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Do Cigarette Taxes Make Smokers Happier

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Jonathan H. Gruber and Sendhil Mullainathan

Abstract

Some policy makers justify cigarette taxes by arguing that they actually make smokers better off. This argument has been hard to evaluate because behavioral data, such as that showing reduced cigarette consumption following a tax hike, cannot resolve the issue of whether smokers are made better off by the reduction or not. In this paper, we directly assess the effect of cigarette taxes on well-being, using subjective well-being data. We model the differential impact of excise taxes on those with a propensity to smoke, relative to others, in order to control for omitted correlations between happiness and excise taxation. Using US data on happiness and state-level changes in excise taxes, we find consistent evidence that excise taxes make those who have a propensity to smoke happier. To assess robustness, we repeat the exercise using Canadian data, which has independent information on well-being and also much larger tax changes, and find the exact same pattern. Moreover, these impacts are present for cigarette excise taxes, but not for other excise taxes. These results suggest that the welfare effects of cigarette taxation are far more complex than simple rational economic models might predict.

KEYWORDS: tobacco, excise taxes, happiness

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Economists have a well-established framework for understanding the welfare consequences of taxing goods that don't create externalities. Taxes create dead-weight loss by causing consumers to distort their consumption away from their preferred choices. This cost is weighed against the benefits of government revenue. As established in the seminal analysis of Becker and Murphy (1988), this argument applies equally well to both addictive and nonaddictive goods. The same type of revealed preference arguments that suggest that taxes reduce the welfare of consumers of nonaddictive goods can be extended to "rational addicts": agents decide to smoke by trading off the long-term costs of consumption against the immediate pleasures of consuming, all the while taking into account the addictive properties of the good in question. This model therefore suggests that the only justification for taxing addictive goods is the interpersonal externalities associated with consumption of those goods.

When debating tax policy towards addictive goods, however, policy makers often use an argument that falls outside of this framework. A classic example is smoking. Anti-smoking advocates argue that smoking is an unwanted habit, citing evidence such as the fact that eight in ten smokers expressing a desire to quit, and that the typical smoker tries to quit every eight and a half months (Gruber and Koszegi, 2001). Thus, they argue, cigarette taxes can actually make those at risk of smoking better off. By helping them quit an unwanted habit or prevent them from starting a bad one, the distortion in behavior caused by the taxes may actually raise welfare among those who are at risk of smoking. Some anecdotal evidence supports this claim. For example, following a 70 cent tax hike in New Jersey, one resident said "he is not bitter about the tax hike. Anything that can motivate me to quit, or motivate anyone else to quit, is worth having,"[1]

Despite its prevalence, this argument is hard to convincingly evaluate. For example, there is wide agreement that consumption of cigarette is fairly price sensitive (Chaloupka and Warner, 2001). But this fact doesn't shed light on welfare. If tax hikes reduce consumption where the benefits to the consumer exceed the costs, then the consumer is worse off, exactly as in standard incidence models. If, on the other hand, tax hikes are reducing an "unwanted habit", then smokers may be better off as some policy makers argue. Data on behavior alone, therefore, cannot resolve the issue.

1 http://www.newstribune.com/stories/071302/bus_0713020036.asp

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In this paper, we propose a new approach to evaluate this important argument. We suggest using a source of data that is sometimes used in other disciplines but rarely by economists: data on self-reported happiness.[2] In principle, happiness is a direct welfare measure that can overcome the limitations of other approaches to welfare analysis of policies such as excise taxation. Moreover, happiness measures have been repeatedly validated as a strong correlate of well-being, using alternative psychological, physiological, and economic measures of well-being.

Practically, we use data from the General Social Survey, which since 1973 has collected data on self-reported well-being for a nationally representative sample of respondents, matched to data on the level of state cigarette excise taxes. Since states have independently changed their taxes over time, we have significant variation with which to estimate the effect of cigarette tax changes on self-reported happiness. However, looking at how these tax changes affect happiness in the state as a whole would cause problems if other factors are changing along with these taxes. We therefore examine how tax changes differentially affect the happiness of those in a state who have a propensity to smoke. This strategy in essence uses those who are not likely to be smokers as a way of controlling for other shocks contemporaneous with cigarette tax changes. It also measures the effect of the tax on those who are hypothesized to be helped by it: smokers, former smokers, and potential smokers.

Our results are striking: those with a propensity to smoke are significantly happier when excise taxes rise. To corroborate this finding, we use parallel data from Canada, a country that has seen much larger excise tax changes in the recent past, and we obtain a very similar finding. Moreover, these results are robust to a battery of specification checks in both countries. Most importantly, we find that, while cigarette taxes make those with a propensity to smoke happier, other taxes do not. The fact that these conclusions emerge so clearly in two independent data sets, with different distributions of underlying happiness indicators, is quite striking.

We then turn to trying to offer some (loose) interpretation of these results. We argue that it is hard to rationalize these findings within a rational addiction framework. Rather, they appear to be much more consistent with alternative models, such as those that feature time inconsistency in decision making.

Our paper proceeds as follows. In Part I, we discuss the use of subjective well being indicators as a measure of welfare. In Part II, we discuss our data source and our

[2]Easterlin (1974) provides an important early exception. Recent examples of work by economists using happiness data include Blanchflower and Oswald (1996), Clark and Oswald (1994), DiTella, MacCulloch and Oswald (2001) and Easterlin (1995).

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empirical strategy. Part III presents our basic results, and Part IV presents a battery of specification checks, including results for Canada. Part V then discusses the interpretation of these findings. Part VI concludes.

Part I: What does Happiness Measure?

Our methodology relies on using subjectively reported happiness measures in empirical work. But how much can such measures be trusted? Economists worry about the validity of such questions and to some extent the scientific evidence supports these worries (Bertrand and Mullainathan 2001). A large array of evidence has shown that subjective survey questions are prone to significant reporting error. For example, studies have found that the placement of well-being questions affects how they are answered. If they are preceded by a question, for example, that asks about dating behavior, people are more likely to report unhappiness. Beyond order choice, instantaneous mood at the time of survey is also found to have a large effect on how people answer such questions. Schwarz and Strack (1999) provide a nice survey of these effects.

Yet such results only tell us that there is measurement error in these questions. There is also measurement error in the numerous other variables that economists study. What is more relevant for our purposes is that the evidence is clear that these questions also contain significant true signal about well-being. Evidence of this kind comes in several varieties but they all follow a similar methodology: find a more objective measure of well-being and see how well this measure correlates with the self-report. And strong positive correlations have been found for a large set of such variables. For example, outsider's assessments of a person's happiness or independent counts of smiles correlate positively with self-reported happiness. Moving to much more physiological measures, everything from heart rate, blood pressure, skin resistance measures of responses to stress, to even level of activity in the left versus right prefrontal lobe all are found to correlate with subjective reports of well being (Kahneman 1999; Gardner and Oswald, 2001). These studies all suggest that despite the measurement error inherent in this attitudinal question, it nevertheless correlates effectively with well being.

Moreover, the small happiness literature in economics also has uncovered interesting patterns further bolstering the idea that these variables in fact measure well-being. In cross-sections, happiness generally rises with factors that economists would associate with improved well being, such as higher incomes. The income effect appears to be causal, as it is present for lottery winners and those receiving inheritances (Gardner and Oswald, 2001). Self-reported well-being is also lower for the unemployed, and for those who are divorced (Blanchflower and Oswald, 2000); interestingly, however,

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the reduction in happiness due to unemployment is mitigated when there is a larger "reference group" of unemployed (Clark, forthcoming). Despite the increased use of this measure, there has been no attempt to date of which we are aware that uses these subjective well-being measures to draw welfare conclusions about particular tax or spending interventions. As a whole, therefore, the available evidence suggests that while subjective well-being measures do contain noise, they also contain significant signal and are a fruitful area for empirical exploration.

A more subtle concern raised in Bertrand and Mullainathan (2001) is that the measurement error in self-reported well-being may be correlated with other variables. This makes it hard to assess whether a factor is affecting happiness or simply the measurement error in happiness. But in our framework, for this to drive our results, the measurement error in happiness would have to change in specific states coincident with cigarette taxes and in such a way that it only affects those who are at most risk of smoking. It is hard to see how his could be driven by the considerations cited in Bertrand and Mullainathan (2001) as generating correlated measurement error, considerations such as cognitive dissonance and social reference effects.

Therefore, it seems plausible that happiness measures can be used for interpersonal comparisons in our current context. Our particular happiness measure is a 3-level indicator for happiness. Our use of this measure does not require a full cardinalization of utility, but rather simply that individuals accurately reflect their utility rankings across these three categories.

Part II: Data

Our primary data set is the General Social Survey (GSS). This is a nationally representative survey that has been administered to 1500 to 2500 households in most years since 1972; we use data from 1973 (the first year where state identifiers are available) through 1998. The GSS asks a variety of standard economics questions, but its use has mostly been in other disciplines, since the survey's main focus is on questions not traditionally used by economists: attitudes towards current events or political parties; religious devotion; and psychological measures such as happiness. It is the last measure that forms our key dependent variable. In particular, in each year the GSS asks respondents "Taken all together, how would you say things are these days -- would you say that you are very happy, pretty happy, or not too happy"?

A major advantage of the GSS for our purposes is that it has been carried out for many years. Over this long time period there have been very significant changes in the real excise tax rates charged by the states, both absolutely and relatively to each other. It is these changes that provide the identifying variation for our model. Data on state cigarette excise taxes come from the publication *The Tax Burden on*

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Tobacco. We use state excise tax values as of February of each year, as the GSS data were collected over the February-April period.

The first three columns of Table 1 show the means and standard deviations of the key variables for our analysis; we first show the means for all respondents, and then separately by smokers and nonsmokers. We use three dummy variables as our dependent variables for measuring happiness, corresponding to the three possible answers to the happiness question above. Over our entire sample, in the United States 32% of respondents report themselves to be very happy, 55% are pretty happy, and 12% are not happy.

The GSS collected data directly on smoking behavior, but only from 1977 to 1993. In those years, 35% of the U.S. sample reports themselves as smokers, which is consistent with prevalence rates over this time period. Table 1 also summarizes the data sets based on whether the person reports being a smoker or a non-smoker. Smokers are somewhat less happy than non-smokers; while this consistent with the notion that they would like to quit but cannot, it is equally consistent with heterogeneity in smoking behavior by underlying happiness.

The average real (in 1999 dollars) excise tax rate on cigarettes in the US is 31.6 cents, with a standard deviation of 15.8 cents. There is wide variation in excise taxes across states, over time, and within states over time; 25% of the variation in excise taxes is within states over time. This allows us to control for fixed state differences in cigarette taxes and happiness in our analysis below.

Table 1 also shows the means for the key control variables used in our analysis. Some interesting features are worth noting. Smokers are less educated; for example, they have a high school dropout rate of 33% compared to 25% for non-smokers. They are also more likely to be unemployed and less likely to be out of the labor force, although this likely largely reflects the fact that the smoking rate is much higher among males.

Finally, income is available only categorically in the GSS, in fine gradations until the top of the income distribution, then in larger intervals and finally a top code. In order to create a smooth income measure, we have used data from each year's Current Population Survey to impute values to each of these larger ranges and the top-coded range.

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Part III: Basic Results

For our initial regression analysis, we follow the literature on the impact of taxation on smoking, simply replacing the dependent variable, traditionally smoking, with happiness. Let H_{ijt} be the happiness of individual i who lives in state j at time t, and T_{jt} be the real level of cigarette taxes in state j at time t. A simple regression that relates happiness to cigarette taxes in the state would be:

$$(1) \qquad H_{ijt} = \alpha + \beta_j + \eta_t + \delta T_{jt} + \zeta X_{ijt}$$

where β_j are state fixed effects and η_t are year fixed effects. These fixed effects completely control for any fixed differences between states and between years, which means that only within-state variation in cigarette taxes is used in the estimation. We include a variety of X variables to control for other determinants of happiness: five-year age category and gender interactions; household income quartile dummies; education categories (high school dropout, high school graduate, some college, and college graduate); education of the respondents mother and father (by the same categories); race (white, black, and other); marital status (married, divorced/separated, widowed, never married); dummies for number of children; dummies for full time work, part time work, unemployed, out of labor force, and whether ever worked; religious attendance (a nine value index that rises monotonically with attendance frequency); and the state/year unemployment rate.

Since we have three discrete levels of reported happiness, we simply estimate three linear probability models; the results are similar if probit models are estimated instead. In estimating all our equations, we adjust the standard error to allow for both auto-correlation and the grouped data, as suggested by Bertrand, Duflo and Mullainathan (2001). We do this by performing a White correction that allows for an arbitrary variance-covariance matrix within states.

The results of estimating a regression such as this are presented in the first three columns of Table 2. The results here are somewhat mixed. There is a negative effect on being very happy, a positive effect on being somewhat happy, and a negative effect on being unhappy. The last of these is the closest to significance, but none of the coefficients are large relative to their standard errors. From this series of regressions, one would conclude that there is no consistent impact of cigarette taxes on happiness.

These columns also show the coefficients on many of the regressors included in this happiness equation (we do not show the coefficients on the age/sex, state, and year controls). Blacks are less happy, as are those with children. Married persons are much happier, as are those with higher incomes.

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Those who are unemployed are less happy, but those who are not in the labor force are happier. Those who attend religious services more frequently are also much happier.

But this simple regression has several problems. First, if states are changing cigarette taxes at different points in their state business cycle (in a manner which is not captured by our state/year unemployment rate control), the estimated "effect" may instead reflect the effect of these economic conditions on happiness. Another potential omitted factor from this model is the state spending (or reduced other taxes) that is financed by cigarette taxation. If we find that higher cigarette taxes lead to a general rise in happiness, this could simply reflect the fact that these revenues are used in a welfare-enhancing way. Finally, not all members of the state are at risk of smoking. The arguments made by policy makers rely on helping people quit or preventing them from starting, suggesting that those who are at risk of smoking should have the largest happiness effects.

To address this problem, we follow a long-standing approach in labor economics, particularly for the evaluation of welfare programs: we contrast the impact of taxes on those likely to be affected, relative to those unlikely to be affected.[3] That is, we exploit the fact that cigarette taxes should only affect the happiness of those who are at smokers, former smokers, and potential smokers. We can therefore compare the effect of taxes on this group to the impact on those who are at a lower risk of smoking.

We cannot do so by using direct data on smoking, because smoking is itself a function of the tax rate. This leads to a potential sample selection bias: if those who stop smoking when tax rates go up are happier on average than those who continue smoking, this would bias us towards finding a reduction in happiness among (remaining) smokers as taxes rise. It is for this reason that the earlier literature does not generally compare those who receive welfare to those who do not, but rather those who are likely to receive welfare (e.g. low educated single mothers) to those who are not (e.g. highly educated married women).

In principle, we could surmount this problem by instrumenting smoking. But, in practice, there are also two additional concerns with using direct smoking information in our context. First, the happiness effect in our model should operate through current smokers (who are potentially reducing their quantity smoked in response to the tax), former smokers (who have quit in response to the tax), and even

3 A classic examples of this approach is Ellwood and Bane (1985), who use demographic characteristics to distinguish those whose family structure and living arrangements are likely and unlikely to be affected by welfare benefit changes.

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potential smokers (whose happiness may change through the reduced opportunity to smoke). That is, it is directly the *propensity to be a smoker* that is of interest in our context, not being a current smoker per se. Therefore, by using current smokers only, we would obtain a misleading answer as to the total effect of the tax. Second, smoking data are only available for a subset of years in the GSS, severely limiting our sample size.

We therefore follow the strategy of earlier work in comparing the impact of excise taxation on those with a propensity to smoke. Specifically, we first estimate a regression that relates smoking behavior to the observable predictors of smoking we see in the GSS data, which are the set of Xs included in the regression above (and listed above). We estimate such an equation for each year that has smoking information, and use that to form a predicted probability of smoking ($PSMOKE_{ijt}$).[4] The R-squared on these prediction equations vary between 0.13 and 0.2, with F-statistics on the regression that are always highly significant. Appendix Table 1 shows a sample regression, from 1977 (the first year of the data). While we cannot perfectly predict smoking, there are some clear correlates in the data, most notably age/sex, education, and religious participation.

The simplest means of comparing those who do and do not have a propensity to smoke is to split the sample based on this predicted smoker index. In the first and second rows of Table 3, we show the results of our regression when we split the sample into those at a high propensity to smoke (predicted odds of smoking more than the sample median), and those with a low propensity (predicted odds of smoking less than the sample median). Doing this split, we see that there is an equal insignificant impact on being very happy in both samples. There is a stronger but insignificant effect of being somewhat happy in the high propensity sample, but not in the low propensity sample. Most notably, there is now a negative and marginally significant impact of taxes on unhappiness in the high propensity group, and a positive and insignificant impact in the low propensity group. These coefficients are significantly different from each other, showing that higher taxes reduce unhappiness more for those with a propensity to smoke.

These regressions are somewhat difficult to interpret, however, and they do not use the full information available from our smoking risk variable. We therefore next estimate equations of the form:

4 In years before the first smoking information is available (1973-76), we use the first available year of information to form the prediction. In years after the last smoking information is available (1994-98 in U.S), we use the last available year. In years between, we interpolate from years that have the smoking information.

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$$(2) \qquad H_{ijt} = \alpha + \beta_j + \eta_t + \delta T_{jt} + \theta PSMOKE_{ijt} + \gamma T_{jt} * PSMOKE_{ijt} + \zeta X_{ijt}$$

where the coefficient of interest is now γ, the coefficient on the interaction of the tax rate and the smoking propensity index. We also include the set of covariates, X, that were used to predict smoking for our smoking propensity measure, and which may have independent effects on happiness, as well as a full set of state and year dummies. Note that when these X variables are included, the coefficient θ is still identified by the fact that the prediction equations are estimated year-by-year, while we don't allow for year-specific coefficients on the Xs. But this is a very tenuous source of identification, and as a result we don't focus our analysis on the direct effect of PSMOKE. Rather, the key coefficient is γ on the interaction between PSMOKE and T. This is well identified by the interaction between changing taxes within states over time and the predicted smoking risk. That is, we are now asking whether deviations in cigarette taxes from their state-specific mean cause a relative change in the happiness of those who have a high propensity to smoke relative to those who have a low propensity to smoke.

To see the intuition of this approach, imagine that we had simply contrasted the impact of taxation on those with low education (relatively likely to smoke) and high education (relatively unlikely to smoke). The coefficient on education says nothing about the impact of taxes on happiness; it is included as a control, the role played by PSMOKE in equation (2). But the interaction of education with taxes would tell us whether taxes have a larger impact on those with a propensity to smoke. Our approach uses a full set of variables to get a richer prediction of smoking behavior, but the intuition is the same. The Appendix discusses this approach relative to an IV approach, and shows that our results are of a similar sign and significance, although larger in magnitude (as we predict) from this alternative approach.

Of course, this approach still makes a critical identifying assumption, which is that omitted factors impact those with a propensity to smoke and those without a propensity to smoke in the same way. We will provide some sensitivity tests below to demonstrate the validity of this assumption.

The second panel of Table 3 shows our findings from this approach. There is a positive but insignificant interaction between cigarette taxes and the propensity to smoke for the equations modeling "very happy" and "somewhat happy". But there is a negative and very significant effect on the probability of answering "Not happy". Specifically, the interaction term between the predicted smoking variable and the tax rate in column (3) is significantly negative. This suggests that cigarette taxes especially reduce unhappiness amongst those with a propensity to smoke. There are no significant effects on those unlikely to be smokers (as shown by the insignificant

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main effect on the tax variable in the regression); that is, as we see when we split the sample, higher taxes reduce the unhappiness of those with a propensity to smoke, with no effect on those less likely to be smokers. Given that the tax effect is shown most clearly in terms of a reduction in unhappiness, we focus on this dependent variable for the remainder of our analysis.

Interpreting the magnitude of these effects requires care. Since we are interested in comparing the effect on those with a propensity to smoke, relative to those without a propensity to smoke, one cannot simply compare those with PSMOKE =1 and PSMOKE =0. Instead, one should compare those demographic groups with high and low propensities to smoke. An easy way to do this is to focus on the demographics of actual smokers and non-smokers. The mean rate of predicted smoking (based on demographics) in our sample of smokers is 0.46, and the mean rate in our sample of non-smokers is 0.30. So a reasonable comparison is between those with values of PSMOKE of .46 and .30. This is the equivalent of comparing two demographic groups, one of which represents those very likely to be smokers and the other of which represents those unlikely to be smokers. With this comparison, a $1 rise in taxes reduces unhappiness of those with a propensity to smoke by 0.16*-0.156 or roughly 2.5 percentage points, relative to those unlikely to smoke.

There are two ways to gauge the size of this effect. The first is to contrast it to the impact of other predictors of happiness. For example, we find that, conditional on all other Xs, moving from the bottom to the third income quartile reduces unhappiness by about 4 percentage points. So a $1.60 tax (roughly five times the sample mean) would be equivalent to moving someone with a propensity to smoke from the bottom to the third income quartile. Of course, this comparison is difficult because the impact of income on happiness is not well identified in our models. But our income coefficient is strikingly similar to that obtained by Gardner and Oswald (2001) in their study of the impact of random windfalls on happiness in the U.K., suggesting that this cross-sectional approach can yield useful estimates of the impact of income on happiness.[5]

[5] Gardner and Oswald find that a $75,000 windfall reduces happiness by 0.2 standard deviations in their more convincing differences model. We estimate that moving from the bottom to the top income quartile would raise income by $75,000 in real terms, and is associated with a 0.24 standard deviation drop in unhappiness.

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A different comparison is to consider what these results imply if the effects on happiness is only through reducing smoking. Estimates of the impact of excise taxes on tobacco expenditures are generally in the range of -0.5, although Gruber and Koszegi (2002) obtain a higher elasticity of roughly -0.6 using more recent data. These estimates suggest that each 10 cent increase in price leads to a 6% decline in smoking. Our happiness regressions suggest that this tax rise is also associated with a decrease in 0.25 percentage points of unhappiness amongst those with a propensity to smoke, or roughly 1.6% of the baseline level of unhappiness of those who are smokers. Extrapolating, then, these findings suggest that reducing smoking by 100% would remove 27%of the unhappiness of smokers in the U.S. Alternatively, we find that reducing smoking by 100% would remove roughly two-thirds of the six percentage point unhappiness "gap" between smokers and non-smokers shown in Table 1. Obviously, though, these will be over-estimates since the taxes also operate in other ways, such as helping ex-smokers stay quit or preventing some from starting smoking in the first place.

Part IV: Specification Checks

Contrast to Canada

Our first check of these striking results is to replicate them in a fully independent setting, Canada. Like its U.S. counterpart, Canada's General Social Survey has periodically since 1985 collected data on the happiness of a large sample of Canadians; we use all available surveys that include a happiness question (1985, 1986, 1989, 1990, 1991, 1996, and 1998).[6] The Canadian GSS question asks "Would you describe yourself as very happy, somewhat happy, somewhat unhappy, or very unhappy", and there is also an option for "no opinion". Since only a very small share of the sample responds that they are very unhappy or no opinion, we combine those responses with somewhat unhappy to form our unhappiness category.[7] Moreover, as in the U.S., there have been very significant excise tax changes in Canada over this time period. Data on Canadian tobacco taxes were collected by

6 The U.S. GSS survey is a random sample and requires no weighting, but the Canadian GSS is not nationally representative unless weighted, so that all of our regression estimates use survey weights.

7 The wording of the Canadian question changes somewhat over time. In 1986, the question adds "Presently, would you describe yourself as…", and the 1991 and 1996 surveys add "usually, would you describe yourself as…". These wording changes appear to affect the distribution of responses across the very and somewhat happy categories, but do not impact the share of the sample saying that they are unhappy, which is the category upon which we focus. Any overall impacts from wording changes will be captured in the year dummies included in the regression.

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Gruber, Sen and Stabile (2002), and incorporate both federal and provincial excise and sales taxes on cigarettes.

We use the tax rate as of the month of the survey, since the Canadian GSS was collected in various months of the year over time.

The second set of columns of Table 1 shows the means of our data for the Canadian GSS. Canadians report much higher levels of happiness than the U.S.: only 5% of the people report being unhappy, 34% report being "somewhat happy" and 59% report being "very happy" (with 2% missing). These differences are consistent with the literature reviewed earlier, which discusses the sensitivity of the happiness responses across countries or types of wording. But the consistent impacts of cigarette taxation we will see in both countries below confirm that these differences do not interfere with our tests.

Data on smoking is collected in the Canadian GSS in 1986, 1991, and 1996. In those years, 30% of Canadians report smoking. As in the U.S., smokers are less happy on average, less educated, and less attached to the labor force. We have endeavored to use as much as possible a common set of control variables in the two data sets, but the available variables are not identical (e.g. there are no consistent labor supply measures in the Canadian GSS).

The prediction equation for smoking in the Canadian GSS for 1986 is shown in Appendix Table 1 as well. The R-squared of the predictions regressions is roughly 0.1 in all three years, with age/sex, marital status, education, religious participation, and income (measured at both the household and individual level) significant predictors of smoking.

Taxes are higher in Canada, with a mean tax rate of over $1 (Canadian) per pack. The average PPP exchange rate over our time period was 1.26 (OECD, 2002), so that this is a much higher tax per pack even in constant dollar terms. Moreover, the tax in Canada is much more variable over this time period, with a standard deviation that is 2.5 times as large as in the U.S.

Table 4 shows a parallel set of regressions to the U.S. results, using our Canadian data. The first three columns show the results from regression specification (1), regressing happiness on the tax rate. Once again, these simple regressions do not show a very significant pattern of results, although in this case there is a marginally significant reduction in overall unhappiness when taxes are higher.

We therefore once again attempt to increase the efficiency of this test by interacting the excise tax with our smoker propensity index.

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So we reestimate (2) for this Canadian sample, including the tax rate, PSMOKE (estimated now for Canada, as shown for one year in the second column of the Appendix table), and the interaction of the two. As the second set of columns in Table 4 show, we once again see a very statistically significant reduction in the odds of being unhappy from high taxes for those with a propensity to smoke; the interaction of PSMOKE and TAX in the last column is highly significant. There is a reduction in the odds of being somewhat happy as well, and a rise in the odds of being very happy.

In absolute value, the magnitudes from this test in Canada are much smaller than in the U.S. Here, we find that (at the mean difference in predicted smoking rates among smokers and nonsmokers), the estimates imply that a $1.00 tax increase would reduce unhappiness by 0.6 percentage points. This implies that it would take a $3.57 tax to parallel the happiness effects of moving from the bottom to the third income quartile (a 2.8 percentage point reduction in unhappiness, accounting for both the personal and household income effects).

But, relative to baseline prices, which are about three times as large in Canada as the U.S., the effects are remarkably similar across countries. Using the estimated elasticity of consumption of -0.45 from Gruber, Sen and Stabile (2002), we estimate that a ten cent tax increase would reduce smoking by 3% in Canada. This same tax increase would reduce unhappiness by 0.06 percentage points, or 0.94% of baseline. Thus, similarly to the U.S., the results imply that reducing smoking by 100% would remove roughly 30% of the unhappiness of smokers. Alternatively, reducing smoking by 100% would remove roughly all of the gaps in unhappiness between smokers and nonsmokers.

One possible interpretation of this result could come from the higher level of base prices in Canada, relative to the U.S. Given these high taxes already in place in Canada, the remaining pool of smokers may be those with the largest self-control problems. These smokers may need much larger tax changes to dissuade them from smoking.

Beyond the exact magnitudes, what is critical here is that we have replicated this exercise in a completely independent data set, with a different (and more dramatic) set of policy changes. Yet we find a very similar pattern of results. This confirms that our findings are not driven by any peculiarities of the U.S. GSS, or institutional particularities of our country. For the remainder of the paper, we show our remaining analysis for both countries where possible.

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Control Group Validity

One specific concern that is not fully addressed by our use of the Canadian data is that those who do not have a propensity to smoke don't form a legitimate control group for assessing the impact of taxes on those with a propensity to smoke. For example, these groups may be impacted differently by business cycle effects that are correlated with tax rate setting. Or, if government spending is more redistributive than excise taxation, or at least valued more by the type of individuals who have a propensity to smoke, our finding could reflect the happiness effects of spending, not excise taxation.

We address this concern in two ways, focusing on the "unhappy" variables in both countries, and the specification that interacts taxes with predicted smoking (equation (2)). First, in Table 5, we assess whether there are omitted interactions of other types that might explain the observed significant interaction of taxes and predicted smoking. In the first column, we show our basic results from Tables 3 and 4 for comparison. In the second column, we interact the state/year unemployment rate with PSMOKE, to capture any differential impacts of the cycle on the happiness of those with different smoking propensities; this has no impact on our estimates. In the third column, we interact a linear time trend with each of the state dummies, to allow for any slow moving correlations between state tax setting and well-being in that state; this serves to strengthen our results somewhat, particularly for Canada. In the fourth column, we interact a linear time trend with the predicted smoking measure, to allow for separate trends in well-being for those with different propensities to smoke; once again, there is little impact. In the fifth column, we interact each state dummy with PSMOKE to allow for the effect of smoking propensities to vary by state; this reduces the estimate somewhat in the U.S., but raises it in Canada. Overall, our findings are reasonably robust to these controls for differential sensitivity to cyclical conditions, slow-moving trends in the data, or heterogeneity in populations across states.

Table 6 presents an even more convincing test: comparing the impact of cigarette excise taxes to other taxes. If we are finding a spending effect, or even some other type of political economy effect, we should see a similar effect of an interaction of these other taxes with the propensity to smoke. To address this point, we have gathered data on three other state or province taxes: the excise taxes on gas and

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alcohol, and the state or province sales tax rate.[8] We have also gathered data on state or province real revenues per capita. We add to our regression specification these tax variables, as well as their own interactions with PSMOKE. If we are finding a general tax effect, then these interactions should be at least as important in our regression as the interaction of the cigarette excise tax with smoking propensities. But, if we are finding a cigarette tax-specific effect, then our existing results should not much change, and these additional interactions should be insignificant. As in Table 5, we focus on the model of unhappiness in both the U.S. and Canada.

In column (1), we see the effect of the beer tax, in column (2), the effect of the gas tax, in column (3) the effect of the sales tax and in column (4), the effect of total state revenues. In all four cases and in both countries, we see that the inclusion of these variables does not much affect the initial estimate of the cigarette tax*smoking propensity interaction. Moreover, the new interaction terms with other taxes themselves are never negative and significant, although, for the U.S., there is a marginally significant negative effect of revenues per capita. For the beer tax in the U.S., and for the gas and sales taxes in Canada, the interaction with smoking propensity is actually positive and significant, suggesting that higher tax rates on those items raise unhappiness among those with a propensity to smoke. This may reflect the fact that these regressive taxes are targeted to those low income persons most likely to smoke. But, if anything, they suggest a bias against our finding for cigarette taxation. Thus there is little evidence that it is *spending* of tax money (rather than the tax itself) that is affecting smoker happiness.

Yet another possibility is that cigarette taxes are somehow spent differently than other kinds of taxes, so that there remains a happiness effect through the revenue side. We have investigated this possibility by gathering data on the composition of public spending in the U.S. over the 1977-1999 period, decomposing total spending into spending on: educational services, social services, transportation, public safety, environment and housing, government administration, utility expenditures and other spending. We then regressed each of these spending categories on the different taxes to determine whether the marginal effect of cigarette taxes was different than the other taxes we have studied. No significant pattern was found. This suggests that differential spending of cigarette tax revenues does not drive our results.

8 The sales tax rate in both countries is an ad valorem rate; the gasoline tax is cents per gallon in the U.S. and cents per litre in Canada; the alcohol tax is dollars per case of beer in the U.S. and ad valorem rate in Canada. All dollar tax rates are expressed in real terms.

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Part V: Interpretation

The analysis thus far has demonstrated that higher cigarette taxes are clearly associated with a rise in the reported well-being of those with a propensity to smoke. How do we interpret these results? On their face, they appear *inconsistent with the standard rational addiction model*. In that model, there is no reason for smokers to see an increase in utility when taxes are higher.

If higher taxes were to raise utility, then the rational addict could achieve the same outcome by simply reducing smoking by the amount that the tax does, i.e. by emulating the tax. So cigarette taxes should reduce the happiness of Becker-Murphy rational addicts.

But there are alternative interpretations that could rescue the rational addiction model. One possibility is to argue that it is not smokers who are made happier but instead the spouses and relatives of smokers. Since our identification strategy compares those with a propensity to smoke to those without this propensity, our estimates would also include this externality effect if spouses and relatives have similar background characteristics.

Of course, if higher taxes made family members better off, then this would indicate another potential failure of the standard model: imperfect family utility maximization. That is, by the same logic that shows that rational addicts cannot be made better off by a higher tax, families of smokers cannot be made better off by a higher tax if the smoker was maximizing family utility. If family utility was being maximized, and family members were on net better off with less smoking, then smoking would have already fallen. But the failure to maximize family utility is a very different type of failure than that discussed earlier, so it is important to distinguish whether this is driving our results.

We have investigated this possibility in two ways. First, we separately estimated our models by demographic groups. If our effects are due to individual internalities, there is no *a priori* reason to believe the self-control problem ought to be greater for any particular group. If, on the other hand, our effects were due to intra-family externalities, one would expect differences. Specifically, one would expect married people to show bigger effects since they are more likely to experience the externalities of smoking. Moreover, since men smoke more, wives should experience a bigger externality than husbands. In fact, however, we found no

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consistent pattern of results across the two countries; the effects were indeed largest for married women in the U.S., but they were largest for single men in Canada.

We therefore explored an alternative approach in the U.S. data. Using the available information on spousal characteristics in the survey data, we estimate spousal predicted smoking as a function of the same set of covariates as above, but using the spouse's education and labor supply in place of the respondent. This allows us to augment our regression model by also including the predicted smoking of the spouse and it's interaction with the tax. If our results are driven by within-family externalities, then we would see effects through spousal smoking propensities, and reduced or zero effects through own smoking propensities.

Unfortunately, this exercise cannot be carried out in Canada due to the paucity of information on spousal characteristics.

As Table 7 shows, we do find some weak evidence for a role for spousal smoking in determining own happiness. The first column of Table 7 presents our model estimated only for married persons; the effect here is somewhat larger than in the full sample. We then include spousal smoking propensity and its interaction with the tax rate; in these models, we are also including as X variables the additional spousal characteristics that determine spousal smoking propensities. The interaction of spousal smoking propensities and the tax is negative and marginally significant for all married persons; that is, married couples where the spouse is more likely to smoke are also made marginally better off by the tax. But the inclusion of this term has no effect on our key estimated interaction of respondent's smoking propensity with the tax. Thus, within-family externalities may be present, but they appear unable to explain our results.

A second, more fundamental, complication in interpreting these results as evidence against the rational addiction model is the timing of the happiness gains. For a rational addict, the cost of quitting smoking today is clearly negative, but the effect on future happiness can be positive. This is because reducing smoking today can raise future utility. Put another way, the tax inducing someone to reduce smoking is analogous to an investment in which he bears a cost today (immediate pain of withdrawal) and reaps a benefit in the future (longer life expectancy). The net effect of this investment on utility (using the smoker's discount rate) is negative in the rational addiction model.

But it is possible that our measure is under-measuring the short-run costs and over-weighting the long-run benefits, thereby producing an illusory positive gain to smokers. The problem is that our existing test does not measure the immediate impacts of the tax, but rather the average impacts over time. Since we are regressing

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current happiness on current taxes, our estimated coefficients include the immediate effect of taxes on happiness. But if taxes are correlated over time, they will also include the lagged effect. Specifically, the more auto-correlated are cigarette taxes, the more the estimated effect in equation (2) includes the effect of lagged taxes. Thus, our test cannot rule out that time consistent smokers are being made better off in the long run, which through serially correlated tobacco taxes appears as an effect of the current tax on happiness.

A stronger test would be to assess the *immediate* impact of tax increases on well-being; this should clearly be negative for the rational addict.[9] Specifically, we would like to model the short run impact of taxes on happiness, examining period to period changes rather than deviations from long run state means. But doing so increases our data requirements dramatically. To measure the average effect over time in a fixed effects framework, all we require is that, summed over all periods before and after a tax changes in a state, we have sufficient observations to identify an impact of a tax change. But, to examine an immediate impact requires having data in one period on enough observations to distinguish the impact of taxation. This is impossible in the U.S. GSS. This data has the advantage of many years of information, but the typical sample size in any year is fewer than 2000 observations, which is then divided over 50 states. When years are pooled, our state specific sample sizes are sufficient to identify average tax effects. But identifying short run effects is impossible.

The Canadian GSS, however, does permit a short run comparison. Our Canadian GSS data have between 9300 and 27,600 observations per year. Moreover, these are divided over only 10 provinces, so that the average province/year cell size is over 2000 observations. Thus, we can aggregate these data to the province/year level and estimate changes regressions that allow us to examine short run impacts of tax changes.

To do so, we divide our Canadian GSS sample into those with a propensity to smoke, and those without a propensity to smoke; the former group is composed of those

9 In fact, ideally, we would examine the *ex-ante* impact of taxes on well-being, before the tax is even in place, to avoid any issue of quitting costs versus gains in health. One approach to doing so would be to assess the impact of excise taxes in the period when they are enacted, but not yet effective, following Gruber and Koszegi (2001); but the data requirements for such a test (large samples at a monthly frequency) are not met here. Another alternative would be to look at the stated preferences of smokers for cigarette taxation, or their patterns of voting on cigarette –related propositions. While such data is hard to find, it is even harder to interpret when it does exist. What would constitute evidence for the argument that smokers are made happier by cigarette taxes? Would a 60% support rate be enough? Smokers may favor cigarette taxes because they help them quit, but oppose such taxes on general political/redistributive grounds or other principles. Clearly this number would need to be bench-marked but it is not clear against what.

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above the 75th percentile of the predicted smoker distribution (a 41% chance of smoking or greater), while the latter is composed of those below the 25th percentile (a 19% chance of smoking or smaller). We then create a data set where each observation measures the mean level of unhappiness and excise taxes for each smoking propensity group in each province in each survey year. Using these data, we regress the *change* in mean happiness on the *change* in mean excise taxes separately for each group, including a full set of year dummies to capture time trends in happiness changes.

By using changes, rather than fixed effects, we measure the short run, rather than the average longer run, effects of tax changes on well-being.

Estimating this changes model for those with a propensity to, we obtain a coefficient of -.044 (0.016). This result confirms that, for those with a propensity to smoke, there is a short-run negative effect of higher taxes on unhappiness; when taxes rise, happiness falls. For those without a propensity to smoke, on the other hand, this changes model yields an estimate of –0.009 (0.008), which is both statistically insignificant, and statistically significantly lower than the estimate for those with a propensity to smoke, confirming the causal interpretation of our finding for predicted smokers. Thus, it appears that, inconsistent with the rational addiction view, even in the short run smokers are made happier.

Of course, one could always argue that we are not looking short run enough. Perhaps the negative effects of quitting occur at much higher frequencies. If the costs of quitting are high enough and/or discount rates are high enough, even within one year a rational addict could be made better off from reducing smoking. But it seems unlikely that the cost of quitting could be that high, relative to the long-term benefits of having quit. For example, our estimates imply that quitting smoking would reduce unhappiness by about the same amount as moving from the bottom to the third income quartile, a rise in income of $15,000. For a discount rate of 5%, the costs of quitting would have to be *over $275,000* for a rational addict to be made better off within a year from quitting smoking. Even if the discount rate is 10%, the cost of quitting would have to be almost $170,000 for the rational addict to be better off. These estimates seem implausibly large. Within a rational addiction framework, it is highly unlikely that smokers could be made better off in the short run.

If our results contradict the rational addiction model, which models do they support? One is the model of Gruber and Koszegi (2001, 2002), who embed within the Becker-Murphy stock addiction framework preferences that are *time inconsistent*, following Laibson (1997) and O'Donoghue and Rabin (1999). The Becker-Murphy model features exponential discounting, where consumption k periods ahead is

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discounted by δ^k, for any k. In this quasi-hyperbolic formulation used by Gruber and Koszegi, the discounting is exponential starting next period, but that entire future is discounted by an extra factor from today's perspective. More specifically, next period is discounted by $\beta\delta$, the following period by $\beta\delta^2$, and k periods in the future by $\beta\delta^k$, where $\beta<1$ is an extra discount factor that changes the discounting of this period relative to the entire future.

The key feature of such a hyperbolic model is that individuals will have self-control problems. Specifically, individuals would like to smoke less in the future than he actually can. The problem arises because he is patient about the future (the relative discount rate between future periods is δ), but impatient about the present (the relative discount rate between today and tomorrow is $\beta\delta<\delta$). So when planning about the future, the hyperbolic person makes patient Plans (e.g. smoke less) but when the time comes to act they always end up making impatient choices (e.g. smoke more).

Because of this time inconsistency, cigarette taxes can raise the discounted utility of sophisticated hyperbolic consumers. The reason is that the tax serves as a self-commitment device.[10] By forcing a reduction in the smoking in the future, the tax allows the hyperbolic agent to do something they would not be otherwise be able to do.[11] Our findings are therefore potentially consistent with this alternative to the rational addiction model.

Moreover, quantitatively the findings can give guidance about choosing between different hyperbolic models. One key variable in hyperbolic models is the degree of naivete agents have about their self-control problems. This is readily modeled by assuming that agents think their discount function is described by a hyperbolic (β', δ). They have complete self-knowledge of their self-control problem if $\beta'=\beta$, and have no self-knowledge if $\beta'=1$, with intermediate values parameterizing the extent of naivete. Our results above are most consistent with intermediate cases: $\beta < \beta' < 1$. If $\beta' = \beta$, since agents are fully aware of their self-control problems, they should be demanding self-control devices quite strongly since our results suggest the happiness gains are very large, yet we do not see smokers organizing to lobby en masse for cigarette taxes. At the other extreme, if $\beta' = 1$, agents would not experience a large immediate utility gain from a cigarette taxes since they (incorrectly) presume that they would have stopped smoking anyway. The large utility gains are, therefore, best explained by models where smokers recognize they have self-control problems but

[10] As Gruber and Koszegi (2002) discuss, this government-provided commitment device is valued by consumers because the private sector cannot plausibly provide true commitment.

[11] Of course, a sophisticated time inconsistent consumer's first choice would be a tax that started next period, but Gruber and Koszegi show that even a tax that starts this period would make time inconsistent smokers better off.

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under-appreciate their magnitude ($\beta < \beta' < 1$). Thus they are made even happier by taxes than they would have forecast.

Of course, the Gruber and Koszegi model is not the only alternative which highlights self-control problems. In the temptation model of Bernheim and Rangel (2001), the failures of rational addition are not due to preferences, but rather due to cognitive processing. Agents essentially face "tempted" and "untempted" states, and, in the tempted state, they lose self-control and overconsume the addictive goods. But there is, by definition, no price elasticity in the "tempted" state, so that higher prices serve no self-control purpose; thus, higher prices only make them worse off. Similarly, in the model of Gul and Pessendorfer (2001), there is a direct disutility from being tempted; but, so long as the agent can afford the product which is tempting them, there is no reduction in this disutility from higher prices. Thus, on their face, neither of these models is consistent with the findings of this paper. But variations on these models which introduce price elasticities into the "tempted" state could deliver similar implications for well-being to those derived from the sophisticated hyperbolic model.

Thus, the results presented here are consistent with several self-control alternatives to the Becker-Murphy model. Further distinguishing among these particular alternatives is difficult. But the key point is that most alternatives which imply a rise in well-being from higher excise taxation have radical implications for government policy. Under the rational addiction model, there is no rationale for government regulation of addictive bads other than interpersonal externalities; for cigarettes, in fact, most estimates of the externalities from smoking are well below the existing average level of excise taxation (Gruber and Koszegi, 2002). But alternatives such as time inconsistent models suggest that the optimal excise tax is greater than zero even absent externalities, due to the self-control benefits to time inconsistent agents. For example, in their specific model Gruber and Koszegi (2002) find that the optimal tax, even with very modest time inconsistency, is well above $1 per pack, above and beyond externalities. If time inconsistency is the explanation for the large findings here, it seems likely that the optimal tax would be much higher.

Part VI: Conclusions

The results in this paper have potentially important implications for how policy makers should view smoking in general and cigarette taxes in particular. In particular, they suggest that smokers themselves may be made better off by cigarette taxes.

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This result is inconsistent with rational views of smoking that would view such a tax as a pure hindrance on smokers, and more consistent with alternatives such as behavioral time-inconsistent models in which these taxes may serve as self-control devices.

The methodology used in this paper should also have broader interest. Economists are often concerned with welfare, with how policies affect the happiness of people. Yet there are few tools for empirically assessing welfare. In the case of smoking, as with many other behaviors, behavioral reactions to changes in the environment can only provide limited insight into the welfare implications of policy interventions. Theories that have very different policy implications can accommodate a variety of behaviors and, as a consequence, empirical work on behavioral responses can leave us in the dark about welfare.

Subjective well-being measures provide a possible way to directly address welfare questions. As our analysis shows, this direct approach is empirically feasible. Happiness measures may be noisy, but in our case at least, they contain sufficient signal to discern effects of moderate size policies. This is heartening because happiness data is abundant. In the US, the GSS is available in moderately large samples for many years. Looking beyond the US, the Canada data we use is not the exception but rather the rule: many countries, notably in Europe, collect cross-sections and panel data on happiness. In short, the results in this paper suggest that by using happiness data, economists may be able to directly assess the impacts of public policy on well-being.

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Table 1: Summary Statistics

	US Data			Canadian Data		
	All	Smoker?		All	Smoker?	
		No	Yes		No	Yes
Report "Very Happy"	0.320	0.355	0.277	0.588	0.720	0.646
	(.466)	(.479)	(.448)	(.492)	(.449)	(.478)
Report "Pretty Happy" (US) or "Somewhat Happy" (Canada)	0.554	0.543	0.559	0.338	0.226	0.281
	(.497)	(.498)	(.497)	(.473)	(.418)	(.450)
Report "Not Too Happy" (US) or "Unhappy" (Canada)	0.119	0.091	0.152	0.050	0.044	0.064
	(.323)	(.288)	(.359)	(.217)	(.205)	(.245)
Real Tax Rate	0.316	0.280	0.278	1.170	1.227	1.211
	(.158)	(.115)	(.117)	(.394)	(.494)	(.490)
Predicted Smoker	0.352	0.298	0.456	0.300	0.247	0.365
	(.190)	(.171)	(.174)	(.154)	(.147)	(.148)
Smoke?	0.352	0.000	1.000	0.276	0.000	1.000
	(.478)			(.447)		
White	0.835	0.844	0.832			
	(.371)	(.363)	(.374)			
Black	0.135	0.128	0.144			
	(.342)	(.334)	(.351)			
Married	0.565	0.583	0.548	0.552	0.543	0.515
	(.496)	(.493)	(.498)	(.497)	(.498)	(.500)
Separated or Divorced	0.145	0.113	0.191	0.084	0.066	0.126
	(.353)	(.317)	(.393)	(.278)	(.249)	(.331)
Widowed	0.185	0.179	0.183	0.138	0.204	0.117
	(.388)	(.384)	(.387)	(.345)	(.403)	(.321)
High School Dropout	0.265	0.247	0.332	0.370	0.389	0.403
	(.441)	(.431)	(.471)	(.483)	(.488)	(.491)
High School Graduate	0.321	0.317	0.348	0.150	0.136	0.171
	(.467)	(.465)	(.476)	(.357)	(.343)	(.376)
Some College	0.217	0.213	0.199	0.147	0.134	0.152
	(.412)	(.410)	(.399)	(.354)	(.340)	(.359)
College Graduate	0.194	0.220	0.118	0.301	0.327	0.265
	(.396)	(.414)	(.323)	(.459)	(.469)	(.441)
Full Time Worker	0.490	0.464	0.536			
	(.500)	(.499)	(.499)			
Part Time Worker	0.101	0.106	0.089			
	(.301)	(.307)	(.284)			
Unemployed	0.029	0.019	0.044			

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	(.169)	(.138)	(.206)			
Not in Labor Force	0.344	0.379	0.287			
	(.475)	(.485)	(.452)			
Church Attendance Index	3.884	4.457	3.076			
	(2.694)	(2.676)	(2.445)			
Church Attendance 1				0.238	0.281	0.130
				(.426)	(.450)	(.336)
Church Attendance 2				0.121	0.122	0.098
				(.326)	(.328)	(.298)
Church Attendance 3				0.267	0.248	0.290
				(.443)	(.432)	(.454)
Unemployment Rate	9.556	10.396	10.569	6.595	6.833	6.947
	(2.772)	(2.626)	(2.698)	(2.113)	(1.952)	(1.994)
Number of Observations	36421	10279	5583	100663	35990	13742

Notes: Standard deviations in parenthesis. Columns 1-3 are for the US data and column 4-6 are for Canadian data. Columns 1 and 4 are full sample means. Columns 2 and 3, and columns 5 and 6, restrict to sample of non-smokers and smokers respectively. Smoker data is only available for a subset of the full sample in both data sets.

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Table 2: Relation Between Cigarette Taxes and Unhappiness

	Very Happy	**Pretty Happy**	**Not Happy**
Tax	**-0.011**	**.030**	**-0.018**
	(.019)	**(.021)**	**(.015)**
Married	0.177	-0.080	-0.095
	(.009)	(.011)	(.008)
Separated/Divorced	0.018	-0.018	-0.003
	(.009)	(.012)	(.009)
Widowed	0.036	0.005	-0.041
	(.012)	(.015)	(.010)
High School Dropout	0.050	0.012	0.030
	(.049)	(.042)	(.028)
High School Graduate	0.053	0.032	0.007
	(.047)	(.043)	(.028)
Some College	0.058	0.036	0.002
	(.049)	(.046)	(.029)
College Graduate	0.072	0.020	0.001
	(.047)	(.045)	(.029)
Father High School Dropout	0.004	0.006	-0.009
	(.004)	(.005)	(.004)
Mother High School Dropout	-0.008	0.008	0.002
	(.007)	(.007)	(.005)
Father High School Graduate	0.009	0.015	-0.022
	(.007)	(.007)	(.004)
Mother High School Graduate	0.004	0.007	-0.009
	(.008)	(.010)	(.006)
Father Some College	0.011	0.001	-0.010
	(.012)	(.011)	(.007)
Mother Some College	0.004	0.012	-0.014
	(.013)	(.014)	(.007)
Father College Graduate	0.024	-0.002	-0.021
	(.010)	(.010)	(.007)
Mother College Graduate	0.027	-0.008	-0.016
	(.014)	(.013)	(.008)

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Lowest Household Income Quartile	-0.045 (.011)	0.025 (.012)	0.028 (.010)
2nd Household Income Quartile	-0.024 (.010)	0.045 (.011)	-0.013 (.010)
3rd Household Income Quartile	0.009 (.012)	0.033 (.011)	-0.033 (.009)
Top Household Income Quartile	0.055 (.011)	-0.002 (.010)	-0.048 (.009)
White	-0.005 (.016)	0.031 (.013)	-0.019 (.009)
Black	-0.085 (.017)	0.041 (.014)	0.044 (.015)
One Child	-0.031 (.007)	0.016 (.009)	0.019 (.005)
Two Children	-0.019 (.007)	0.003 (.009)	0.021 (.006)
Three Children	-0.035 (.008)	0.013 (.010)	0.028 (.006)
Four Children	-0.019 (.011)	0.004 (.012)	0.021 (.009)
Five or More Children	-0.021 (.011)	0.005 (.010)	0.021 (.008)
Full Time Worker	0.029 (.012)	0.043 (.013)	-0.068 (.011)
Part Time Worker	0.022 (.012)	0.039 (.014)	-0.057 (.010)
Unemployed	-0.031 (.015)	-0.041 (.019)	0.080 (.017)
Not In Labor Force	0.036 (.011)	0.020 (.012)	-0.052 (.011)
Ever Worked	0.008 (.010)	-0.010 (.011)	0.003 (.007)
Unemployment Rate	-0.004 (.003)	0.004 (.002)	0.001 (.002)

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Church Attendance Index	0.017 (.001)	-0.009 (.001)	-0.008 (.001)
Age*Sex Dummies	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Sample Size	36421	36421	36421

Notes: Dependent variable is a dummy variable indicating which answer people chose to a happiness question. The dependent variable in columns (1) is the dummy for people reporting "very happy"; in column (2) it is the dummy for people reporting being "pretty happy" (2); and in column (3) it is the dummy for people reporting being "not happy". Standard errors, which are corrected to allow for grouped error terms at the state-level, are in parentheses.

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Table 3: Distinguishing Impacts of Tax By Propensity to Smoke

	Very Happy	Somewhat Happy	Unhappy
Tax Rate - High Propensity	**-0.005 (0.042)**	**0.050 (0.045)**	**-0.055 (0.029)**
Tax Rate - Low Propensity	**-0.005 (0.040)**	**0.003 (0.040)**	**0.011 (0.017)**
Tax Rate	-0.027 (.033)	-0.005 (.034)	0.032 (.020)
Propensity to Smoke	-0.069 (.038)	-0.014 (.040)	0.075 (.026)
Propensity to Smoke * Tax Rate	**0.047 (.078)**	**0.109 (.070)**	**-0.156 (.045)**

Notes: Dependent variable is a dummy variable indicating which answer people chose to a happiness question. The dependent variable in columns (1) is the dummy for people reporting "very happy"; in column (2) it is the dummy for people reporting being "pretty happy" (2); and in column (3) it is the dummy for people reporting being "not happy". Standard errors, which are corrected to allow for grouped error terms at the state-level, are in parentheses. Regressions include all controls listed in Table 2. First two rows split the sample into those with above and below median propensities to smoke. Bottom three rows estimate equation (2), including the tax rate, smoking propensities, and an interaction of the two.

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Table 4: Effects of Cigarette Taxes on Happiness in Canada

	Very Happy	Somewhat Happy	Unhappy	Very Happy	Somewhat Happy	Unhappy
Tax	0.021 (.024)	-0.003 (.023)	-0.013 (.007)	0.000 (.029)	0.013 (.023)	0.000 (.011)
Propensity to Smoke				0.198 (.051)	0.194 (.055)	0.096 (.040)
Propensity to Smoke * Tax				**0.072 (.062)**	**-0.058 (.052)**	**-0.048 (.020)**
Married	0.119 (.005)	-0.098 (.003)	-0.020 (.004)	0.118 (.005)	-0.098 (.004)	-0.020 (.004)
Separated/Divorced	-0.005 (.008)	-0.014 (.007)	0.027 (.002)	-0.029 (.008)	-0.025 (.009)	0.023 (.004)
Widowed	-0.004 (.008)	-0.028 (.010)	0.025 (.004)	-0.010 (.009)	-0.034 (.009)	0.023 (.004)
High School Dropout	0.155 (.017)	0.152 (.019)	0.024 (.003)	0.135 (.013)	0.144 (.018)	0.022 (.005)
High School Graduate	0.199 (.017)	0.127 (.020)	0.013 (.004)	0.191 (.014)	0.123 (.019)	0.012 (.004)
Some College	0.205 (.023)	0.121 (.015)	0.014 (.005)	0.210 (.021)	0.124 (.014)	0.015 (.005)
College Graduate	0.201 (.027)	0.126 (.018)	0.014 (.003)	0.220 (.027)	0.135 (.017)	0.017 (.003)
Lowest Household Income Quartile	-0.038 (.024)	0.041 (.015)	0.022 (.010)	-0.049 (.023)	0.036 (.015)	0.021 (.009)
2nd Household Income Quartile	-0.021 (.011)	0.042 (.008)	0.002 (.004)	-0.026 (.011)	0.039 (.008)	0.001 (.004)
3rd Household Income Quartile	-0.009 (.004)	0.021 (.005)	0.006 (.003)	-0.010 (.004)	0.020 (.005)	0.006 (.003)
Top Household Income Quartile	0.039 (.005)	-0.013 (.003)	-0.010 (.003)	0.048 (.007)	-0.009 (.003)	-0.008 (.003)
Lowest Personal Income Quartile	-0.019 (.009)	0.028 (.007)	0.023 (.006)	-0.016 (.009)	0.029 (.007)	0.023 (.006)

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2nd Personal Income Quartile	-0.008 (.008)	0.022 (.006)	0.017 (.003)	-0.018 (.007)	0.018 (.005)	0.015 (.003)
3rd Personal Income Quartile	0.017 (.006)	0.018 (.007)	-0.000 (.004)	0.007 (.006)	0.013 (.007)	-0.002 (.005)
Top Personal Income Quartile	0.030 (.012)	0.007 (.007)	-0.006 (.002)	0.030 (.012)	0.008 (.006)	-0.006 (.002)
Household Size 2	0.018 (.014)	-0.008 (.013)	-0.009 (.006)	0.014 (.013)	-0.011 (.013)	-0.010 (.006)
Household Size 3	0.005 (.008)	0.006 (.005)	-0.011 (.004)	-0.005 (.008)	0.001 (.006)	-0.013 (.003)
Household Size 4+	0.005 (.011)	0.005 (.012)	-0.013 (.005)	0.003 (.011)	0.004 (.012)	-0.013 (.005)
Unemployment Rate	-0.004 (.005)	0.005 (.003)	0.000 (.002)	-0.004 (.005)	0.005 (.003)	0.000 (.002)
Attend Church Weekly	0.100 (.005)	-0.055 (.005)	-0.020 (.002)	0.147 (.011)	-0.034 (.007)	-0.012 (.007)
Attend Church Monthly	0.059 (.006)	-0.023 (.006)	-0.014 (.003)	0.086 (.007)	-0.011 (.009)	-0.010 (.006)
Attend Church Annually Or Less	0.029 (.012)	0.000 (.010)	-0.010 (.001)	0.039 (.013)	0.004 (.010)	-0.009 (.002)
Born in Canada	0.054 (.009)	-0.005 (.004)	-0.009 (.002)	0.031 (.010)	-0.016 (.004)	-0.012 (.005)
Live in House	0.025 (.010)	0.000 (.009)	0.011 (.003)	0.036 (.011)	0.006 (.009)	0.013 (.005)
Live in Apartment	0.031 (.008)	0.020 (.007)	0.016 (.001)	0.038 (.009)	0.023 (.007)	0.017 (.001)
Own Dwelling?	0.046 (.009)	0.009 (.003)	-0.013 (.004)	0.065 (.012)	0.018 (.002)	-0.011 (.002)
Speak English at Home	-0.012 (.020)	0.010 (.018)	-0.000 (.003)	-0.015 (.019)	0.008 (.018)	-0.001 (.003)
Age*Sex Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes

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Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	36421	36421	36421	100663	100663	100663

Notes: Dependent variable is a dummy variable indicating which answer people chose to a happiness question. The dependent variable in columns (1) and (4) is the dummy for people reporting "very happy"; in columns (2) and (5) it is the dummy for people reporting being "somewhat happy"; and in columns (3) and (6) it is the dummy for people reporting being "unhappy". Standard errors, which are corrected to allow for grouped error terms at the province-level, are in parentheses. The first three columns estimate equation (1); the second three columns estimate equation (2), including propensity to smoke and its interaction with the tax rate.

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Table 5: Robustness Checks					
Panel A: US Data					
Tax	0.032	0.033	0.036	0.070	0.015
	(.020)	(.020)	(.022)	(.021)	(.022)
Propensity to Smoke	0.075	-0.006	0.011	0.073	-0.190
	(.026)	(.036)	(.059)	(.025)	(.025)
Propensity to Smoke*Tax	**-0.156**	**-0.152**	**-0.167**	**-0.152**	**-0.104**
	(.045)	**(.049)**	**(.046)**	**(.042)**	**(.077)**
Panel B: Canadian Data					
Tax	0.000	0.000	0.010	0.018	0.003
	(.011)	(.011)	(.009)	(.016)	(.015)
Propensity to Smoke	0.096	0.072	0.180	0.097	0.096
	(.040)	(.061)	(.061)	(.040)	(.051)
Propensity to Smoke*Tax	**-0.048**	**-0.048**	**-0.082**	**-0.048**	**-.057**
	(.020)	**(.021)**	**(.026)**	**(.020)**	**(.031)**
Demographic Controls	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes
Propensity to Smoke*Unemployment Rate	No	Yes	No	No	No
State Dummies*Trend	No	No	Yes	No	No
Propensity to Smoke*Trend	No	No	No	Yes	No
State Dummies*Propensity to Smoke	No	No	No	No	Yes

Notes: Standard errors are in parenthesis and are corrected to allow for correlation within states or provinces. The dependent variable in each column is a dummy for unhappiness. "Propensity to Smoke*Unemployment Rate" means that the effect of propensity to smoke was allowed to depend on the unemployment rate in the state. "State Dummies*Trend" means each state was allowed to have its own linear time trend. "Propensity to Smoke*Trend" means that the regression includes an interaction of propensity to smoke with a linear time trend. "State Dummies*Propensity to Smoke" means that propensity to smoke was allowed to have a different effect in each state. Regressions include all of the control variables shown in Tables 2 (for U.S.) and 4 (for Canada).

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Table 6: "Effect" of Other Taxes

Panel A: US Data

	Beer Tax	Gas Tax	Sales Tax	Total Revenues
Cigarette Tax	0.038	0.035	0.033	0.029
	(.024)	(.020)	(.020)	(.019)
Other Tax	-0.017	-0.001	0.003	-0.004
	(.008)	(.001)	(.004)	(.023)
Propensity to Smoke	0.055	0.060	0.060	0.125
	(.031)	(.048)	(.033)	(.038)
Propensity to Smoke*Cigarette Tax	**-0.181**	**-0.162**	**-0.159**	**-0.144**
	(.055)	**(.043)**	**(.045)**	**(.043)**
Propensity to Smoke*OtherTax	**0.034**	**0.001**	**0.003**	**-0.037**
	(.014)	**(.003)**	**(.006)**	**(.021)**

Panel B: Canadian Data

	Beer Tax	Gas Tax	Sales Tax	Total Revenues
Cigarette Tax	0.003	0.008	0.004	0.002
	(.008)	(.006)	(.010)	(.009)
Other Tax	-0.006	-0.002	-0.004	-0.006
	(.002)	(.001)	(.001)	(.004)
Propensity to Smoke	0.082	0.072	0.067	0.059
	(.048)	(.044)	(.041)	(.034)
Propensity to Smoke*Cigarette Tax	**-0.045**	**-0.047**	**-0.048**	**-0.049**
	(.020)	**(.021)**	**(.019)**	**(.020)**
Propensity to Smoke*OtherTax	**0.001**	**0.002**	**0.004**	**0.009**
	(.002)	**(.001)**	**(.001)**	**(.007)**
Demographic Controls	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes

Notes: Standard errors are in parenthesis and are corrected to allow for correlation within states or provinces. The dependent variable in each column is a dummy for unhappiness. "Other Tax" refers to a different tax in each column. It refers to a beer or alcohol tax in column (1), gas tax in column (2), sales tax in column (3) and Total state/province revenues in column (4). Regressions include all of the control variables shown in Tables 2 (U.S.) and 4 (Canada).

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Table 7: Effect by Demographic Group in US GSS

	Married	Married
Tax		
	0.075	0.104
	(.021)	(.027)
Propensity to Smoke		
	0.071	0.061
	(.038)	(.043)
Propensity to Smoke*Tax		
	-0.219	**-0.194**
	(.065)	**(.044)**
Spouse's Propensity to Smoke		0.046
		(.082)
Spouse Propensity to Smoke*Tax		**-0.126**
		(.072)
Demographic Controls	Yes	Yes
State Dummies	Yes	Yes
Year Dummies	Yes	Yes

Notes: Standard errors are in parenthesis and are corrected to allow for correlation within states. The dependent variable in each column is a dummy for unhappiness. The first column repeats our basic estimates from equation (2) for the sample of married persons. Column 2 augments equation (2) by adding spouse's propensity to smoke, and an interaction of spouse's propensity to smoke with the tax rate. Regressions include all of the control variables shown in Table 2.

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Appendix: Alternative Empirical Approaches

We argue above that the correct empirical approach to addressing our question of interest is to interact the cigarette tax with a measure of the propensity to smoke. In this appendix, we show the results of alternative empirical approaches for comparison.

A natural alternative would be to interact cigarette taxes with whether an individual is actually a smoker. As we argue above, this is an inappropriate specification for two reasons. First, smoking is endogenous, so that the composition of happiness smokers may change as taxes change for reasons unrelated to the tax change. Second, taxes may affect the happiness of former smokers (by making it easier to resist the temptation to resume smoking) or prospective smokers (by making it easier to never start smoking in the first place). As a practical matter, we also lack data on who smokes for the full sample. Nevertheless, we present the results from this alternative specification in the first column of Appendix Table 2, for both the US and Canada. We find that there is a negative effect on unhappiness for smokers when cigarette taxes rise, but the effects are not significant. One reason for this lack of significance is the reduced sample size: the smoking data are only available for a subset of survey years in both countries.

We can address the first of the concerns expressed above by instrumenting smoking with predicted smoking. Of course, doing so will not address the second concern, which is that the effects come from groups of non-smokers as well, in particular from recent former smokers. Because of this, our instrumental variables estimate will *overstate* the true impact on the happiness of all groups; that is, the effect of all groups is assumed to come through only one group (current smokers), so that this estimate will be too large.

Column (2) of Appendix Table 2 shows the results of this IV approach. The coefficient from the U.S. indicates that being a $1 tax lowers unhappiness by 30 percentage points among smokers, an estimate which is very large. In Canada, a $1 tax lowers unhappiness by 3.8 percentage points among smokers, which is a more modest but still a large effect.

For the U.S., part of this large effect comes from the restricted set of years in which smoking data are available. Column (3) shows the results of estimating the base predicted smoker interaction model used in the paper for this subsample of years. The coefficient is twice as large as the coefficient in Table 3. Extrapolating, that implies that the effect of a tax on the happiness of smokers over all years would be only half as large as in the second column of Appendix Table 2, or 15 percentage points. This is still very large, but it is once again upward biased by the fact that much of the impact may be on nonsmokers. In Canada, the estimate is somewhat lower in the restricted set of years, so that the coefficient in column (2) should perhaps be increased. Indeed, doing so, we get a quite similar ratio of the impacts in Canada to those in the U.S. (about 1 to 4) that we see in the main results.

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Appendix Table 1: Smoking Prediction Equations

	US Data (1977)	Canadian Data (1985)
Married	-0.045 (.045)	0.014 (.015)
Separated/Divorced	0.076 (.056)	.118 (.021)
Widowed	-0.026 (.062)	.079 (.019)
High School Dropout	-0.045 (.167)	-0.042 (.039)
High School Graduate	-0.123 (.167)	-0.074 (.040)
Some College	-0.187 (.169)	-0.119 (.041)
College Graduate	-0.303 (.170)	-0.180 (.040)
Father High School Dropout	-0.057 (.031)	
Mother High School Dropout	0.093 (.036)	
Father High School Graduate	-0.020 (.042)	
Mother High School Graduate	0.087 (.042)	
Father Some College	-0.025 (.063)	
Mother Some College	0.054 (.064)	
Father College Graduate	-0.021 (.065)	
Mother College Graduate	0.070 (.075)	

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Lowest Household Income Quartile	-0.029 (.043)	0.022 (.015)
2nd Household Income Quartile	0.006 (.038)	0.029 (.014)
3rd Household Income Quartile	0.025 (.035)	0.040 (.015)
Top Household Income Quartile	0.054 (.011)	-0.001 (.016)
2nd Personal Income Quartile		0.007 (.015)
3rd Personal Income Quartile		0.011 (.016)
Top Personal Income Quartile		-0.050 (.018)
White	0.250 (.122)	
Black	0.005 (.126)	
One Child	-0.059 (.043)	
Two Children	-0.015 (.042)	
Three Children	-0.054 (.046)	
Four Children	-0.143 (.054)	
Five or More Children	-0.100 (.052	
Household Size 2		0.027 (.015)
Household Size 3		0.048 (.017)
Household Size 4+		0.023 (.017)

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Full Time Worker	0.095 (.065)	
Part Time Worker	-0.010 (.078)	
Unemployed	.241 (.069)	
Not In Labor Force	0.074 (.011)	
Ever Worked	-0.002 (.048)	
Unemployment Rate	-0.12 (.009)	0.003 (.001)
Church Attendance Index	-0.044 (.005)	
Attend Church Weekly		-0.143 (.012)
Attend Church Monthly		-0.001 (.012)
Attend Church Annually Or Less		0.039 (.013)
Born in Canada		0.075 (.012)
Live in House		-.029 (.013)
Live in Apartment		0.001 (.016
Own Dwelling?		-0.075 (.014)
Speak English at Home		0.012 (.012)
Age*Sex Dummies	Yes	Yes
State Dummies	Yes	Yes

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Year Dummies	Yes	Yes
R-squared	0.16	0.11
Sample Size	36421	100663

Notes: Dependent variable is a dummy variable for being a smoker. Standard errors, which are corrected to allow for grouped error terms at the state/province-level, are in parentheses. The first column uses US data for 1977 while the second column uses Canadian data for 1985.

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Appendix Table 2: Alternative Specifications

Panel A: US Data

	Baseline	Smoker	Smoker IV	Propensity, Smoke Years
Cigarette Tax	0.032 (.020)	0.074 (.031)	0.108 (.034)	0.163 (.044)
Propensity to Smoke	0.075 (.026)			0.113 (.037)
Smoker		0.050 (.017)	0.153 (.042)	
Propensity to Smoke*Cigarette Tax	**-0.156** **(.045)**			**-0.319** **(.102)**
Smoker*Cigarette Tax		**-0.067** **(.052)**	**-0.305** **(.099)**	

Panel B: Canadian Data

	Baseline	Smoker	Smoker IV	Propensity, Smoke Years
Cigarette Tax	0.000 (.011)	-0.002 (.006)	0.005 (.007)	0.005 (.008)
Propensity to Smoke	0.096 (.041)			0.050 (0.048)
Smoker		0.029 (.019	0.049 (.046)	
Propensity to Smoke*Cigarette Tax	**-0.048** **(.020)**			**-0.038** **(.024)**
Smoker*Cigarette Tax		**-0.010** **(.014)**	**-0.038** **(.024)**	
Demographic Controls	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes

Notes: Standard errors are in parenthesis and are corrected to allow for correlation within states or provinces. The dependent variable in each column is a dummy for unhappiness. "Other Tax" refers to a different tax in each column. It refers to a beer or alcohol tax in column (1), gas tax in column (2), sales tax in column (3) and Total state/province revenues in column (4). Regressions include all of the control variables shown in Tables 2 (U.S.) and 4 (Canada).

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