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Anti-Smoking Policies and Smoker Well-Being: Evidence from Britain*

ANDREW LEICESTER† and PETER LEVELL‡

†*Frontier Economics – work carried out while at the Institute for Fiscal Studies (andrew.leicester@frontier-economics.com)*
‡*Institute for Fiscal Studies; University College London (peter_l@ifs.org.uk)*

Abstract

Anti-smoking policies can in theory make smokers better off, by helping smokers with time-inconsistent preferences commit to giving up or reducing the amount they smoke. We use almost 20 years of British individual-level panel data to explore the impact on self-reported psychological well-being of two policy interventions: large increases in tobacco excise taxes and bans on smoking in public places. We use a difference-in-differences approach to compare the effects on well-being for likely smokers and non-smokers. We find robust evidence that increases in tobacco taxes raise the relative well-being of likely smokers. Exploiting regional variation in the timing of the smoking ban across Britain, we find no evidence that it raised smoker well-being. Our

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findings give some support to the view that tobacco taxes are at least partly justifiable because of the benefits they have for smokers themselves.

Policy points

- Theories of time-inconsistent behaviour imply that smokers with self-control problems may benefit from policies that increase the cost of smoking. This is because such measures can help smokers achieve their goals of cutting down or quitting.
- We find evidence in a British panel covering the years 1991–2008 that the subjective well-being of likely smokers increases more than that of people less likely to smoke when tobacco taxes increase. While the sizes of the estimated effects are modest, they are indicative of self-control problems among smokers. We do not find similar effects for a smoking ban.
- These results suggest an additional justification for tobacco taxation: such taxes correct not only for costs that smokers impose on others but also for costs that smokers with self-control problems impose on themselves.
- If poorer smokers suffered from greater self-control problems, then it would suggest that the costs and benefits of tobacco taxation are more progressive than a standard analysis would suggest. However, we do not find evidence that smokers with low education (and hence lower lifetime income) suffer from greater self-control problems.

I. Introduction

It has long been recognised that policies to reduce smoking behaviour can be justified by the negative externalities associated with smoking.[1] Costs, including passive smoking and the net cost to public health care, are borne by wider society but not the smoker, leading to excessively high levels of smoking from a socially optimal perspective. Policies that raise the cost of smoking, such as tobacco excise taxes, can therefore improve overall social welfare, though are usually assumed to make smokers individually worse off. However, recent insights from behavioural economics suggest that smokers themselves may benefit from anti-smoking interventions. In particular, when smokers are time inconsistent, they may not be acting in their *own* best interests in making their smoking decisions. They make plans to give up smoking in the future but are unable to act on them when the future comes and they are faced with the immediate decision to smoke again.[2] In such cases, anti-smoking

[1] A summary of the evidence on the magnitude of smoking-related externalities is provided in Crawford, Keen and Smith (2010).

[2] Around two-thirds of adult smokers in England say they would like to give up (NHS Information Centre, 2012).

policies could help smokers commit to giving up, meaning the policies could be further rationalised by the commitment benefits they confer on smokers.

It is hard to determine straightforwardly whether or not there is such a rationale for intervention. Empirical predictions about how smoking behaviour responds to price increases (now or expected in the future), for example, are the same whether or not smokers are assumed to be time inconsistent. A small emerging literature, beginning with Gruber and Mullainathan (2005), has started to look at how the self-reported well-being of smokers responds to policy reforms. Assuming that self-reported well-being is informative about individual welfare, evidence that smokers are made relatively better off when anti-smoking reforms are enacted would be suggestive of time inconsistency. The key methodological challenge of this literature is to overcome the fact that smoking behaviour is in itself partly determined by policy reforms.

This paper contributes to this emerging literature in a number of ways. Most significantly, we make use of a long individual-level panel data set that includes information on smoking behaviour alongside self-reported well-being. Previous studies looking into the same question have made use of cross-sectional data, relying on a modelled propensity to smoke (assumed exogenous to policy changes) to identify the impact on well-being. Using panel data allows us to make use of individuals' *own* smoking histories to construct treatment groups of people likely to benefit from the commitment value of anti-smoking policies, but where treatment status is not itself determined by policy changes. We also explore the differential impact of the policies across education groups. If people with low education (a proxy for low lifetime income) are more prone to time inconsistency, they may see greater welfare benefits from anti-smoking policies. This has implications for the distributional effects of such policies.

We look at the effect both of tobacco excise taxes and of bans on smoking in public places. Bans have been implemented in a number of OECD countries in recent years, as anti-smoking policies move beyond price-based incentives toward more direct regulation of smoker behaviour. We exploit time-specific variation in cigarette taxation and regional variation in when smoking bans were implemented in the UK to identify the impact of these measures on smoker well-being. Our study provides the first evidence on this issue for Britain, which has been active both in implementing bans on smoking and in raising tobacco taxes. As at July 2013, British excise taxes on cigarettes were the second-highest (behind Ireland) in the European Union.[3]

To preview briefly our key results: we find evidence that higher real tobacco taxes increase the relative well-being of likely smokers, suggestive of time inconsistency in smoker behaviour. These results are broadly robust to different definitions of treatment groups and measures of well-being. However, we find no evidence that bans on smoking in public places raise smoker well-being.

[3]HM Revenue & Customs, 2013.

The rest of the paper proceeds as follows. Section II recaps the recent evolution of UK tobacco taxes and bans on smoking in public places, our main policies of interest. Section III then describes the main economic and conceptual ideas around smoker behaviour and the implications of anti-smoking policies for smoker welfare. Section IV develops the empirical strategy and describes the data used for the analysis. Section V describes the main results and Section VI concludes.

II. Policy background

1. Smoking taxation

Excise taxes have long formed an important part of government policy to limit tobacco consumption. Cigarette taxation in the UK includes both a specific component (£3.79 per pack of 20 cigarettes in 2015) and an *ad valorem* component (16.5 per cent of the tax-inclusive price).[4] Tobacco taxes are forecast to raise around £9.1 billion in financial year 2015–16, 1.4 per cent of total receipts.[5]

Figure 1 shows real-terms excise taxes on cigarettes in the UK between 1988 and 2012, based on data from HM Revenue & Customs (2012). We convert the specific and *ad valorem* components into a single rate per pack of 20 cigarettes using information on cigarette prices from the Office for National Statistics (ONS) to place a cash value on the *ad valorem* component.[6] This is then converted to real June 2012 values using the all-items retail prices index (RPI).

Real tax rates more than doubled over the period. Between 1993 and 2000, there was an explicit ‘escalator’ policy to increase the specific tax in real terms, by 3 per cent above inflation at first and then, from July 1997, by 5 per cent. Over this period, real tobacco taxes increased by around 64 per cent from £2.32 to £3.81 per pack. Beginning with the 2001 Budget, tobacco taxes were then frozen in real terms. In December 2008, tax rates increased to ‘offset’ a temporary reduction in the rate of value added tax (VAT) from 17.5 per cent to 15 per cent enacted as a fiscal stimulus (though note that this increase was not reversed when VAT rates rose again in January 2010). Real taxes then rose by 1 per cent in 2010, 5 per cent in 2011 and 2012, 3 per cent in 2013 and 4 per cent in 2014.

A large empirical literature suggests that prices are an effective instrument to reduce smoking rates. A meta-study by Gallet and List (2003) found an average price elasticity of demand for cigarettes of around –0.4 to –0.5.

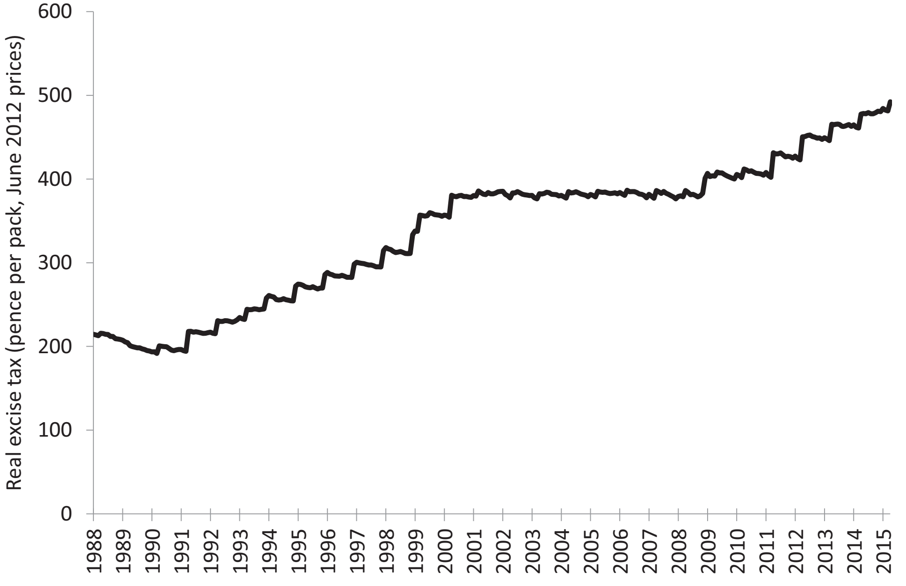
[4]Other tobacco products generally face specific taxes by weight. Rates can be found at https://www.gov.uk/tax-on-shopping/alcohol-tobacco.

[5]HM Treasury, 2015.

[6]Series CZMP at http://www.ons.gov.uk/ons/datasets-and-tables/data-selector.html?table-id=3.1&dataset=mm23.

FIGURE 1

Real-terms cigarette excise tax per pack of 20



Source: ONS data on prices (series CZMP at http://www.ons.gov.uk/ons/datasets-and-tables/data-selector.html?table-id=3.1&dataset=mm23). UKtradeinfo data on taxes (http://www.uktradeinfo.com). Converted to June 2012 prices using the all-items retail prices index.

Chaloupka and Warner (2000) found that price increases reduce demand along both the extensive and intensive margins (i.e. the propensity to smoke at all and how much is smoked conditional on smoking anything) and that increases in tobacco taxes tend to be passed through more than one-for-one into final retail prices. A number of papers have highlighted other behavioural responses to price rises, such as substitution to higher-tar and higher-nicotine cigarettes and smoking more intensively,[7] which could partially or wholly offset the impact of reduced smoking on the intensive margin. Smokers could also substitute by trading down to cheaper brands or forms of tobacco, such as hand-rolling tobacco (HRT) rather than pre-rolled cigarettes. There is evidence of a switch towards HRT in England. Data from the NHS Information Centre (2012) suggest that in 1990, around 91 per cent of adult smokers smoked mostly pre-rolled cigarettes whilst 10 per cent smoked hand-rolled. By 2010, the proportions were 69 per cent and 31 per cent. Note, though, that the taxation of HRT and cigarettes have moved in very similar ways over time.[8] Further,

[7]Evans and Farrelly, 1998; Farrelly et al., 2004; Adda and Cornaglia, 2006.
[8]HM Revenue & Customs, 2011.

given the overall decline in smoking rates, the prevalence of HRT smokers amongst all adults rose only from 3 per cent to 6 per cent over the same period whilst the proportion of cigarette smokers fell from 26 per cent to 14 per cent.

Smokers may also respond by purchasing illicit tobacco (for example, from low-tax jurisdictions by people living near borders or through increased organised smuggling). Stehr (2005) uses US data and finds cigarette purchases respond more than cigarette consumption to tax changes, ascribing much of the difference to evasion. Price is not the only determinant of evasion, which will also depend on resources devoted to enforcement. Estimates for the UK suggest that the illicit market share for cigarettes fell from 17 per cent in 2004–05 to 9 per cent in 2012–13 (from 61 per cent to 36 per cent for HRT over the same period),[9] following a policy strategy to reduce smuggling including greater enforcement at borders and punishments for those caught.[10] We discuss the possible implications for our analysis in Section V.

2. Bans on smoking in public places

Over the last decade or so, a number of countries have implemented bans on smoking in public places. In the UK, a ban was first introduced in Scotland from 26 March 2006, followed by Wales (2 April 2007), Northern Ireland (30 April 2007) and finally England (1 July 2007). De Bartolome and Irvine (2010) develop a model in which bans raise the cost of smoking by limiting the ability of smokers to take in a smooth stream of nicotine throughout the day. Bans can also raise the hassle costs of smoking. A review by Hopkins et al. (2010) finds that a small but consistent effect of ‘smoke-free’ policies including bans is to reduce smoking at the extensive and intensive margins. By contrast, a number of recent studies[11] have used quasi-experimental approaches exploiting regional variation in the timing of bans and found no significant effect on overall smoking prevalence or intensity, though some evidence of heterogeneous responses amongst particular groups (for example, those most likely to frequent bars and restaurants or heavy smokers). Shetty et al. (2011) find no impact of smoking bans in workplaces on hospitalisation rates or mortality from heart attacks, comparing areas where bans were introduced and control areas where they were not. These results suggest bans tend to displace where people smoke. Adda and Cornaglia (2010) draw on time-use data to suggest that following bans, smokers spent more time at home. They argue that bans may have increased children’s exposure to second-hand smoke as a result, though the findings have been disputed by Carpenter, Postolek and Warman (2011) drawing on self-reported records of exposure by non-smokers.

[9]HM Revenue & Customs, 2010 and 2014.
[10]HM Customs & Excise and HM Treasury, 2000.
[11]Adda and Cornaglia, 2010; Carpenter, Postolek and Warman, 2011; Anger, Kvasnicka and Siedler, 2011; Jones et al., 2011.

III. Economic models of smoker behaviour and welfare implications of policy interventions

The workhorse economic model of smoking has been the ‘rational addiction’ framework.[12] Consumption of an addictive good such as tobacco builds up a ‘stock’ of addiction. Utility in each period depends not only on current consumption of tobacco and other goods, but also on the accumulated addiction stock. This introduces a non-separability in preferences across periods, such that current smoking will be influenced by expectations of future tobacco prices. Addiction is characterised by ‘adjacent complementarity’: the higher the addiction stock, the higher the marginal utility from smoking. Consumers pick a path for current and future consumption of tobacco and the non-addictive good, taking into account how current decisions affect future utility. In the absence of any unexpected shocks, they will follow that plan through. A key implication of the rational addiction framework is that increases in the cost of smoking (such as higher tobacco taxes or public smoking bans) will reduce smokers’ well-being.

The rational addiction model has been extended in a number of ways to account for smokers expressing regret about their smoking behaviour or failing to give up when they express a preference to do so, whilst maintaining the basic assumption that smoking decisions are the result of utility-maximising behaviour. For example, if consumers make boundedly rational decisions with informational uncertainty about their tendency to become addicted,[13] then taking up smoking might look optimal given some perceived risk of addiction which later turns out to be wrong, but continued smoking is then optimal given the addiction stock that has been built up. There is some empirical evidence that young people in particular make smoking choices without full information on the risk of addiction.[14,15] Suranovic, Goldfarb and Leonard (1999) build in an adjustment cost of reducing smoking (perhaps reflecting the pain of withdrawal), which may not be fully anticipated by people making smoking decisions. Jehiel and Lilico (2010) argue that consumption decisions may be taken with limited foresight: consumers may make smoking choices only considering some proportion of the future rather than the whole lifetime. In this framework, it is possible that the lifetime optimal choice would be not to smoke but the limited foresight optimal choice is to smoke. The authors show that if the foresight horizon increases with age (perhaps reflecting increased

[12]Becker and Murphy, 1988.

[13]Orphanides and Zervos, 1995.

[14]Gruber and Zinman, 2001; Loewenstein, O’Donoghue and Rabin, 2003; Schoenbaum, 2005.

[15]It is also possible that people may misperceive the health risks of smoking, which would cause them to come to regret their decisions as and when their information changed. Sloan and Platt (2011), though, suggest that, if anything, young people tend to *over*estimate the risk of health harms from smoking.

learning or maturity), older people will give up or, in some cases, cycle between smoking and not smoking.

Whilst these ideas suggest why people come to regret past decisions or change their behaviour in the light of new information or experience, they do not clearly explain why people say they plan to give up but fail to do so.[16] Another development of the theory has considered that people suffer from a *time inconsistency* problem. The rational addiction framework assumes that there is a constant discount rate: from today's perspective, utility in two periods' time is discounted twice as much as utility next period. If, instead, people discount the immediate future more heavily than the far-distant future, then it is possible that a plan to quit looks optimal from today's perspective but is no longer optimal when the time comes to follow that plan through. As a result, the passage of time alone is enough to change behaviour; hence the expression 'time inconsistency'. A key implication of time inconsistency is that consumers place positive value on mechanisms that allow them to commit to a particular plan of action. Bryan, Karlan and Nelson (2010) provide evidence on the demand for commitment devices in a number of contexts. Frederick, Loewenstein and O'Donoghue (2002) and DellaVigna (2009) survey evidence for time inconsistency in lab and field experiments respectively.

The hyperbolic discounting model of Laibson (1997) builds this change in discount factors into standard models of people making intertemporal choices. O'Donoghue and Rabin (1999) look at this in the context of procrastination over whether or not to carry out a discrete action such as giving up smoking. Gruber and Kőszegi (2001 and 2004) build hyperbolic discounting into the rational addiction framework to think about continuous choices over cigarette consumption. Their model forms the basis for our empirical approach – we outline the main points below but full details of the derivation and proofs can be found in their papers.

Consumers allocate income across two goods in each period t – addictive tobacco a_t and a non-addictive consumption good c_t. Utility is assumed to be additively separable in the two goods. Consuming the addictive good builds up an addiction stock S_t, which depreciates at a constant rate $0 < d < 1$ in each period. Utility from consuming the addictive good depends on the accumulated stock as well as on the current consumption. Thus the utility function takes the form

$$(1) \qquad U_t = v(a_t, S_t) + u(c_t)$$

$$(2) \qquad S_{t+1} = (1 - d)(S_t + a_t).$$

[16] Assuming, of course, that a stated preference to give up is genuine and does not reflect a bias of survey respondents to give 'socially acceptable' answers.

Addiction in this model arises when $v_{aS} > 0$, i.e. the marginal utility of current tobacco consumption increases in the addiction stock. The harmful health consequences of tobacco can be modelled by assuming $v_S < 0$.

Under quadratic utility, the subutility functions can be written as

$$(3) \qquad v(a_t, S_t) = \alpha_a a_t + \alpha_S S_t + \alpha_{aS} a_t S_t + \frac{\alpha_{aa}}{2} a_t^2 + \frac{\alpha_{SS}}{2} S_t^2$$

$$(4) \qquad u(c_t) = \alpha_c c_t,$$

with $\alpha_a, \alpha_c, \alpha_{aS} > 0$ and $\alpha_{aa}, \alpha_{SS}, \alpha_S < 0$. Tobacco is sold at a price p in each period and the non-addictive good is sold at a normalised price of 1. Consumption of the non-addictive good is therefore given as residual income, $c_t = I_t - pa_t$.

Consumers maximise discounted current and future utility. Utility for those making decisions in period t is given as

$$(5) \qquad U_t + \beta \sum_{i=1}^{T} \delta^i U_{t+i},$$

with $\beta, \delta \in (0, 1)$. Hyperbolic discounting in this model arises because from the period-t perspective, utility in the next period ($t + 1$) is discounted by $\beta\delta$ whereas utility between periods $t + 1$ and $t + 2$ (or any two future consecutive periods) is discounted only by δ. In other words, the immediate future is more heavily discounted than the distant future. In the next period, the discount rate between $t + 1$ and $t + 2$ will change to $\beta\delta$.

Consumers are assumed to be aware that their discount rates are time-varying in this way. It is also assumed that from a welfare perspective, what matters for consumers are their long-run preferences (i.e. ignoring the additional immediate discount factor β).[17] This means that *welfare* from period t is given as

$$(6) \qquad W_t(S_t, p) = \sum_{i=1}^{T} \delta^i \left(v(a_{t+i}, c_{t+i}) + u(c_{t+i}) \right).$$

The question is then how an increase in the price of tobacco affects consumer welfare. Since (conditional on income) utility will depend on the price of

[17]This is, of course, something of a controversial assumption: it may be thought that at least some weight should be given to the short-term preference as well. For a discussion, see Bernheim and Rangel (2005).

tobacco and the addiction stock in this model, from today's perspective, the expression of interest is

$$(7) \qquad \frac{d}{dp}W_1(S_1, p) = \frac{d}{dp}\left[v(a_1, S_1) + \alpha_c(I_1 - pa_1) + \delta W_2(S_2, p)\right].$$

Gruber and Kőszegi (2004) show, using an iterative procedure, that the derivative of discounted utility with respect to the price of tobacco is given by

$$(8) \qquad -\alpha_c\left(\sum_{j=1}^{T}\delta^{j-1}a_j\right) - (1-\beta)\sum_{j=1}^{T}\delta^{j-1}\frac{\partial a_j}{\partial p}\frac{v_a(a_j, S_j) - p\alpha_c}{\beta}.$$

The first term is the standard result where there is no time inconsistency: higher tobacco prices reduce utility by increasing the cost of smoking. The second term is the commitment benefit of higher prices. In each period, consumers smoke more than they would 'like' on the basis of their long-term preferences because the immediate future is more heavily discounted. Higher prices induce a consumption response $\partial a_j/\partial p < 0$, which helps time-inconsistent consumers to act more in line with their long-run preferences. The whole second expression is positive, i.e. the utility cost of higher prices is mitigated by the self-control benefit.

To reiterate: the important implication of building time inconsistency into the addiction framework is that increases in the cost of addictive goods (such as tobacco) need not reduce individual welfare. With time inconsistency, higher costs provide a 'commitment' benefit to smokers, reducing future consumption towards levels that would be optimal in a time-consistent model.[18] Gruber and Kőszegi (2004) carry out a calibration exercise to show that higher prices are particularly likely to be welfare-improving when the hyperbolic discount factor is low (i.e. when people are very impatient over the immediate future). This suggests that increases in tobacco tax rates can make smokers better off, giving a rationale for smoking taxes even without negative externalities. A

[18]As noted in Gruber and Mullainathan (2005), other 'behavioural' models besides time inconsistency can generate a demand for commitment. Bernheim and Rangel (2004) discuss 'cue-based' consumption models, where consumption decisions depend on particular environmental signals (such as intending not to smoke but being influenced to do so when in the company of other smokers). Smokers who are aware of this would demand mechanisms that help them avoid those cues. The model of Gul and Pesendorfer (2001 and 2007) considers the 'self-control' costs that people face to avoid 'temptation' – think of the willpower effort needed not to smoke when cigarettes are easily available – and so demand mechanisms that reduce self-control costs. In both cases, simple versions of these models imply that higher prices make smokers worse off because consumption in the presence of a cue or the temptation value placed on smoking is essentially unresponsive to price. However, allowing cue-based demand or temptation utility to vary with price can restore the interpretation that smoker welfare improves when the costs of smoking rise.

similar argument can be made for other policies that raise the cost of smoking, such as bans on smoking in public places.

If we can interpret measures of self-reported well-being in surveys as a measure of individual welfare (or at least assume that they are positively correlated with welfare), then this model implies that when smokers are very time inconsistent, their reported well-being will rise when smoking costs increase.[19] Non-smokers, on the other hand, should be unaffected. This forms the basis of our empirical strategy outlined in the next section.[20]

IV. Empirical methods

1. Empirical strategy

We carry out a reduced-form analysis of the impact of anti-smoking policy interventions (increases in cigarette excise taxes and bans on smoking in public places) on self-reported individual measures of well-being. In particular, we compare whether these policies have a differential impact on the well-being of the people most likely to benefit from the commitment value they generate: smokers or groups of people who, on the basis of their observable characteristics or past smoking behaviour, are likely smokers. We exploit time-varying real excise taxes and time- and location-varying bans on smoking alongside detailed long-term panel data reporting happiness and smoking measures for a large sample of British adults to conduct our analysis. The data are described in more detail in Section IV.2.

We draw on a number of recent studies, most notably the pioneering work of Gruber and Mullainathan (2005). Using US and Canadian data, they find evidence that higher real cigarette excise taxes significantly reduce the tendency of likely smokers to report being unhappy relative to those unlikely to smoke. Their results for the US imply that an excise tax of $1.60 has the same effect on the happiness of likely smokers as moving from the poorest income quartile to the next-poorest quartile. More recently, additional evidence has emerged on the effect of tobacco control policies on the relative well-being of smokers. Odermatt and Stutzer (2013) exploit variation across time and across European countries in the cost of cigarettes, controlling for other anti-smoking

[19] Larsen and Fredrickson (1999) and Stutzer and Frey (2010) summarise evidence on the relationship between survey-based measures of happiness and other indicators of well-being (including observer-reports of happiness and economic indicators).

[20] There are, of course, reasons why non-smokers may be affected by higher tobacco taxes as well. Reduced smoking rates may have positive benefits for non-smokers in the presence of negative externalities, for example. The use of increased tobacco tax revenues to fund public spending or to reduce other taxes may also benefit non-smokers. However, it might be expected that these gains are second-order in magnitude relative to the direct gains for time-inconsistent smokers. Smoking bans may, on the other hand, be associated with more substantial gains for non-smokers. For this reason, when evaluating bans, our strategy will be to compare *smokers* in regions where the bans are and are not in effect (rather than smokers and non-smokers).

measures. They find a negative effect of increasing cigarette prices on likely smokers relative to those with a lower propensity to smoke. However, they also find a positive effect of smoking bans on smokers who recently tried to quit (a group more likely to be suffering from self-control problems). Brodeur (2013) uses county-level data from the US and finds a positive effect of smoking bans.

Other studies have provided empirical support for the idea that smokers are in favour of anti-smoking policies. Hersch (2005) uses US data to look at support for restrictions on smoking in six different types of public place among current smokers, distinguishing those who say they want to quit (who may therefore reveal themselves to be suffering from time inconsistency) from those who do not. The results show stronger support for the bans amongst those who want to give up. Interestingly, the support is even stronger for those who have tried and failed to quit than for those who are planning to try for the first time. The author suggests this is evidence that those who have failed before have higher quit costs (such as the pain of withdrawal or difficulty in obtaining support for giving up elsewhere) and so would place even greater value on the policies. Kan (2007) finds evidence in Taiwanese data that smokers who want to quit express more support for higher tobacco taxes and smoking bans in public places or at work. Badillo Amador and López Nicolás (2013) find similar results in Spanish data.

a) Cigarette excise taxes

In looking at the impact of tobacco excise taxes, we compare how they affect the self-reported well-being of likely smokers relative to non-smokers. The basic estimating equation takes the form

$$(9) \qquad H_{it} = \alpha + \beta_t + \gamma T_t + \delta S_{it} + \xi(T_t S_{it}) + \boldsymbol{\theta}\mathbf{X}_{it} + \mu_i + \varepsilon_{it},$$

where H_{it} is a measure of well-being (i indexing individuals and t indexing the time – year and month – in which the individual is observed), T_t is the inflation-adjusted tobacco tax rate, S_{it} is a variable indicating smoker status, $\mathbf{X}_{it}$ is a vector of individual-specific observable characteristics that could affect well-being, μ_i is an individual-level fixed effect capturing unobserved heterogeneity and β_t is a time (year) effect. We detail the set of covariates in Section IV.2. The parameter of interest is ξ, which measures the differential impact of real excise taxes on the well-being of smokers relative to non-smokers. A positive coefficient would be suggestive of the commitment benefits from higher taxation seen in time inconsistency models of smoker behaviour.

Simply using current smoker status as the measure of S_{it} in the model may lead to problems since smoking behaviour today will be endogenous to current tax rates. This leads to a selection effect: if those who continue to smoke following a tax rise have lower happiness than those who quit, this will bias downward the coefficient ξ.

We consider a number of approaches to deal with this selection problem. The first closely follows that of Gruber and Mullainathan (2005). In place of current smoking status, they use a modelled estimate of an individual's *propensity* to smoke based on observable characteristics, where smoking propensity is not a function of current tobacco tax rates. Using the first wave of data (from 1991), we estimate a probit model of the propensity to smoke where the dependent variable is a dummy variable for current smoker status and the independent variables are the same covariates $\mathbf{X}_{it}$ used in the happiness equation.[21] We use the parameter estimates to predict the likelihood that individuals observed in each year would have been smokers had they been observed with those covariates in the 1991 sample. This propensity P_{it}^{91} is then used in place of S_{it} in the well-being equation.[22] There are two small differences between Gruber and Mullainathan's approach and our first method. First, we model the propensity only using the first year of data rather than estimating separate propensity models for each year of data. The idea is that modelled propensity to have been a smoker in 1991 is exogenous to tax rates in later years. Second, we account for the fact that we use a two-step method and use bootstrapping techniques to calculate standard errors in the happiness equation. Not doing this gives misleadingly small standard errors, which could give rise to spurious significance of the key terms of interest.

The fact we are using a long panel data set of individual smoking and happiness data allows us to consider two further approaches. Rather than modelling the likelihood that individuals are smokers, we are able to draw on each individual's *own smoking history* to assign them to a 'treatment' group who would benefit from the commitment value of higher taxation under time inconsistency or to a 'control' group who would not. First, we define those who are smokers in the initial 1991 wave of data as treated and track how their well-being responds to changes in cigarette excise taxes relative to those who were not smokers in 1991. Second, we define those who are ever observed to smoke over the entire data period (1991 to 2008) as treated and those who never smoke

[21]The results from this probit are detailed in the online appendix. Controlling for other observables, women are less likely to smoke than men. People in poorer households and with lower education are more likely to smoke, as are those who are unemployed. Divorced, widowed or cohabiting people are more likely to smoke than married people. Private and social renters are more likely to smoke than people of other tenure types. Those with breathing problems are more likely to be smokers.

[22]Note that, as discussed in Gruber and Mullainathan (2005), we do not use P_{it}^{91} as an *instrument* for S_{it}; rather, it directly replaces it as the object of interest in the estimating equation. Further, as we use the same covariates in the happiness equation and the first-stage probit equation, identification of the θ parameter in the happiness equation is rather tenuous, as it is driven entirely by the fact that the propensity score is modelled non-linearly whereas the parameters enter linearly into the happiness equation. However, these covariates in the happiness equation are not really of direct interest and serve merely as controls: the key parameter of interest is ξ, which is identified by the interaction of smoking propensity with real tax rates.

as controls. Clearly, those who smoked in 1991 are a subset of those who are ever observed to smoke over the whole period. It may be that some of those who were not smoking in 1991 but later took it up were also at risk of starting in earlier years and so would also have valued the commitment benefit of taxation. Rather than estimating a propensity weight, these approaches essentially boil down to a straightforward difference-in-differences model where treatment status is exogenous to policy reforms.[23]

One particular implication of the economic model is that likely smokers who exhibit a greater degree of time inconsistency will see the largest welfare gains from increased taxes, since they will value the commitment mechanism more highly. There is relatively little evidence on whether time inconsistency varies directly with observable individual characteristics. Paserman (2008) finds that unemployed workers who were previously on low incomes exhibit more time inconsistency than those previously on high incomes. Choi et al. (2014) find evidence that better-off consumers make decisions consistent with economic rationality, but they do not test time consistency directly. We investigate the issue by exploring whether there is any differential in the relative impact of taxes on self-reported happiness for smokers in different education groups, where education is assumed to reflect differences in permanent income across households. This involves further interacting the policy and smoker status interaction with education dummies for those who achieved qualifications at compulsory education level and those who achieved post-compulsory-level qualifications. Finding larger effects for low-education individuals could be suggestive evidence that time-inconsistency problems are larger for poorer people.

From Figure 1 earlier, it is clear that there is serial correlation in T_t, making it important to account for any serial correlation in the errors if we are to avoid overstating the precision of our estimates. We account for this in our propensity score specification by implementing a block bootstrap procedure, resampling individuals rather than observations (combinations of individual and survey wave). In our other specifications, we cluster our standard errors at the individual level.

[23]We considered other approaches to determining treatment status. For example, in 1999, individuals were asked about their smoking histories over their whole life and we could have classified anyone who had ever smoked as treated. However, around two-thirds of people had smoked at some point, and we found little difference between modelled smoking propensity across treatment and control individuals using this definition. It was also unclear that experimental youth smoking would be a good determinant of whether or not people suffered from time inconsistency problems as adults. We also could have used any past smoking behaviour as a current treatment indicator, such as smoker status in the previous year, rather than smoker status in the first wave. However, the assumption that smoking status a year ago is exogenous to current policy changes is perhaps less credible than assuming exogeneity of initial smoker status to current policy, particularly where people may have had reasonable expectations about how policy would change in the near future either during the tobacco tax escalator or around the time that smoking bans were being introduced in different regions.

b) Bans on smoking in public places

We adopt a similar approach to assessing the well-being effect of the ban on smoking in public places. We exploit regional variation in the timing of the smoking ban. We restrict our sample to those who are defined as likely smokers and we compare changes in smoker well-being for those living in Scotland and for those living in England and Wales in the period following the ban being implemented in Scotland. Again, this is a straightforward difference-in-differences estimation where the sample is restricted to individuals likely to benefit from the commitment value of the ban and where 'treatment' is now defined across regions rather than people.

We define groups of likely smokers using individual observed smoking histories in a similar way to that described above.[24] First, we take smoking status in the first year in which individuals are observed. In the analysis of the smoking ban, we begin in 1999 (rather than in 1991 as for the tax analysis) in order to make use of regional booster samples in the data, which significantly increase the Scottish sample size. Second, we take smoking status over the whole data period beginning from 1999 and define likely smokers as those who smoked at any time. As above, treatment status should be exogenous to the policy reforms.

The estimating equation is of the form

$$(10) \qquad H_{it} = \alpha + \beta SCOT_{it} + \gamma POST_{it} + \xi(SCOT_{it}POST_{it}) + \theta \mathbf{X}_{it} + \mu_i + \varepsilon_{it},$$

where $SCOT_{it}$ is a dummy variable for individuals living in Scotland and $POST_{it}$ is a dummy for someone observed in March 2006 or later, following the ban being introduced in Scotland. The coefficient ξ on the interaction term then represents the relative impact on well-being for a 'smoker' living in Scotland following the ban there compared with one living in England or Wales before the ban was extended to the rest of Britain.

2. Data

Our data come from the first 18 waves of the British Household Panel Survey (BHPS), which cover the period September 1991 to April 2009. The BHPS is an annual survey that initially sampled around 5,500 households (10,000 individuals) in 1991. The survey attempts to follow the same individuals and their natural descendants through successive waves, even if they move home. All adult members (aged 16+) of the sample households are interviewed each

[24]Note that we do not use a propensity score approach in this part of the analysis. We could model smoking likelihood and take some arbitrary cut-off value to define those 'likely' smokers over whom to make regional comparisons in well-being, but it is not clear what the appropriate cut-off would be.

year, including adults who move into the household after the start of the survey and children who reach adulthood. Most interviews (around 97 per cent) take place between September and November each year, though a small number take place between December and May. The survey initially only covered England, Scotland and Wales. Booster samples of around 1,500 households each from Scotland and Wales were added in 1999 to allow regional analysis.[25] Information on individual and household socio-economic characteristics is collected each year, along with physical and mental health status and current smoking behaviour.

The survey asks several questions on individuals' subjective well-being. However, the only questions that have been asked consistently across waves are the General Health Questionnaire (GHQ) questions of Goldberg (1972).[26] These are a suite of 12 questions originally used to screen patients to detect signs of psychiatric disorders. They ask about recent feelings of stress, self-worth, confidence and happiness. Respondents answer each question on a four-point scale, reflecting their recent feelings relative to 'usual'. These responses are converted to a 0–3 numeric value, where 3 represents the highest distress and 0 the lowest. Values for each question are then simply added up to give a total score between 0 and 36, with higher values representing greater distress. Answers to these questions are recorded by the respondents on a self-completion questionnaire and are not interviewer-delivered.

We draw on the GHQ measure in two ways to define the dependent variable in the well-being equation. First, we use the total GHQ score (subtracted from 36 to get what we will refer to as a *well-being score*, which increases as distress decreases from a minimum of 0 to a maximum of 36) and estimate the well-being model using simple ordinary least squares (OLS). Second, we select one of the 12 questions that make up the GHQ: 'Have you recently been feeling reasonably happy, all things considered?'. Respondents can answer 'more so than usual', 'the same as usual', 'less so than usual' or 'much less than usual'. We generate three dummy variables for 'happier than usual', 'the same as usual' and 'less happy than usual' (the last includes those reporting 'much

[25]A sample of around 2,000 households from Northern Ireland were included from the 11th wave (2001) to allow UK-wide analysis; because the region is not covered across all waves, we exclude Northern Ireland observations from all our results. Further details of the BHPS can be found at https://www.iser.essex.ac.uk/bhps. Note that the last BHPS sample was surveyed in 2008. Since then, the BHPS sample has been subsumed into the wider survey Understanding Society, which has much larger sample sizes. The first wave of Understanding Society to incorporate the BHPS sample was in 2010. However, information on smoking is not routinely collected in Understanding Society; rather, smoking history information will be gathered every three years. This means it has not been possible to extend the analysis beyond 2008.

[26]A seven-point measure of general life satisfaction is only available from 1996 to 2000 and 2002 to 2008, and so misses a large period during which real-terms cigarette excise taxes were rising rapidly (see Figure 1).

less than usual', which is relatively rarely observed) and then estimate three separate linear probability models for each outcome.[27]

Note that the GHQ specifically asks respondents to consider their feelings relative to 'usual'. There may be some concern about this. If time-inconsistent smokers felt their 'usual' happiness rise following anti-smoking policy interventions, we may not pick this up with the questions as phrased. However, what would be particularly hard to account for outside of a time inconsistency framework is a *short-term* increase in happiness following an anti-smoking intervention, which is precisely what this sort of question would capture. Within the standard rational addiction model, for example, smokers could be happier (have higher utility) if they did not smoke than if they did, but given their accumulated addiction stock and the short-term costs of quitting, the optimal decision is to continue to smoke. Higher taxes may help smokers to give up, and eventually attain a higher baseline happiness level, but we would expect the short-term effect on happiness to be negative whilst the costs of quitting are endured. If we find evidence that anti-smoking measures make likely smokers happier *than usual*, this could be even more persuasive evidence for time inconsistency if we interpret it as a short-term gain in happiness. Note too that we are not the first to use the GHQ as a straightforward happiness measure[28] or as a measure of happiness relating to smoking behaviour in the BHPS: Moore (2009) finds that increases in smoking behaviour are correlated with reduced happiness using the same GHQ happiness question.

We include in the model a number of covariates that we might expect to influence well-being. Amongst these is real net equivalised annual household income, based on the income derivations for the BHPS provided by Bardasi et al. (2012). Rather than using absolute income, we construct within-wave income deciles to allow for the possibility that well-being is affected by relative income position rather than by absolute income and to allow for non-linear relationships between income and well-being.[29] Individual-level covariates include age (and age squared), gender, the presence and number of children of different age groups in the household, highest education level, employment status, marital status, housing tenure type, and a number of physical and mental health problems as recorded by the individual in the self-completion questionnaire. Controlling for age effects allows us to strip out life-cycle changes in well-being scores. Clark and Etilé (2002) note that deterioration in health is correlated with quitting smoking. Health outcomes are therefore likely to be related both to smoking status and to self-reported well-being, suggesting that excluding them could bias our results.

[27]This follows Ai and Norton (2003), who warn of the difficulty of interpreting interaction terms in non-linear models.

[28]See, for example, Oswald (1997) and Clark and Oswald (2002).

[29]Clark and Oswald (2002) discuss absolute versus positional measures of income as determinants of well-being.

The BHPS data are supplemented with real-terms smoking excise duties as described in Section II. These are merged into the BHPS data at a monthly level, expressed in June 2012 prices based on the all-items RPI measure of the price level.

a) Sample selection

As described in Section IV.1, we consider three main approaches to explore the relationship between anti-smoking policies and the relative well-being of likely smokers. Here we outline the samples selected from the data for the different approaches.

Cigarette excise taxes

All of our analysis of excise taxes is restricted to the 'original' BHPS sample (individuals who were interviewed in the initial 1991 wave). We start with a naive analysis using current smoker status; here our only restriction on the sample is to include individuals who are observed in 1991 and have non-missing smoker information in a given year.

For the model using smoking propensity in place of smoker status, we also restrict the sample to individuals observed in 1991. This is largely because we bootstrap the estimates to account for the two-stage nature of the estimation. To ensure that the number of observations in the first-stage probit model does not change in each iteration, we have to condition on individuals who are observed in that year.

For the model using 1991 smoking status as a treatment indicator, it is obvious that we need to restrict the sample to those individuals observed in that year with non-missing smoking status. Conditional on that, we do not make any other sample selection. This means that we can include observations in later years where smoking status is missing (unlike the naive model), since the treatment indicator has been defined for that observation.

For the model using observed smoking status over the period 1991 to 2008 as a treatment indicator, we condition on the individual reporting their smoking status in at least 15 waves (including 1991).[30] If they ever report a positive response to current smoking behaviour, we classify them as treated individuals in all years; otherwise, they are classed as controls. Essentially, we are willing to assume that someone who reports being a non-smoker in at least 15 separate years is unlikely to be a smoker in the years in which their smoking status is not observed. Since this is the only specification that conditions on smoking status being observed a given number of times, the sample sizes for this specification are substantially lower than those for the other specifications (see Table 1).[31]

[30]Conditioning on observing individuals in 1991 excludes new entrants to the survey whose smoking status may depend on tax rates in the years they join.

[31]This selection does not appear to materially affect things: our empirical findings are not sensitive to using this more restricted sample with the other approaches. Results are available on request.

TABLE 1

Numbers of observations and individuals in each specification

Specification:	Tax			Ban	
	1991 propensity	*Smoked 1991–2008*	*Smoked 1991*	*Smoked 1999*	*Smoked 1999–2008*
No. of observations	109,552	85,525	111,446	23,312	24,061
No. of individuals	9,525	5,012	9,744	3,875	3,243

Across all specifications, we further condition on individuals having a full set of right-hand-side control variables without missing values. We also exclude proxy respondents.

Bans on smoking in public places

Our analysis of smoking bans relies explicitly on regional variation in the timing of when bans were introduced. Since the original BHPS sample contains relatively small numbers of Scottish observations, relying on this sample alone makes it much less likely that we would pick up any differential effect on well-being across regions. Thus, for this analysis, we make use of the regional booster samples. Since these were introduced in 1999, we start our analysis in that year. All our analysis of smoking bans ends before the ban was extended to England and Wales in 2007. Since our smoking ban models use region as a treatment indicator and are restricted to ‘smokers’ from a subset of the whole data period, the sample sizes are much smaller than for the tax-based models, even though we make use of the regional boosters.

As noted in Section IV.1b, we do not use a propensity model for the smoking ban analysis, since our interest is in regional variation in the well-being of likely smokers and it is not clear what the appropriate cut-off point would be for classifying individuals as likely smokers based on a modelled propensity. Thus we define smokers based on observed behaviour: those who were smokers in 1999, and those who ever smoked in the period 1999 to 2008 with no more than two missing observations over that period.

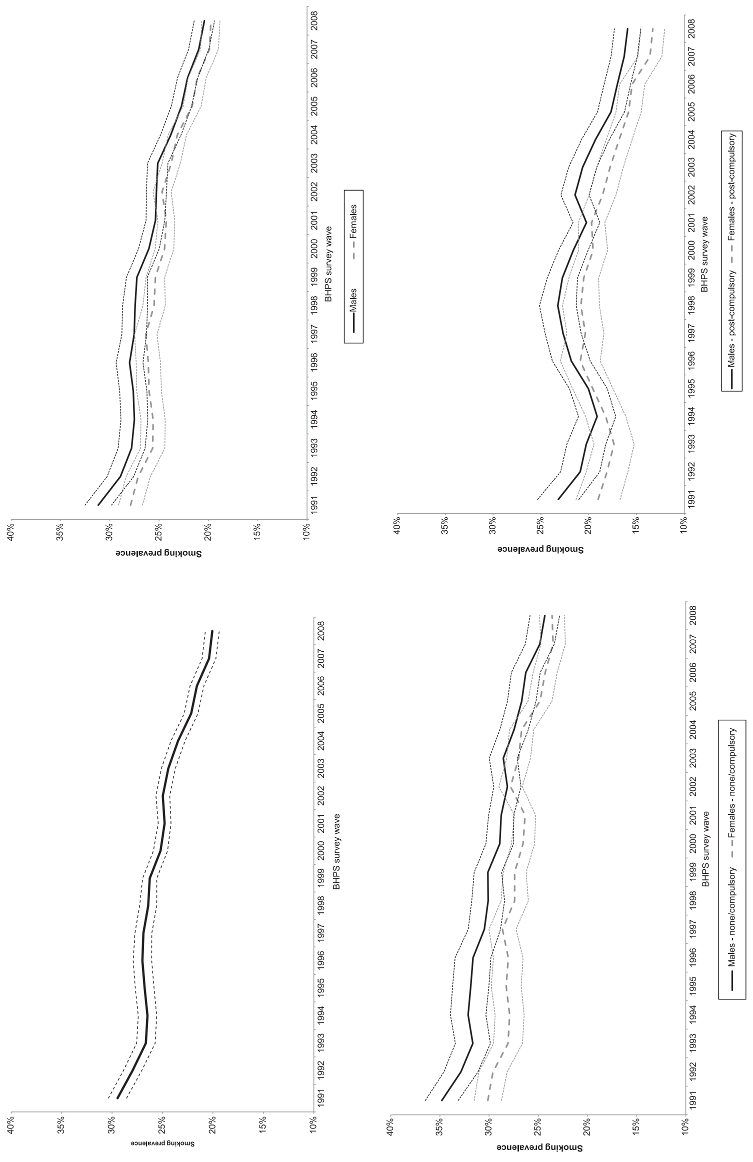
Table 1 shows the number of individuals and the number of observations for each specification following these various selection criteria.

b) Descriptive statistics: smoking and happiness

Smoking rates in the BHPS sample have fallen over time. Figure 2 compares trends in overall smoking prevalence (top-left panel), by gender (top-right), and then further splitting by education distinguishing those with no more than compulsory education (bottom-left) and those with post-compulsory education

FIGURE 2

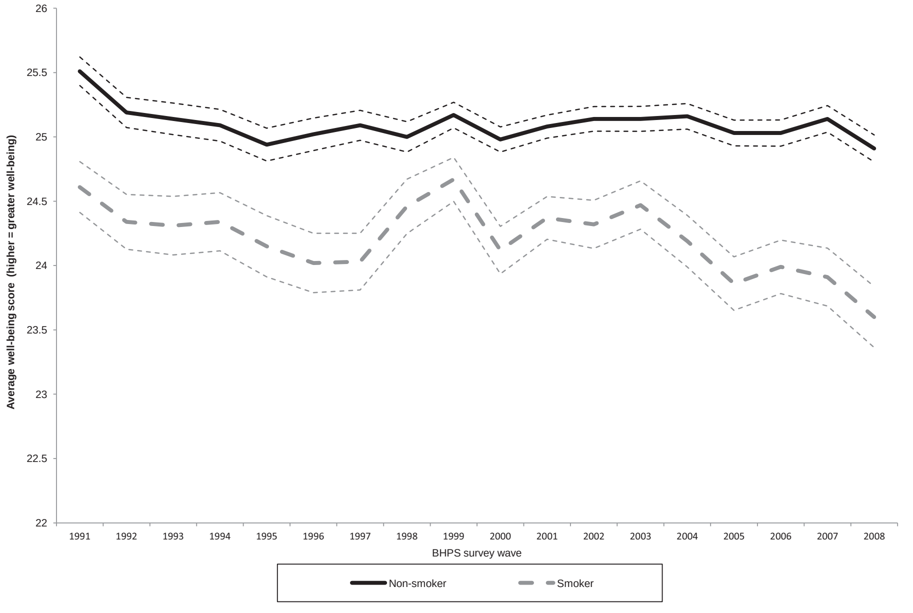
BHPS smoking prevalence by gender and education level



Source: Authors' estimates from British Household Panel Survey data. Dotted lines are 95 per cent confidence intervals.

FIGURE 3

Well-being score by current smoker status



Source: Authors' estimates from British Household Panel Survey data. Dotted lines are 95 per cent confidence intervals.

(bottom-right).[32] Overall smoking prevalence fell from 29 per cent in the 1991 survey to 20 per cent in the 2008 survey. Males have remained slightly more likely to smoke than females, though both have seen similar declines in participation over time. These figures and trends are very close to those reported in official smoking statistics based on data from the General Lifestyle Survey.[33] Males are also more likely to smoke than females conditional on education level. Whilst those with post-compulsory education are considerably less likely to smoke than those without, there have been strikingly different trends over time in smoking rates by education. For those with low education, smoking rates have declined fairly consistently over the whole period. By contrast, for those with higher education, prevalence actually rose in the mid-1990s before falling back in the 2000s.

There appears to be a negative correlation between smoking and self-reported well-being. Figure 3 shows well-being scores (between 0 and 36) by

[32]These graphs are based on the full samples in each year rather than on the selected samples for our empirical specification. Regional booster samples are included where available. All figures are weighted to national totals.

[33]Dunstan, 2012.

current smoker status and survey year. Because higher GHQ scores indicate greater distress, a higher score on the measure in the graph reflects greater well-being. Across all waves, smokers report on average a well-being score of 24.2 (a GHQ of 11.8) and non-smokers a score of 25.1 (a GHQ of 10.9). For both groups, there has been a slight downward trend over time in the average score, though the recent trend is stronger for smokers such that the gap has widened in the most recent years having narrowed in the late 1990s. Of course, as is clear from Figure 2, compositional changes over time in those who are smokers mean we cannot draw clear conclusions from these trends alone about how anti-smoking policies have affected the relative well-being of smokers. Hence we turn to our empirical results.

V. Results

1. Cigarette excise taxes

Table 2 shows the proportion of current smokers, non-smokers and all individuals who are classified as ever having been a smoker over the period 1991–2008 or were smokers in 1991. These two definitions of likely smoker are used to form our treatment and control groups. The treatment groups are intended to capture individuals who may benefit from higher excise taxes as a commitment device. The table also shows the average smoking propensity score (pscore) from the probit model of smoking status using the 1991 sample for current smokers. Those with higher propensity scores should have a higher likelihood of benefiting from the commitment that excise taxes may provide.

The table shows that the average propensity score is higher for individuals observed smoking in any given year than for those observed not smoking, though the difference is not huge (0.35 compared with 0.24). Thirty-five per cent of the sample were seen smoking at one point in our sample and are included in our ever smoked group. Interestingly, only 16 per cent of those observed not smoking in any given year are seen smoking on some other

TABLE 2

Likely smoker variables

	Current smoker			*Everyone*	*No. of observations*	*No. of individuals*
	Yes	No	No answer			
Average pscore	0.35	0.24	0.32	0.27	109,552	9,525
Ever smoked 1991–2008	100%	16%	35%	35%	85,525	5,012
Smoked 1991	89%	9%	27%	29%	111,446	9,744

Note: Averages among those who have non-missing overall well-being scores.

TABLE 3

Cigarette taxes and smoker well-being: overall well-being score

	(1) Current smoker	*(2) 1991 propensity*	*(3) Smoked 1991–2008*	*(4) Smoked 1991*
Smoker main effect	–0.641***	–8.893***	-	-
	(0.235)	(1.312)		
Cigarette tax	–0.964*	–1.071*	–0.693	–0.985*
	(0.564)	(0.582)	(0.692)	(0.565)
Interaction	0.162**	0.667**	0.139**	0.145**
	(0.072)	(0.275)	(0.069)	(0.067)
Observations	111,399	109,552	85,525	111,446
Individuals	9,744	9,525	5,012	9,744
R^2	0.064		0.065	0.064

Note: Standard errors are given in parentheses. Standard errors in column 2 are based on 750 block bootstrap replications. In all other models, they are clustered at the individual level. Additional controls are included for age, sex, children, education, employment, marital status, income, health, tenure, region and individual fixed effects. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

occasion, suggesting that most non-smokers are essentially never smokers. Twenty-nine per cent of the sample were seen smoking in the first year of data in 1991.

All of the well-being equations include year dummies,[34] the interaction of the smoker status variable (the treatment/control indicator or the propensity score) with the real excise tax rate (measured in pounds per pack of 20 cigarettes), the same set of covariates as the smoking propensity model and individual fixed effects. Controlling for fixed effects allows us to eliminate sources of unobserved heterogeneity, which may otherwise bias our results (addressing, for instance, concerns that attrition or mortality differ between happy smokers and unhappy smokers). As discussed in Section IV.1, the other covariates serve merely as controls in the model. Therefore, for reasons of space, we do not present the full model results, and instead present only key results of interest. A full set of results are available on request.

Table 3 shows results from a regression with the overall well-being score (on the 0–36 scale) as the dependent variable. If higher excise taxes were associated with an increase in the relative well-being of the smoker groups, we would expect a positive coefficient on the interaction term. Each column shows a different specification of the definition of the 'smoker' variable as outlined

[34]We do not control specifically for month – whilst we may expect seasonal variation in reported well-being, recall that almost all the interviews in the BHPS take place between September and November in a given year.

in Section IV.1. Column 1 shows a naive specification using current smoker status, likely to be endogenous to the current tax rate. Column 2 replaces current status with the propensity score based on the 1991 smoker probit. Column 3 replaces current smoker status with a treatment indicator set to 1 for those who were smokers at any time between 1991 and 2008. Column 4 sets the treatment indicator to 1 for those who smoked in 1991. Because fixed effects will be collinear with any time-invariant characteristics, the constant smoker status variables in the 'Smoked 1991–2008' and 'Smoked 1991' specifications are omitted.

Columns 1 and 2 suggest that being a smoker (or likely smoker) is associated with lower well-being. Current smokers have an average well-being score around 0.64 points lower than non-smokers. It is hard to compare this with the propensity score specification (column 2) since here the 'smoker' variable is just the propensity score in the range 0 to 1. Taking the average observed propensity score for current smokers in the sample (0.35) compared with the average propensity score for non-smokers (0.24) and multiplying the difference by the coefficient gives that an average smoker in this specification has well-being score around 1 point lower than an average non-smoker.

The interaction terms are consistently positive and statistically significant in all specifications. These results lend support to the view that a treatment group of smokers see their well-being rise relative to a control group of non-smokers as taxes increase. Taking the treatment/control specifications, for example, a £1 rise in taxes would be associated with a relative increase in the well-being score of smokers of around 0.14 points. This compares with an average well-being score for the sample of 24.9 and a standard deviation of 5.3.

As discussed in Section IV.1, an open empirical question is whether poorer consumers exhibit a greater degree of time inconsistency. If so, one implication is that smokers with low education (where low education is a proxy for low lifetime income) would see the largest benefits from the commitment value of higher taxation. Table 4 repeats the analysis, further interacting the smoker status and tax interaction with three education groups (post-compulsory, compulsory only and no formal qualifications). The effects appear greatest for those with no qualifications (i.e. the least-educated group). However, F-tests cannot reject the hypothesis that the coefficients on the interaction terms are all the same, or even that the coefficient for the lowest education group is significantly different from that of the highest education group (except for the propensity score model, where the difference between the coefficients for the lowest and highest education groups is significant with a p-value of 0.04).

The results so far have focused on the broad well-being measure derived from 12 separate questions that make up the GHQ. As we discussed in Section IV.2, the single sub-question that forms part of the overall GHQ asking about current happiness relative to usual may provide a more concrete

TABLE 4

Cigarette taxes and smoker well-being with education interactions: overall well-being score

	(1) Current smoker	*(2) 1991 propensity*	*(3) Smoked 1991–2008*	*(4) Smoked 1991*
Smoker main effect	–0.656***	–8.055***	-	-
	(0.235)	(1.310)		
Cigarette tax	–0.968*	–1.092*	–0.697	–0.989*
	(0.564)	(0.581)	(0.692)	(0.564)
Interaction: no qualifications	0.186**	0.902***	0.266***	0.244**
	(0.080)	(0.285)	(0.101)	(0.096)
Interaction: compulsory	0.168**	0.677**	0.118	0.081
	(0.080)	(0.293)	(0.091)	(0.088)
Interaction: post-compulsory	0.147*	0.344	0.049	0.097
	(0.077)	(0.338)	(0.087)	(0.086)
Observations	111,399	109,552	85,525	111,446
Individuals	9,744	9,525	5,012	9,744
R^2	0.064		0.065	0.064

Note: Standard errors are given in parentheses. Standard errors in column 2 are based on 750 block bootstrap replications. In all other models, they are clustered at the individual level. Additional controls are included for age, sex, children, education, employment, marital status, income, health, tenure, region and individual fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

test of the rational addiction model. We run separate linear probability models for those responding being ‘happier than usual’ (Table 5) or ‘less happy than usual’ (Table 6).[35] We would expect the interaction term to be positive in Table 5 and negative in Table 6.[36]

The results are broadly similar to those we saw for the overall well-being score. Higher taxes have a statistically significant positive effect on the probability of smokers reporting being ‘happier than usual’ relative to non-smokers for the propensity score and ‘Smoked 1991–2008’ models (columns 2 and 3 of Table 5). This effect is still positive but only significant at the 10 per cent level for the ‘Smoked 1991’ specification (and is insignificant for the naive ‘Current smoker’ specification). In addition, those with a higher propensity to smoke are also less likely to report being ‘less happy than usual’ when taxes increase. However, the magnitudes of these effects are small. For instance, those we observe smoking at any time between 1991 and 2008 are

[35]Note that the most consistent significant findings reported by Gruber and Mullainathan (2005) were a reduced probability that likely smokers reported being unhappy.

[36]We also ran a linear probability model where the outcome was ‘same happiness as usual’. The interaction effects were insignificant in this specification. Results are available on request.

TABLE 5

Relationship between cigarette taxes and being happier than usual

	(1) Current smoker	*(2) 1991 propensity*	*(3) Smoked 1991–2008*	*(4) Smoked 1991*
Smoker main effect	–0.029**	–0.159**	-	-
	(0.014)	(0.066)		
Cigarette tax	0.032	0.015	0.007	0.030
	(0.040)	(0.039)	(0.048)	(0.039)
Interaction	0.006	0.070***	0.009**	0.007*
	(0.004)	(0.015)	(0.004)	(0.004)
Observations	113,770	113,821	86,938	113,821
Individuals	9,816	9,816	5,012	9,816
R^2	0.006		0.007	0.006

Note: Standard errors are given in parentheses. Standard errors in column 2 are based on 750 block bootstrap replications. In all other models, they are clustered at the individual level. Additional controls are included for age, sex, children, education, employment, marital status, income, health, tenure, region and individual fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE 6

Relationship between cigarette taxes and being less happy than usual

	(1) Current smoker	*(2) 1991 propensity*	*(3) Smoked 1991–2008*	*(4) Smoked 1991*
Smoker main effect	0.027*	0.391***	-	-
	(0.015)	(0.078)		
Cigarette tax	0.045	0.056	0.030	0.047
	(0.041)	(0.041)	(0.049)	(0.041)
Interaction	–0.007	–0.046***	–0.005	–0.005
	(0.005)	(0.016)	(0.004)	(0.004)
Observations	113,770	113,821	86,938	113,821
Individuals	9,816	9,816	5,012	9,816
R^2	0.029		0.031	0.029

Note: Standard errors are given in parentheses. Standard errors in column 2 are based on 750 block bootstrap replications. In all other models, they are clustered at the individual level. Additional controls are included for age, sex, children, education, employment, marital status, income, health, tenure, region and individual fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

just less than 1 per cent more likely to report being happier than usual for a £1 increase in taxes.

In summary, the results based on the single happiness question point in the same direction as the results based on the overall well-being measure: likely smokers appear to be made relatively *better off* from higher real excise taxes.

Taxes had a positive and significant effect on the relative well-being of likely smokers for all of our treatment groups at the 5 per cent level. Ignoring the likely endogenous current smoker specification, all treatment groups were also more likely to report being happier than usual with at least 10 per cent significance. However, we do not find robust evidence that the effect is greater for low-education individuals. Although the magnitude of the interaction term is quite low relative to the mean or standard deviation of happiness measures, the fact that our various models point consistently in the same direction is noteworthy and goes against the predictions of the rational addiction model.

2. Ban on smoking in public places

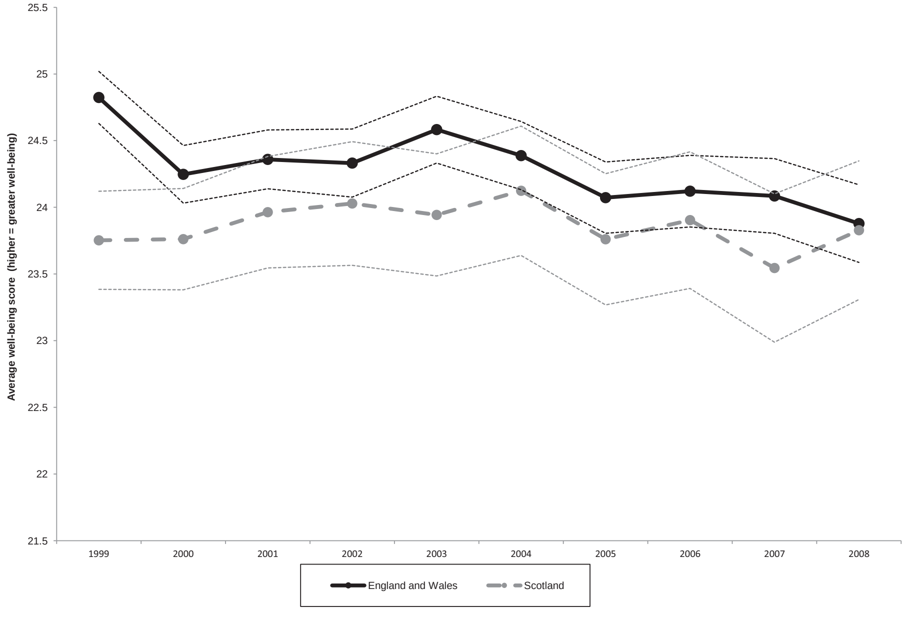
For the analysis using regional variation in the timing of the smoking ban, the sample is restricted to those classified as likely smokers, comparing the well-being of smokers in Scotland with that in England and Wales in the period following the ban being implemented north of the border. We restrict our sample to stop in March 2007, before the ban is extended to other regions. We show results for the specification classifying likely smokers based on their 1999 smoking behaviour. Results from the alternative strategy outlined in Section IV drawing on observed smoking status over the whole period 1999 to 2008 are quantitatively very similar and are available on request.

As always with difference-in-differences studies, we make the common trends assumption. This means we assume that changes in the well-being of likely smokers in England and Wales would be a good predictor of changes in Scotland absent the smoking ban having been implemented there. Figure 4 shows trends in the average well-being score by region (Scotland versus England and Wales) over time among those who smoked in 1999. The trends are broadly similar: flat or increasing well-being scores in both regions up to around 2003 with some evidence of decreasing scores since then. The striking difference is 1999 to 2000, when there was a large decrease in the well-being score in England and Wales not seen in Scotland.

Table 7 shows the main results, again focused on the coefficients of key interest: the coefficients on the Scotland dummy, a dummy for the period between March 2006 and March 2007 (when the ban was in place in Scotland but not elsewhere) and the interaction term between the two, which is the difference-in-differences estimate. Columns here represent different dependent variables: linear probability models (LPMs) for the three categorical responses to the single happiness question and a model for the overall well-being score. We find no significant effects of the ban on the relative probability of a smoker being more, less, or no more or less happy than usual or on the overall well-being score of those who were seen smoking in 1999.

FIGURE 4

Average well-being for those observed to smoke in 1999, by year and region



Source: Authors' estimates from British Household Panel Survey data. Dotted lines are 95 per cent confidence intervals.

TABLE 7

Impact of smoking ban on well-being of likely smokers

	(1) *LPM:* *'happier'*	*(2)* *LPM:* *'same as usual'*	*(3)* *LPM:* *'less happy'*	*(4)* *OLS:* *0–36 well-being*
Scotland	–0.056	–0.105	0.126*	0.192
	(0.080)	(0.108)	(0.071)	(1.064)
Scottish ban	0.040	0.016	–0.023	–0.155
	(0.058)	(0.071)	(0.044)	(0.517)
Scotland × Ban	0.002	–0.001	–0.013	0.316
	(0.016)	(0.022)	(0.017)	(0.236)
Observations	23,312	23,312	23,312	22,790
R^2	0.008	0.016	0.038	0.079

Note: Clustered (on the individual) standard errors are given in parentheses. Note the sample is restricted to those who smoked in 1999. Additional controls are included for age, sex, children, education, employment, marital status, income, health, tenure, region and individual fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

3. Discussion

Our results lend some support to the view that anti-smoking policies raise the relative well-being of likely smokers, as suggested by models of time inconsistency in which smokers value the commitment benefits of such policies. Using different methods to classify individuals as likely smokers, we find consistent evidence that higher real-terms cigarette excise taxes raise the relative self-reported well-being of smokers. This holds for two different measures of well-being: an index of distress and a specific question on current happiness relative to usual (though the evidence is slightly less robust for the latter). That the results hold across different specifications and different outcome variables, and correspond to those of Gruber and Mullainathan (2005) using data from other countries (the US and Canada), is suggestive that this effect is quite empirically robust. The relative increase in well-being for likely smokers is, however, quite small compared with the mean or standard deviation of observed responses.

On the other hand, we find no significant effects of the ban on smoking in public places on the well-being of smokers in Scotland relative to those in England and Wales during the short period in which the ban was only in operation in Scotland. Possible explanations for this finding include that the ban was less effective as a self-control device than increases in tobacco taxes (perhaps because smokers can substitute toward smoking more away from public places) or that the costs of complying with the ban in terms of increased ‘hassle’ negated any benefits it might provide by serving as a self-control device. Moreover, while the ban on smoking in 2007 was only in place in Scotland, smokers in England and Wales could have anticipated bans based on legislation passing through national parliaments at the time. This means that forward-looking time-inconsistent smokers may have already adjusted to an expected future ban even before its implementation – which would violate the common trends assumption and bias our results.

A possible concern with our approach is that there may have been other changes in anti-smoking policies at the same time as tax changes (or the smoking ban). If so, without explicitly measuring and controlling for other policy changes, we might attribute some of their impact on relative smoker well-being to our policies of interest. It is common to see policies packaged together as part of a wider anti-smoking strategy.[37] However, there is no clear evidence that other significant anti-smoking reforms were introduced at the same time as the main increase in real excise taxes in the 1990s under the duty escalator. Cigarette advertising had been banned on UK television since 1965, and advertising in other media was progressively banned between

[37]For example, the 1998 White Paper *Smoking Kills* (Department of Health, 1998) looked at measures including advertising restrictions and clamping down on smuggling.

2003 and 2005 as part of the Tobacco Advertising and Promotion Act 2002, following the end of the escalator period. Health warnings on cigarette packets were introduced compulsorily in July 1991, before the excise escalator, and increased in prominence and visibility from 2002, after the escalator had ended. The legal purchase age for cigarettes was also raised after the escalator period, in October 2007.

A final consideration that deserves some discussion is that taxes can also affect the well-being of smokers through their effects on the illicit (untaxed) market for tobacco. As noted in Section II.1, illicit tobacco appeared to command a relatively high share of the market in the 1990s when tax rates were increasing rapidly. If tax increases lead to a rise in the availability of illicit tobacco, this could actually reduce the cost of smoking and make smokers better off even under a time-consistent framework. This would undermine the power of our test to discriminate between time inconsistency and rational addiction. Note, though, that for this to hold would require the cost of obtaining tobacco to *fall* when the tax rate went up, where the cost of illicit tobacco includes not only the purchase price but also the costs of obtaining it (which may include the hassle of dealing with illicit sellers and any moral costs). In general, we might expect that people turning to illicit tobacco might be paying more than they had paid for tobacco before the tax increase, just less than the cost of obtaining licit (duty-paid) tobacco following the tax rise. However, it is possible that the cost of smoking fell as a result of tax increases if, for instance, the growth in the size of the illicit market allowed tobacco smugglers to benefit from economies of scale or if more widespread use of illicit tobacco reduced any social stigma or moral costs from consuming it. It is, of course, hard to determine whether these sorts of effects exist or how important they are without data on who smokes illicit tobacco and how the price of licit and illicit products changed over this period.

VI. Conclusions

When people suffer from time inconsistency problems that lead them to overconsume harmful products, policies that restrict their behaviour can in principle make them individually better off. We find quite strong evidence of such an effect of tobacco taxes using British panel data recording smoking status and self-reported measures of well-being over a long period. The effect appears to be fairly modest in size, but is robust to different treatment indicators and measures of well-being, and points in the opposite direction to the predictions of standard theory. However, we find no evidence of a similar effect for a ban on smoking in public places introduced in Scotland.

From a policy perspective, our findings suggest a possible ‘internality’ rationale for tobacco taxation as well as the more usual externality rationale. This could suggest that optimal tobacco excise taxes exceed those that would be justified by estimates of the marginal external cost imposed on others, though we are unable to quantify the size of the internality on the basis of our results. Drawing on a theoretical framework, Gruber and Kőszegi (2001) estimated that the internality-correcting tax could be around $1 per pack of cigarettes compared with an excise tax at the time of around 65 cents. They also argued that if poorer individuals were more likely to suffer from time inconsistency, increases in taxes could be progressive in terms of their welfare impact. However, we only find weak evidence that effects are greater for low-education (which, by proxy, implies poorer) consumers of tobacco and this evidence is not robust to the choice of specification.

Supporting information

Additional supporting information may be found in the online version of this paper on the publisher's website:

- Appendix. Propensity to smoke: results

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