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Smoking behaviour and individual well-being: a fresh look at the effects of the 2005 public smoking ban in Italy

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Abstract

This paper investigates the impact of the public smoking ban which came into effect in Italy in January 2005 on individual smoking behaviour. Current empirical evidence supports the existence of a negative effect of the Italian ban on smoking prevalence and consumption in the general population. Our analysis shows that the apparent success of the ban is due to the fact that existing results do not take into account seasonal differences in smoking behaviour. Using quarterly data from the 1999/2000 and 2004/2005 Italian Health Surveys and adopting a difference-in-difference approach that nets out monthly variation in smoking rates, we show that the Italian smoking ban had no impact on smoking behaviour for the population as a whole but only on some subgroups. This result notwithstanding, we find that the smoking ban increased the overall well-being of non-smokers.

JEL classifications: C31, I12, I18, K32.

1. Introduction

The increased awareness of the damage caused by tobacco smoking has led numerous countries to prohibit tobacco advertising and introduce partial or total bans on smoking in workplaces and—most recently—in all public areas. The main rationale behind these policies is that smoking bans reduce non-smokers' exposure to second-hand smoking and at the same time create a supportive environment for those who want to quit or decrease their tobacco consumption.[1]

1 Tobacco smoking is a well-known cause of several diseases, including lung cancer and cardiovascular and respiratory diseases (Doll *et al.*, 1994). Cigarette smoking is also considered the single most important modifiable factor affecting birth weight and the risk of preterm birth (Shiono and Behrman, 1995). Growing evidence indicates that both active and passive smoking affect cardiac problems (Law *et al.*, 1997; Raupach *et al.*, 2006) and increase the severity of asthma as well as the probability of developing this condition in adulthood (Stapleton *et al.*, 2011).

Several studies investigate the effect of smoking bans on smoking or its health consequences. Recent epidemiological evidence shows that public smoking bans in some US states and European countries have significantly reduced acute myocardial infarctions and asthma exacerbations (Barone-Adesi *et al.*, 2006; Juster *et al.*, 2007; Pell *et al.*, 2008), with some positive effects on birth outcomes (Mackay *et al.*, 2012). What appears to be less clear-cut, however, is whether public smoking bans have had a significant impact on individual smoking behaviour.[2] In Spain, Guerrero *et al.* (2011) find a lower than expected smoking prevalence one year after the implementation of a partial smoking ban. Evidence for Scotland shows a significant reduction in smoking prevalence 3–6 months before a law banning smoking in public places came into force (Mackay *et al.*, 2011). By contrast, studies conducted in the UK (Elton and Campbell, 2008; Lee *et al.*, 2011; Jones *et al.*, 2015), the Netherlands (Verdonk-Kleinjan *et al.*, 2011), Germany (Anger *et al.*, 2011), and Ireland (Mullally *et al.*, 2009) find no significant or limited impact of similar smoking policies on individual smoking habits.

In this paper we investigate the impact of the smoking ban in public places introduced in 2005 in Italy on individual smoking prevalence and cigarette consumption. From January 2005, the Italian government banned smoking in all indoor public places, including cafés, restaurants, airports, railway stations, as well as all public and private workplaces. Public support for the ban was widespread and enforcement was considered successful (Gallus *et al.*, 2006; Gorini, 2011). The existing empirical evidence strongly supports a negative short-run effect of the ban on both smoking prevalence and total consumption of cigarettes (Cesaroni *et al.*, 2008; Federico *et al.*, 2012; Buonanno and Ranzani, 2013). This evidence is mainly based on before-after comparisons.

We show that the existing evaluation results for Italy are not robust to alternative identification strategies. Using both aggregate data on cigarette sales and monthly data on individual smoking prevalence and consumption, we first document the existence of a seasonal pattern in smoking behaviour. We then show that netting out these seasonal effects dramatically affects previous evaluations of the smoking ban. Specifically, we find that the ban had no statistically significant effect on the general population, and some negative impacts on smoking prevalence and consumption only among specific subgroups, such as young never-married women.

Using the same data and the same empirical approach, we also provide new evidence about the welfare effects of the ban on the population of smokers and non-smokers. As a measure of well-being, we use an indicator derived from participants' responses to the SF-12 module of the Italian Health Survey, a battery of questions specifically designed to elicit satisfaction with physical and psychological health conditions (Ware *et al.*, 1996). Although the Italian smoking ban had a limited effect on overall smoking prevalence and cigarette consumption, it may still have had a significant impact on the well-being of non-smokers. This effect could be positive if the ban reduced exposure to second-hand smoking (Pell *et al.*, 2008; Meyers *et al.*, 2009), or negative if the ban reduced smoking in public areas and increased its prevalence in unregulated environments (Adda and Cornaglia,

2 Early studies in economics investigated the effect on smoking prevalence of workplace bans and found that these significantly reduced smoking prevalence and consumption among employed smokers (Evans *et al.*, 1999). However, bans on smoking in public places are intrinsically different from workplace bans in several respects. For example, a smoker will generally have more discretion over time spent in hospitality premises than over time spent in the workplace.

2010). A recent literature also argues that there could be positive effects of a smoking ban on smokers when the ban acts as a self-control device and individuals are time inconsistent (Odermatt and Stutzer, 2015; Leicester and Levell, 2016). Our results show that the ban had a positive effect on the well-being of the population of non-smokers, and that these effects are felt by many different subgroups of the population. By contrast, we find no effects of the ban on the well-being of smokers.

Our study offers a new, robust, and broader evaluation of the consequences of the smoking ban in Italy. We also make two substantive contributions to the literature on the effects of public health policies. First, our empirical strategy points out the importance of taking into account seasonality in smoking behaviour for the estimation of robust policy effects of anti-smoking policies. Specifically, our results suggest that in the presence of seasonal variation in smoking, a before/after approach might lead to biased estimates of the effect of interest and that using a short window of time around the cut-off date is no panacea. Similar issues arise in all studies where the outcome of interest exhibits seasonal variation, as for alcohol consumption (Cho *et al.*, 2001), mental health (Ayers *et al.*, 2013), or fertility (Buckles and Hungerman, 2013).

Second, we examine the welfare implication of the Italian smoking ban on the well-being of both smokers and non-smokers and for various subgroups of the population. Our results show positive welfare effects of the smoking ban in Italy. These appear to be modest in magnitude (about 4% of a standard deviation) and are only seen for non-smokers. However, while we observe changes in smoking behaviour mainly among young women who are single or not employed, the welfare benefits are felt across larger sections of the population of non-smokers, including married individuals and those in employment. This suggests that even though the ban had a limited effect on smoking prevalence and consumption, it might have changed smoking behaviour in a more general sense (when and where it is acceptable to smoke, for example), and this resulted in a significant improvement in general well-being.

2. The 2005 ban on smoking in public places in Italy

On 10 January 2005, a total ban on smoking in public places came into effect in Italy.[3] The ban prohibited smoking from enclosed workplaces and hospitality premises, including bars, cafés, restaurants, and clubs.

The ban received wide support both before and after its implementation (La Vecchia *et al.*, 2001; Gallus *et al.*, 2004; Gallus *et al.*, 2006). Enforcement controls showed that compliance was good, with fewer than 100 (1.5%) violations in about 6,000 checks by the police (Gallus *et al.*, 2006).

Several pieces of evidence suggest that the Italian ban was successful at reducing passive as well as active smoking. Barone-Adesi *et al.* (2006) find a significant decline in rates of hospital admission for acute myocardial infarction among individuals under 60 (11%), while Cesaroni *et al.* (2008) document a 4% decline in acute coronary events in the population under 70. Gallus *et al.* (2006) report a significant decline in both smoking prevalence (2.3%) and cigarette consumption (5.5%), which was particularly pronounced among women and young people.

3 The law was passed on 16 January 2003 (the so-called ‘Legge Sirchia’), but came into effect only 2 years later.

Although some of this evidence is based on relatively small samples (e.g., the survey used in Gallus *et al.*, 2006, has about 3,100 individuals), two recent studies by Federico *et al.* (2012) and Buonanno and Ranzani (2013) based on data from large national surveys also report statistically significant effects of the ban on individual smoking behaviour. Federico *et al.* (2012) analyse 11 waves from the Multiscopo Survey (conducted by ISTAT, the Italian Office of National Statistics, with a sample size of about 30,000 observations for each year), from 1999 to 2010 and show evidence of a decrease in smoking prevalence for men (2.6%) as well as an increase in quit rates for both men (3.3%) and women (4.5%) in the year immediately after the introduction of the ban. Similarly, Buonanno and Ranzani (2013) show that smoking prevalence decreased by 1.3% and the number of cigarettes smoked declined by almost 8% as a consequence of the ban. Their analysis is based on a sample of more than 120,000 individuals from the 2004–2005 Italian Health Survey (*Condizioni di salute e ricorso ai servizi sanitari*).

The main shortcoming of these studies is that these estimates may partially reflect seasonality in smoking behaviour. All waves of the Multiscopo Survey for the years up to 2004 are carried out during the last months of the calendar year (mainly November), while those for the period 2005–2010 are collected during the months of February and March. Clearly, any effect of the ban identified by Federico *et al.* (2012) is going to confound seasonal and policy-induced variation in smoking. The study by Buonanno and Ranzani (2013) is also vulnerable to seasonal effects since their results rely on a comparison between smoking prevalence and cigarette consumption measured in December 2004 (before the ban) *versus* March–September 2005 (after the ban). If smoking behaviour is subject to seasonal variation—due for example to tax increases, weather conditions, and timing of quitting efforts (e.g., New Year's resolutions)—and smoking incidence or cigarettes consumption is highest (lowest) during the last (first) months of the year, then the effect of the ban estimated by these studies is going to be larger than the true effect. In the next section, we will present our data and show evidence of seasonal effects in smoking behaviour. Section 4 proposes an empirical strategy which takes into account seasonal effects in smoking behaviour; in Section 5, we show that this leads to a very different evaluation of the effectiveness of the Italian smoking ban. Section 6 investigates broader welfare implications.

3. Our data

The Italian Health Survey (IHS) is a cross-sectional survey carried out approximately once every 5 years. In this paper we use data from 1999/2000 and 2004/2005. Interviews took place in the month of December of one year (1999 or 2004), and the months of March, June, and September of the following year (2000 or 2005), on a representative sample of households. The survey contains detailed information about the respondents' smoking status and cigarettes consumption, as well as a large amount of demographic information about the individual and the household.[4]

After excluding people under the age of 15 and over 65, our sample size reduces to 178,472 individuals (93,853 in 1999/2000 and 84,619 in 2004/2005), with more than

4 The IHS provides information on the age (in years) of smoking initiation and cessation. However, since the age of the respondent is also expressed in years, we cannot identify start or quitting date by month or quarter of the year. This makes impossible to study initiation or cessation with the data at hand. For an example of a study investigating smoking transitions, see DeCicca *et al.* (2008).

20,000 interviews in each month (Appendix Table A1). The data show a negative trend over time in smoking prevalence and consumption. This is consistent with official data documenting that sales of cigarettes dropped from 100.4 tons in 2000 to 92.8 tons in 2005, a decrease of 7.6%.

Appendix Table A2 shows the variation in smoking behaviour by gender and individual socio-economic characteristics. There is a large gender gap in smoking. Men are almost 50% more likely to smoke than women and consume on average twice the amount of cigarettes per day. Smoking is more prevalent in the Centre and the South, but for the South this is mainly due to high rates of smoking among men. The incidence is higher among single men, those with a low level of education, the young, and the employed. Among women, the main differences in terms of smoking incidence are by marital and activity status, with single and employed women exhibiting the highest rates. By contrast, there is not much of a gap according to education level and smaller differences by age.

Figure 1 shows average smoking rates for each month in the 1999/2000 and 2004/2005 waves of the IHS. Although the confidence intervals around each point indicate that the differences within a year are mostly insignificant from a statistical point of view, there is a clear seasonal pattern which is repeated over time. Smoking is almost always more prevalent in the months of December and September than in the months of March or June.

Although it is possible that the decrease in smoking rates between December 2004 and March 2005 was a consequence of the smoking ban, the fact that there was a similar decline between the months of December 1999 and March 2000 is an important piece of evidence to consider. Performing an analysis on the 2004/2005 data alone in the absence of a

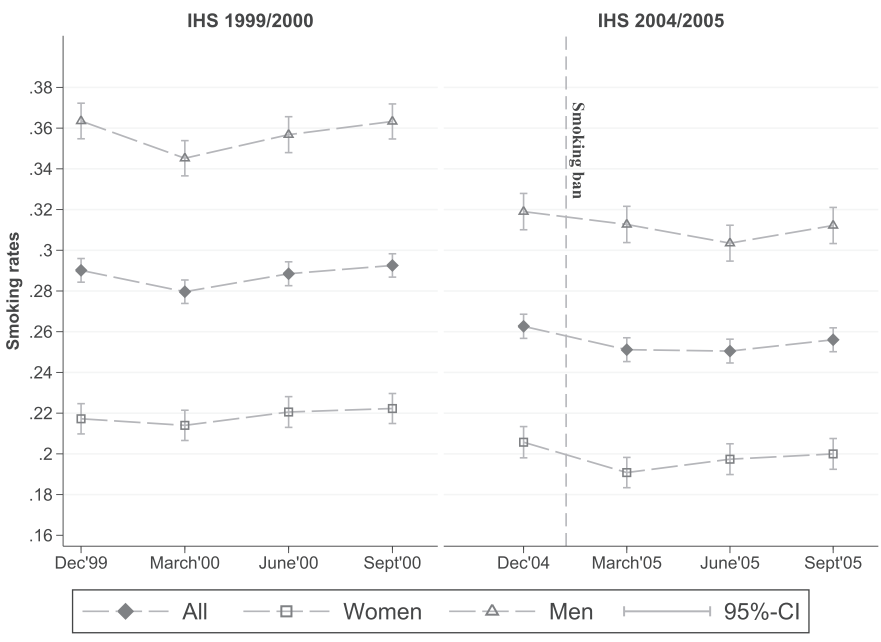

Fig. 1. Mean smoking rates by month

Source: Italian Health Survey for 1999/2000 and 2004/2005. Sample restricted to individuals aged 15–65.

Notes: All data points and confidence intervals (vertical lines) are calculated using sampling weights.

control group and in the presence of seasonal effects might lead to overestimating the effect of the ban. Similar considerations apply to cigarette consumption data (Appendix Fig. A1) and to the proportion of heavy smokers, i.e. those smoking 10 or more cigarettes per day (Appendix Fig. A2).

These graphs also indicate that female monthly smoking patters are different from those of males and exhibit less seasonal variation in the period preceding the implementation of the ban (1999/2000 wave). Further analysis by subgroups (defined according to age, level of education, etc.) indicates that there is substantial heterogeneity in seasonal smoking patterns. It will therefore be particularly important to examine heterogeneity in the effects of the ban.

The seasonal patterns in smoking shown in Fig. 1 and Appendix Figs A1 and A2 are also reflected in aggregate data on cigarettes sales shown in Fig. 2 and may have several explanations. For example, there might be a correlation between average temperatures and smoking, as people tend to visit hospitality venues more frequently in the spring than in the winter. A second possibility is that the price of cigarettes is subject to seasonal variation due to regular changes in excise duty (Fig. 3). Alternatively, the decline in smoking between December and March could be the result of New Year's resolutions, whereby some people might decide to quit smoking and/or adopt healthier behaviours. These are alternative hypotheses that we will take into account in Section 5.4.

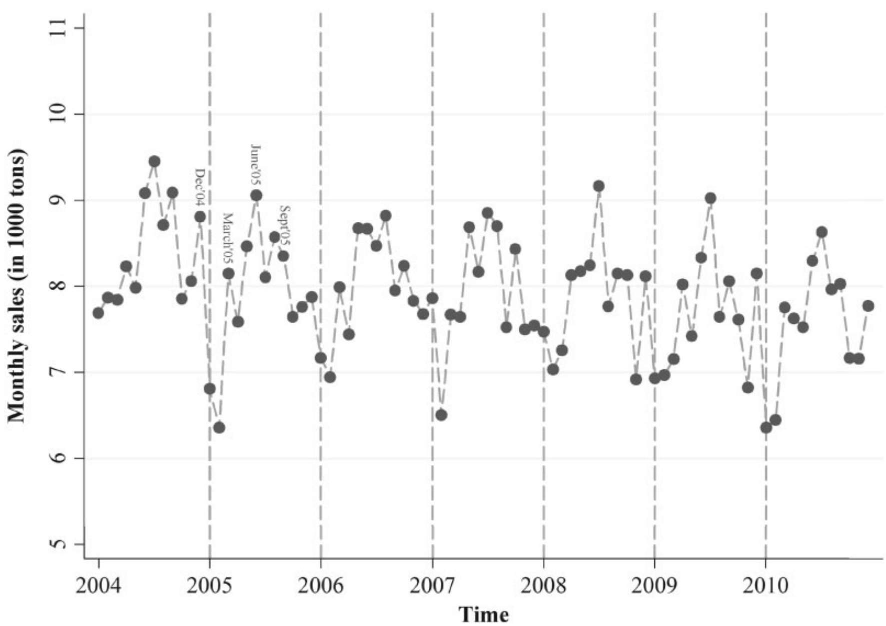

Fig. 2. Sales of cigarettes by month
Source: ISTAT. Statistical Yearbooks (*Annuario Statistico Italiano*) 2004–2010.
Notes: The figure includes the quantity of tobacco products transferred from the depositary warehouses to warehouses of distributors.

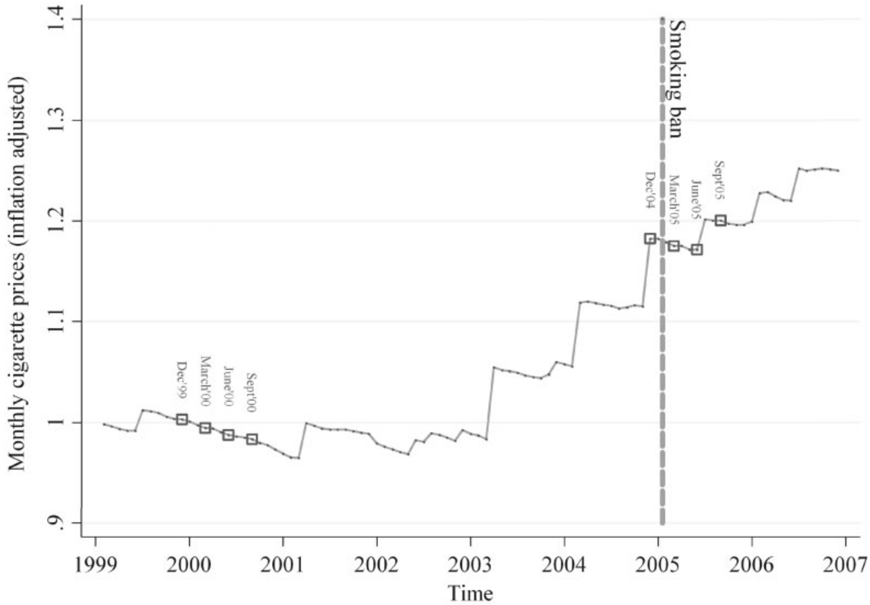

Fig. 3. Price of cigarettes
Source: ISTAT. Consumer Price Statistics (*Servizio delle statistiche ai prezzi al consumo*).
Notes: Prices are inflation adjusted and normalized to the level observed in January 1999.

4. Empirical model

In order to estimate the effects of the smoking ban, we start by estimating the following regression using the 2004/2005 Italian Health Survey, which interviews individuals in December 2004 and March, June, and September 2005:[5]

$$Y_{it} = \alpha + \beta D_{it} + \mu X_{it} + u_{it} \tag{1}$$

where Y_{it} represents the smoking status or the number of cigarettes smoked by individual i at time t, and the variable D assumes value 0 for all individuals interviewed before January 2005 (control group) and value 1 for those interviewed afterwards (treatment group), thus capturing the effect of the smoking ban. The covariates in X include: age, age squared, household size, an indicator variable for being female, the presence of children of age below 8 in the household, an indicator variable for being married, a full set of dummy variables for educational attainment (elementary school or less, junior high school diploma, high school diploma, and missing information on educational attainment), being employed, being inactive, having household economic conditions that are adequate or excellent, and a full set of regional dummies.

The key identifying assumption here is that in the absence of the ban (the treatment) the difference in smoking behaviour between individuals in the control and treatment groups is not statistically significant from zero. However, there are at least two plausible reasons why this assumption might not hold. The first is that we might be in the presence of

5 This approach is the one followed by most analyses of the Italian smoking ban, such as the one performed by Buonanno and Ranzani (2013).

long-term trends in smoking behaviour which, especially if negative, might lead us to overestimate the impact of the ban. The second problem is that smoking, like many other activities, might exhibit seasonal variation which if not taken into account might confound the effect of the ban.

One way to overcome the first problem is to restrict the analysis to a small interval before and after the introduction of the ban—effectively comparing smoking in March 2005 to that observed in December 2004. However, while this strategy might be an effective way of dealing with the presence of a long-term trend, it makes the estimate more vulnerable to the presence of seasonal effects.

One possible strategy to control for seasonal effects is to combine variation in smoking between December 2004 and March 2005 with variation in smoking between December 1999 and March 2000 in one single regression. This is a straightforward difference-in-difference estimation where the 'treatment' is now defined across periods rather than people. The estimating equation is of the form:

$$Y_{it} = \alpha + \beta D_{it} + \gamma S_{it} + \delta D_{it} \times S_{it} + \mu X_{it} + u_{it} \quad (2)$$

where S_{it} is a variable which takes value 1 if the individual is observed in 2004–2005 and value 0 if he or she is observed in 1999–2000, and D_{it} is—as previously—a dummy that takes the value 1 if the individual is interviewed in March (June or September) and 0 if he or she is interviewed in December. The coefficient γ captures the effect on Y of time, i.e. general changes in the economic and social context across the two waves. Notice that the coefficient β picks up differences in smoking behaviour between March (June or September) and December, independently of the year of interview, i.e. it represents seasonal effects. The coefficient of interest is δ, because it captures the differential impact of the smoking ban on individuals interviewed before and after the reform net of possible seasonal effects.

To account for seasonality, we need to assume the existence of a common trend in the treatment and control groups in the pre-treatment period. In our context, this is equivalent to assuming that the trend in smoking consumption or prevalence in the years before 1999/2000 (control group) is the same or very similar to the trend in the years before 2004/2005 (treated group). In order to check this is true, we need annual data on smoking consumption and prevalence over a longer horizon.

The main source of annual data on smoking consumption and prevalence comes from the *Indagine Multiscopo Aspetti della Vita Quotidiana*, an annual cross-sectional survey on a representative sample of the Italian population which starts in 1993. Appendix Fig. A3 shows smoking prevalence for all individuals aged 14+ by gender. As we can see, smoking prevalence has been declining among men and has been fairly constant among women over the period 1993–2010. Similar trends are shown in Gualano *et al.* (2014), who use data from a different survey carried out by the National Institute of Health.

5. Results

5.1 Estimates based on the before/after approach

We now show that if seasonal differences in smoking behaviour are not adequately controlled for, a before/after evaluation of new regulations on smoking behaviour will lead to incorrect estimates of the size of the effects. In order to demonstrate this is the case, we first discuss the results obtained in the previous literature for both the short and the medium term using specification (1).

The short-term estimates are obtained comparing individual smoking in March 2005 against December 2004 and are reported in columns (1)–(3) of Table 1. All estimates are performed using linear regressions, even when the outcome is binary. All standard errors are clustered at the household level to take into account potential correlation in smoking behaviour among individuals living in the same household.[6]

The estimates show that the short-term effect of the ban is negative across the whole population, with a decrease of 1.2 percentage points in smoking prevalence (or a 4.7% reduction w.r.t. the 2004/2005 overall incidence), a 0.27 reduction in the number of cigarettes smoked (7.6%), and a reduction of 1.3 percentage points in the incidence of heavy smokers, here identified by the proportion of people smoking more than 10 cigarettes per day (6.9%). The medium-term effects, obtained by comparing smoking behaviour in March, June, and September 2005 against December 2004, are less pronounced, but equally statistically significant.

Analysis by subgroups shows that the effect of the ban was relatively stronger among women than men (6.1% for women and 3.8% for men, as obtained by dividing the coefficients in Table 1 by the mean prevalence by gender shown in Appendix Table A2) and mainly concentrated among single individuals. Most notably, single, low-educated, young, as well as young and single individuals are the groups most affected by the ban both in terms of prevalence and intensity. These results are not surprising since young and single individuals tend to visit hospitality venues more often. Differences by level of education are more difficult to explain, but could be consistent with the fact that a larger proportion of low-educated individuals work in the hospitality sector (in our data, 20.6% of all low-educated individuals are employed in this sector, against 16.3% of high-educated individuals).[7] Corresponding medium-term estimates are very similar.

5.2 Estimates based on a DiD approach

We now estimate the model using specification (2), which nets out seasonal effects using the variation in smoking behaviour observed over a period not affected by the ban, i.e. the months of March 2000 and December 1999. In other words, we use data from the 1999–2000 IHS to construct a control group.[8]

6 We also performed all our regressions using non-linear estimators. Specifically, we used a probit model for the proportion of smokers and heavy smokers, and a poisson regression for the number of cigarettes smoked. All the results reported here and in the following tables are robust to these checks.

7 Previous evidence on whether the effects of smoking control policies differ by education level is scarce, with most of the analyses conducted either on the general population (MacKay *et al.*, 2011) or on very specific subgroups (e.g., Mullally *et al.*, 2009).

8 We conduct extensive checks to verify that the 1999–2000 survey offers a valid control group for the 2004–2005 survey. In particular, we test for the presence of significant differences in the characteristics (e.g., age, gender, level of education, geographic distribution, household composition and economic position, etc.) of the sample of individuals interviewed in December and March, respectively, in the two surveys. We can find none. We also test for the presence of statistically significant differences in the December vs. March change in these characteristics across surveys (in the spirit of our DiD empirical specification). Again, we cannot see any significant change in the underlying composition of the sample.

Table 1. Smoking: Before/after estimates

	Short-term			Medium-term		
	Smoker[a] (1)	#Cig[b] (2)	Cig10+[c] (3)	Smoker (4)	#Cig (5)	Cig10+ (6)
All	–0.012*	–0.267*	–0.013*	–0.010**	–0.168*	–0.010**
	(0.005)	(0.080)	(0.004)	(0.004)	(0.066)	(0.003)
N	42,255	41,128	41,128	84,619	82,333	82,333
Subgroups:						
Male	–0.012	–0.323*	–0.015	–0.011*	–0.214*	–0.013**
	(0.007)	(0.124)	(0.006)	(0.005)	(0.102)	(0.005)
Female	–0.012	–0.215*	–0.011	–0.008	–0.119	–0.006
	(0.006)	(0.080)	(0.005)	(0.005)	(0.067)	(0.004)
Married	–0.007	–0.234	–0.010	–0.005	–0.111	–0.007
	(0.006)	(0.105)	(0.005)	(0.005)	(0.087)	(0.004)
Single	–0.018	–0.306*	–0.016*	–0.015**	–0.236*	–0.013**
	(0.007)	(0.114)	(0.006)	(0.006)	(0.094)	(0.005)
Low educated	–0.017*	–0.313*	–0.018*	–0.015**	–0.192*	–0.015**
	(0.006)	(0.112)	(0.006)	(0.005)	(0.092)	(0.005)
High educated	–0.006	–0.187	–0.006	–0.003	–0.119	–0.003
	(0.006)	(0.104)	(0.006)	(0.005)	(0.087)	(0.005)
Age 15–39	–0.016	–0.260	–0.012	–0.016**	–0.237**	–0.011*
	(0.006)	(0.102)	(0.006)	(0.005)	(0.085)	(0.005)
Age 40–65	–0.009	–0.277	–0.014	–0.004	–0.106	–0.008
	(0.006)	(0.112)	(0.006)	(0.005)	(0.092)	(0.005)
Young&Single	–0.022*	–0.365*	–0.017	–0.019**	–0.307**	–0.015**
	(0.008)	(0.119)	(0.007)	(0.006)	(0.100)	(0.006)
Employed	–0.014	–0.291*	–0.014	–0.009	–0.152	–0.009
	(0.006)	(0.109)	(0.006)	(0.006)	(0.090)	(0.005)
Not employed	–0.009	–0.224	–0.011	–0.009*	–0.166*	–0.009*
	(0.006)	(0.100)	(0.005)	(0.005)	(0.083)	(0.004)
Young&Not empl.	–0.014	–0.276	–0.012	–0.017*	–0.318**	–0.011*
	(0.009)	(0.127)	(0.007)	(0.007)	(0.107)	(0.006)

Source: Italian Health Survey for 2004/2005. Sample restricted to 15–65 years old.
Notes: [a]Ratio of smokers to total population; [b]Mean number of daily cigarettes smoked (0 for non-smokers); [c]Ratio of individuals smoking 10 or more cigarettes per day to total population. Each number represents a separate estimate of the coefficient β in eq. (1), and indicates whether the individual was interviewed after the smoking ban came into effect. Columns 1–3 report results of the short-term estimates, where we compare individuals interviewed in March 2005 with those interviewed in December 2004; columns 4–6 refer to medium-term estimates, obtained by comparing individuals interviewed in March, June and September 2005 with those interviewed in December 2004. In columns (2) and (3), and (5) and (6), we exclude from our sample individuals who smoke but do not provide valid information on the number of cigarettes smoked. All estimates are obtained via a linear probability model and standard errors (in parentheses) are clustered at the household level, with * $p < 0.05$, ** $p < 0.01$. The covariates include: age, age squared, household size, indicator variables for (i) being female, (ii) the presence of children of age below 8 in the household, (iii) being married, (iv) educational attainment (elementary school or less, junior high school diploma, high school diploma and missing information on educational attainment), (v) being employed, (vi) being inactive, (vii) having household economic conditions which are adequate or excellent, and a full set of regional dummies.

Table 2. Smoking: Short-term difference-in-difference estimates

	All			Men			Women		
	Smoker [a] (1)	#Cig[b] (2)	Cig10+[c] (3)	Smoker (4)	#Cig (5)	Cig10+ (6)	Smoker (7)	#Cig (8)	Cig10+ (9)
All	–0.007	–0.137	–0.009	–0.003	–0.062	–0.007	–0.012	–0.225*	–0.012
	(0.007)	(0.114)	(0.006)	(0.009)	(0.178)	(0.009)	(0.008)	(0.112)	(0.006)
N	88,988	87,722	87,722	44,112	43,344	43,344	44,876	44,378	44,378
Subgroups:									
Married	–0.000	–0.017	–0.003	0.001	0.076	–0.002	–0.002	–0.119	–0.004
	(0.008)	(0.150)	(0.007)	(0.012)	(0.245)	(0.011)	(0.009)	(0.137)	(0.008)
Single	–0.016	–0.309	–0.018*	–0.009	–0.261	–0.014	–0.025*	–0.376*	–0.024*
	(0.010)	(0.160)	(0.009)	(0.014)	(0.246)	(0.013)	(0.012)	(0.178)	(0.010)
Low educated	–0.014	–0.184	–0.016*	–0.017	–0.216	–0.023	–0.013	–0.183	–0.010
	(0.008)	(0.155)	(0.008)	(0.012)	(0.248)	(0.012)	(0.010)	(0.151)	(0.009)
High educated	0.002	–0.062	–0.001	0.015	0.119	0.011	–0.010	–0.255	–0.014
	(0.009)	(0.155)	(0.008)	(0.013)	(0.246)	(0.012)	(0.012)	(0.162)	(0.010)
Age 15–39	–0.012	–0.197	–0.012	–0.009	–0.139	–0.011	–0.016	–0.291*	–0.014
	(0.009)	(0.143)	(0.008)	(0.013)	(0.227)	(0.012)	(0.011)	(0.145)	(0.009)
Age 40–65	–0.003	–0.069	–0.007	0.001	–0.013	–0.005	–0.007	–0.139	–0.010
	(0.009)	(0.163)	(0.008)	(0.012)	(0.263)	(0.012)	(0.010)	(0.160)	(0.009)
Young&Single	–0.022*	–0.392*	–0.021*	–0.018	–0.341	–0.019	–0.029*	–0.485**	–0.025*
	(0.011)	(0.168)	(0.009)	(0.015)	(0.253)	(0.014)	(0.014)	(0.185)	(0.011)
Employed	–0.002	–0.019	–0.006	0.007	0.170	0.002	–0.016	–0.318	–0.020
	(0.009)	(0.164)	(0.008)	(0.011)	(0.225)	(0.011)	(0.013)	(0.186)	(0.011)
Not employed	–0.011	–0.247	–0.012	–0.020	–0.519	–0.026	–0.008	–0.141	–0.006
	(0.008)	(0.137)	(0.007)	(0.015)	(0.276)	(0.013)	(0.009)	(0.135)	(0.008)
Young&Not empl.	–0.021	–0.347*	–0.018	–0.024	–0.363	–0.027	–0.022	–0.414*	–0.018
	(0.012)	(0.171)	(0.010)	(0.020)	(0.315)	(0.017)	(0.014)	(0.182)	(0.011)

Source: Italian Health Survey for 1999/2000 and 2004/2005. Sample restricted to 15–65 years old.
Notes: [a]Ratio of smokers to total population; [b]Mean number of daily cigarettes smoked (0 for non-smokers); [c]Ratio of individuals smoking 10 or more cigarettes per day to total population. Each number represents a separate estimate of the coefficient δ in eq. (2), which captures the effect of the ban net of seasonal effects. Estimates refer to short-term effects, obtained by comparing individuals interviewed in March with those interviewed in December. In columns (2) and (3), (5) and (6), and (8) and (9), we exclude from our sample individuals who smoke, but who do not provide valid information on the number of cigarettes smoked. All estimates are obtained via a linear probability model and standard errors (in parentheses) are clustered at the household level, with $^{*}p < 0.05$, $^{**}p < 0.01$. For main covariates, see notes to Table 1.

Comparing the results in Table 1 and Table 2, it is immediately obvious that by applying a DiD strategy a large part of the impact of the ban found in the previous analysis is washed out. Table 2 shows that the short-run effects of the ban are much smaller in magnitude and no longer statistically significant for the overall population and for men in particular. For women, there is some evidence that the negative effects of the ban on smoking are still present, although these are not always as precisely estimated as before. Similar results hold for the medium-term effects (see Table 3), where we see no change in smoking incidence in the population as a whole and in male smoking behaviour, and only some effects on the smoking intensity for women.

Table 3. Smoking: Medium-term difference-in-difference estimates

	All			Men			Women		
	Smoker [a] (1)	#Cig[b] (2)	Cig10+[c] (3)	Smoker (4)	#Cig (5)	Cig10+ (6)	Smoker (7)	#Cig (8)	Cig10+ (9)
All	–0.008	–0.174	–0.009	–0.005	–0.140	–0.008	–0.011	–0.205*	–0.010
	(0.005)	(0.094)	(0.005)	(0.007)	(0.147)	(0.007)	(0.006)	(0.093)	(0.005)
N	178,472	175,892	175,892	88,391	86,793	86,793	90,081	89,099	89,099
Subgroups:									
Married	–0.000	–0.029	–0.002	0.001	–0.004	–0.002	–0.001	–0.058	–0.003
	(0.007)	(0.124)	(0.006)	(0.010)	(0.203)	(0.009)	(0.008)	(0.114)	(0.007)
Single	–0.018*	–0.371**	–0.018**	–0.011	–0.312	–0.015	–0.026*	–0.420**	–0.021*
	(0.008)	(0.132)	(0.007)	(0.011)	(0.203)	(0.010)	(0.010)	(0.148)	(0.009)
Low educated	–0.020**	–0.299*	–0.019**	–0.019	–0.282	–0.022*	–0.021**	–0.317*	–0.015*
	(0.007)	(0.127)	(0.006)	(0.010)	(0.205)	(0.010)	(0.008)	(0.125)	(0.007)
High educated	0.008	0.012	0.004	0.014	0.066	0.010	0.002	–0.051	–0.003
	(0.008)	(0.128)	(0.007)	(0.011)	(0.203)	(0.010)	(0.010)	(0.135)	(0.008)
Age 15–39	–0.012	–0.260*	–0.012	–0.010	–0.179	–0.010	–0.014	–0.343**	–0.013
	(0.007)	(0.118)	(0.006)	(0.011)	(0.188)	(0.010)	(0.009)	(0.120)	(0.007)
Age 40–65	–0.004	–0.081	–0.006	–0.001	–0.112	–0.006	–0.008	–0.062	–0.006
	(0.007)	(0.135)	(0.006)	(0.010)	(0.218)	(0.010)	(0.008)	(0.133)	(0.007)
Young&Single	–0.023*	–0.435**	–0.020*	–0.017	–0.319	–0.015	–0.029*	–0.577**	–0.026**
	(0.009)	(0.139)	(0.008)	(0.012)	(0.209)	(0.011)	(0.012)	(0.154)	(0.009)
Employed	–0.001	–0.039	–0.004	0.003	0.016	–0.001	–0.006	–0.136	–0.010
	(0.007)	(0.135)	(0.007)	(0.009)	(0.186)	(0.009)	(0.010)	(0.155)	(0.009)
Not employed	–0.015*	–0.293**	–0.013*	–0.018	–0.423	–0.021	–0.014	–0.229*	–0.009
	(0.007)	(0.112)	(0.006)	(0.012)	(0.228)	(0.011)	(0.008)	(0.112)	(0.006)
Young&Not empl.	–0.024*	–0.484**	–0.019*	–0.023	–0.391	–0.022	–0.027*	–0.567**	–0.020*
	(0.010)	(0.142)	(0.008)	(0.016)	(0.260)	(0.014)	(0.011)	(0.154)	(0.009)

Source: Italian Health Survey for 1999/2000 and 2004/2005. Sample restricted to 15–65 years old.
Notes: [a]Ratio of smokers to total population; [b]Mean number of daily cigarettes smoked (0 for non-smokers); [c]Ratio of individuals smoking 10 or more cigarettes per day to total population. Each number represents a separate estimate of the coefficient δ in eq. (2), which captures the effect of the ban net of seasonal effects. Estimates refer to medium-term effects, obtained by comparing individuals interviewed in March, June and September with those interviewed in December. In columns (2) and (3), (5) and (6), and (8) and (9), we exclude from our sample individuals who smoke, but who do not provide valid information on the number of cigarettes smoked. All estimates are obtained via a linear probability model and standard errors (in parentheses) are clustered at the household level, with $^{*}p < 0.05$, $^{**}p < 0.01$. For main covariates, see notes to Table 1.

Looking at the analysis by subgroups is very informative. There is a clear indication that the effects are restricted to very specific sections of the population. Table 2 shows that in the short term, most of the effects are concentrated among individuals who are young and single, with some effects on the number of cigarettes smoked by the young and not employed. Estimates of medium-term effects (Table 3) indicate that the ban reduced smoking also among single, low-educated, and not employed individuals more generally, and that we can attribute most of these effects to women. The most pronounced short-term effects are to be found for young and single women, where the incidence of smoking is reduced by 2.9 percentage points (–14.0%) and the intensity falls by almost 0.5 cigarettes a day (–23.6%). Also the group of young and not employed women appears to be affected by the

Table 4. Seasonality in tobacco consumption or sales over the years

	Tobacco expenditure Q4/1985–Q3/1996 (1)	Tobacco sales Q4/2005–Q3/2010 (2)
Q1	–0.057**	–0.079**
	(0.014)	(0.023)
Q2	–0.044**	0.041
	(0.014)	(0.023)
Q3	–0.028*	0.071**
	(0.014)	(0.023)
Control for cohort effects	Yes	Yes
Control for regions	Yes	No
N	391,407	60

Sources: The dependent variable of column (1) is the logarithm of tobacco expenditure from October 1985 to September 1996 according to the Italian Consumption Survey (*Indagine sui consumi delle famiglie*). The dependent variable of column (2) is the logarithm of official tobacco sales (in 1000 tons) in Italy from October 2005 to September 2010.
Notes: Standard errors in parenthesis, with $^{*}\ p < 0.05$, $^{**}\ p < 0.01$. Both equations control for cohort effects, which are meant to capture long-term trends in a flexible way. The variables Q1, Q2, and Q3 refer to the first three quarters of the year. Therefore, the last quarter (of the previous year) serves as reference category.

ban. This is consistent with the fact that these are the groups most likely to visit hospitality venues and that women are more likely to show compliance to rules than are men.[9] The same results hold when looking at the interaction of the smoking ban effect with individual characteristics in a fully interacted model (not shown).

5.3 Robustness checks

As discussed above, our DiD estimator represents a way to correct for seasonal variation in smoking behaviour. Another approach to address the impact of seasonality on before/after estimates of the smoking ban is to look at monthly variation in (i) tobacco expenditure recorded in the Italian consumer survey (*Indagine sui consumi delle famiglie*), and (ii) cigarette sales derived from national data. Both data sources have drawbacks. The data from the Italian consumer survey have been seasonally adjusted as from 1997, so that we can only consider years between 1985 and 1996. Monthly data on official tobacco sales are available only from 2004 (here we will use information from 2005—after the smoking ban—to 2010) and may not reflect actual consumption, as they do not take into account illicit sales and hoarding behaviour (Momperousse *et al*., 2007).

The regressions shown in Table 4 confirm the existence of a considerable amount of seasonal variation in these data. Both per capita consumption of tobacco and the value of cigarette sales are 6 to 8% lower in the first quarter of one year compared to the last quarter of the previous year, which in our data would translate into a reduction in the number of cigarettes due to seasonal effects of between 0.21 and 0.28 per day (the number of

9 For example, some studies have found that women show more tax compliance than men (see Kastlunger *et al.*, 2010), they are less likely to engage in drunk driving (Scott-Parker *et al.*, 2014), and among pedestrians, males violate more rules than females (Tom and Granié, 2011), already at a young age (Granié, 2007).

Table 5. Smoking: Placebo test

	All			Men			Women		
	Smoker [a] (1)	#Cig[b] (2)	Cig10+[c] (3)	Smoker (4)	#Cig (5)	Cig10+ (6)	Smoker (7)	#Cig (8)	Cig10+ (9)
Ban	0.000	–0.115	0.000	0.000	–0.225	–0.003	0.001	0.012	0.003
	(0.007)	(0.115)	(0.006)	(0.009)	(0.180)	(0.009)	(0.008)	(0.113)	(0.0007)
N	88,440	87,134	87,134	43,803	42,997	42,997	44,637	44,137	44,137

Source: Italian Health Survey for 1999/2000 and 2004/2005. Sample restricted to 15–65 years old.
Notes: [a]Ratio of smokers to total population; [b]Mean number of daily cigarettes smoked (0 for non-smokers); [c]Ratio of individuals smoking 10 or more cigarettes per day to total population. Each number represent a separate estimate of the coefficient δ in eq (2) obtained comparing March 2000–June 2000 vs March 2005–June 2005. In columns (2) and (3), (5) and (6), and (8) and (9), we exclude from our sample individuals who smoke, but who do not provide valid information on the number of cigarettes smoked. All estimates are obtained via a linear probability model and standard errors (in parentheses) are clustered at the household level, with $^{*} p < 0.05$, $^{**} p < 0.01$. For main covariates, see notes to Table 1.

cigarettes consumed is about 3.6 per day in 2004–2005). As the before/after estimates of the ban is –0.267 (as shown in Table 1, column 2), this implies that a very large part of the effect could be in fact attributed to the season. Indeed, a before/after estimate purged of seasonal effects using these estimates would range between –0.057 and +0.013, showing small to no effects of the ban on the number of cigarettes smoked, which is consistent with our results.

Another way of showing that our DiD estimator is correctly capturing seasonal variation in smoking is to perform a *placebo* test by comparing the change in smoking between the months of March 2005 and June 2005 with the change in smoking between the months of March 2000 and June 2000. If our DiD strategy is adequately capturing seasonal variation in smoking, and this is stable across time, we would expect to find completely insignificant coefficients in this case. As anticipated, the DiD estimate on smoking prevalence is virtually zero in terms of magnitude and statistically insignificant (Table 5). This is true for the general population, but also for specific subgroups of individuals, such as young and single women, for whom we did find some effects of the smoking ban (not shown).

5.4 Seasonality in smoking behaviour

We now consider whether the seasonal effect in smoking behaviour is due to New Year's resolutions, price changes, or climate. First, we investigate the hypothesis that the March vs. December effect is due to New Year's resolutions (Norcross *et al.*, 2002). Typical examples of New Year's resolutions (besides quitting smoking) are going on a diet, joining a gym, or starting some (heavier) form of physical exercise. We use information on diet and physical exercise available in the IHS. We construct a variable indicating whether an individual is on a diet (excluding a diet prescribed by a physician) and a variable which indicates whether the individual carries out regular physical exercise. We then estimate the effect of the ban on these two outcomes.[10] The coefficient δ in eq. (2) can be interpreted

10 The proportion of individuals aged 15–65 who declare themselves to be on a diet (not for medical reasons) is 5.5 (overall), 3.9 for men, and 7.1 for women. The proportion of those indicating they carry out regular physical activity is 52.2 overall, 55.8 for men, and 48.7 for women.

here as the New Year effect rather than post-ban effect. Estimates of the model in eq. (1) show no significant changes in the prevalence of diet or physical exercises with the New Year. Only for women do we find some evidence that they tend to practice more sports in March than in December. Using a DiD approach, as per eq. (2), even this effect disappears.

Second, we consider whether changes in cigarette prices might explain seasonal patterns in consumption. Cigarettes prices in Italy are regulated at the national level. Figure 3 reports the time series of cigarette price over the period 1999–2007. We see sharp increases in real cigarette prices, due to changes in excise duty, usually followed by gradual declines, due to inflation. As excise duty is usually set after the budget is announced, and this usually happens at the same time within each calendar year, changes in cigarette prices could contribute to explain seasonal variation in smoking prevalence and consumption (Momperousse *et al.*, 2007). However, because of Italy's rather unpredictable political cycle and multiple budget announcements within a year, we think that this is unlikely. Indeed, we see irregularly spaced changes in cigarettes prices. So, it is very improbable that the seasonal pattern in smoking documented in Fig. 1 and Appendix Figs A1 and A2 is caused by changes in prices.

Finally, we check for the presence of climatic effects on smoking behaviour. There might be a correlation between average temperatures and smoking, as people tend to visit hospitality venues more frequently in the spring than in the winter.[11] We use data on average temperatures and rainfall by month and region.[12] We find that these variables explain a substantial amount of seasonal variation in smoking behaviour. Substituting average temperature and rainfall for the ban variable in eq. (1), we calculate that climate alone explains 50–80% of the reduction in overall smoking prevalence and intensity that Buonanno and Ranzani (2013) attributed to the smoking ban. The remaining 20–50% not explained by climate is consistent with the magnitude of our DiD estimates. As a further check, we add the two climatic variables to our DiD specifications. Our main findings do not change significantly.[13]

6. Welfare effects

Did the smoking ban have effects other than changes in smoking prevalence or cigarette consumption? In this section, we investigate whether the introduction of the ban had welfare implications by looking at measures of individual well-being. A small but rapidly expanding literature has investigated the effects of anti-smoking policies on individual well-being. This literature usually focuses either on the effects for smokers or on the effects for non-smokers, and its findings are still very mixed.

Anti-smoking policies may have an effect on smokers' well-being when smokers hold time-inconsistent preferences (O'Donoghue and Rabin, 1999; Gruber and Köszegi, 2001),

11 In the IHS of 1999/2000, respondents reported whether they visited a club in the previous quarter of the year. In December, 25.1% answered with 'yes', while in March (27.5%), June (25.8%), and September (29.3%); the ratio was on average 27.5%.

12 The data are downloaded from http://www.ilmeteo.it/portale/archivio-meteo.

13 We can identify the effects of temperatures and rainfall from the effect of the ban because the former vary by month and region while the ban only varies by month. In other words, this check relies on the assumption that the effect of the smoking ban is homogeneous across regions.

and the policy change acts as a self-control device that helps individuals reconcile short-term and long-term goals. Support for this hypothesis can be found in Gruber and Mullainathan (2005), who show that higher taxes improve self-reported well-being of individuals with a higher propensity to smoke compared with those with a lower propensity to smoke. By contrast, Leicester and Levell (2016) show no significant association between smoking bans and individual well-being in the UK. Odermatt and Stutzer (2015) find that smoking bans in Europe are welfare enhancing only for individuals who would like to quit smoking.

As for the effects on non-smokers, these could be positive if the ban reduces exposure to second-hand or passive smoking with positive effects on health or general well-being (Pell *et al*., 2008; Meyers *et al*., 2009), or negative if the ban reduces smoking in public areas at the expense of smoking in unregulated private places, such as the home (Adda and Cornaglia, 2010). Here again the evidence is rather mixed. Odermatt and Stutzer (2015) find no significant effect of smoking bans on the well-being of non-smokers, while Yang and Zucchelli (2015) finds large welfare effects for both smokers and non-smokers, particularly among married couples with children. Interestingly, the latter study shows smaller but still statistically significant effects of the UK bans on non-smokers married to other non-smokers.

In order to investigate the welfare implication of the smoking ban, we use a mental well-being indicator derived from the SF-12 module of the IHS. The SF-12 is a short battery with 12 questions selected from a longer instrument (the SF-36) introduced in the USA during the 1980s (Ware *et al*., 1996) to elicit self-reported measures of mental and physical health. We combine the answers to the SF-12 questions using principal component analysis. This reveals the existence of two latent variables. The first variable, which is mostly correlated to questions measuring physical well-being, explains about 48% of the overall variance in the SF-12, while the second, which constitutes our mental well-being variable, explains an additional 13%.[14]

Like smoking, our well-being measure exhibits seasonal variation (Fig. 4). Specifically, we see that well-being is usually significantly higher during June than in December or March. The pattern is less clear for the month of September, where in the year 2005 well-being appears to continue an upward trend. This may be an effect attributable to the ban, but also to time-varying factors, such as weather conditions, which have been shown to have a significant impact on measures of life-satisfaction (see Feddersen *et al*. [2016] for an example). For this reason, we will perform some checks to ensure that our results are robust to the introduction of weather conditions.[15]

14 The mental well-being indicator thus obtained is highly correlated with (i) the physical well-being variable predicted using the same model, and (ii) an alternative indicator of well-being constructed by using individual answers to a 6-question battery capturing how often the individual has been (a) happy, (b) anxious, (c) depressed, (d) motivated, (e) exhausted, or (f) tired in the past 4 weeks (these questions are asked in the 2004/05 survey but not in the 1999/2000 one). Our indicator of well-being also appears to be strongly related to other individual characteristics, such that we observe a higher level of well-being for women, a positive relationship with education, and a positive correlation with the indicator for good economic conditions.

15 Note that a comprehensive analysis of seasonal variation in well-being or life satisfaction would require knowledge of the exact timing of the interview. Unfortunately, this is not available in our data. We simply intend to show here how the introduction of weather conditions affects our

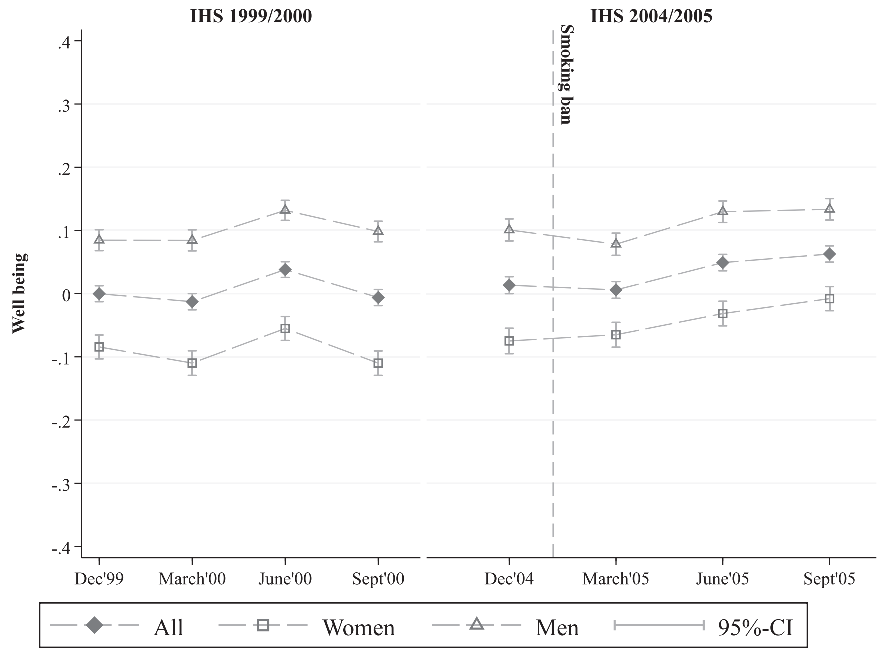

Fig. 4. Average mental well-being indicator by month
Source: Italian Health Survey for 1999/2000 and 2004/2005. Sample restricted to individuals aged 15–65.
Notes: All data points and confidence intervals (vertical lines) are calculated using sampling weights.

As for the analysis on smoking, before implementing our DiD strategy we use the *Indagine Multiscopo Aspetti della Vita Quotidiana* 1993–2012 to investigate long-term trends in well-being and check the common trends assumption. This survey does not contain the SF-12, but we use instead questions from a battery intended to capture various aspects of life satisfaction over the past 12 months. Specifically, we consider individual responses to questions about satisfaction with (i) health, (ii) relationships with family members, (iii) relationship with friends, and (iv) leisure activities. Respondents answer these questions using a 4-item Likert scale. We combine these answers into an indicator of 'life satisfaction' using principal component analysis. Appendix Fig. A4 shows that there is a slight decrease in this indicator from 1993 to 1997 (with the year 1998 being a possible outlier), but the trend from 1998 to 2012 is pretty flat, with few and not very significant fluctuations.

As we want to consider the effect of the ban on smokers and non-smokers, we need to define these two populations. Following Yang and Zucchelli (2015), we define the non-smokers as those individuals who have never smoked in their life up to the time when they are interviewed; the population of smokers consists of all those individuals who are either currently smoking or have been a smoker at some point in the past; we call them *potential smokers*.[16]

coefficient of interest; a full analysis of the relationship between self-reported measures of well-being or life satisfaction and the climate is beyond the scope of this paper.

16 We cannot, unfortunately, distinguish individuals who have tried to quit smoking over the past year (they could have been considered marginal smokers, as in Odermatt and Stutzer [2015]), as this information is not available for the 1999/2000 survey.

Table 6. Well-being: Before/after and difference-in-difference estimates

	Before/After			DiD		
	All (1)	Men (2)	Women (3)	All (4)	Men (5)	Women (6)
Panel A: Short-term						
All:ban	0.028	0.028	0.027	0.012	0.011	0.012
	(0.014)	(0.016)	(0.023)	(0.018)	(0.020)	(0.026)
Ban + Ban*never smoked	0.042**	0.049**	0.038*	0.026	0.032	0.023
	(0.013)	(0.017)	(0.016)	(0.016)	(0.020)	(0.021)
N	42,255	20,893	21,362	88,988	44,112	44,876
Panel B: Medium-term						
All:ban	0.030*	0.029*	0.033	0.023	0.016	0.032
	(0.012)	(0.013)	(0.018)	(0.014)	(0.016)	(0.021)
Ban + Ban*never smoked	0.046**	0.063**	0.036**	0.039**	0.049**	0.035*
	(0.010)	(0.014)	(0.013)	(0.013)	(0.016)	(0.017)
N	84,619	41,846	42,773	178,472	88,391	90,081

Source: Italian Health Survey for 1999/2000 and 2004/2005. Sample restricted to 15–65 years old.
Notes: The dependent variable is a measure of well-being derived from the SF-12 module of the survey. Each number in columns 1–3 represents a separate estimate of the coefficient β in eq. (1). Each number in columns 4–6 represents a separate estimate of the coefficient δ in equation (2), which captures the effect of the ban net of seasonal effects. Estimates in Panel A refer to short-term effects, obtained by comparing individuals interviewed in March *versus* those interviewed in December. Estimates in Panel B refer to medium-term effects, obtained by comparing individuals interviewed in March, June or September *versus* those interviewed in December. All estimates are obtained via a linear probability model and standard errors (in parentheses) are clustered at the household level, with $^{*}p < 0.05$, $^{**}p < 0.01$. For main covariates, see notes to Table 1.

In Table 6, we first present results adopting a before/after strategy, then we use a DiD strategy to take into account seasonal effects. We consider both short-term and medium-term effects. Results for subgroups of the population are reported in Appendix Table A3. Notice that here we interact the effect of the ban with an indicator variable equal to 1 if the individual is a non-smoker. This implies that the effect of the variable ‘ban’ is the effect of the ban for the potential smokers, while the sum of the effect of the variable ‘ban’ and its interaction with the non-smoker indicator is the effect of the ban for the non-smokers.

Looking at the before/after estimates in the short term first, we see that there are significant effects of the ban for non-smokers only, and these are seen both at the aggregate level and for several subgroups of the population. The short-term DiD estimates are smaller in magnitude and reveal no effects of the ban on either smokers or non-smokers in the population as a whole, with some limited evidence of significant impacts for non-smokers in employment. The DiD estimates in the medium term are a bit higher, reflecting the higher levels of well-being observed in September 2005, and reach statistical significance in the population as a whole as well as for several subgroups of non-smokers. To check for the presence of omitted time-varying variables which are not necessarily captured by seasonal effects, we investigate whether factors related to the average weather conditions during the interview month may influence our estimates. Our results (not shown) indicate that the inclusion of temperature and rainfall variables does not have any impact on our main estimates.

In respect of the magnitude, we see that the effect on the overall population of non-smokers is equal to 0.04 of a standard deviation. This is not a large amount, but it is comparable to the association between well-being and being married (0.07) or having a high school diploma (–0.04) as opposed to a degree, and is to be interpreted in the light of the relatively small and limited effect of the ban on actual smoking prevalence and cigarette consumption.

Looking at the heterogeneity effects for the medium term DID estimates in Appendix Table A3, we see that there are positive and statistically significant effects of the ban for the well-being of the non-smokers in a variety of groups, including individuals who are married, relatively young (15–39), and employed. These are groups where smoking consumption did not respond to the ban. For the subgroups where the ban changed smoking behaviour, we see quite large positive effects of the ban on the well-being of female smokers (0.06 for the young and single, and 0.06 for the young and not employed), but these coefficients do not reach statistical significance.

We draw two conclusions from this analysis. First, it would appear that the welfare-enhancing effects of the ban are more widely distributed across the population than the observed changes in smoking behaviour; second, that they are mainly observed among non-smokers.

7. Conclusions

In this paper, we offer a new and comprehensive evaluation of the 2005 ban on smoking in indoor public areas implemented in Italy. We ask two questions. First, we consider whether smoke-free policies affecting public spaces and workplaces can modify individual smoking behaviour, inducing smokers to quit smoking or to reduce the number of cigarettes consumed. According to the results of this study, these policies should not be thought of as a general tool to reduce the overall incidence of smoking. They can, however, effectively decrease smoking prevalence and intensity among subgroups of young people, a very important target of anti-smoking campaigns. Specifically, we show that in the period from December 2004 and March 2005, smoking prevalence among young and single women decreased by almost 3 percentage points while the number of cigarettes smoked dropped by 0.5 units as a result of the ban.

These results are in contrast with several previous evaluations of the 2005 smoking ban in Italy that have shown more pervasive effects of the smoking ban (Federico *et al.*, 2012; Buonanno and Ranzani, 2013). We argue that these previous studies confound the effects of the policy with seasonal variation in smoking behaviour.

Our second research question considers the direct effects on individual well-being. This is a way to measure the social welfare effect of a policy change (Gruber and Mullainathan, 2005). Here we find evidence that the ban had benefits for the well-being of non-smokers in the general population and in many of its subgroups. This could be explained by the fact that the ban might have changed general smoking habits (where and when people smoke), rather than the quantity or incidence of smoking, with a consequent reduction in second-hand smoking exposure for the non-smokers. Although we have no direct evidence to support this latter interpretation, as we do not observe where people smoke but only the quantity of cigarettes smoked, our evidence is in line with the other evidence which points out the positive health implications of the ban (Cesaroni *et al.*, 2008; Barone-Adesi *et al.*, 2011).

With the exception of a recent analysis by Yang and Zucchelli (2015), previous studies have either not considered or failed to find positive well-being effects for the non-smoking population. This is, however, a very important aspect to document, as it is crucial to evaluate the success of anti-smoking policies. It is also relevant to understand the wide public support that these policies have enjoyed so far and the popularity of new proposals aimed at extending the reach of the ban on smoking to private cars and outdoor spaces (Gallus *et al.*, 2012; Martínez-Sánchez *et al.*, 2014).

Supplementary material

The Appendix and the data files are available online at the OUP website.

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