the same time, significantly increase the number of datapoints we have available (to 100 instead of just 10).

2.23 For these reasons, this is the route we chose to pursue.

Figure 2.14: Cross-section and time-series dimensions based on a sample of 100 data points

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
NFLD	30.0	28.0	22.0	22.0	22.0	21.0	19.0	16.0	17.0	15.0
PEI	28.0	21.0	20.0	19.0	20.0	17.0	13.0	14.0	13.0	14.0
NS	30.0	25.0	27.0	20.0	18.0	20.0	13.0	15.0	13.0	14.0
NB	27.0	30.0	25.0	18.0	22.0	18.0	18.0	16.0	17.0	14.0
QUE	36.0	30.0	29.0	32.0	26.0	24.0	23.0	18.0	17.0	17.0
ONT	25.0	25.0	19.0	19.0	14.0	17.0	16.0	12.0	13.0	13.0
MAN	29.0	25.0	28.0	23.0	20.0	21.0	20.0	20.0	20.0	17.0
SASK	31.0	24.0	27.0	29.0	28.0	25.0	25.0	21.0	22.0	20.0
ALB	26.0	24.0	24.0	19.0	18.0	16.0	19.0	15.0	20.0	16.0
BC	20.0	18.0	17.0	15.0	14.0	13.0	14.0	12.0	9.0	15.0

Source: Canadian Tobacco Use Monitoring Survey (CTUMS) 1999-2008 (as availabe from Health Canada)

Ban in effect (red)

Ban not in effect (black)

2.24 Using this panel, we are able to model the effects of display bans in two broad ways.

(a) First, we consider a '*simple trend model'* in which we assess what difference, if any, the presence of a display ban makes to the numbers of smokers as a percentage and average numbers of cigarettes smoked for the whole 15+ population and just those aged 15-19, versus a straightforward linear trend.

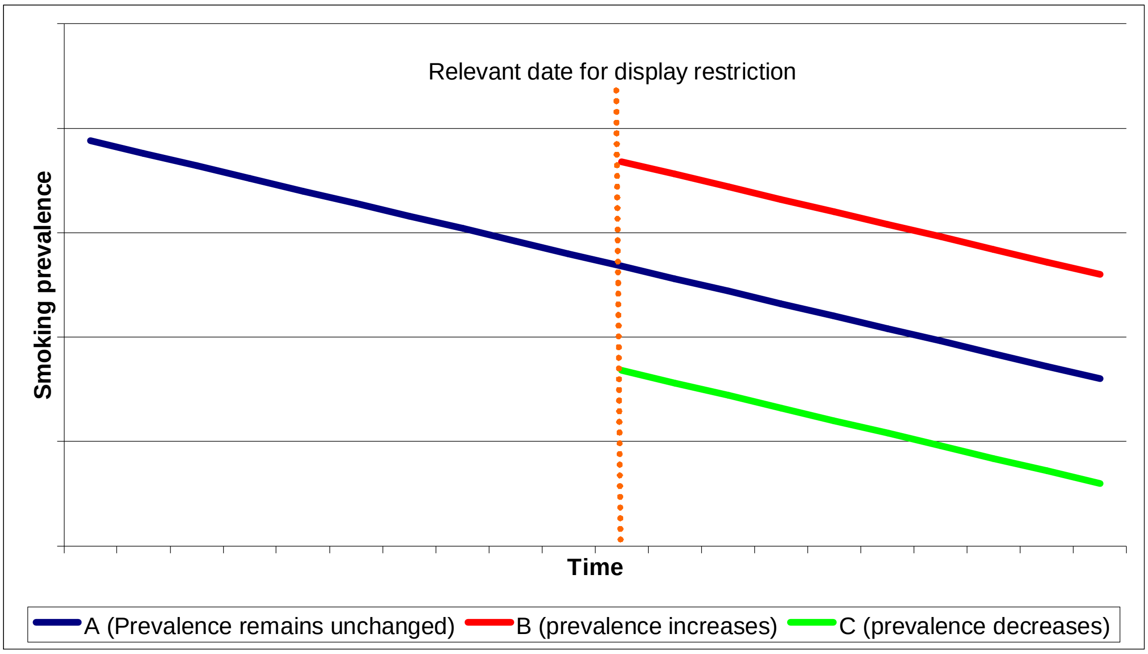
(b) Secondly, we consider a more elaborate '*standard economic factors'* model in which changes in the prevalence and the average number of cigarettes smoked are tested to consider if correlations exist between cigarette prices and income per capita as well as the presence or introduction of a display ban.

Simple Trend Models

2.25 Our first model considers the statistical impact of display bans when the number of cigarette smokers and average number of cigarettes smoked in Canada during 1999 to 2008 are otherwise understood as simply reflecting a trend. Although depending solely upon such a linear trend model might arguably be methodologically naïve in ignoring potentially important economic factors (and indeed we shall below consider a more elaborate model), identifying the purest statistical effect of display bans is an instructive starting point and can be useful to aid the explanation and interpretation of later more complicated results.

2.26 We illustrate in Figure 2.15 below the linear analysis represented by the Simple Trend Model.

Figure 2.15: What a simple test examines



2.27 We see in Figure 2.15 that this test models the display restriction as potentially affecting the level of the variable (whatever is on the y axis – say, the number of smokers aged 15-19) either by raising it (line B), lowering it (line C), or leaving it unchanged (line A) but, crucially, with no effect on the slope of the trend.[8]

2.28 The key weakness of this first, simplistic trend model is that there may be a number of features that change through time in ways that are not random, and which are closely correlated to smoking prevalence or the average number of cigarettes smoked.

Results of Simple Trend Model

2.29 Our model based on simply correlating smoking prevalence and cigarette consumption with a linear trend and the presence or absence of a display ban produces the following results for the Canadian panel:

(a) For both age groups, the introduction of a display ban is associated, statistically, with a rise (*NB* a rise, not a fall) in smoking prevalence. That is to say, the situation broadly resembles the transition from trend line A in Figure 2.15 before the display ban to trend line B afterwards.

[8] Of course, a change in trend is what we have tested for with a Chow test.

(b) For both age groups, the average number of cigarettes consumed is unchanged. In broad graphical terms, the trend in the average number of cigarettes smoked is rather like line A both before and after the introduction of the display ban.

2.30 As noted above, although it is useful to perform this initial statistical test, it is very simplistic and does not, in our view, allow us to say anything definitive about the display ban. More specifically, the fact that this statistical test has the result that the display ban increased smoking prevalence for the population as a whole and for the 15-19 age group, does not, by itself, allow us to conclude that the introduction of the ban is associated with increased prevalence. The reason we cannot jump to such an inference (even at the level of correlation, let alone causation) is that this simplistic statistical test does not control for a number of important and obvious factors that might affect prevalence.

2.31 Specifically, we have not yet controlled for any of the statistical effects that price or income might have upon prevalence or on the average number of cigarettes consumed. We turn to that now.

Standard Economic Factors Models

2.32 Our next group of models employs two of the insights of the economic theory of consumer demand that would be most widely acknowledged by economists. Specifically, we consider the possibility that consumption of cigarettes (both in terms of prevalence and in terms of average number of cigarettes smoked) might be correlated with the price of cigarettes and the average incomes of smokers.

Rationale for the standard economic factors models

2.33 There are a number of economic mechanisms via which the prices of any good (not just cigarettes) might be correlated with the number of consumers of that good. One is that when the number of people that are regular consumers of a good falls, the willingness-to-pay of those that remain rises and hence the price rises. Another is that when the price of a good rises, people's ability to afford to be consumers of it may fall. For example, if computer games cost £10,000 each, then fewer people would buy computer games (at least *legal* computer games) than if computer games cost £30 each. A third is that when the price of a good rises, people may switch from that good into near substitutes (e.g. perhaps a pipe smoker might become a cigar smoker if the cost of pipe tobacco rose). Similarly, there are a number of mechanisms via which the prices of a good might be correlated with the amounts of that good consumed.

2.34 Again, there are a number of economic mechanisms via which income per capita might be correlated with the number of consumers of a good. If computer games cost £30 each, then, even for given preferences, households with a per capita income of £10,000 per week are more likely to be able to be computer games purchasers than are households with a per capita income of £20 per week. Alternatively, periods of reduced consumption might be periods of reduced income, if what has happened is that consumers in the economy as a whole cut back and that then drove a period of recession.

(Such "Keynesian" mechanisms have been discussed recently in the context of the Great Recession.)

2.35 If indeed prices and average incomes are correlated with prevalence and/or average number of cigarettes smoked, then ignoring such effects could introduce spurious statistical results.

The dataset used

2.36 The standard economic factors models were estimated using four different panels:

(a) *Smoking prevalence and average number of cigarettes smoked (per person per day) for the general population and for those aged 15-19* — Data was provided by CTUMS 1999-2008, as available from Health Canada.[9]

(b) *Cigarette prices* — The panel was constructed by combining data obtained from two different sources. The first source of data is the Consumer Price Index (CPI) for the period 1999-2008, as provided by Statistics Canada.[10] The second data source is the average price level of 200 cigarettes in each province as of 2008, and was provided by the Smoking and Health Action Foundation,[11] as available from the Canadian Council for Tobacco Control.[12] The price level data provided an indication of price variation across provinces and was combined with the CPI data from Statistics Canada in order to extrapolate the price level of cigarettes (relative to other commodities) in each province considered, for the period 1999-2008.

(c) *GDP per capita* — The panel was obtained by combining historic data on GDP for each province considered as available from Statistics Canada, with historic data on the population of each province, as available from the OECD.[13]

(d) *The display ban* — For each province considered, the presence of the display ban was accounted for by using a dummy variable which takes value "one" if the display ban is in place and value "zero" otherwise.

The estimation technique

2.37 The estimation technique employed to analyse the potential impact of the display ban is a *first difference estimator with fixed effects*, and for simplicity, hereafter we refer to the standard economic factors model estimated using this technique as a "*first difference fixed effects model*". The first difference fixed effects model is an internationally recognised statistical technique and is widely used in order to assess the impact of regulatory

9 Link: http://www.hc-sc.gc.ca/hc-ps/tobac-tabac/research-recherche/stat/index-eng.php
10 Link: http://www.statcan.gc.ca
11 Link: http://www.nsra-adnf.ca/cms/file/pdf/cigarette_prices_Canada_24_Apr_2008.pdf
12 Link: http://www.cctc.ca/cctc/EN/tcrc/multimedia/tcmultimedia.2008-05-09.4904552730
13 Links: http://www.statcan.gc.ca and http://stats.oecd.org/OECDregionalstatistics

interventions (such as the introduction of display bans in our context) upon a given outcome variable (in the case of our study, smoking prevalence and cigarette consumption). A key advantage of this model is that it is particularly effective in dealing with what in statistics is known as the 'omitted variables problem'. The ten different Canadian provinces in our sample have their own unique features such as cultural differences, differing age distributions, differing geographies, and so on. These province-specific factors are not observable — because we have no data — but might be statistically relevant in accounting for smoking behaviour. The first difference fixed effects model allows us to control for the potential effect these factors might have, without explicitly introducing them into the model.

2.38 Of course, besides the first difference fixed effects model there are also other models that could be used to assess the impact of a display ban. When a first difference fixed effects model identifies a correlation, we can be the most confident (amongst the models in the table) that there really is such a statistical correlation. On the other hand, when the first difference fixed effects model does not identify a correlation, we can be the least confident in asserting that no correlation exists — the data might simply be inadequate to stand up to the statistical scrutiny of the first difference fixed effects test.

2.39 Because of this, it is important to note that the substance of our key results, in terms of when the display restriction is statistically correlated with a change in smoking prevalence or average number of cigarettes smoked, arises in other less data demanding models, as well as in the first difference fixed effects model.

2.40 In Appendix 1 we provide a description of the most important of these alternative models and — for completeness and as a cross-check to our preferred first difference fixed effects model — we report the results obtained by employing the most relevant of them. The reader should note that these cross-checks produce results that, in respect of the statistical effect of the display ban, are - in substance - the same as those below.

2.41 It is unclear over precisely what time period the display ban is intended to reach maturity in its effects. We therefore test for:

(a) continuing effects produced by the presence of a display ban;

(b) immediate effects of a display ban; and

(c) effects that do not begin until one year after the introduction of the display ban.

2.42 The distinction between the presence of the display ban, the presence of the display ban with a one year delay, and the introduction of the display ban can be seen most straightforwardly by considering an example (we use the province of Manitoba, where the display ban was enforced in 2005):

Table 2.1: Three potential expressions of the display ban in Manitoba

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
Display ban	0	0	0	0	0	0	1	1	1	1
Display ban with delay	0	0	0	0	0	0	0	1	1	1
Introduction of the display ban	0	0	0	0	0	0	1	0	0	0

Results of the standard economic factors model

2.43 Our results concerning smoking prevalence require more discussion than those of the average number of cigarettes smoked, and hence we report the latter first.

Average number of cigarettes smoked

2.44 The following table denotes which variable has a statistically significant impact on the average number of cigarettes smoked.

Table 2.2: Results of the '*first difference fixed effects model'* concerning the impact of the display ban on the average number of cigarettes smoked (15-19 and 15+)

	(15-19)	(15+)
Price	no effect	(- -)
GDP per capita	no effect	no effect
Display ban	no effect	no effect
Display ban with delay	no effect	no effect
Introduction of the display ban	no effect	(- -)

(+ +), (- -) = *significance at 95%* + = *increase in consumption*

(+), (-) = *significance at 90%* - = *decrease in consumption*

(a) For the general population, we find that changes in cigarette prices are negatively correlated with changes in consumption per person per day. In other words, in those periods in which cigarette prices rise, fewer of them are smoked by the general population (as reported as part of the CTUMS data).

(b) There is no correlation of income per capita with the average number of cigarettes smoked both by the general population, and the 15-19 age group.

(c) **The average number of cigarettes consumed changes at the time of introduction of the display ban for the general population.** The introduction of the display ban is statistically associated with a decrease (estimated to be of the order of half a cigarette[14]) in the average number of cigarettes smoked per person per day.

(d) There is **no correlation** of changes in price, the presence (with and without delay), or introduction **of the display ban with changes in the average number of cigarettes consumed by the 15-19 age group**.

2.45 It is important to emphasise that the above results are delivered by a model in which changes in the average number of cigarettes consumed are almost entirely unrelated, in an accounting sense, to the variables with which we model it. In other words, the average number of cigarettes consumed is overwhelmingly accounted for, statistically, by fixed factors for each province combined with random fluctuations.

Smoking prevalence

Table 2.3: Results of the '*first difference fixed effects model'* concerning the impact of the display ban on smoking prevalence (15-19 and 15+)

	(15-19)	(15+)
Price	no effect	**(- -)**
GDP per capita	no effect	no effect
Display ban	**(+ +)**	no effect
Display ban with delay	no effect	no effect
Introduction of the display ban	**(+ +)**	no effect

(+ +), (- -) = *significance at 95%* **+** = *increase in prevalence*

(+), (-) = *significance at 90%* **-** = *decrease in prevalence*

2.46 The degree of statistical accounting for changes in the percentages that are cigarette smokers is much higher in our model than that for changes in the average number of cigarettes smoked per person per day. Key results are:

(a) Changes in **price are statistically correlated with changes in prevalence for the general population**. For the 15-19 age group there is no statistically significant correlation between price changes and prevalence, though the result is sufficiently close to significance to make a statistician suspect that more detailed investigation might, in principle, identify a correlation.

(b) Changes in income are uncorrelated to changes in smoking prevalence for either the general population or the 15-19 age group.

[14] We refer to Appendix 2 for all the estimation results.

(c) **The presence of the display ban has no statistical correlation with changes in prevalence, either with or without a year's delay for effect, for the general population (those 15+ years old)**.

(d) **The display ban is strongly and materially correlated with <u>increased</u> prevalence amongst 15-19 year olds**. Specifically, **where the display ban is present, smoking prevalence increases by 2 percentage points**.

2.47 Given that smoking prevalence in Canada amongst 15-19 year olds is 15 per cent, a 2 percentage point increase is obviously very significant. Taken at face value, this result would imply that the presence of display bans in Canada increased the numbers of 15-19 year olds that are smokers by approximately 13 per cent.[15]

2.48 It might seem, to the lay reader, as if, given the differences in population between different provinces, it should be appropriate to population weight the data during the statistical exercise. The reader might, for example, think of how one would calculate the overall level of smoking prevalence via a weighted sum of the smoking prevalences in individual provinces (when population weighting would indeed be appropriate). However, such weighting would be a serious methodological error in our statistical exercise here. The concept of the panel is that the population of each of the provinces represents, as it were, a sample drawn from the overall Canadian population (generating what a statistician calls an "observation"). To be sure, each sample (the set of people surveyed in each province to find out their smoking behaviour) may have its own idiosyncratic features (that is why we employ fixed effects adjustments), but the validity of the panel is based on the idea that there are some common pan-Canadian features to what occurs — and it is for these that we are testing. Provided that the sample taken in each province is sufficiently large, it is statistically significant by itself. To give provinces that had smaller populations a lower weight in the analysis would be to imply that there was greater doubt as to the accuracy of the estimate from that province — doubt that is not present here. The very essence of the panel approach is that each case counts equally.

Remarks

2.49 Having described the results of our model, it is important to emphasise the potential limitations of the data used, the way in which our results should be interpreted and further issues.

(a) First, we are able to construct data, by province, on the prices and levels of legal cigarettes, by using Consumer Price inflation data on cigarettes, by province, and cross-provincial data on levels of cigarette prices. However, our dataset does not

15 The 13 per cent change is calculated by the formula in Appendix 2. We note that, as ever, even if this at-face-value approach were correct it would say nothing about causation — for example, the introduction of the display ban and the 13 per cent increase in the number of 15-19 year old smokers could be caused by the same common factor even though they bore no causal relation to one another.

include prices of contraband or counterfeit cigarettes, but at the same time our data on prevalence and average number of cigarettes consumed does not distinguish between whether or not those cigarettes are legal. (In particular, the detailed CTUMS breakdown available on the Health Canada website does not tell us, of the average number of cigarettes smoked per person per day, the proportions that are counterfeit or contraband.) If consumption or prevalence is correlated with prices of cigarettes consumed, then it could be affected by changes to the prices of counterfeit or contraband cigarettes that are not matched by changes in the prices of legal cigarettes (we have data only on the prices of legal cigarettes). This might happen, for example, if the prices of contraband cigarettes rose as a result of improved enforcement.

(b) Next, our data on incomes give us only average income for a province as a whole, based on the GDP and population of each province. We have no data on the incomes of individual cigarette smokers or non-smokers. It is possible, in principle, for example, that average incomes for a province could change whilst the average incomes of cigarette smokers were unchanged.

(c) Next, our exercise investigates only correlation, not causality. We are not able, for example, to distinguish between it being more correct to say (i) that rising income leads to greater cigarette consumption; or (ii) that lower cigarette consumption leads to lower income (e.g. through general Keynesian-type macroeconomic mechanisms whereby reduced consumption of goods leads to a general reduction in income levels). Similarly, we are not able, with this statistical exercise, to say if increased price (whether as a result of a tax increase or some other driver) leads to fewer people being cigarette smokers or whether fewer people being cigarette smokers leads to prices rising (e.g. because the remaining smokers have lower price elasticity of demand). In the same way, our exercise can only correlate a display ban with reduced, increased, or unchanged levels of prevalence and consumption. The exercise will not tell us whether the ban is the *cause* of these changes or non-changes.

(d) A careful visual inspection of Figure 2.9 above denotes a sharp increase in smoking prevalence in British Columbia amongst those aged 15-19 from 2007 to 2008. We observe, for example, that CTUMS has raised a question on the reliability of the British Columbia data. This sharp increase might raise the suspicion that our results concerning the impact of the display ban on smoking prevalence for the 15-19 age group is in fact driven by the presence of unreliable British Columbia data. However, we have replicated the model excluding British Columbia from the sample, and our key results remain unchanged.

(e) In addition to display bans, other regulatory developments between 1999 and 2008 — at a federal and a provincial level — might also be considered potentially relevant factors affecting smoking behaviour. However, first, the time span of the dataset is too short to assess statistically the potential impact of federal regulatory intervention using the option of the more powerful statistical test described above.[16] Since these interventions are introduced in all Canadian provinces simultaneously, their impact can be analysed only by exploiting the time-series dimension of the panel which consists of only 10 data points. Unlike the display bans which are enforced on a province by province basis, no form of panel analysis is possible for a federal measure. Secondly, the main advantage of the 'first difference fixed effects model' lies exactly in its ability to control for the potential effect of factors that are not explicitly included in the model. It is in principle possible to conduct a similar panel analysis for other types of provincial regulatory tobacco control measures introduced in the relevant period. However, if such regulation at a provincial level had a material impact on smoking prevalence and cigarette consumption in 1999 to 2008, our model would control for this. Although in principle the omission of other regulatory developments variables (as with any omitted variables) may be of significance, the dataset available do not allow us to make useful statistical tests of their importance or otherwise.

2.50 Nonetheless, we consider the exercise we have been able to conduct to be methodologically robust given the data available.

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[16] In principle, the impact of federal regulatory interventions could be analysed by performing a 'Chow' test on the monthly data series for all ten provinces. However, after a careful inspection of the monthly data series from the same sources discussed in the 2008 Report and for the period from January 2002 (the earliest point at which we have data from such sources), we have concluded that these are too volatile to make the outcome of a Chow test meaningful. Put simply, an analysis of the impact of federal regulatory intervention would require a consideration of the monthly data across all ten provinces of Canada (we do not have such data for the three Canadian territories of Northwest Territories, Yukon and Nunavut and - as noted at paragraph 2.11 above - the CTUMS annual dataset does not include them). Whilst the monthly data considered in the 2008 Report for Saskatchewan, British Columbia and Manitoba for 1999 to 2007 (see paragraphs 6.12 - 6.14 and Figures 6.7 - 6.9 of the 2008 Report) was of a type enabling Chow tests to be conducted, the monthly data for a number of other provinces for the period from January 2002 to August 2009 is too volatile to make the use of a Chow test across all ten provinces meaningful. An example of this volatility can be seen in Figure 5.3 in Appendix 2.

3 ICELANDIC DATA ON PREVALENCE AND AVERAGE NUMBER OF CIGARETTES SMOKED

3.1 In the 2008 Report, commenting on the Statistics Iceland data to 2007, we stated (paragraph 6.2 in the 2008 Report):

> "Smoking prevalence amongst the general population in Iceland...more clearly displays a downward trend over time than amongst those aged 15 to 19 years. Upon visual inspection the display ban would not seem to have caused a structural break in either of these series. Thus, the policy would seem not to have impacted upon these prevalence trends."

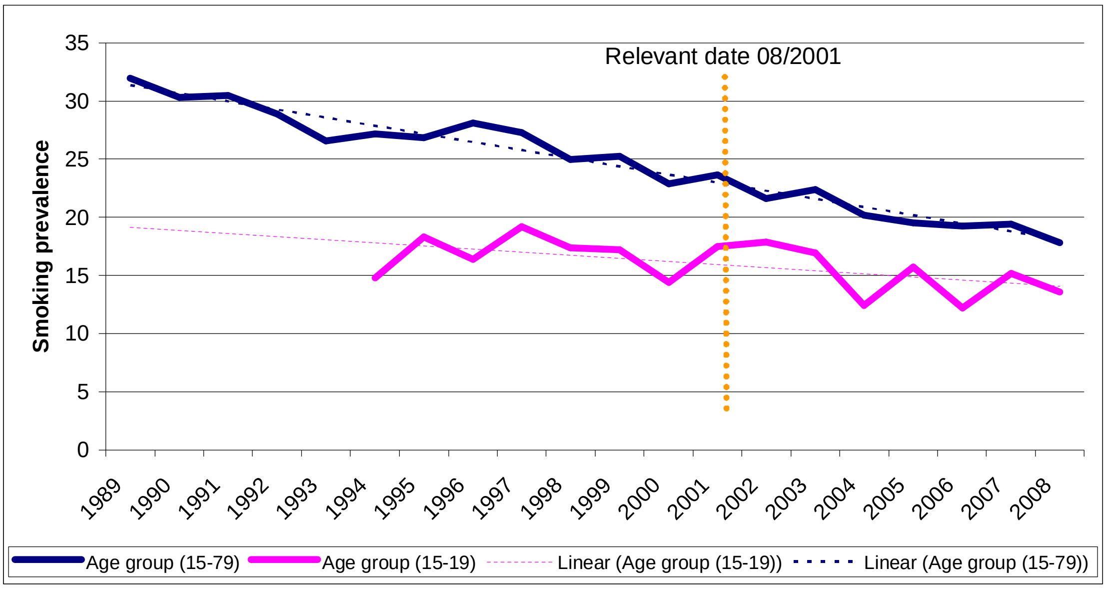
3.2 Commenting on the FTC Document's use of ESPAD data, we stated (paragraph 6.5 in the 2008 Report):

> "we do not find the FTC Document's reference to the reported fall in the number of 16-17 year olds who have ever smoked cigarettes from 61 percent in 1995 to 46 percent in 2003 to be conclusive. In neither of these cases is there consideration of whether these changes might be reflective of a pre-existing trend from prior to the display ban. Furthermore, we can see from the Statistics Iceland data that the series for 15-19 year olds is subject to considerable volatility."

Recent Data

3.3 At the time of the 2008 Report, we performed a visual assessment of Statistics Iceland data (no more elaborate statistical tests were possible). On 21st September 2009, Statistics Iceland data became available for 2008. As can be seen in Figure 3.1 below, the 2008 data provides no visual support to suggest that the display ban which took effect from August 2001 has had an effect on existing trends.

Figure 3.1: Percentage of people that are daily smokers in Iceland



Source: Statistics Iceland based on surveys undertaken by the Public Health Institute of Iceland

3.4 The quality of the Statistics Iceland data has been challenged. It is suggested that the ESPAD survey results for Iceland, discussed in paragraph 6.5 of the 2008 Report, are likely to be more accurate.

3.5 We agree that the ESPAD figures are likely to provide a more accurate snapshot estimate of Icelandic prevalence; however, given that there are only four data points available (1995, 1999, 2003 and 2007) the quality of the snapshot estimate comes at the expense of it being completely useless to a statistician as a basis for assessing the effects of the Icelandic display ban. Two data points either side of the ban are utterly inadequate to provide a basis for any serious-minded statistical assessment.

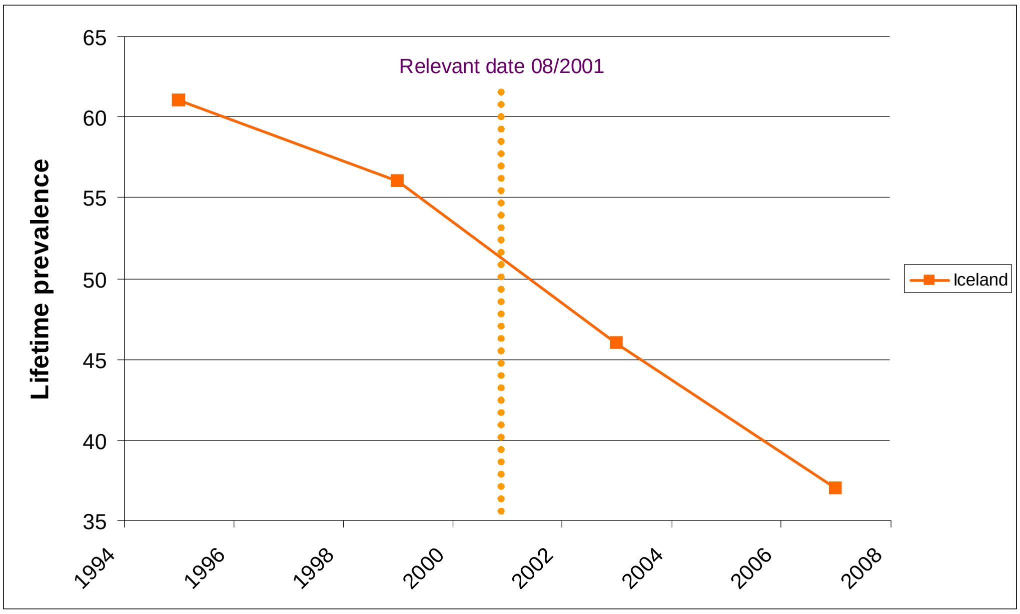
3.6 In contrast, although the Statistics Iceland dataset may give an inferior snapshot estimate, its greater frequency allows us to make a much more meaningful assessment. Indeed, if the nature of the biases in the Statistics Iceland figures are relatively constant, then the dataset might provide a statistically robust basis for assessing how prevalence is changing. However, this point is probably not worth dwelling upon excessively, since even the Statistics Iceland dataset is not adequate at this stage for us to perform proper statistical tests.

The ESPAD Data

3.7 Since an additional data point (2007) is now available from ESPAD to add to the three datapoints that the FTC Document appeared to regard as a statistically significant sample, we thought it would be worth amplifying upon the point of the statistical inadequacy of the ESPAD sample for our purpose.

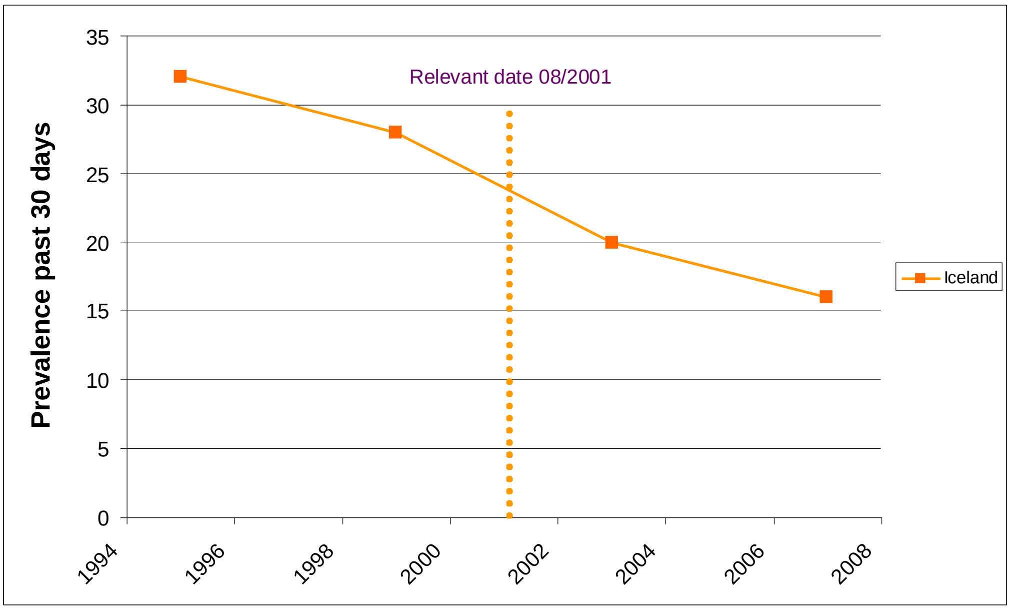
3.8 In Figures 3.2 and 3.3 below we report the latest ESPAD data for Iceland for students with a mean age of approximately 16.

Figure 3.2: Lifetime prevalence in Iceland



Source: ESPAD 1993-2007

Figure 3.3: Prevalence in the past 30 days



Source: ESPAD 1993-2007

3.9 Our suspicion is that certain readers, unacquainted with statistics, might be tempted to engage in the following forms of inference:

(a) Studying Figure 3.2, someone might say that the line from 1995 to 1999 has a negative slope and the steeper sloped from 1999 to 2007 indicates that the display ban has accelerated the rate of decline in prevalence.

(b) Studying Figure 3.3, someone might say that the lines from 1995 to 1999 and 2003 to 2007 have similar slopes, whilst the line from 1999 to 2003 is steeper, indicating that the display ban was the driver of a fall in recent prevalence.

3.10 Such inferences would be extremely naïve. However, in light of the fact that the FTC Document sought to rely upon inferences from three datapoints, we feel it might be useful to unpack a little further what we mean by dismissing the statistical relevance of such claims.

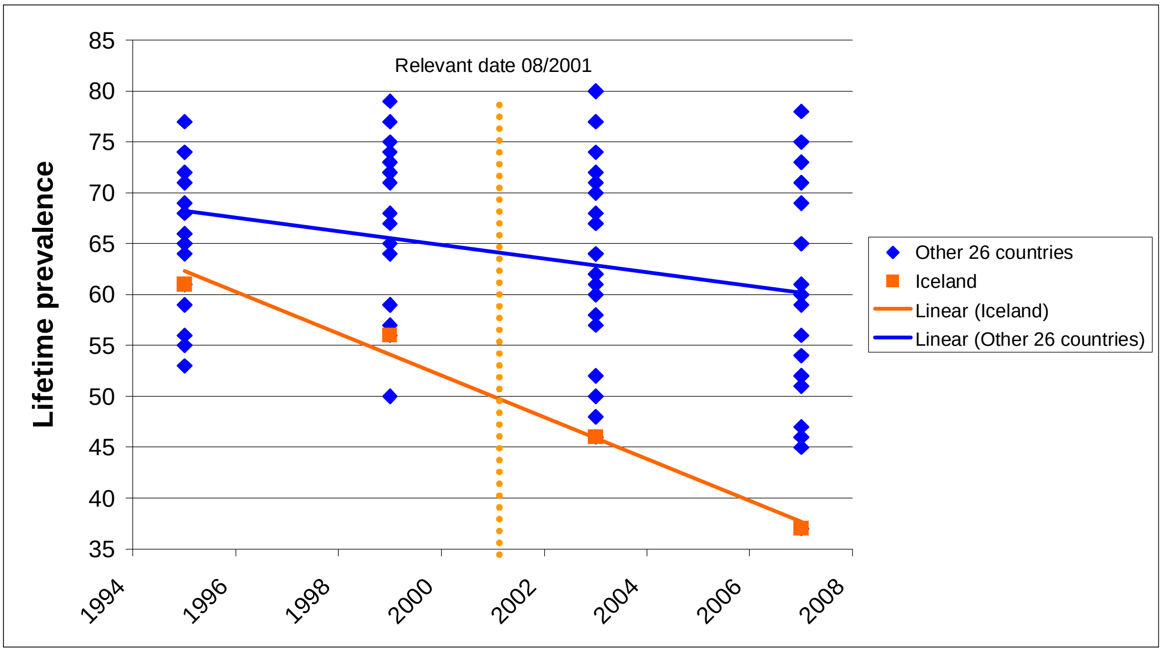
3.11 In Figures 3.4 and 3.5 below we present exactly the same ESPAD data for Iceland as in Figures 3.2 and 3.3 above. The difference is that, instead of simply drawing lines between each of the points for individual years, we have drawn best-fit lines through the series. In that way, insofar as the results for individual years deviate from the best-fit lines they can be regarded as involving either measurement errors or year-to-year random fluctuations.

3.12 A natural question is how legitimate it is to draw such best-fit lines. The Icelandic data points do not lie on a simple line. Perhaps some of them lie far enough away from a trend line to beg explanation in some other way than via a simple linear trend.

3.13 To investigate this issue, we have looked at the results for other countries in the ESPAD dataset, so as to see what is the average year-to-year deviation from trends for other countries that do not have display bans. ESPAD covers 35 countries. However, a few of these, such as Armenia, may not be sufficiently comparable in nature to Iceland to be of relevance. We therefore included the 26 other countries that are members of the European Economic Area.[17]

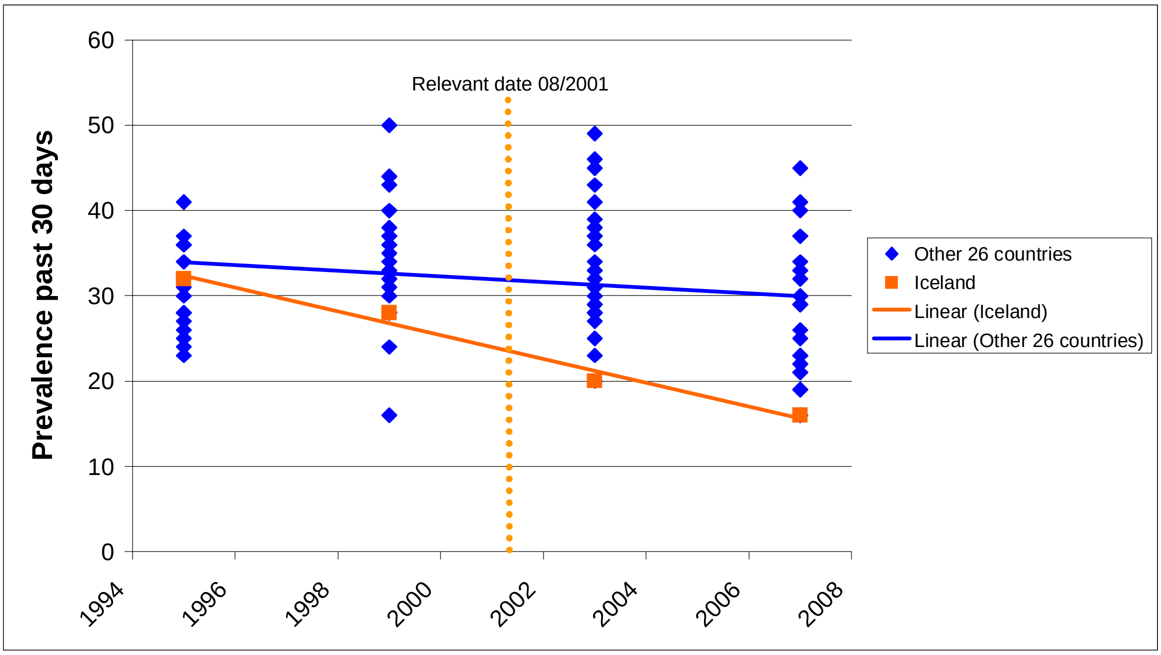
[17] In addition to the 24 EU Member States which firm part the of ESPAD survey (it excludes Latvia, Luxembourg and Spain), we have included Norway and Switzerland.

Figure 3.4: Lifetime prevalence in 27 countries from the ESPAD dataset



Source: ESPAD 1993-2007

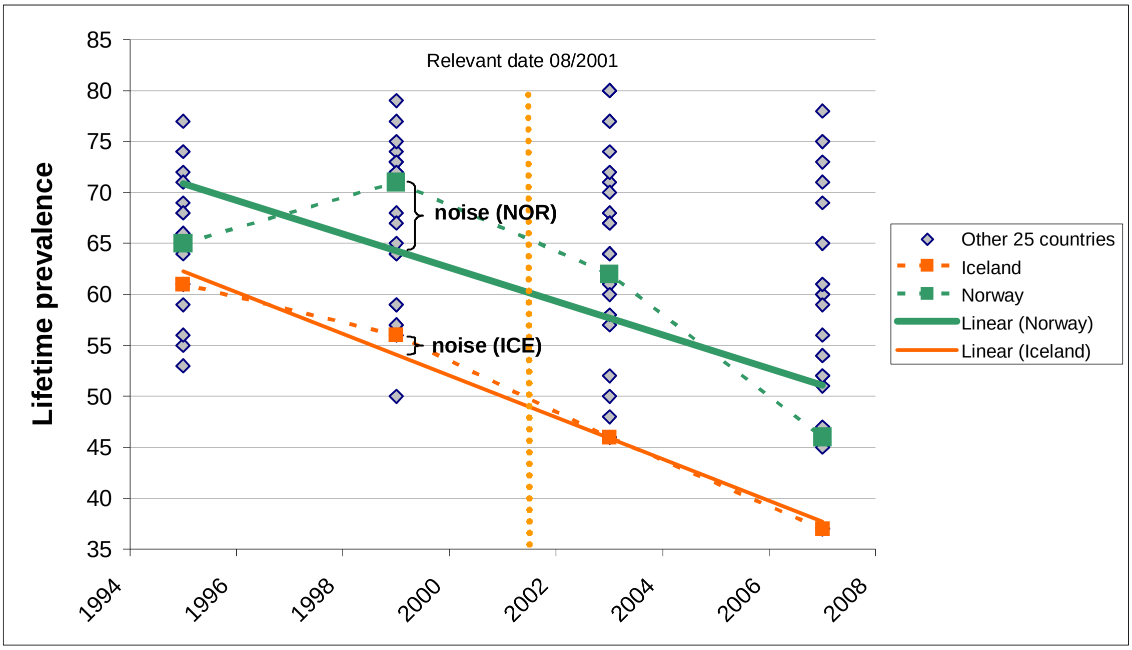
Figure 3.5: Last 30 days prevalence in 27 countries from the ESPAD dataset



Source: ESPAD 1993-2007

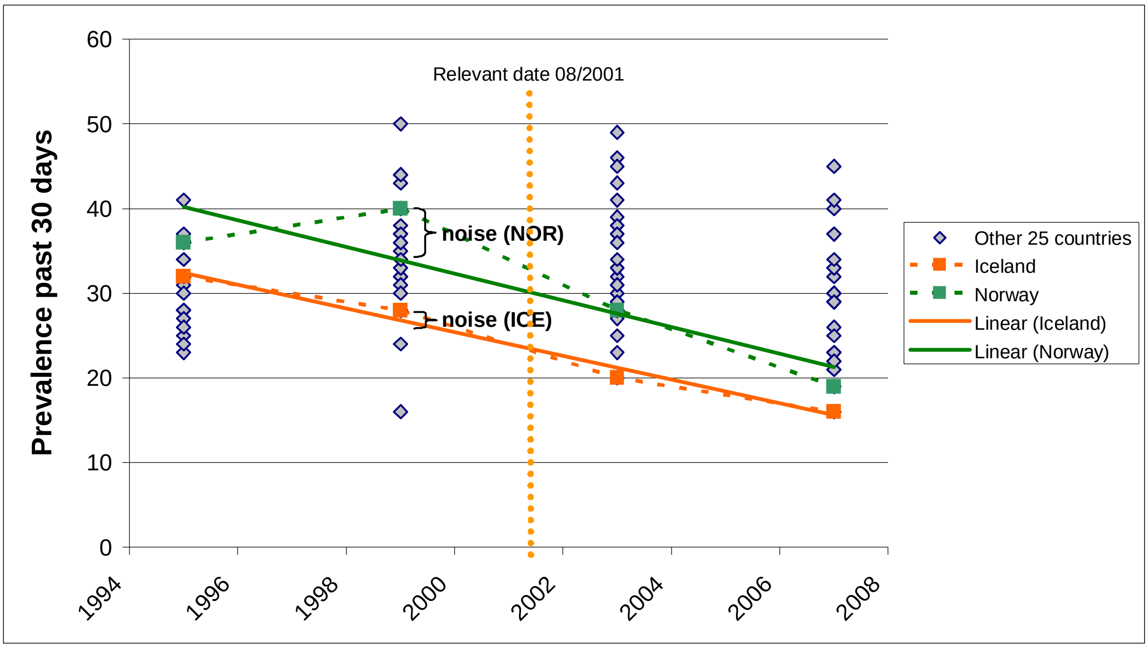
3.14 In Figure 3.6 and Figure 3.7 below, we see that for the case of Norway, year-to-year variation around the trend is much greater than that in Iceland when looking at lifetime prevalence. However, Norway might be a statistically unusual case. We therefore calculated, for all 27 countries, the percentage year-to-year variation from the trend. From these percentage variations for all 27 countries, we calculated the average and the standard deviation. This allows us to consider, for the Icelandic series, whether year-to-year variations from trend are more or less than the average for the ESPAD sample as a whole. Of course, for any sample, some members must be greater than average and some less, so year-to-year variations greater than average would not, in themselves, imply any need for an explanation other than random fluctuation. But 95 per cent of year-to-year fluctuations from trend will be less than 1.96 standard deviations. A normal statistical threshold for there being a need to explain a deviation from an average as something other than mere random fluctuation is the 95 per cent threshold. We call the range within which 95 per cent of cases fall the "statistical confidence interval".

Figure 3.6: Iceland versus other ESPAD countries (1)



Source: ESPAD 1993-2007

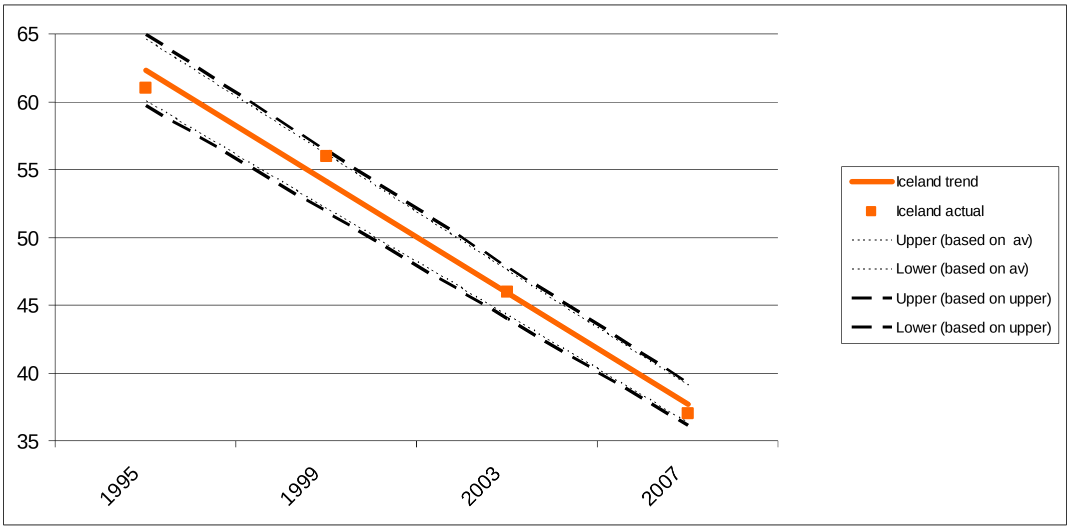
Figure 3.7: Iceland versus other ESPAD countries (2)



Source: ESPAD 1993-2007

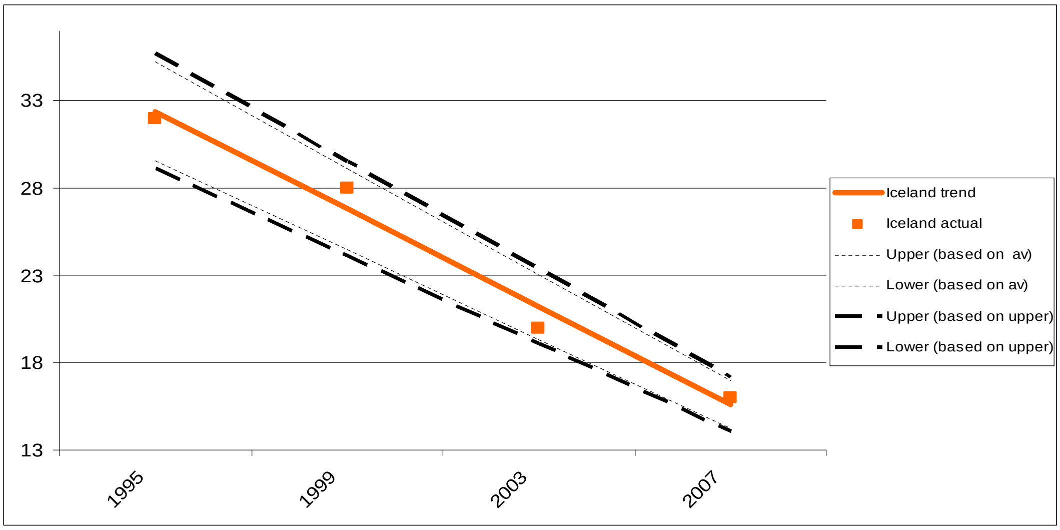
3.15 The results of applying our statistical confidence interval to Iceland's ESPAD series can be seen in Figure 3.8 and Figure 3.9 below. The lighter dashed lines apply to the Icelandic trend the average percentage deviation from trend, whilst the heavier dashed lines give the statistical confidence interval. If the Icelandic data contained points that lay outside the darker dashed lines, that would be an argument that they were not well explained in terms of a simple linear trend.

Figure 3.8: Statistical uncertainty around a notional linear trend in lifetime prevalence in Iceland



Source: ESPAD 1993-2007 and Europe Economics' calculations

Figure 3.9: Statistical uncertainty around a notional linear trend in past 30 day prevalence in Iceland



Source: ESPAD 1993-2007 and Europe Economics' calculations

3.16 As is obvious from the figures, all datapoints for Iceland lie not merely within the confidence interval, but actually within the average degree of variation. In other words, there is very little variation for Iceland from trend.

3.17 We do not go on to contend that this evidence suggests that the Icelandic data series *provides evidence* that Iceland's smoking prevalence (either over the whole of life or the past 30 days) is subject to a simple linear trend. Our contention is that nothing useful can be inferred from these four data points, and we continue to believe that the Statistics Iceland data series is more useful for this time series purpose (albeit not as robust in generating a cross-section snapshot estimate).

Conclusion

3.18 We remain of the same view that we put forward in the 2008 Report: there is no statistical basis for believing that the display ban has affected prevalence trends or consumption trends in Iceland. Further, there is no basis for believing that the display ban has affected current smoking prevalence amongst the general population or the 15-19 age group.

4 ICELANDIC AND THAI DATA ON COMPETITION AND INNOVATION

4.1 In the 2008 Report our key comments regarding innovation and competition were as follows:

> "an HHI of 1,000 (the threshold for a competitive market) would have been reached *[in Iceland]* in 2014 on the...prerestriction line, but takes until 2037 to reach the same value on the...post-restriction line." (paragraph 5.40)

> "The impact upon innovation of the display bans introduced recently in Iceland and Thailand is as yet unclear." (Finding 9)

> "A display ban would materially impair new innovation." (Finding 8(a))

> "Evidence from Iceland and Thailand suggests that…a display ban, as well as having negative competition effects itself, may also be bringing to realisation a number of the negative competition effects of other measures." (Finding 11)

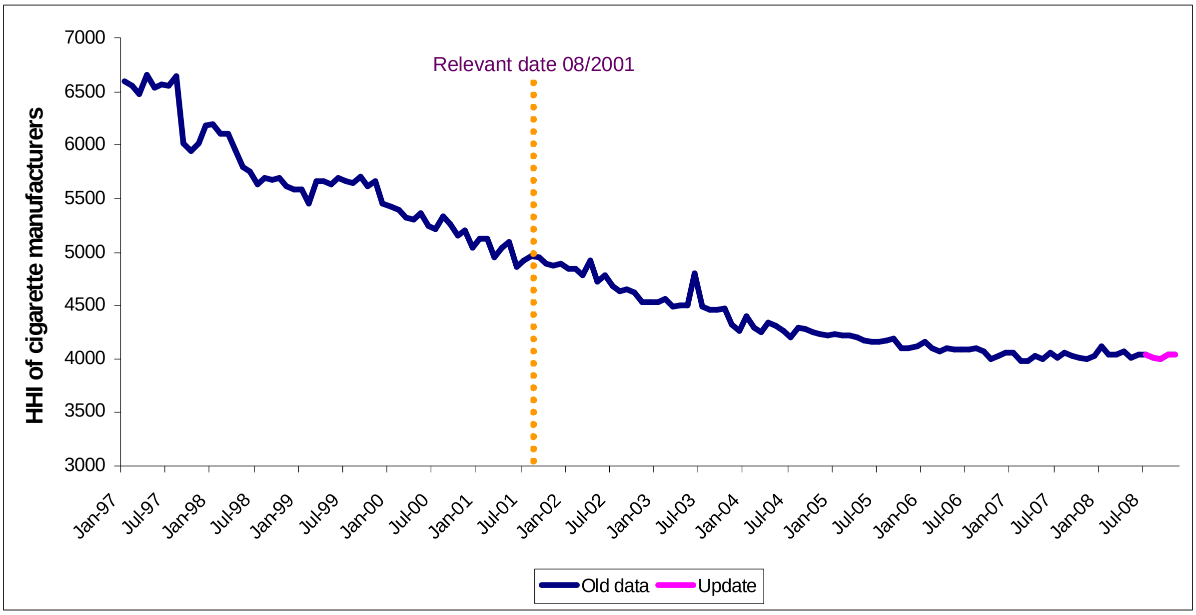
> "Data from Iceland [and] Thailand…suggest that, at least so far, display bans have had no measurable impact upon prevalence of smoking, either among the young or among the population as a whole." (Finding 13(a) and (b))

4.2 Data available since September 2008 has not changed our qualitative position in any of these regards.

Competition in Iceland

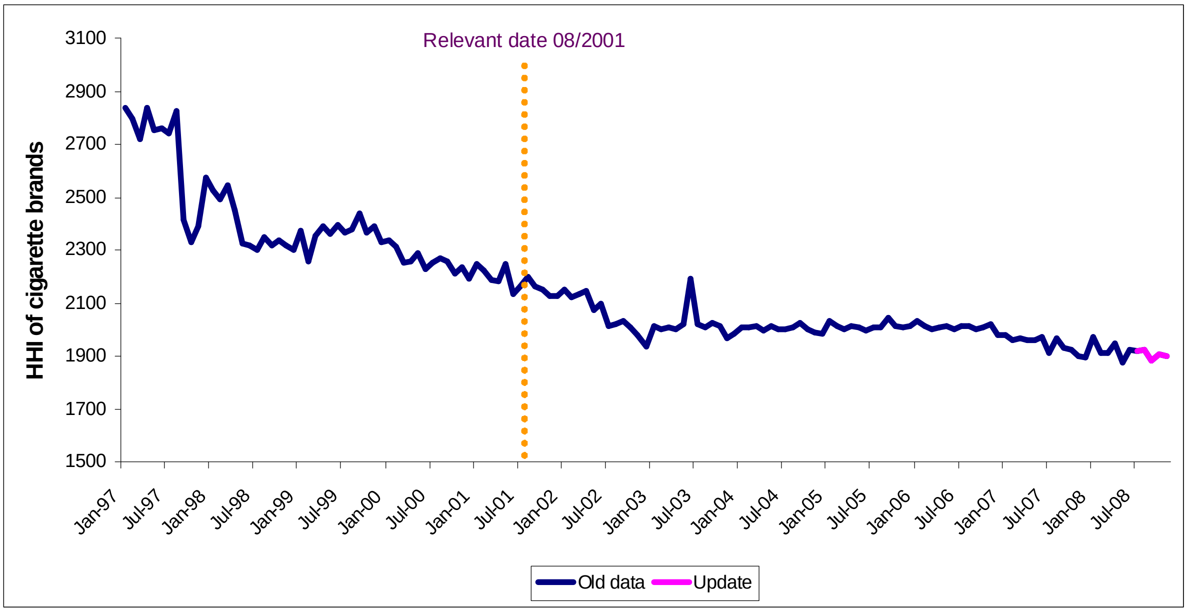
4.3 As in the 2008 Report, we report below the Herfindahl-Hirschman index ("HHI") for cigarette manufacturers, the brands of which are sold in Iceland, and for their brands.

Figure 4.1: HHI of cigarette manufacturers, the brands of which are sold in Iceland



Source: ÁTVR (State Alcohol and Tobacco Store) in Iceland

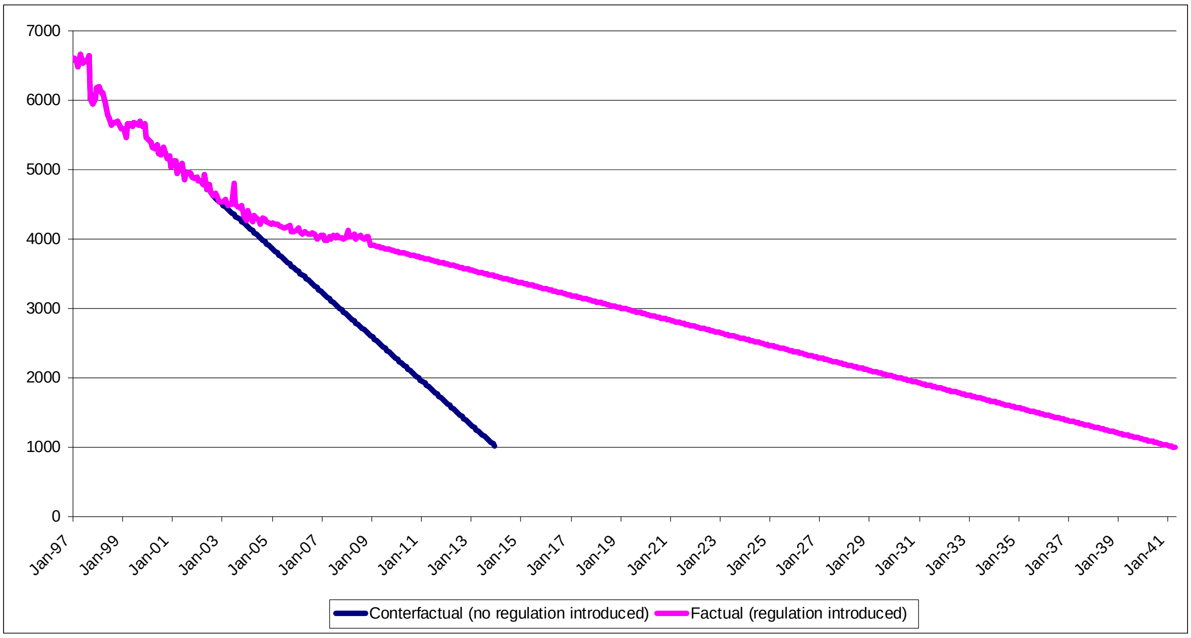
Figure 4.2: HHI of cigarette brands in Iceland



Source: ÁTVR (State Alcohol and Tobacco Store) in Iceland

4.4 As in the 2008 Report, we were able to conduct a Chow test on the series for manufacturers with brands on sale in Iceland. We were able to perform such a test again, incorporating the new data, with the result that the introduction of the display ban is now associated with an even greater slow-down in the rate at which the Icelandic market achieves the HHI threshold for competitiveness. It now takes 27 more years after the introduction of the display ban for the market to become HHI-competitive, over-and-above the trend that existed before the display ban. See Figure 4.3 below.

Figure 4.3: Simulation of the increase in competition in the absence of a display ban

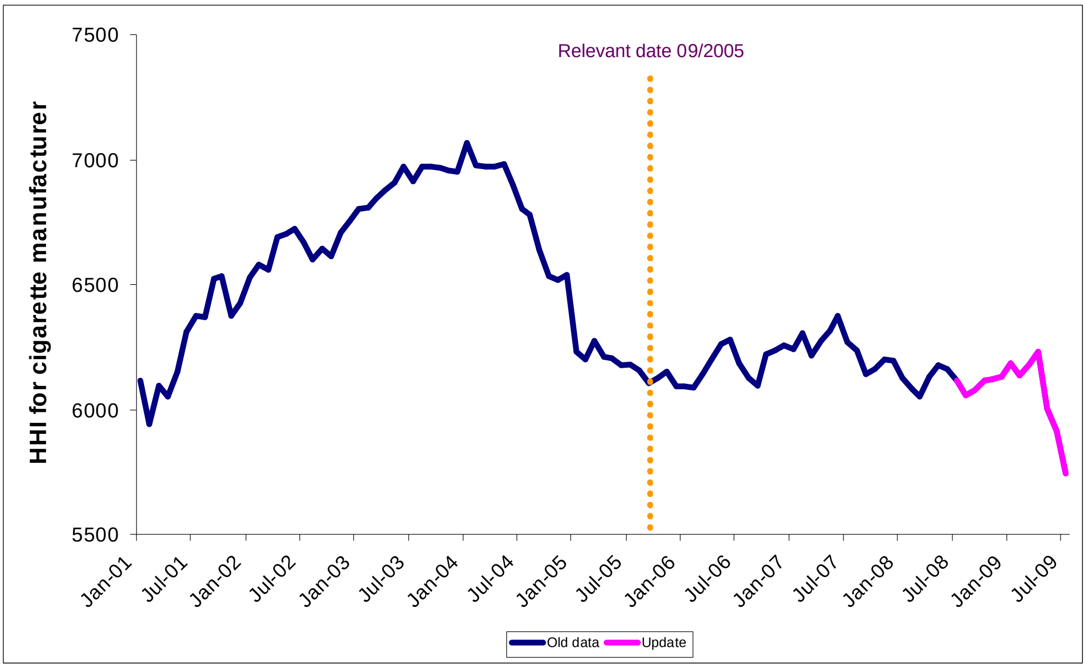


Source: Europe Economics' analysis of ÁTVR (State Alcohol and Tobacco Store) in Iceland

Competition in Thailand

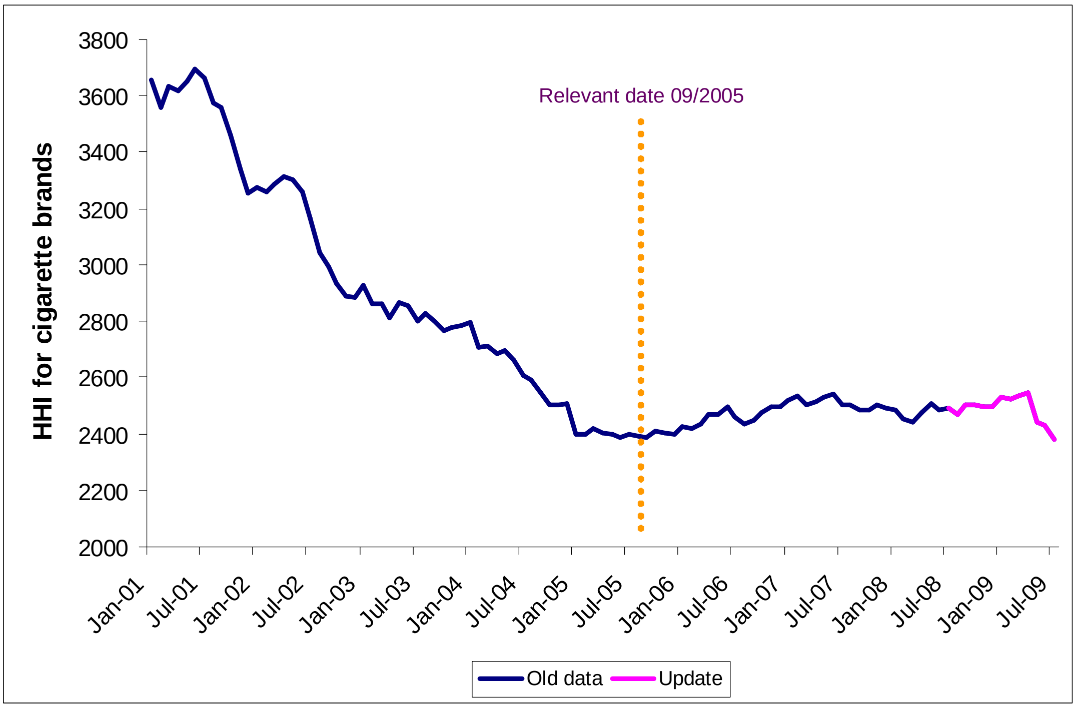
4.5 There is now an additional year's data from Thailand. However, the series still does not allow statistical testing, despite the visual suggestion that the introduction of the display ban is associated with a fall-off in the rate of increase in HHI-competitiveness.

Figure 4.4: HHI for cigarette manufacturers, the brands of which are sold in Thailand



Source: AC Nielsen

Figure 4.5: HHI for cigarette brands in Thailand

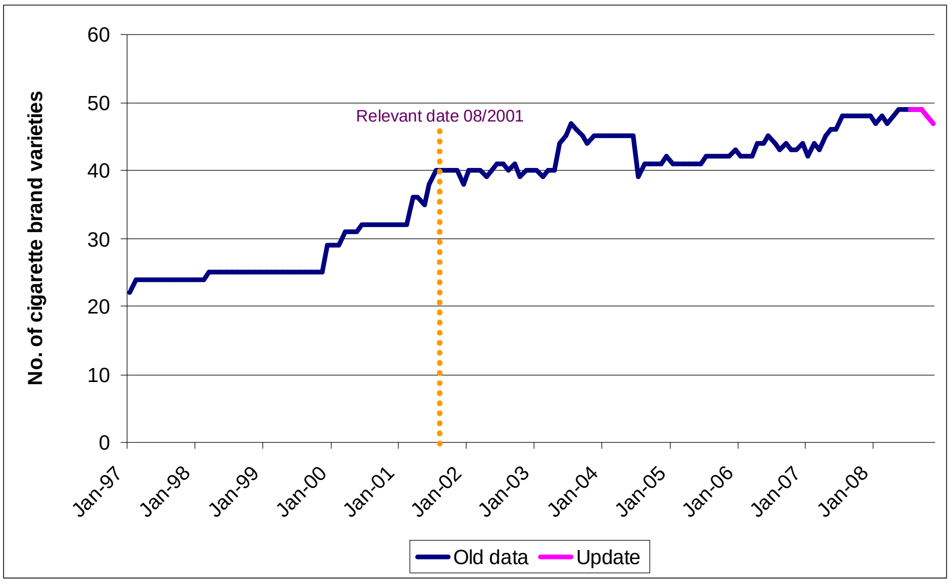


Source: AC Nielsen

Innovation

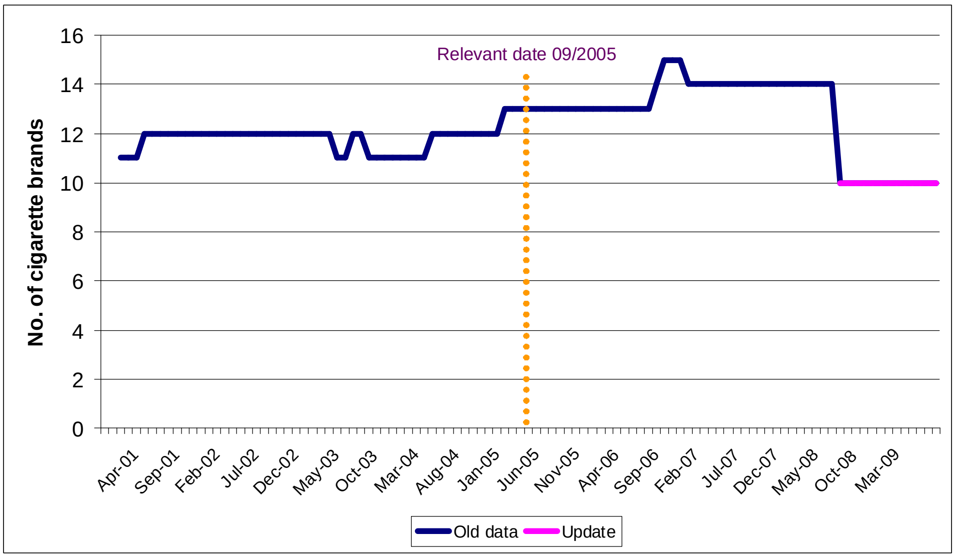
4.6 We remain unable to say anything specific about the data concerning innovation in Iceland or Thailand, and continue to rely upon the theoretical considerations discussed in detail in the 2008 Report. In Figure 4.6 and Figure 4.7 below, the number of brands in Iceland and Thailand is updated from the 2008 Report to include the latest data, but, as can be seen, provide no basis for changing our previous opinion.

Figure 4.6: Iceland



Source: ÁTVR (State Alcohol and Tobacco Store) in Iceland

Figure 4.7: Thailand



Source: AC Nielsen

5 CONCLUSION

5.1 The findings from the 2008 Report remain unchanged, in particular as regards:

(a) "The impact upon innovation of the display bans introduced recently in Iceland and Thailand is as yet unclear." (Finding 9)

(b) "A display ban would materially impair new innovation." (Finding 8(a))

(c) "Evidence from Iceland and Thailand suggests that…a display ban, as well as having negative competition effects itself, may also be bringing to realisation a number of the negative competition effects of other measures." (Finding 11)

(d) "Data from Iceland [and] Thailand…suggest that, at least so far, display bans have had no measurable impact upon prevalence of smoking, either among the young or among the population as a whole." (Finding 13(a) and (b))

5.2 The following finding from the 2008 Report is modified:

> "Data from...relevant Canadian provinces suggest that, at least so far, display bans have had no measurable impact upon prevalence of smoking, either among the young or among the population as a whole."

This modification is necessary as, having considered the recently released 2008 Canadian data using more detailed and powerful statistical tests than were available to us in September 2008 (given the limited data which existed at that point), we have found that, although the presence of the display ban has no statistical correlation with the extent of smoking prevalence for the general population in Canada, the display ban is strongly and materially correlated with increased prevalence amongst 15-19 year olds. This statistical correlation is discussed further at paragraphs 2.46 to 2.47 and 2.49(c) above and in Appendix 2 to this report.

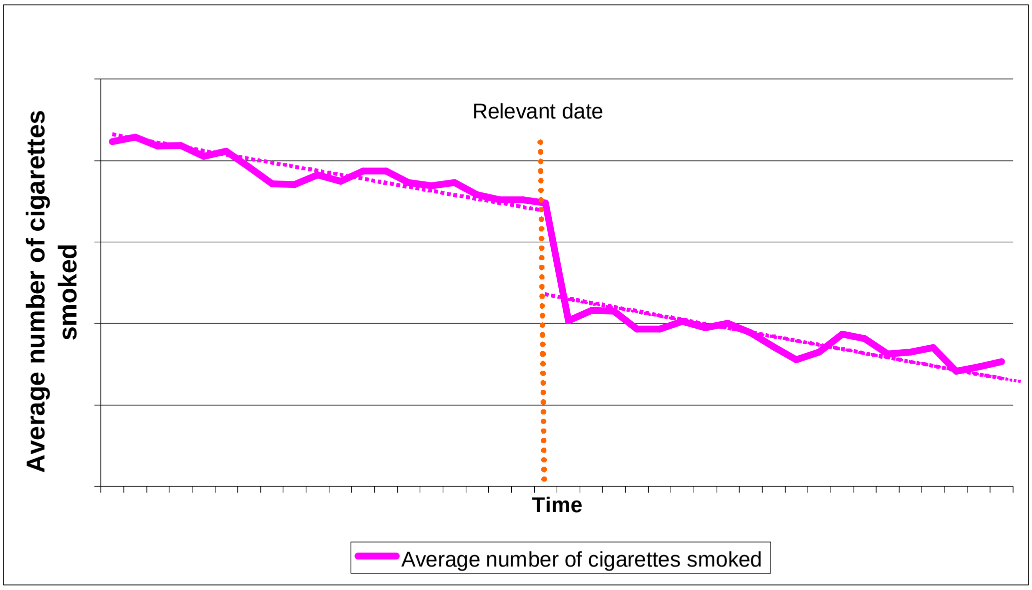
5.3 Overall, our conclusion remains that there is, as yet, no credible statistical evidence that the introduction of display bans has been associated with reduced smoking prevalence, and in particular, no evidence of such an effect in respect of those aged 15-19.

APPENDIX 1: DESCRIPTION OF THE GENERAL METHODOLOGY FOR THE STANDARD ECONOMIC FACTORS MODELS

A1.1 This section sets out the methodological rationale for having the first difference fixed effects model as our preferred choice. In the section we also discuss the characteristics of other models and, for completeness, we provide their results.

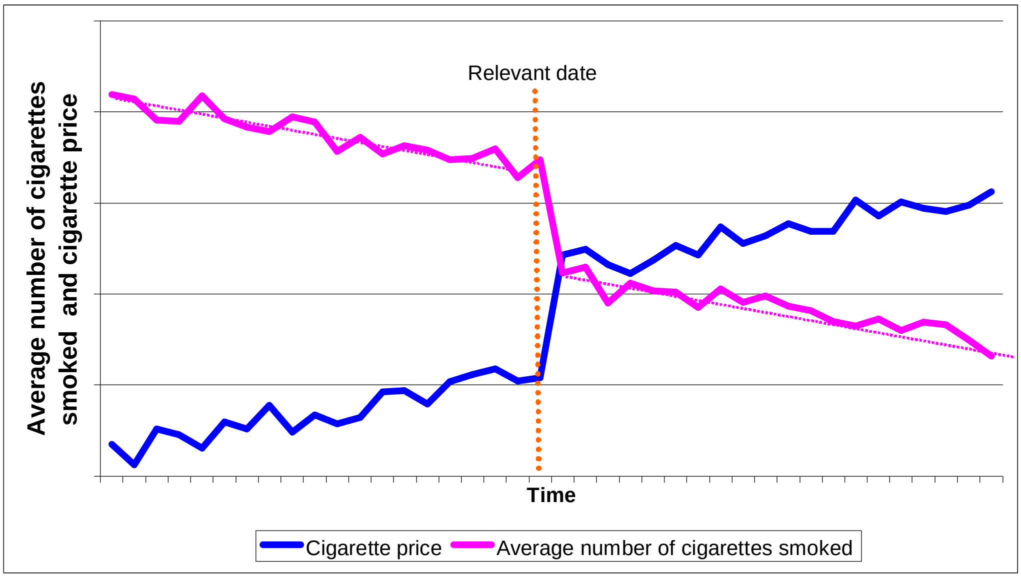
A1.2 Suppose that there were some country in the world in which the average number of cigarettes smoked and the relevant date of the enforcement of a display ban in that country were as depicted in Figure 5.1 below.

Figure 5.1: Example of a simple trend model for a fictional country



A1.3 Assume now that we also had information on the prices of cigarettes for the fictional country and that these were as depicted in Figure 5.2 below, where we assume that the spike upwards in the price of cigarettes is unrelated to the introduction of the display ban (e.g. it might be associated with a poor tobacco harvest and consequently increased costs for cigarette manufacturers).

Figure 5.2: Example of a simple economic factors model for a fictional country



A1.4 In such a model a simple linear test of the sort described in paragraph 2.25 above might find the introduction of the display ban to be relevant in accounting, statistically, for the average number of cigarettes smoked series. But that might merely be because prices rose by coincidence in the same year the display ban was introduced. In other words, by relying only on the information in Figure 5.1 above, we would be neglecting the existence of another factor (i.e. cigarettes prices) that might also have an impact on the average number of cigarettes smoked.

A1.5 Of course, it is not feasible (even in principle) to test every possible correlation that might exist (there is not data for every change that occurs in the universe, and even if there was it would take an infinite amount of time to test it all). But it is natural to consider the most obvious potentially significant economic factors, especially since data on these factors is available or straightforward to construct. A simple model including price and average incomes allows us to estimate the impact of the display ban net of the potential effect of these factors. In statistical terms we say that an economic model like the one we propose "controls" for the potential effect of cigarette prices and consumers' income.

A1.6 As for the linear model, a limitation of a model that accounted for smoking prevalence or average number of cigarettes smoked purely in terms of a trend, the presence or absence of the display ban, cigarette prices and average incomes would be that it fails to control for other unobservable factors — for which we have no data — that might also be statistically relevant in accounting for smoking behaviour. (In statistics this is known as the "omitted variable problem".) In the context of our model the potentially omitted variables can be split into two groups.

(a) Omitted variables that are specific to each province and that might have an effect upon the absolute levels of smoking prevalence and the average number of cigarettes consumed.

(b) Omitted variables that are specific to each province and that might be important not only in explaining the absolute levels of smoking prevalence and cigarettes consumption but also the rate at which these change in time.

A1.7 A standard statistical technique to alleviate problems arising from the omission of the first type of variables is to employ a so-called "fixed effects model". A fixed effects model controls for province-specific unobservable factors that affect the absolute levels of smoking prevalence and average number of cigarettes consumed, by assuming that each province has its own intrinsic level of smoking prevalence and cigarette consumption. So, under a fixed effects model, the implicit assumption is that there is one trend rate of change (in prevalence, in average number of cigarettes smoked) that is the same across all provinces, but that each province starts from its own particular level. We have performed fixed effects tests, as mentioned below.

A1.8 A limitation of the fixed effects model is that it would not address problems arising from the omission of variables that influence trends instead of absolute levels. So, if each province had its own individual trend, rather than the trend being common across all provinces, a fixed effects model would be inadequate. A standard technique to control for omitted variables producing province-specific trends is to use a "first difference" model with fixed effects. In a "first difference" model, instead of correlating the levels of the variables (smoking prevalence, average number of cigarettes smoked, price, the display ban, etc.) with one another, the model correlated changes in the variables of interest (prevalence, average number of cigarettes smoked) with changes in the other variables. This type of model assumes not only that each province has its own intrinsic level of smoking prevalence, but also (potentially) its own specific trend. To test the robustness of certain of the results of our fixed effects modelling, we constructed first difference models with fixed effects.

A1.9 The table below summarises the different models discussed. (We note that, in order to distinguish the more straightforward fixed effects model from the first difference model with fixed effects, we refer to the former hereafter as a "levels fixed effects model", though this is not a formal statistical term.)

Table 5.1: Description of Models

	Control for economic factors: - price - consumers' income	**Control for unobserved factors that affect levels of smoking prevalence and average number of cigarettes smoked**	**Control for unobserved factors that affect the rate at which smoking prevalence and average number of cigarettes smoked change**
Simple trend model	no	No	no
Simple model with economic factors	yes	No	no
Levels fixed effects model	yes	Yes	no
First difference fixed effects model	yes	Yes	Yes

A1.10 We rank these models in terms of how sophisticated they are, the simple trend model being the least complex, and the first difference fixed effects model being the most complex.

A1.11 It is important to understand that there is a correlation between the level of sophistication and the power of the statistical test. The first difference fixed effects model is the most "unforgiving" of those above, in that it is the most likely to reject the hypothesis that there is a statistically significant correlation between the variables considered. When a first difference fixed effects model identifies a correlation, we can be the most confident (amongst the models in the table) that there really is such a statistical correlation. On the other hand, when the first difference fixed effects model does not identify a correlation, we can be the least confident in asserting that no correlation exists — the data might simply be inadequate to stand up to the statistical scrutiny of the first difference fixed effects test.

A1.12 Because of this, it is important to note that the substance of our key results, in terms of when the display restriction is statistically correlated with a change in smoking prevalence or average number of cigarettes smoked, arises in both a levels fixed effects model and in the first difference fixed effects model. A comparison of the results obtained with the two models is provided in the tables below.[18]

[18] Note that the tables for the levels fixed effects model include also a time trend variable which is however not present in the first difference fixed effects model. The reason is that the time trend is automatically removed by the first difference transformation, and we refer to Appendix 2 for a formal mathematical derivation of this result.

Table 5.2: Results of the '*first difference fixed effects model'* concerning the impact of the display ban on average cigarette consumption (15-19 and 15+)

	(15-19)	(15+)
Price	no effect	(- -)
GDP per capita	no effect	no effect
Display ban	no effect	no effect
Display ban with delay	no effect	no effect
Introduction of the display ban	no effect	(- -)

(+ +), (- -) = *significance at 95%* + = *increase in consumption*

(+), (-) = *significance at 90%* - = *decrease in consumption*

Table 5.3: Results of the '*levels fixed effects model'* concerning the impact of the display ban on average cigarette consumption (15-19 and 15+)

	(15-19)	(15+)
Price	(- -)	(- -)
GDP per capita	no effect	no effect
Time trend	no effect	no effect
Display ban	(+ +)	(-)
Display ban with delay	(+ +)	no effect
Introduction of the display ban	no effect	(- -)

(+ +), (- -) = *significance at 95%* + = *increase in consumption*

(+), (-) = *significance at 90%* - = *decrease in consumption*

A1.13 As we can see, the levels fixed effects model suggests that the display ban is associated with an increase in the average number of cigarettes smoked for the 15-19 age group. However we should be cautious in trusting the statistical reliability of this result as it does not pass the scrutiny of the more demanding first difference fixed effects model specification.

Smoking prevalence

Table 5.4: Results of the '*first difference fixed effects model'* concerning the impact of the display ban on smoking prevalence (15-19 and 15+)

	(15-19)	(15+)
Price	no effect	(- -)
GDP per capita	no effect	no effect
Display ban	(+ +)	no effect
Display ban with delay	no effect	no effect
Introduction of the display ban	(+ +)	no effect

(+ +), (- -) = *significance at 95%* + = *increase in prevalence*

(+), (-) = *significance at 90%* - = *decrease in prevalence*

Table 5.5: Results of the '*levels fixed effects model'* concerning the impact of the display ban on smoking prevalence (15-19 and 15+)

	(15-19)	(15+)
Price	(- -)	(- -)
GDP per capita	no effect	(- -)
Time trend	(- -)	no effect
Display ban	(+ +)	no effect
Display ban with delay	(+ +)	no effect
Introduction of the display ban	no effect	no effect

(+ +), (- -) = *significance at 95%* + = *increase in consumption*

(+), (-) = *significance at 90%* - = *decrease in consumption*

A1.14 As expected, the levels fixed effects model identifies more correlation between variables than the first difference fixed effects model. It is worth noting that, while the enforcement of the display ban is significant in the first difference fixed effects model, it is not significant in the levels fixed effects model. In contrast, the presence of the display ban is statistically and strongly associated with an increase in smoking prevalence amongst those aged 15-19 in both model specifications.

A1.15 Finally, we acknowledge that (in addition to the fixed effects approach) there exists another approach, the "random effects" approach, which also deals with the problem of unobservable characteristics. The most common practice in panel studies drawing on large geographic areas, such as countries or states, is to use a fixed effects approach, and our preferred models are fixed effects models. However, we have compared the results of the levels fixed effects model with those of the levels random effects model and the key results coincide.

APPENDIX 2: ESTIMATION RESULTS

A2.1 This section sets out the econometric methodology adopted for the quantitative analysis of the impact of the display ban, and provides the estimation results.

A2.2 The section is divided into two sub-sections:

(a) Analysis of the impact of the display ban on smoking prevalence and smoking consumption; and

(b) Analysis of the impact of the display ban on the intensity of competition in the market.

Analysis of smoking prevalence and smoking consumption

Canada: the new panel data analysis

The dataset

A2.3 The econometric analysis is based on a panel with yearly data covering the period 1999—2008 and the following ten provinces (acronyms in brackets):

- *Alberta (ALB)*
- *British Columbia (BC)*
- *Manitoba (MAN)*
- *New Brunswick(NB)*
- *Newfoundland and Labrador (NFLD)*
- *Nova Scotia (NS)*
- *Ontario (ONT)*
- *Prince Edward Island (PEI)*
- *Québec (QUE)*
- *Saskatchewan (SASK)*

A2.4 The variables used in the model are the following:

(a) Dependent variables:

- *gprev* = smoking prevalence of the general population (15+ years old)
- *yprev* = smoking prevalence of the 15-19 year-old age group

- *gcons* = average number of cigarettes smoked among the general population (15+ years old)
- *ycons* = average number of cigarettes smoked among the general age group (15+ years old)

(b) Explanatory variables:

- *price* = price of cigarettes
- *GDPc* = GDP per capita
- *ban* = a dummy variable which takes value one if the display ban is in place and value zero otherwise
- *ban2* = a dummy variable which takes value one if the display ban is in place for at least one year, and vale zero otherwise
- *t* = is a linear trend

A2.5 All variables above are in the same panel format, i.e. we have yearly data for each province for all the variables.

General methodology

A2.6 In general, the impact of the display ban on smoking prevalence can be inferred in two ways:

(a) From a comparison of the provinces where the display ban is enforced with those where it is not enforced (cross-sectional dimension); and

(b) By a comparison of the smoking prevalence of a province before and after the enforcement of the display ban (time-series dimension).

A2.7 In order for either of these approaches to be effective one needs to have a sufficient number of data points, i.e., either many provinces with the ban in place and many provinces without the display ban in place (for the cross-sectional approach), or data for many years before and after the display ban was enforced (for the time series-approach).

A2.8 In contrast, a panel data approach allows us to make systematic use of the two dimensions and to increase significantly the sample size. Given we have data for ten provinces and for ten years, a panel approach results in a sample size of 100 observations as opposed to the sample size of 10 that we would have if we adopted a simple cross-section or time-series approach.

The Simple Linear Trend Model

A2.9 The first exercise we have conducted consists of regressing (through a standard OLS) smoking prevalence and cigarette consumption of each province on a linear trend and the presence of the ban. The results, for the two age groups (15-19 and 15+), are provided below.

Table 5.6: Smoking prevalence (15+) accounted for by a trend and the display ban

Dependent Variable: GENPREV(i,t)
Method: Pooled Least Squares
Sample: 1999 2008
Included observations: 10
Number of cross-sections used: 10
Total panel (balanced) observations: 100
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
T	-0.846890	0.056846	-14.89790	0.0000
BAN(i,t)	1.193127	0.467855	2.550209	0.0125
Fixed Effects				
_NFLD--C	27.31100			
_PEI--C	25.35307			
_NS--C	26.97238			
_NB--C	27.11100			
_QUE--C	27.49169			
_ONT--C	22.99169			
_MAN--C	25.43375			
_SASK--C	26.93375			
_ALB--C	25.69169			
_BC--C	20.09169			

Table 5.7: Smoking prevalence (15-19) accounted for by a trend and the display ban

Dependent Variable: YOUNGPREV(i,t)
Method: Pooled Least Squares
Sample: 1999 2008
Included observations: 10
Number of cross-sections used: 10
Total panel (balanced) observations: 100
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
T	-1.553110	0.096662	-16.06736	0.0000
BAN(i,t)	2.344063	0.794879	2.948955	0.0041
Fixed Effects				
_NFLD--C	28.18900			
_PEI--C	24.18578			
_NS--C	26.02018			
_NB--C	27.48900			
_QUE--C	31.95459			
_ONT--C	24.05459			
_MAN--C	28.35137			
_SASK--C	31.25137			
_ALB--C	26.45459			
_BC--C	21.45459			

Table 5.8: Cigarette consumption (15+) accounted for by a trend and the display ban

Dependent Variable: GENCONS**(i,t)**
Method: Pooled Least Squares
Sample: 1999 2008
Included observations: 10
Number of cross-sections used: 10
Total panel (balanced) observations: 100
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
T	-0.226986	0.029661	-7.652552	0.0000
BAN(i,t)	-0.155154	0.216194	-0.717662	0.4749
Fixed Effects				
_NFLD--C	16.60144			
_PEI--C	17.47798			
_NS--C	17.33247			
_NB--C	17.90144			
_QUE--C	17.69695			
_ONT--C	16.81695			
_MAN--C	15.94350			
_SASK--C	16.16350			
_ALB--C	16.57695			
_BC--C	16.21695			

Table 5.9: Cigarette consumption (15-19) accounted for by a trend and the display ban

Dependent Variable: YOUNGCONS(i,t)
Method: Pooled Least Squares
Sample: 1999 2008
Included observations: 10
Number of cross-sections used: 10
Total panel (balanced) observations: 100
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
T	-0.133589	0.045757	-2.919530	0.0045
BAN(i,t)	0.565463	0.361871	1.562608	0.1217
Fixed Effects				
_NFLD--C	13.79115			
_PEI--C	14.00151			
_NS--C	13.56806			
_NB--C	14.65115			
_QUE--C	12.80460			
_ONT--C	13.06460			
_MAN--C	11.59496			
_SASK--C	11.77496			
_ALB--C	12.65460			
_BC--C	12.14460			

A2.10 The results above would suggest the following:

(a) For both age groups, there exists a clear declining trend in both smoking prevalence and cigarette consumption.

(b) The decline in smoking prevalence seems more marked for the 15-19 age group as opposed to the 15+ age group. The reverse is true for the decline in cigarette consumption which is more marked for the general population as opposed to the 15-19 age group.

(c) For both age groups the display ban seems to be positively correlated with smoking prevalence, while it is not correlated with cigarette consumption.

A2.11 It is important to stress that the regressions provided above are a very naïve and unsophisticated approach and do not allow to draw any statistically robust conclusion concerning the potential impact of the display ban. They simply provide a confirmation of the existence of a declining trend for both smoking prevalence and cigarette consumption, and confirm that the introduction of the display ban did not result in a decrease in the level of smoking prevalence. This could be spotted by a simple visual inspection of the graphs. In contrast, if an indicative suggestion must be drawn, this would be that the display ban is associated with an increase in smoking prevalence for both age groups considered.

A2.12 In order to be able to draw a more definite conclusion concerning the impact of the display ban, we have constructed a more sophisticated model which aims to explain smoking prevalence and cigarette consumption by a richer set of variables. The main idea behind this approach is to control for factors other than the display ban that could be important in explaining smoking behaviour. As a result of this, we should be able to isolate the impact of the display ban from that of other variables.

The standard economic factors model

A2.13 In general, the model that we propose aims to explain smoking prevalence and cigarette consumption by the following variables:

(a) *Cigarette prices*: this would control for substitution effects, i.e. the idea that demand for cigarettes (both in terms of individual smoking and average number of cigarettes smoked) is sensitive to a change in the price of cigarettes;

(b) *GDP per capita*: this would control for income effect, and reflects the idea that demand for cigarettes might be dependent upon individual wealth;

(c) *A time trend*: this would control for consumers' evolving preference for cigarettes;

(d) *Display ban*: this would allow to estimate the effect of the display ban in isolation from the effects of the variables listed above; and

(e) *Fixed effects:* fixed effects are constant coefficients that allow controlling for other province-specific characteristics that we do not observe in the data but could play an important role in explaining absolute levels smoking prevalence and cigarette consumption.

A2.14 In general, the model that we proposed has the following mathematical formulation (reported below in relation to smoking prevalence among the 15-19 age group):

(M1) $yprev_{i,t} = \alpha_i + \beta_1 price_{i,t} + \beta_2 gdpc_{i,t} + \beta_3 t + \beta_4 ban_{i,t} + \varepsilon_{i,t}$ [**Levels Fixed Effects**] where $i = \{ALB,...,SASK\}$ indicates the province, and $t = \{1999,...,2008\}$ indicates the year, and $\varepsilon_{i,t}$ is the error term.

A2.15 The direct estimation of the model (M1) — *which is the first difference fixed effects model* — above poses a series of econometric issues. In particular, since all variables are expressed at their absolute level, there is a risk that the model relies upon spurious correlations. A standard way to solve this problem is to take a *first difference* transformation. This consists of expressing the models as follows:

(M2) $$dyprev_{i,t} = \beta_1 dprice_{i,t} + \beta_2 dgdpc_{i,t} + \beta_3 + \beta_4 dban_{i,t} + \eta_{i,t}$$

where $dyprev_{i,t} = (yprev_{i,t} - dyprev_{i,t-1})$ is the prevalence in year t minus the prevalence in year $t-1$, $dprice_{i,t} = (price_{i,t} - price_{i,t-1})$ the price in year t minus the price in year $t-1$, and so on. The model explains to what extent *changes* in prevalence (as opposed to *absolute levels* of prevalence) can be accounted for by changes in prices, changes in GDP per capita, etc.

A2.16 The model (M2) above has been further modified to take the following form:

(M3) $dyprev_{i,t} = \gamma_{i,t} + \beta_1 dprice_{i,t} + \beta_2 dgdpc_{i,t} + \beta_4 ban_{i,t} + \eta_{i,t}$ [**1ST Diff Fixed Effects**]

A2.17 The differences to model (M2) are the following. First, the constant intercept β_3 in (M2) has been replaced by the fixed effect $\gamma_{i,t}$ in (M3). The introduction of a fixed effect in the first difference model allows us to control for the existence of unobservable characteristics which are specific to each province and that might affect the rate at which smoking preface changes over time. In other words, the fixed effect $\gamma_{i,t}$ allows us to account for the possibility that province-specific factors exist that play a potential role in accelerating or slowing down the decline in smoking prevalence. Second, in (M3) we have replaced $dban_{i,t}$ with $ban_{i,t}$. The reason is that we are interested in studying the impact of the *presence of the ban*, whereas the $dban_{i,t}$ implies that we would be studying the impact of the *introduction of the ban*. However, for completeness, we have estimated the model including both $ban_{i,t}$ and $dban_{i,t}$.

Results for the first difference fixed effects model

A2.18 The tables below provide the model estimates of the average number of cigarettes smoked for both age groups considered.

Table 5.10: Change in average number of cigarettes smoked (15+)

Dependent Variable: DGCONS(i,t)

Method: GLS (Cross Section Weights)
Sample: 2000 2008

Included observations: 9				
Number of cross-sections used: 10				
Total panel (balanced) observations: 90				
Convergence achieved after 6 iteration(s)				
White Heteroskedasticity-Consistent Standard Errors & Covariance				
Variable	Coefficient	Std. Error	t-Statistic	Prob.
DPRICE (i,t)	-0.028480	0.014369	-1.982056	0.0506
DGDPC(i,t)	-3.28E-05	0.000107	-0.306463	0.7600
BAN (i,t)	-0.249175	0.177799	-1.401448	0.1646
Weighted Statistics				
R-squared	0.002262	Mean dependent var		-0.271862
Adjusted R-squared	-0.020674	S.D. dependent var		0.922857
S.E. of regression	0.932348	Sum squared resid		75.62672
F-statistic	0.098640	Durbin-Watson stat		2.766675
Prob(F-statistic)	0.906170			
Unweighted Statistics				
R-squared	0.002562	Mean dependent var		-0.256667
Adjusted R-squared	-0.020367	S.D. dependent var		0.922996
S.E. of regression	0.932348	Sum squared resid		75.62674
Durbin-Watson stat	2.791624			

Table 5.11: Change in average number of cigarettes smoked (15-19)

Dependent Variable: DYCONS(i,t)				
Method: GLS (Cross Section Weights)				
Sample: 2000 2008				
Included observations: 9				
Number of cross-sections used: 10				
Total panel (balanced) observations: 90				
Convergence achieved after 4 iteration(s)				
White Heteroskedasticity-Consistent Standard Errors & Covariance				
Variable	Coefficient	Std. Error	t-Statistic	Prob.
DPRICE(i,t)	-0.023032	0.015586	-1.477783	0.1431
DGDPC(i,t)	-2.80E-05	0.000114	-0.245726	0.8065
BAN(i,t)	0.333569	0.210327	1.585954	0.1164
Weighted Statistics				
R-squared	0.035056	Mean dependent var		-0.032820
Adjusted R-squared	0.012874	S.D. dependent var		1.400682
S.E. of regression	1.391637	Sum squared resid		168.4888
F-statistic	1.580354	Durbin-Watson stat		2.730209
Prob(F-statistic)	0.211755			
Unweighted Statistics				
R-squared	0.023431	Mean dependent var		-0.037778
Adjusted R-squared	0.000981	S.D. dependent var		1.392321
S.E. of regression	1.391637	Sum squared resid		168.4890
Durbin-Watson stat	2.972221			

A2.19 The results suggest that, among the variables analysed, the only one to have some effect on cigarette consumption is the price of cigarettes (significance just below 95 per cent). This is negatively correlated with the average number of cigarettes smoked, but only among the general population. None of the variables are significant in explaining cigarette consumption among the age group 15-19. The presence of the display ban had no impact on the average number of cigarettes consumed. We run the model replacing the variable *ban* with *ban2* (i.e. a ban with delay) and we obtained similar results.

A2.20 However, when we replaced *ban* with *dban* we found there the introduction of the display ban had a negative impact on cigarette consumption for the 15+ population (see table below)

Table 5.12: The impact of the *introduction* of the display ban on smoking prevalence (15+)

Dependent Variable: DGCONS(i,t)				
Method: GLS (Cross Section Weights)				
Date: 10/01/09 Time: 14:55				
Sample: 2000 2008				
Included observations: 9				
Number of cross-sections used: 10				
Total panel (balanced) observations: 90				
Convergence achieved after 5 iteration(s)				
White Heteroskedasticity-Consistent Standard Errors & Covariance				
Variable	Coefficient	Std. Error	t-Statistic	Prob.
DPRICE(i,t)	-0.026572	0.014756	-1.800777	0.0752
DGDPC(i,t)	-6.04E-05	0.000109	-0.553704	0.5812
DBAN(i,t)	-0.551901	0.188268	-2.931467	0.0043
Weighted Statistics				
R-squared	0.029267	Mean dependent var		-0.270954
Adjusted R-squared	0.006951	S.D. dependent var		0.921680
S.E. of regression	0.918471	Sum squared resid		73.39222
F-statistic	1.311497	Durbin-Watson stat		2.748455
Prob(F-statistic)	0.274689			
Unweighted Statistics				
R-squared	0.032032	Mean dependent var		-0.256667
Adjusted R-squared	0.009780	S.D. dependent var		0.922996
S.E. of regression	0.918471	Sum squared resid		73.39227
Durbin-Watson stat	2.802323			

A2.21 The estimation method used is a feasible Generalised Least Square ("GLS") because of the potential presence of cross-section heteroskedasticity. Since changes in average number of cigarettes smoked tend to be limited and very similar across provinces, neither the inclusion of fixed effects nor that of a common intercept were justified. It should be noted that the R^2 and the adjusted R-squared are extremely low. This is partially due to the fact that the model is in first differences (which generally leads to much lower goodness of fit compared to the models where the variables are expressed in absolute levels) and partially due to the fact that the variation in average number of cigarettes smoked is very low. In fact, we have estimated the model in which all variables are expressed at their absolute levels and we have obtained an extremely high goodness of fit (more than 0.95). However, for the reasons explained before, we prefer to rely only on the results of a first difference model, even if this comes at the cost of a poor goodness of fit. In essence, the very low R^2 can be interpreted as suggesting that very little of the changes in average number of cigarettes smoked is accounted for by changes in prices, changes in income per capita or the display ban.

A2.22 The following tables provide estimates of smoking prevalence among the two age groups considered. The use of a feasible GLS estimator was justified by the potential presence of cross-section heteroskedasticity. Also, the model specification includes fixed effects because yearly changes smoking prevalence tend to be different across provinces.

Table 5.13: Change in smoking prevalence (15+)

Dependent Variable: DGPREV(i,t)
Method: GLS (Cross Section Weights)
Sample: 2000 2008
Included observations: 9
Number of cross-sections used: 10
Total panel (balanced) observations: 90
Convergence achieved after 7 iteration(s)
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
DPRICE(i,t)	-0.117301	0.033324	-3.519973	0.0007
DGDPC(i,t)	-4.08E-05	0.000114	-0.357304	0.7218
BAN(i,t)	-0.106170	0.328222	-0.323471	0.7472
Fixed Effects				
_ALB--C	-0.004022			
_BC--C	-0.047625			
_MAN--C	0.426124			
_NB--C	-0.109710			
_NFLD--C	-0.295041			
_NS--C	-0.278095			
_ONT--C	-0.059675			
_PEI--C	-0.045044			
_QUE--C	-0.689996			
_SASK--C	0.011616			

Weighted Statistics			
R-squared	0.166761	Mean dependent var	-0.866072
Adjusted R-squared	0.036905	S.D. dependent var	1.879502
S.E. of regression	1.844494	Sum squared resid	261.9663
F-statistic	7.705227	Durbin-Watson stat	2.698857
Prob(F-statistic)	0.000890		

Unweighted Statistics			
R-squared	0.089130	Mean dependent var	-0.733333
Adjusted R-squared	-0.052824	S.D. dependent var	1.797626
S.E. of regression	1.844495	Sum squared resid	261.9663
Durbin-Watson stat	2.748624		

Table 5.14: Change in smoking prevalence (15-19)

Dependent Variable: DYPREV(i,t)
Method: GLS (Cross Section Weights)
Sample: 2000 2008
Included observations: 9
Number of cross-sections used: 10
Total panel (balanced) observations: 90
Convergence achieved after 9 iteration(s)
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
DPRICE(i,t)	0.117999	0.073436	1.606821	0.1122
DGDPC(i,t)	0.000164	0.000304	0.540757	0.5902
BAN(i,t)	2.742715	0.781832	3.508062	0.0008
Fixed Effects				
_ALB--C	-2.169456			
_BC--C	-1.443360			
_MAN--C	-3.229903			
_NB--C	-2.072637			
_NFLD--C	-2.434563			
_NS--C	-3.153951			
_ONT--C	-2.281012			
_PEI--C	-3.229972			
_QUE--C	-2.996844			
_SASK--C	-3.170839			

Weighted Statistics			
R-squared	0.154476	Mean dependent var	-1.560497
Adjusted R-squared	0.022706	S.D. dependent var	3.127720
S.E. of regression	3.092007	Sum squared resid	736.1591
F-statistic	7.033883	Durbin-Watson stat	2.810363
Prob(F-statistic)	0.001564		
Unweighted Statistics			
R-squared	0.028539	Mean dependent var	-1.411111
Adjusted R-squared	-0.122857	S.D. dependent var	2.917959
S.E. of regression	3.092013	Sum squared resid	736.1621
Durbin-Watson stat	2.966542		

A2.23 The results for the 15+ age group indicate that cigarette prices are associated with a reduction in smoking prevalence. The display ban appears to have no impact on the percentage of smokers among the general population. In contrast, for the 15-19 age group, the presence of the display ban is associated with an increase (plus 2.7 percentage points) in smoking prevalence. The same model has been re-estimated using a standard OLS (see Table 5.15 below) and the presence of the display ban is still significant at the 90 per cent confidence. The significance of the display ban increases if we remove *dprice* and *dgdpc* (those variables that are not significant) from the model (see Table 5.16 below.)

Table 5.15: Change in smoking prevalence (15-19) – OLS estimation 1

Dependent Variable: DYPREV(i,t)
Method: Pooled Least Squares
Sample(adjusted): 2000 2008
Included observations: 9 after adjusting endpoints
Number of cross-sections used: 10
Total panel (balanced) observations: 90
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
DPRICE(i,t)	0.035388	0.087740	0.403329	0.6878
DGDPC(i,t)	-3.61E-05	0.000461	-0.078264	0.9378
BAN(i,t)	1.566052	0.916579	1.708584	0.0916
Fixed Effects				
_ALB--C	-1.442695			
_BC--C	-0.846309			
_MAN--C	-2.182151			
_NB--C	-1.585835			
_NFLD--C	-1.778957			
_NS--C	-2.311279			
_ONT--C	-1.669570			
_PEI--C	-2.264938			
_QUE--C	-2.419590			
_SASK--C	-2.071471			

R-squared	0.052829	Mean dependent var	-1.411111
Adjusted R-squared	-0.094782	S.D. dependent var	2.917959
S.E. of regression	3.053114	Sum squared resid	717.7559
F-statistic	2.147345	Durbin-Watson stat	2.975351
Prob(F-statistic)	0.123737		

Table 5.16: Change in smoking prevalence (15-19) – OLS estimation 2

Dependent Variable: DYPREV(i,t)
Method: Pooled Least Squares
Sample(adjusted): 2000 2008
Included observations: 9 after adjusting endpoints
Number of cross-sections used: 10
Total panel (balanced) observations: 90
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
BAN(i,t)	1.432692	0.798888	1.793359	0.0767
Fixed Effects				
_ALB--C	-1.270299			
_BC--C	-0.714744			
_MAN--C	-1.970085			
_NB--C	-1.444444			
_NFLD--C	-1.666667			
_NS--C	-2.096154			
_ONT--C	-1.492521			
_PEI--C	-2.033120			
_QUE--C	-2.270299			
_SASK--C	-1.858974			
R-squared	0.050083	Mean dependent var		-1.411111
Adjusted R-squared	-0.070160	S.D. dependent var		2.917959
S.E. of regression	3.018585	Sum squared resid		719.8365
Durbin-Watson stat	2.964701			

A2.24 All the results presented above are conclusive with indicating that the display ban is significantly and positively correlated with an increase in smoking prevalence among those aged 15-19. Concerning the magnitude of this, the following GLS model (in which, different from the model of Table 5.14 above, redundant variables have been removed) suggests that the presence of the display ban is associated with an increase of approximately 2 percentage points in smoking prevalence among the age group 15-19.

Table 5.17: The impact of the display ban on smoking prevalence (15-19)

Dependent Variable: DYPREV(i,t)
Method: GLS (Cross Section Weights)
Sample: 2000 2008
Included observations: 9
Number of cross-sections used: 10
Total panel (balanced) observations: 90
Convergence achieved after 8 iteration(s)
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
BAN(i,t)	2.125602	0.752926	2.823124	0.0060
Fixed Effects				
_ALB--C	-1.347289			
_BC--C	-0.791734			
_MAN--C	-2.278046			
_NB--C	-1.444444			
_NFLD--C	-1.666667			
_NS--C	-2.250134			
_ONT--C	-1.569511			
_PEI--C	-2.264090			
_QUE--C	-2.347289			
_SASK--C	-2.166934			
Weighted Statistics				
R-squared	0.104231	Mean dependent var		-1.490361

Adjusted R-squared	-0.009158	S.D. dependent var	3.016411
S.E. of regression	3.030192	Sum squared resid	725.3829
Durbin-Watson stat	2.816999		
Unweighted Statistics			
R-squared	0.042762	Mean dependent var	-1.411111
Adjusted R-squared	-0.078408	S.D. dependent var	2.917959
S.E. of regression	3.030195	Sum squared resid	725.3846
Durbin-Watson stat	2.967993		

A2.25 We have then re-estimated the model replacing *ban* with *ban2* and the presence of the display ban ceases to be significant. However, if we replace *ban* with *dban* (which means we assess the impact of the *introduction of the display ban* rather than the *presence of the display ban*) the significance (at the 99 per cent level) is restored.

Table 5.18: The impact of the *introduction* of the display ban on smoking prevalence (15-19)

Dependent Variable: DYPREV(i,t)
Method: GLS (Cross Section Weights)
Sample: 2000 2008
Included observations: 9
Number of cross-sections used: 10
Total panel (balanced) observations: 90
Convergence achieved after 10 iteration(s)
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
DPRICE(i,t)	0.057397	0.066505	0.863047	0.3908
DGDPC(i,t)	0.000136	0.000292	0.466775	0.6420
DBAN(i,t)	2.990439	0.944126	3.167416	0.0022
Fixed Effects				
_ALB--C	-1.855168			
_BC--C	-1.209841			
_MAN--C	-2.028512			
_NB--C	-1.781074			
_NFLD--C	-2.118169			
_NS--C	-2.512346			
_ONT--C	-1.998553			
_PEI--C	-2.284432			
_QUE--C	-2.752265			
_SASK--C	-1.952748			

Weighted Statistics			
R-squared	0.118166	Mean dependent var	-1.517215
Adjusted R-squared	-0.019262	S.D. dependent var	3.073394
S.E. of regression	3.102854	Sum squared resid	741.3329
F-statistic	5.159027	Durbin-Watson stat	2.794186
Prob(F-statistic)	0.007896		
Unweighted Statistics			
R-squared	0.021713	Mean dependent var	-1.411111
Adjusted R-squared	-0.130747	S.D. dependent var	2.917959
S.E. of regression	3.102858	Sum squared resid	741.3349
Durbin-Watson stat	2.980531		

A2.26 The results suggests that the display ban is associated with an increase in prevalence among the 15-19 age group, and that such increase in prevalence is either attributable to the presence of the display ban from the beginning or to the act of introducing it, rather than there being an effect that emerges only gradually over time, say a year after its introduction.

A2.27 All the first difference models of smoking prevalence reported above are characterised by low R^2. As we have already explained, this is mainly due to the fact that the model is expressed in first differences. In order to illustrate this point we report below the results of the models of smoking prevalence of the two age groups considered where the variables are included at their absolute levels.

Table 5.19: Absolute smoking prevalence (15+)

Dependent Variable: GENPREV(i,t)
Method: GLS (Cross Section Weights)
Sample: 1999 2008
Included observations: 10
Number of cross-sections used: 10
Total panel (balanced) observations: 100
Convergence achieved after 17 iteration(s)
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
PRICE(i,t)	-0.148896	0.016100	-9.248367	0.0000
GDPC(i,t)	-0.000149	7.05E-05	-2.113874	0.0374
T	0.153050	0.136530	1.121004	0.2654
BAN(i,t)	-0.294447	0.300480	-0.979921	0.3299
Fixed Effects				
_NFLD--C	38.63266			
_PEI--C	35.67042			
_NS--C	37.11114			
_NB--C	36.31117			
_QUE--C	36.25978			
_ONT--C	32.84410			
_MAN--C	37.33344			
_SASK--C	39.19666			
_ALB--C	38.68774			
_BC--C	31.54962			

Weighted Statistics			
R-squared	0.988757	Mean dependent var	26.88343
Adjusted R-squared	0.987058	S.D. dependent var	12.44136
S.E. of regression	1.415374	Sum squared resid	172.2823
F-statistic	2521.141	Durbin-Watson stat	1.849368
Prob(F-statistic)	0.000000		

Unweighted Statistics			
R-squared	0.854061	Mean dependent var	21.93000
Adjusted R-squared	0.832000	S.D. dependent var	3.453164
S.E. of regression	1.415376	Sum squared resid	172.2828
Durbin-Watson stat	1.738248		

Table 5.20: Absolute smoking prevalence (15-19)

Dependent Variable: YOUNGPREV(i,t)
Method: GLS (Cross Section Weights)
Date: 09/28/09 Time: 13:36
Sample: 1999 2008
Included observations: 10
Number of cross-sections used: 10
Total panel (balanced) observations: 100
Convergence achieved after 10 iteration(s)
White Heteroskedasticity-Consistent Standard Errors & Covariance

Variable	Coefficient	Std. Error	t-Statistic	Prob.
PRICE(i,t)	-0.075370	0.037620	-2.003472	0.0483
GDPC(i,t)	0.000207	0.000194	1.067196	0.2889
T	-1.244166	0.279313	-4.454371	0.0000
BAN(i,t)	2.445366	0.659223	3.709464	0.0004
Fixed Effects				
_NFLD--C	25.63184			
_PEI--C	22.17754			
_NS--C	23.65961			
_NB--C	24.85068			
_QUE--C	27.93148			
_ONT--C	18.59039			
_MAN--C	25.77916			
_SASK--C	27.53013			
_ALB--C	19.67997			
_BC--C	18.21237			
Weighted Statistics				
R-squared	0.914086	Mean dependent var		21.60471
Adjusted R-squared	0.901099	S.D. dependent var		7.169604
S.E. of regression	2.254732	Sum squared resid		437.2083
F-statistic	305.0011	Durbin-Watson stat		2.070032
Prob(F-statistic)	0.000000			
Unweighted Statistics				
R-squared	0.857249	Mean dependent var		20.35000
Adjusted R-squared	0.835670	S.D. dependent var		5.562092
S.E. of regression	2.254738	Sum squared resid		437.2104
Durbin-Watson stat	1.919010			

A2.28 As the tables above show, the most immediate effect of including variables at their levels is that of tremendously increasing the goodness of fit (the R^2 is always above 0.9). Also, different from the first difference models, GDP per capita becomes significant (with a negative sign) in explaining prevalence among the general population, and *price* becomes significant (with a negative sign) in explaining prevalence among those aged 15-19. We are however very cautious in drawing any conclusion concerning these variables because there is the risk that these models rely on spurious correlations. Interestingly, the significance and the coefficients of the display ban are in line with those obtained in the first difference models.

A2.29 The same level models have been specified with a random effects specification and we have obtained broadly the same results.

Calculation of the increase in the absolute numbers of smokers aged 15-19 associated with the display ban.

A2.30 Let B_{2008} be the set of provinces in which a display ban is in place as of 2008, and let $prev_{i,2008}^{(15-19)}$ and $pop_{i,2008}^{(15-19)}$ be, respectively, the smoking prevalence and the population[19] of those aged 15-19 in each province as of 2008. In those provinces where the display ban is in place in 2008, the aggregate number of smokers aged 15-19 is therefore:

19 Historic data on population by province and age group was provided by Statistics Canada.

$$Fn^{(15-19)}_{i,2008} = \sum_{i \in B_{2008}} prev^{(15-19)}_{i,2008} * pop^{(15-19)}_{i,2008}$$

A2.31 The interpretation of the coefficient of $ban_{i,t}$ implies that, in the counterfactual scenario in which display bans were not in place in those provinces belonging to B_{2008}, the aggregate number of smokers would be:

$$CFn^{(15-19)}_{i,2008} = \sum_{i \in B_{2008}} \left(prev^{(15-19)}_{i,2008} - 0.02 \right) * pop^{(15-19)}_{i,2008}$$

Thus, the number of 15-19 year old smokers attributable to display bans being in place in 2008 is $\delta_{2008} = \left(Fn^{(15-19)}_{i,2008} - CFn^{(15-19)}_{i,2008} \right)$, i.e., an increase in the number of smokers (relative to the counterfactual scenario with no display bans) is equal to:

$$\frac{\delta_{2008}}{CFn^{(15-19)}_{i,2008}}$$

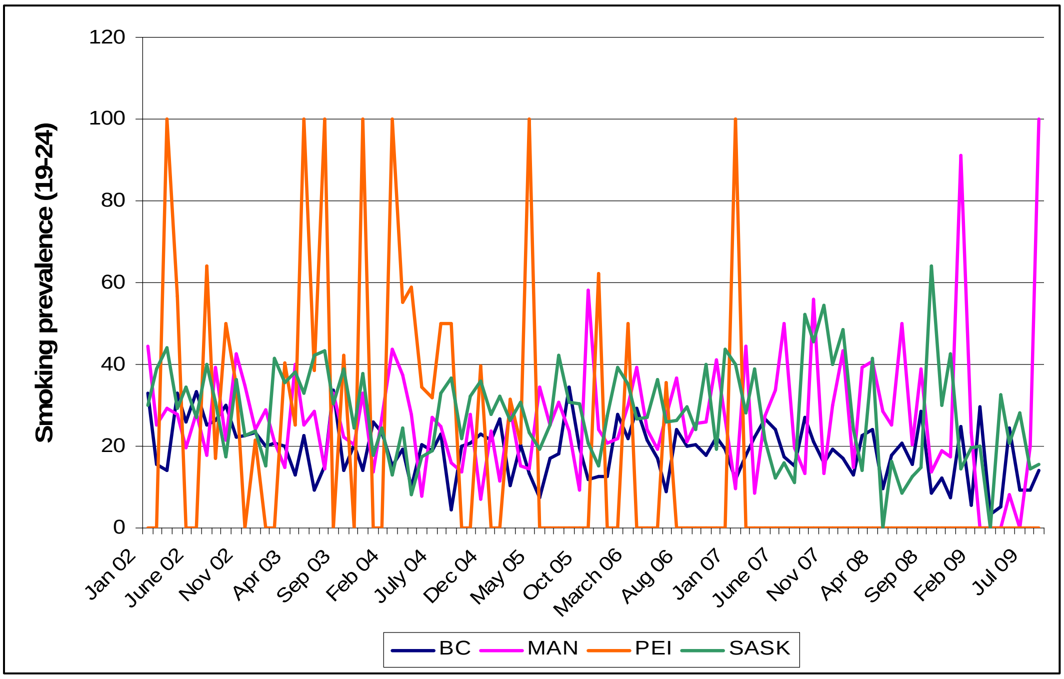
A2.32 The last formula provides the 13 per cent outcome set out at paragraph 2.47 above.

Canada: impossibility of analysing the impact of federal regulatory intervention by performing a meaningful Chow test on monthly data

A2.33 After a careful inspection of the monthly data for all ten Canadian provinces from the same sources discussed in the 2008 Report and for the period from January 2002 (the earliest point at which we have data from such sources), we have concluded that these are too volatile to make the use of a Chow test meaningful.

A2.34 An example of this volatility can be seen in Figure 5.3 below, which illustrates the monthly data for smoking prevalence amongst 19-24 year olds in Prince Edward Island (PEI) by reference to the same type of data for Saskatchewan (SASK), British Columbia (BC) and Manitoba (MAN):

Figure 5.3: Smoking prevalence in BC, MAN, PEI, and SASK (19-24)

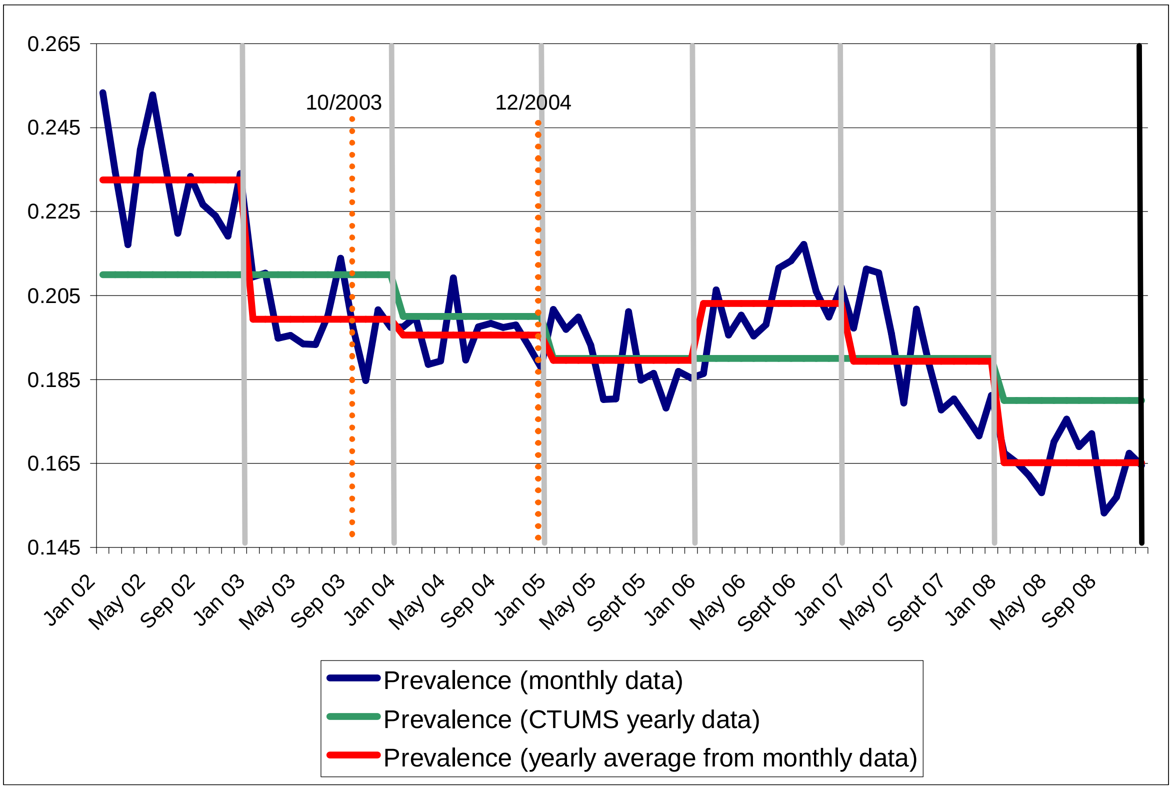


A2.35 We have attempted to perform a Chow test also on the monthly data on smoking prevalence of the general population in Canada (the series was calculated by aggregating data on smoking prevalence for the ten provinces considered).

A2.36 In fact, the Chow test performed on this data suggests the presence of several structural breaks (at least five) throughout the monthly series from January 2002. One break occurs around October 2003 (which broadly coincides with the enforcement of the federal tobacco sponsorship ban), and another in December 2004 (which broadly coincides with the enforcement of the Hazardous Products Act). However, we have concluded that the series is inadequate to make use of such a Chow test meaningful and that measurement errors are likely to be the main drivers behind these sudden changes in trends (i.e. structural brakes) observed in the monthly series (this is discussed further below).

A2.37 In order to illustrate this point, we report below a graph where monthly data on smoking prevalence of the general population in Canada is compared to the yearly CTUMS data. We have shown in this graph the October 2003 and December 2004 points discussed in the previous paragraph.

Figure 5.4: Smoking prevalence, monthly and yearly data (a comparison for the ten Canadian provinces)



A2.38 From a visual inspection of Figure 5.4 above, two conclusions can be drawn:

(a) The monthly data is characterised by pronounced spikes; and

(b) The yearly average prevalence calculated from the monthly data is often significantly different from the yearly prevalence reported by the CTUMS data.

A2.39 While the former aspect of the monthly series (the pronounced spikes) denotes only a high volatility of the data (which in principle does rule out the possibility of performing a meaningful Chow test), the latter aspect is far more problematic as it suggests the presence of systematic and time dependent measurement errors in the monthly data for a number of provinces. In other words, it seems that monthly smoking prevalence has been systematically overestimated for certain provinces in some years (i.e. 2002 and 2006), and underestimated in other years (i.e. 2003 and 2008). Such measurement errors are likely to be the main drivers behind the sudden changes in trends (i.e. structural brakes) observed in the monthly series.

Canada: update of last year time-series analysis

A2.40 The analysis is based on the same methodology adopted last year and relies on the study of the following series:

(c) *Genprevgen_MAN*: general prevalence in Manitoba

(d) *Genprevgen_NFLD*: general prevalence in Newfoundland and Labrador

(e) *Genprev_SASK:* general prevalence in Saskatchewan

A2.41 Compared to the 2008 Report, the series of Newfoundland and Labrador has replaced that of British Columbia because, in light of the data recently made available, British Columbia no longer constitutes a useful control, because a substantial portion of the prevalence series for British Columbia covers a period subsequent the introduction of display ban in that jurisdiction.

A2.42 The following tests confirm that the three series are stationary.

Table 5.21: Stationarity test for Manitoba

ADF Test Statistic	-6.634073	1% Critical Value*	-4.0625
		5% Critical Value	-3.4597
		10% Critical Value	-3.1557

*MacKinnon critical values for rejection of hypothesis of a unit root.

Augmented Dickey-Fuller Test Equation
Dependent Variable: D(GENPREV_MAN)
Method: Least Squares
Sample(adjusted): 2002:03 2009:08
Included observations: 90 after adjusting endpoints

Variable	Coefficient	Std. Error	t-Statistic	Prob.
GENPREV_MAN(-1)	-0.958238	0.144442	-6.634073	0.0000
D(GENPREV_MAN(-1))	0.043651	0.104199	0.418923	0.6763
C	21.41764	3.352710	6.388158	0.0000
@TREND(2002:01)	-0.041712	0.014865	-2.806095	0.0062

R-squared	0.457807	Mean dependent var	-0.016667
Adjusted R-squared	0.438893	S.D. dependent var	4.275939
S.E. of regression	3.202981	Akaike info criterion	5.209467
Sum squared resid	882.2814	Schwarz criterion	5.320570
Log likelihood	-230.4260	F-statistic	24.20500
Durbin-Watson stat	1.994769	Prob(F-statistic)	0.000000

Table 5.22: Stationarity test for Newfoundland and Labrador

ADF Test Statistic	-5.539202	1% Critical Value*	-4.0625
		5% Critical Value	-3.4597
		10% Critical Value	-3.1557

*MacKinnon critical values for rejection of hypothesis of a unit root.

Augmented Dickey-Fuller Test Equation
Dependent Variable: D(GENPREV_NFLD)
Method: Least Squares
Sample(adjusted): 2002:03 2009:08
Included observations: 90 after adjusting endpoints

Variable	Coefficient	Std. Error	t-Statistic	Prob.
GENPREV_NFLD(-1)	-0.830480	0.149928	-5.539202	0.0000
D(GENPREV_NFLD(-1))	-0.065538	0.111118	-0.589804	0.5569
C	21.93057	4.046192	5.420052	0.0000

@TREND(2002:01)	-0.064907	0.021818	-2.974883	0.0038

R-squared	0.433553	Mean dependent var	-0.182222
Adjusted R-squared	0.413793	S.D. dependent var	6.276630
S.E. of regression	4.805648	Akaike info criterion	6.020887
Sum squared resid	1986.105	Schwarz criterion	6.131990
Log likelihood	-266.9399	F-statistic	21.94119
Durbin-Watson stat	1.933975	Prob(F-statistic)	0.000000

Table 5.23: Stationarity test for Saskatchewan

ADF Test Statistic	-5.597790	1% Critical Value*	-3.5039
		5% Critical Value	-2.8936
		10% Critical Value	-2.5836

*MacKinnon critical values for rejection of hypothesis of a unit root.

Augmented Dickey-Fuller Test Equation
Dependent Variable: D(GENPREV_SASK)
Method: Least Squares
Sample(adjusted): 2002:03 2009:08
Included observations: 90 after adjusting endpoints

Variable	Coefficient	Std. Error	t-Statistic	Prob.
GENPREV_SASK(-1)	-0.692336	0.123680	-5.597790	0.0000
D(GENPREV_SASK(-1))	-0.025599	0.104593	-0.244747	0.8072
C	15.20325	2.752162	5.524111	0.0000

R-squared	0.360947	Mean dependent var	-0.063333
Adjusted R-squared	0.346256	S.D. dependent var	3.936617
S.E. of regression	3.182930	Akaike info criterion	5.186247
Sum squared resid	881.4010	Schwarz criterion	5.269574
Log likelihood	-230.3811	F-statistic	24.56943
Durbin-Watson stat	2.009946	Prob(F-statistic)	0.000000

A2.43 After a model selection exercise (based on the Schwarz criterion) we can conclude that the series *Genprev_MAN* is better explained by a linear trend with intercept (which implies that the oscillations around the trend can be interpreted as pure noise).

Table 5.24: Model for general prevalence in Manitoba

Dependent Variable: GENPREV_MAN
Method: Least Squares
Sample: 2002:01 2009:08
Included observations: 92

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	22.73906	0.677886	33.54407	0.0000
T	-0.049903	0.012867	-3.878284	0.0002

R-squared	0.143192	Mean dependent var	20.46848
Adjusted R-squared	0.133672	S.D. dependent var	3.521361
S.E. of regression	3.277568	Akaike info criterion	5.233580
Sum squared resid	966.8205	Schwarz criterion	5.288401
Log likelihood	-238.7447	F-statistic	15.04109
Durbin-Watson stat	1.757047	Prob(F-statistic)	0.000200

A2.44 The best model describing *Genprev_MAN* is such that it allows a direct application of the Chow test. We have conducted a series of Chow tests at several relevant dates (specifically, the date of enforcement of the display ban in Manitoba, August 2005, one year later, and two years later) and were unable to reject the hypothesis of a lack of a structural break.

Table 5.25: Chow test at August 2005

Chow Breakpoint Test: 2005:08			
F-statistic	2.992430	Probability	0.055296
Log likelihood ratio	6.053320	Probability	0.048477

Table 5.26: Chow test at August 2006

Chow Breakpoint Test: 2006:08			
F-statistic	2.734438	Probability	0.070451
Log likelihood ratio	5.546842	Probability	0.062448

Table 5.27: Chow test August 2007

Chow Breakpoint Test: 2007:08			
F-statistic	1.102337	Probability	0.336635
Log likelihood ratio	2.276494	Probability	0.320380

A2.45 The series *Genprevgen_NFLD*: and *Genprevgen_SASK* are better explained by a simple trend with intercept (which, again, confirms the high volatility of the data) and an AR(1) process with intercept respectively.

Table 5.28: Model for Genprev_NFLD

Dependent Variable: GENPREV_NFLD
Method: Least Squares
Sample: 2002:01 2009:08
Included observations: 92

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	26.23380	0.980098	26.76652	0.0000
T	-0.074083	0.018604	-3.982147	0.0001

R-squared	0.149800	Mean dependent var	22.86304
Adjusted R-squared	0.140354	S.D. dependent var	5.110982
S.E. of regression	4.738755	Akaike info criterion	5.970925
Sum squared resid	2021.022	Schwarz criterion	6.025747
Log likelihood	-272.6626	F-statistic	15.85750
Durbin-Watson stat	1.735789	Prob(F-statistic)	0.000138

Table 5.29: Model for Genprev_SASK

Dependent Variable: GENPREV_SASK
Method: Least Squares
Sample(adjusted): 2002:02 2009:08
Included observations: 91 after adjusting endpoints
Convergence achieved after 3 iterations

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	22.00658	0.479750	45.87093	0.0000
AR(1)	0.308519	0.098626	3.128155	0.0024

R-squared	0.099057	Mean dependent var	22.04286

Adjusted R-squared	0.088934	S.D. dependent var	3.313412
S.E. of regression	3.162645	Akaike info criterion	5.162427
Sum squared resid	890.2066	Schwarz criterion	5.217611
Log likelihood	-232.8904	F-statistic	9.785352
Durbin-Watson stat	2.042972	Prob(F-statistic)	0.002378
Inverted AR Roots	.31		

A2.46 In order to test for the potential presence of a break in *Genprevgen_SASK* we have constructed the auxiliary series *Control_SASK* which is constructed as *Genprevgen_SASK* minus *Genprevgen_NFLD*. This series allows us to test for a structural break in the data while using the province of Newfoundland and Labrador as a control.

A2.47 The usual model selection exercise leads us to conclude again that the series behaves like simple noise around a trend.

Table 5.30: Model for Control_SASK

Dependent Variable: CONTROL_SASK
Method: Least Squares
Sample: 2002:01 2009:08
Included observations: 92

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	-2.802314	1.169859	-2.395428	0.0187
T	0.045106	0.022206	2.031278	0.0452

R-squared	0.043836	Mean dependent var	-0.750000
Adjusted R-squared	0.033212	S.D. dependent var	5.752582
S.E. of regression	5.656249	Akaike info criterion	6.324898
Sum squared resid	2879.383	Schwarz criterion	6.379720
Log likelihood	-288.9453	F-statistic	4.126091
Durbin-Watson stat	1.747237	Prob(F-statistic)	0.045177

A2.48 We have conducted a series of Chow tests and we have found a structural break one year after the date of enforcement of the display ban in Saskatchewan, i.e. January 2006.

Table 5.31: Chow test at January 2006

Chow Breakpoint Test: 2006:01

F-statistic	0.071091	Probability	0.931431
Log likelihood ratio	0.148531	Probability	0.928425

A2.49 We have therefore run two separate models; before and after the break point. The model before the break indicates that the series is explained as a simple mean (the trend ceases to be significant probably because of the reduced sample size).

Table 5.32: Model for Control_SASK before the break

Dependent Variable: CONTROL_SASK
Method: Least Squares
Sample: 2002:01 2005:12
Included observations: 48

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	-3.204422	1.370478	-2.338178	0.0238
T	0.063575	0.050239	1.265456	0.2121

R-squared	0.033641	Mean dependent var	-1.710417
Adjusted R-squared	0.012634	S.D. dependent var	4.852604
S.E. of regression	4.821853	Akaike info criterion	6.024967
Sum squared resid	1069.512	Schwarz criterion	6.102934
Log likelihood	-142.5992	F-statistic	1.601380
Durbin-Watson stat	2.196980	Prob(F-statistic)	0.212080

A2.50 However there is no model that can explain the behaviour of the series after the break, i.e. the series behaves as a pure noise around zero. We consider the best explanation for this to be that the statistical break in the series is due to the poor quality of the data after January 2006.

A2.51 Thus, as in the 2008 Report, our overall conclusion is that there is no evidence of the display ban being responsible for any structural break in the monthly series considered.

Analysis of competition and innovation

Iceland

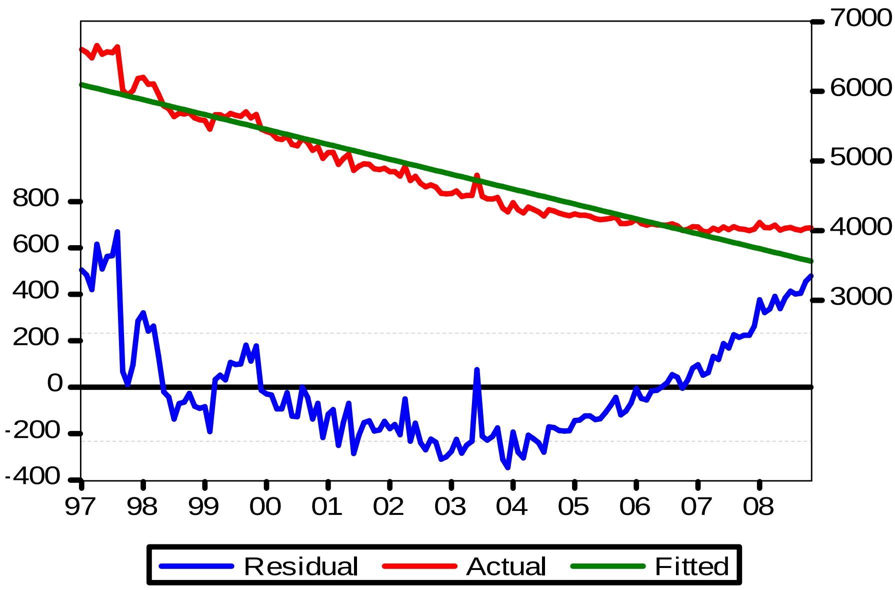
A2.52 The analysis of Iceland is based on the same methodology adopted in the 2008 Report. The series analysed are:

(f) *HHI_Producers*: the HHI of cigarette manufacturers the brands of which are sold in Iceland

(g) *HHI_Brands*: the HHI of cigarettes brands sold in Iceland.

A2.53 The following figure shows the de-trending of the series *HHI_Producers* and the resulting residuals.

Figure 5.5: De-trending of HHI_Producers



A2.54 We have then performed an Augmented Dickey-Fuller test in order to check that the residuals (denoted *U* in the table below) are stationary. The output of the test is displayed below and allows rejecting the null hypothesis of a unit root (i.e., non-stationary) at the 95 per cent confidence interval.

Table 5.33: Stationarity test for residuals

ADF Test Statistic	-2.000817	1% Critical Value*	-2.5802
		5% Critical Value	-1.9421
		10% Critical Value	-1.6169

*MacKinnon critical values for rejection of hypothesis of a unit root.

Augmented Dickey-Fuller Test Equation
Dependent Variable: D(U)
Method: Least Squares
Sample(adjusted): 1997:03 2008:11
Included observations: 141 after adjusting endpoints

Variable	Coefficient	Std. Error	t-Statistic	Prob.
U(-1)	-0.069315	0.034644	-2.000817	0.0474
D(U(-1))	-0.227122	0.082441	-2.754967	0.0067

R-squared	0.093940	Mean dependent var	-0.033373
Adjusted R-squared	0.087421	S.D. dependent var	94.88013
S.E. of regression	90.63803	Akaike info criterion	11.86571
Sum squared resid	1141920.	Schwarz criterion	11.90753
Log likelihood	-834.5324	Durbin-Watson stat	2.062481

A2.55 As in the 2008 Report, the series *HHI_Producers* can is best modelled by an AR(1) process with trend and intercept.

Table 5.34: Model for HHI_Producers

Dependent Variable: HHI_PRODUCERS
Method: Least Squares
Sample(adjusted): 1997:02 2008:11
Included observations: 142 after adjusting endpoints
Convergence achieved after 4 iterations

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	5818.602	239.4617	24.29868	0.0000
T	-14.54557	2.591147	-5.613564	0.0000
AR(1)	0.914225	0.033985	26.90060	0.0000
R-squared	0.985541	Mean dependent var		4815.697
Adjusted R-squared	0.985333	S.D. dependent var		762.6278
S.E. of regression	92.35969	Akaike info criterion		11.91016
Sum squared resid	1185713.	Schwarz criterion		11.97261
Log likelihood	-842.6213	F-statistic		4737.227
Durbin-Watson stat	2.457487	Prob(F-statistic)		0.000000
Inverted AR Roots	.91			

A2.56 We have performed a Chow break point test at one year after the introduction of the display ban i.e., August 2008 and the results (reported below) lead us to reject the hypothesis of a lack of break at the 95 per cent confidence level. In a Chow test, rejection of the null hypothesis implies the existence of a break in the series and, thus, the need to estimate two separate regressions.

Table 5.35: Chow test for HHI_Producers at August 2002

Chow Breakpoint Test: 2002:08

F-statistic	6.798572	Probability	0.000264
Log likelihood ratio	19.84232	Probability	0.000183

A2.57 We have therefore re-estimated the series separately for the two samples. The estimation output for the pre-regulation sample and for the post-regulation samples are, respectively:

Table 5.36: The model for HHI_Producers prior to August 2002

Dependent Variable: HHI_PRODUCERS
Method: Least Squares
Sample(adjusted): 1997:02 2002:08
Included observations: 67 after adjusting endpoints
Convergence achieved after 3 iterations

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	6395.293	84.26540	75.89465	0.0000
T	-26.48074	2.058606	-12.86343	0.0000
AR(1)	0.665875	0.091439	7.282201	0.0000
R-squared	0.961823	Mean dependent var		5500.783
Adjusted R-squared	0.960630	S.D. dependent var		545.6793
S.E. of regression	108.2727	Akaike info criterion		12.25093
Sum squared resid	750271.0	Schwarz criterion		12.34964
Log likelihood	-407.4060	F-statistic		806.2058
Durbin-Watson stat	2.136729	Prob(F-statistic)		0.000000

Table 5.37: The model for HHI_Producers after August 2002

Dependent Variable: HHI_PRODUCERS
Method: Least Squares
Sample: 2002:09 2008:11
Included observations: 75
Convergence achieved after 3 iterations

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	4992.795	115.6370	43.17647	0.0000
T	-7.522065	1.058693	-7.105051	0.0000
AR(1)	0.677788	0.087763	7.722924	0.0000

R-squared	0.900319	Mean dependent var	4203.687
Adjusted R-squared	0.897550	S.D. dependent var	195.0579
S.E. of regression	62.43384	Akaike info criterion	11.14527
Sum squared resid	280654.9	Schwarz criterion	11.23797
Log likelihood	-414.9476	F-statistic	325.1509
Durbin-Watson stat	2.335698	Prob(F-statistic)	0.000000
Inverted AR Roots	.68		

A2.58 Based on the results provided above, we have simulated the future decrease in the concentration after the regulation has been introduced (the “factual scenario”). This appears in red in Figure 5.6 below. Next we simulated how the concentration index would have decreased had the regulation not been introduced (the “counterfactual scenario”). As the figure below shows, under the factual scenario the market is projected to become highly competitive (HHI_Producer index below 1000) in the year 2041. However, had the regulation not been introduced in 2001, the market would have become highly competitive by 2014. Thus the impact of the regulation has been to delay the arrival of a competitive market by 27 years.

Figure 5.6: Simulation of the increase in competition in the absence of regulation

A2.59 In what follows we replicate exactly the same methodology for the series *HHI_Brands.*

Table 5.38: Model for HHI_Brands

Dependent Variable: HHI_BRANDS
Method: Least Squares
Sample(adjusted): 1997:02 2008:11
Included observations: 142 after adjusting endpoints
Convergence achieved after 3 iterations

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	2440.297	52.48335	46.49659	0.0000
T	-4.226421	0.606790	-6.965214	0.0000
AR(1)	0.803465	0.045197	17.77683	0.0000

R-squared	0.935539	Mean dependent var	2147.751
Adjusted R-squared	0.934611	S.D. dependent var	220.4587
S.E. of regression	56.37405	Akaike info criterion	10.92280
Sum squared resid	441746.7	Schwarz criterion	10.98524
Log likelihood	-772.5185	F-statistic	1008.665
Durbin-Watson stat	2.348813	Prob(F-statistic)	0.000000
Inverted AR Roots	.80		

A2.60 We have tested for a structural break at August 2008 and the presence of a break has been confirmed.

Table 5.39: Chow test for HHI_Brands at August 2002

Chow Breakpoint Test: 2002:08

F-statistic	5.873694	Probability	0.000842
Log likelihood ratio	17.30046	Probability	0.000613

A2.61 After estimating the model for HHI_Producers on the two separate samples (see tables Table 5.40 and Table 5.41 below), we can conclude that the rate of decline in brand concentration has slowed down significantly after the enforcement of the display ban.

Table 5.40: The model for HHI_Brands prior to August 2002

Dependent Variable: HHI_BRANDS
Method: Least Squares
Sample(adjusted): 1997:02 2002:08
Included observations: 67 after adjusting endpoints
Convergence achieved after 3 iterations

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	2601.841	53.46945	48.66033	0.0000
T	-8.231482	1.301586	-6.324194	0.0000
AR(1)	0.659727	0.088112	7.487341	0.0000
R-squared	0.875725	Mean dependent var		2329.739
Adjusted R-squared	0.871841	S.D. dependent var		193.7932
S.E. of regression	69.37658	Akaike info criterion		11.36072
Sum squared resid	308039.1	Schwarz criterion		11.45944
Log likelihood	-377.5841	F-statistic		225.4929
Durbin-Watson stat	2.112163	Prob(F-statistic)		0.000000
Inverted AR Roots	.66			

Table 5.41: The model for HHI_Brands after August 2002

Dependent Variable: HHI_BRANDS
Method: Least Squares
Sample: 2002:09 2008:11
Included observations: 75
Convergence achieved after 3 iterations

Variable	Coefficient	Std. Error	t-Statistic	Prob.
C	2143.448	31.27139	68.54342	0.0000
T	-1.507512	0.290115	-5.196254	0.0000
AR(1)	0.375286	0.109459	3.428569	0.0010
R-squared	0.520529	Mean dependent var		1985.176
Adjusted R-squared	0.507210	S.D. dependent var		48.33766
S.E. of regression	33.93255	Akaike info criterion		9.925805
Sum squared resid	82902.11	Schwarz criterion		10.01850
Log likelihood	-369.2177	F-statistic		39.08271
Durbin-Watson stat	2.138720	Prob(F-statistic)		0.000000
Inverted AR Roots	.38			

Thailand

A2.62 The series analysed in this section are:

(a) *HHI_Producers*: the HHI of cigarette manufacturers the brands of which are sold in Thailand; and

(b) *HHI_Brands*: the HHI of cigarette brands sold in Thailand.

A2.63 As in the 2008 Report, the analysis of HHI_Producers and HHI_Brands has been limited because we could not reject the hypothesis that both series behave as random walks.

A2.64 We report below the stationarity tests conducted on HHI_Producers, which lead us to conclude that the series is non-stationary at its levels but is however stationary at the first difference (the first difference is denoted by D(HHI_Producers)).

Table 5.42: Stationarity test for HHI_Producers

ADF Test Statistic	-0.823818	1% Critical Value*	-3.4959
		5% Critical Value	-2.8900
		10% Critical Value	-2.5818

*MacKinnon critical values for rejection of hypothesis of a unit root.

Augmented Dickey-Fuller Test Equation
Dependent Variable: D(HHI_PRODUCERS)
Method: Least Squares
Sample(adjusted): 2001:03 2009:07
Included observations: 101 after adjusting endpoints

Variable	Coefficient	Std. Error	t-Statistic	Prob.
HHI_PRODUCERS(-1)	-0.019923	0.024184	-0.823818	0.4120
D(HHI_PRODUCERS(-1))	0.076188	0.101645	0.749551	0.4553
C	125.7513	155.0952	0.810801	0.4194

R-squared	0.010883	Mean dependent var	-1.992690
Adjusted R-squared	-0.009303	S.D. dependent var	75.79239
S.E. of regression	76.14413	Akaike info criterion	11.53239
Sum squared resid	568197.0	Schwarz criterion	11.61006
Log likelihood	-579.3855	F-statistic	0.539125
Durbin-Watson stat	1.809973	Prob(F-statistic)	0.584978

Table 5.43: Stationarity test for D(HHI_Producers)

ADF Test Statistic	-6.074484	1% Critical Value*	-3.4965
		5% Critical Value	-2.8903
		10% Critical Value	-2.5819

*MacKinnon critical values for rejection of hypothesis of a unit root.

Augmented Dickey-Fuller Test Equation
Dependent Variable: D(HHI_PRODUCERS,2)
Method: Least Squares
Sample(adjusted): 2001:04 2009:07
Included observations: 100 after adjusting endpoints

Variable	Coefficient	Std. Error	t-Statistic	Prob.
D(HHI_PRODUCERS(-1))	-0.844926	0.139094	-6.074484	0.0000
D(HHI_PRODUCERS(-1),2)	-0.040212	0.099317	-0.404889	0.6865
C	-3.471936	7.473689	-0.464554	0.6433

R-squared	0.440286	Mean dependent var	-3.274034
Adjusted R-squared	0.428746	S.D. dependent var	98.87093
S.E. of regression	74.72801	Akaike info criterion	11.49513
Sum squared resid	541674.7	Schwarz criterion	11.57328
Log likelihood	-571.7564	F-statistic	38.15140
Durbin-Watson stat	1.926572	Prob(F-statistic)	0.000000

A2.65 As in the 2008 Report, an inspection of the autocorrelation and partial autocorrelation function of the (first difference) integrated series leads us to conclude that it behaves like a white noise process. We therefore concluded that the HHI_Producers is best described by a random walk process.

A2.66 We have replicated the same methods for HHI_Brands and also concluded that it behaves like a random walk process.

APPENDIX 3: CURRICULUM VITAE OF DR ANDREW LILICO

QUALIFICATIONS

St. John's College, Oxford
BA (Hons) Politics, Philosophy and Economics (1989-92)

University College, London
MSc Economics (1997-8)

Birkbeck College, London
MA Philosophy (2005-7)

University College, London
PhD Economics (1998-02)

CAREER DETAILS

May 09 –	**EUROPE ECONOMICS, London** *Principal* **POLICY EXCHANGE, London** *Chief Economist*
July 06 – May 09	**EUROPE ECONOMICS, London** *Managing Director*
Sep 02 - July 06	**EUROPE ECONOMICS, London** *Managing Consultant*
1999 - Sep 02	**UCL, London** *Visiting Lecturer and Teaching Assistant in Economics*
1999-2003	**Welwyn Hatfield District Council** *Councillor and front-bench spokesman*
1994-99	**PlasPET Group, Florida, USA** *Business Consultant (free-lance)*
1998	**The Adam Smith Institute (International Division)** *Economic Consultant (free-lance)*
1997	**Institute of Directors** *International and Public Sector Economist*
1992-94	**Institute for Fiscal Studies, London** *Economist*
1988-89	**ICI Runcorn Heath Site** *Mathematical Research Scientist*

SELECTED PROFESSIONAL EXPERIENCE

EXPERT WITNESS CASES

Japan Tobacco International

The Future of Tobacco Control (June to September 2008)

Assessing the competition and other impacts of the introduction of a plain packs requirement and a visibility ban.

Ranbaxy/Norton Rose

Assistance with legal defence in alleged cartel case (May 2004 to April 2005)

Assisting the defence in critiquing the alleged damages in a court case alleging that generics pharmaceuticals companies colluded in the late 1990s.

TAXES AND BENEFITS

Wine and Spirits Trade Association

Assistance with Budget supervision (January to March 2008)

This project modelled the impact of various proposed changes to alcohol duties.

CPRE

Taxes and Development (May 2004 to September 2004)

This project considers the impact of various tax measures on the incentives for building housing and conducting other sorts of development in the UK.

An East Asian client

Modelled impact of gambling tax changes (2002-3)

This project considered how certain changes to gambling-related taxes might affect the amount of business lost to illegal or “offshore” gambling organisers.

Institute for Fiscal Studies, London

Economist (1992-94)

Main author of the BBC Budget Guide 1993 (including analysis of Petroleum taxes); Contributor to Green Budget 1993 (in respect of income tax and national insurance); Analysing the UK tax and benefit system; Modelling the impact of changes in UK taxes and benefits on the UK Budget and on individual households; Analysing poverty statistics; Econometric modelling of UK childcare; Setting up of General Household Survey dataset.

Welwyn Hatfield District Council
Councillor (1999-2003), Shadow Finance Spokesman (2001/2), Shadow Spokesman for Economic Development (2000/1), Member of Planning Committee (1999/2000), Member of Development Plan Committee (2000/1)
Devised the 2001 & 2002 Budget responses for the Conservative Group.

Permanent Substitute on LGA Economic Regeneration Executive.

FINANCE & FINANCIAL SERVICES

European Parliament
Critique of EC IA on CRD (November to December 2008)
This project critiqued the European Commission's impact assessment on the revision to the Capital Requirements Directive in respect of cross-border supervision and crisis management.

Ofwat
The Cost of Capital for the Water Industry (July 2008 to ongoing)
Director of project assessing the cost of capital for the regulator.

European Commission DG Internal Market
European Credit intermediation (February 2008 to October 2008)
Director of project analysing European credit intermediation.

European Commission DG Internal Market
Cost of complying with EC Financial Services Action Plan (December 2007 to October 2008)
Director of project assessing and quantifying the cost of complying with the EU's financial services action plan.

European Parliament
Impact of EC Financial Services Action Plan and Financial Services White Paper (December 2005 to February 2007)
Project director for large-scale project for DG Internal Policies assessing and quantifying the impact of the EU's financial services framework on all EU Member States, both individually and collectively.

CAA
The Cost of Capital for BAA (February 2006 to April 2008)
Core advisor on project assessing the cost of capital for the regulated entity managing the main London airports.

FSA
Cost and Benefits of the FSA Training & Competence Exams Requirement (September 2006 to February 2007)
Project director for project assessing and quantifying the costs and benefits associated with a particular FSA regulation.

Ofcom
Applicability of Real Options in Regulation of third generation mobile phone termination charges (August 2006)

London Market Insurance Brokers' Committee
Status of General Insurance Intermediaries under the Financial Services Compensation Scheme (May 2006)
Project director for advocacy project responding to an FSA consultation paper.

FSA
Benefits of the Markets in Financial Instruments Directive (September 2005 to April 2006)
Project leader for project assessing and quantifying the benefits associated with a major EC directive.

Ofcom
Cost of Capital, Real Options (May to September 2005)
During a full-time secondment at Ofcom, participating in a major policy statement by Ofcom on its approach to regulatory cost of capital, including being the main author of the section on the application of real options theory in regulation; assessing BT's permitted group beta; assessing the case for granting different costs of capital on different parts of BT's business.

IEA/Sunday Times Shadow Monetary Policy Committee
Member (Ongoing)
Participating in a monthly email poll and attending quarterly committee meetings to debate and vote on a recommendation for Bank of England interest rates.

Staff Working Paper
Why might people take on "too much" debt? (July 2004)
Technical paper considering under what conditions people might take on more debt than, in retrospect, they would consider ideal.

Ofwat
Review of the impact of general decline in pension funds on RPI. (June 2004)
Analysis for UK water regulator.

Staff Working Paper
Regulating markets with short-sighted decision-makers? (March 2004)
Technical paper setting out a formal economic framework within which to consider how markets are affected by shortsightedness, and how to regulate them.

UCL
Lecturing (December 2003 to July 2005)
Undergraduate lectures in Money & Banking (including Banking, Financial Regulation, Monetary Theory, and Monetary Policy) and in Corporate Finance (including Monte Carlo simulation techniques, Efficient Markets, Capital Structure, Financial Contracting, and Real Options).

FSA
The Costs of Compliance (April to June 2003)
Played a leading role in a major study to assess the size and drivers of incremental cost of compliance with FSA regulation since 1998.

FSA
The Costs and Benefits of FSA Authorisation of General Insurance and Mortgage Intermediation (November 2002 to January 2003)
Part of a team assessing the costs and the costs and benefits of authorisation proposals for the FSA regulation of mortgages and general insurance.

Staff Working Paper
The role and regulation of short selling (October 2002)
Technical paper analysing what regulatory issues short selling raises and whether proposed regulations at the time (e.g. FSA DP17) actually addressed them.

PlasPET Group, Florida, USA
Business Consultancy (1994-99)
Strategy advice; Forecasting; Financial modelling; Business analysis; Design and implementation of Management Systems; Writing of Business Plan for Private Offering.

(Part of a team of three that set up the PlasPET Group from scratch. In 1999 half of the company was bought by Graham Packaging — one of the world's largest packaging manufacturers.)

MACROECONOMICS, ECONOMIC DEVELOPMENT, HOUSING, INTERNATIONAL ECONOMICS

Abu Dhabi Water and Electricity Authority (ADWEA)
The impact of inflation (June to October 2008)
Project assessing the impact of inflation upon Abu Dhabi society and the business of ADWEA

Association of Convenience Stores
Effects of the Planning System on Competition in the Retail Grocery Sector (September 2006 to November 2006)
Project leader for preparation of submission to the Competition Commission.

European Commission
Impact of transport policies on growth and productivity in the EU (December 2005 to September 2006)
Project leader for project, conducted for DG TREN, developing a new economic growth model and conducting an econometric study.

MSD / LASM
Article 82 (June 2004)
Competition economics analysis of impact of European Treaty article pertaining to abuse of a dominant position with reference to parallel trade.

PhRMA
Briefing notes and speech preparation for European LAWGs meeting (May 2004)
Work for European branches of US pharmaceuticals association on parallel trade.

CPRE
Analysis of the Barker Review of Housing Supply (January to March 2004)
Analysis of the Barker Review of Housing Supply. The final report of this study was discussed in all major quality UK press, was debated on the radio and TV (with the author interviewed several times) and featured in two UK Parliamentary debates.

Staff Working Paper
When might people pay too much for their housing? (February 2003)
Technical paper considering under what conditions people might pay more for their housing than, in retrospect, they would consider ideal.

EFPIA
Parallel trade with Accession States (November 2002)
Analysis of the effects of parallel trade in pharmaceuticals with Eastern Europe after new Accession States join EU.

UCL
Lecturing and Teaching (1999 - Sep 02)
Teaching Economics of European Integration to university undergraduates.

UCL
MSc. Dissertation (1998)
A Neo-Classical Growth model of the Romanian Economy. This was a mathematical model of a transition economy that identified the principal factors affecting the growth rate of the economy.

The Adam Smith Institute (International Division)
Macroeconomics Consultancy (1998)
Work for the Romanian government. Andrew produced two macroeconomic models for them. In April 1998 he forecast that the Romanian economy would contract noticeably in 1998. This countered most other forecasts at this time, including the official forecasts, which were all suggesting growth. But by June the official forecasts were projecting a 5 per cent contraction on the year.

The second model was a growth model which indicated the main factors explaining the growth performance of the Romanian economy and suggested ways in which growth might be improved.

In addition to producing these models he trained Romanian government staff in macroeconomics and the preparation of macroeconomic models.

Institute of Directors
International and Public Sector Economist (1997)
Economic policy analysis, responses to consultative documents, writing of Economic Comments. Main areas covered: EU Single Market, Common Agricultural Policy, Higher Education, Welfare, Local Government.

PHARMACEUTICALS

Wyeth Portugal
Reimbursement status of Enbrel in Portugal (December 2005 to September 2006)
Advocacy project considering the case for a change in the reimbursement status of a drug.

INFARMED
Redesign of Portuguese Pharmaceuticals Regulation (July 2004 to April 2005)
Redesigning the Portuguese co-participation and pricing systems for the government pharmaceuticals regulator.

MSD / LASM
Article 82 (June 2004)
Competition economics analysis of impact of European Treaty article pertaining to abuse of a dominant position.

PhRMA
Briefing notes and speech preparation for European LAWGs meeting (May 2004)
Work for European branches of US pharmaceuticals association.

Pfizer
Analysis of alternative pharmacist reimbursement schemes (January 2004)
Analysis of alternative pharmacist reimbursement schemes.

Merck Sharp & Dohme
NHS Reforms (August 2003)
Analysis of UK NHS reforms for an industry seminar.

EFPIA
Parallel trade with Accession States (November 2002)
Analysis of the effects of parallel trade in pharmaceuticals with Eastern Europe after new Accession States join EU.

Staff Working Paper
Risk-sharing pricing models in the distribution of pharmaceuticals (February 2003)
Technical paper analysing conditions under which payment-by-results contracts for pharmaceuticals could be advantageous.

SOME PUBLICATIONS

Six issues in pharmaceuticals — Economic Affairs, September 2006

Transforming the information environment in pharmaceuticals — Scrip Magazine, March 2004
The measure of inflation — Economic Affairs, March 2004
Calculated risk? — Parliamentary Monitor, November 2003
Virtuous price discrimination, pharmaceuticals, and parallel trade — Competition Law Insight, May 2003
Could deflation be ideal? — Economic Affairs, March 2003
Risk-sharing pricing models in the distribution of pharmaceuticals — Europe Economics Staff Working Paper, February 2003
When might people pay too much for their housing? — Europe Economics Staff Working Paper, February 2003
The role and regulation of short selling — Europe Economics Staff Working Paper, October 2002
The Liquidity Trap and Price-Level Targeting — Economic Affairs, June 2002
Ireland, The ECB, and the Maastricht Treaty — European Journal, March 2001
US Economic Success - Is it merely a Statistical Mirage?
— Capital Economics/Deloitte & Touche Economic Review, Dec. 2000
A price-stabilising fuel duty — eBow Brief, October 2000
Price-Level Targeting — Economic Affairs, June 2000
Rover, The Euro, and What If…? — European Journal, April 2000
When is it good to join a Customs Union? — European Journal, Midsummer 1999
Is Cyclical Convergence a Good Thing? — European Journal, May/June 1999
Can “Tax Competition” be harmful? — European Journal, Summer 1998
The End of the CAP? — European Journal, February 1998
Is money spent on students wasted? — The Independent, December 4 1997
Does the UK need more graduates? — IoD Economic Comment, September 1997
The Single Market - What more can be achieved? — IoD Economic Comment, August 1997
The BBC Budget Guide 1993 — IFS, February 1993
The IFS/Goldman Sachs Green Budget 1993 (part-author) — IFS, January 1993
Analysis of the 1993 Budget for the Daily Telegraph