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European Tobacco Taxation and Youth Smoking

Findings from two decades of Health Behaviour in School-aged Children surveys

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Abstract

Using data from the Health Behaviour in School-aged Children survey on smoking behaviors for roughly 440,000 European youths over the years 2001-2018, we show that a one euro increase in cigarette excise taxes reduces smoking prevalence (share of children who has smoked) by 2 percentage points using a two-way fixed effects model. This translates to a participation elasticity of -0.1, implying youths are less price sensitive than adults. This aligns with recent research which has questioned the previous consensus that youths are more price sensitive than adults. In line with similar research on American youths, we also show that the effect of cigarette taxes is decreasing over time and has had no significant effect when focusing on 2010-2018. For frequent smoking (once a week or more) we find no statistically significant effect for any time period, although this might be due to the fact that very few 11-15-year olds are regular smokers. We conclude that policymakers might have to consider other tools than taxation if they want to reduce youth smoking participation further.

Keywords: Youth smoking, smoking initiation, tobacco taxation, cigarette taxation, two-way fixed effects.

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1 Introduction

In the last twenty years, youth smoking has decreased steadily at the same time as tobacco taxes have increased. Previously, it was widely accepted that youths are more price sensitive than adults and the WHO still deems excise taxes the most cost-effective way to reduce smoking, especially for youths (2022). However, the consensus of youths' responsiveness to increased tobacco prices has recently been called into question. In their review of the literature on the economics of tobacco regulation, DeCicca et al. (2022) state that many recent studies find that youths are very insensitive to price. Hansen et al. (2017) show that tobacco taxation used to affect youth smoking but has not done so for the last decade in the US. However, most research regarding the effect of tobacco taxation is conducted using American data, which does not necessarily generalize to other parts of the world.

In this paper, we study how cigarette tax increases affect youth smoking participation (defined as having smoked sometime in their lifetime) and frequent smoking (defined as smoking at least once a week) in 15 European countries[1]. The empirical analysis uses data on 440,000 youths' smoking behavior from five waves of the Health Behaviour in School-aged Children survey (2001-2018), which is administered by the WHO. These surveys are unique, as they contain detailed, individual-level information on childrens' smoking habits in all of Europe over many years. We combine this with information on excise taxes on cigarettes from the European Commission.

Figure 1 shows the trends in smoking prevalence and excise yield (excise tax per pack in euros, not including general VAT) in our sample for each survey wave. The excise yield has increased from 1.6 to 4.0 real 2010 euros. At the same time, the share of youths who have ever smoked has decreased from 39 % to 12 %. While it seems plausible that these trends are connected, correlation does not necessarily imply a causal effect of taxes on smoking. For example, increasing anti-smoking sentiment can reduce smoking rates while also increasing the political demand for tobacco tax increases, implying that the causal effect is driven not through tax but rather through other channels.

[1] The 15 countries that had joined the EU up until 2001, consisting of most of western Europe. All countries are listed in section 3

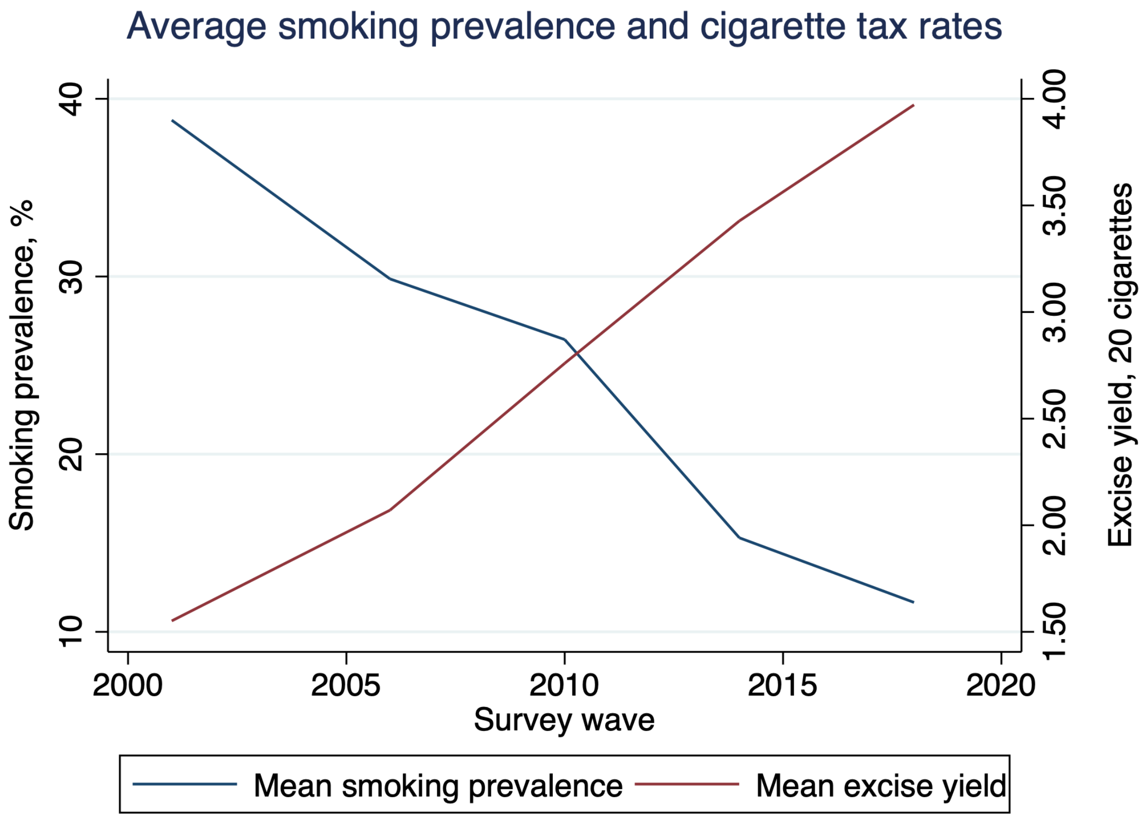

FIGURE 1: Average smoking prevalence and cigarette tax rates

To investigate if tobacco taxes have a causal effect on youth smoking, we use a two-way fixed effects model similar to Hansen et al. (2017). By controlling for time and country-fixed effects, we can obtain a causal effect under the assumptions of parallel trends and the absence of contemporaneous shocks that correlate with tobacco tax rates. These identifying assumptions are difficult to prove in standard ways in this field of research due to the frequent changes in the tax rates in many countries (DeCicca 2022). However, the homogeneity of 15 EU countries we study, the coordination of tobacco regulation at the EU level, the long period of time we study and our tests for heterogenous effects make these assumptions plausible in our view.

Our empirical analysis shows that cigarette taxes have had a small but significant negative effect on smoking participation. A one euro increase in taxes has decreased smoking participation by 2 percentage points during 2001-2018 but has had no statistically significant effect on frequent smoking. We also show that the pattern uncovered by Hansen et al. (2017) holds for the EU15 countries as well; cigarette taxes have significantly reduced youth smoking, but the effect vanishes when looking only at the last decade. We also split the sample based on age and gender and find that the effect differs between boys and girls as well as younger (11-13) and older (15) children.

The rest of this thesis is structured as follows: Section 2 summarizes previous research on the link between youth smoking and tobacco taxation. Section 3 presents the data used, while Section 4 outlines the method used for the analysis. Section 5 presents the main findings and the results from several robustness checks. The thesis is concluded in section 6.

2 Previous Research

The question of how cigarette excise tax affect smoking was recently and extensively reviewed by DeCicca et al. (2022). Instead of redoing their work we will use it as our main point of reference and cite other studies in order to give additional information or highlight things that are particularly relevant for our study. We will begin by summarizing why youth smoking is of high interest in this field of literature. Then we will review the findings on how adults react to cigarette prices before moving on to youths, since the literature on youth smoking centers around if youths are more or less price sensitive than adults.

The question of how to combat smoking is high on the public health agenda. The WHO describes tobacco as an epidemic, deeming it "one of the biggest public health threats the world has ever faced, killing more than 8 million people a year" (2022). Youth smoking is of high interest to health policymakers since most smokers debut as teenagers (Marcon et al., 2018), coupled with the fact that teenage and adult smoking is highly correlated (Gillieskie and Strumpf, 2005). The underlying assumption is that reduced youth smoking will persist into adulthood, which has been difficult to show empirically despite intuitive appeal (DeCicca et al., 2022). However, recent results by Friedson (2023) indicate this might be the case, at least for some cohorts.

How smoking behavior is affected by tax changes can be split into two parts: how much of tax changes are passed onto consumers through increased prices and how the consumer reacts to that price change. When DeCicca et al. review the previous literature, the general conclusion is that most (in many cases all) of the economic burden of tax increases is passed onto consumers through increased prices.

DeCicca et al. choose to focus mainly on smoking participation (the extensive margin) for two reasons: public health policies typically focus on initiation and cessation (picking up and quitting smoking), and the fact that the interpretation of intensive margins (how much a smoker smokes) on health outcomes is difficult since smokers change other behaviors besides the number of cigarettes they buy in response to a price change, such as the strength of the cigarettes or how long they smoke each individual cigarette.

During the last 20 years, the consensus regarding the price elasticity of smoking for adults has shifted. In their 2000 review, Chaloupka and Warner report a consensus price elasticity estimate of -0.4 to -0.7 for adults with intensive and extensive margins contributing equally (meaning that a 1 % price increase reduces demand by 0.4 to 0.7 %). Conversely, later studies with more credible methodology and larger tax changes (yielding more identifying variation) find that demand is more inelastic, in the range of -0.2 to -0.35 (DeCicca et al., 2022). Focusing on the studies DeCicca et al. find the most credible generally shows an elasticity of 0 to -0.1, which they argue indicates an upward bias (away from zero) even in the research done in the 21st century.

The addictive nature of nicotine makes the decision to start or quit buying cigarettes very different from other products, like milk or cereal. DeCicca et al. (2008) demonstrate this fact empirically by developing a model that distinguishes between smoking initiation, cessation, and participation. Using repeated cross-sectional data on American youths, they show that smoking participation (whether you smoke or not) depends on whether you have smoked before. The elasticities reported earlier are shown to be a weighted average of participation and cessation elasticities, where they find no evidence of higher taxes affecting smoking initiation but a positive effect on cessation.

The question of youth smoking has been of high interest for many years. There are different possible reasons that can explain why youths should be more or less sensitive to tobacco prices compared to adults. On the one hand, they often have little money to spend and have not lived long enough to have time to form an addiction to nicotine, making them more price sensitive than adults. On the other hand, smoking can be an important way to gain social acceptance from peers. Youths also are more likely to smoke name-brand cigarettes, indicating that they are less price-sensitive (DeCicca et al., 2022).

In the review by Chaloupka and Warner (2000), the authors find a near consensus stating that youths are more price sensitive than adults. These studies were conducted mostly using cross-sectional data, which can suffer from omitted variable bias if tobacco tax is correlated with other factors that affect smoking and overstate the causal effect (DeCicca 2022).

In contrast with these earlier estimates of elasticity around -1 for youths, DeCicca et al.

(2022) paint a different picture when reviewing the work done in the 21st century. Recent studies find elasticities much closer to zero, which the authors attribute to using data from different places and timeperiods as well as methods with stronger claims of capturing a causal effect. Also the frequency and severity of tax increases between the two periods. Many studies cannot even find participation elasticities that are significantly different from zero.

We want to highlight two studies reviewed by DeCicca et al. as they showcase the development in the research on youth smoking and taxes. Carpenter and Cook (2008) employ a two-way fixed effects approach to examine the effect of tobacco taxation on US youth smoking using the Youth Risk Behavior Surveys (YRBS) over the time period 1991-2005. The authors find price elasticities in the range of -0.23 to -0.56, smaller than in previous cross-sectional studies but still significant. Hansen et al. (2017) build upon Carpenter and Cook by extending the analysis with additional survey waves (2007 - 2013). This reduces the estimated participation elasticities to -0.2 for their main specification, with elasticities becoming insignificant when looking only at the later years. The authors speculate that the reduced importance of tobacco taxation is explained by a composition effect; when smoking participation dropped from 35 % to 25 %, there remains a "'hard-core' group of young people whose smoking initiation decisions are insensitive to cost" (pp. 73).

Most studies reviewed by DeCicca et al. (2022) and Chaloupka and Warner (2000) are based on American data, both for adults and youths. To the best of our knowledge, there are no studies of European youths' responsiveness to tobacco taxation using repeated cross-sectional data, similar to the one conducted by Carpenter and Cook (2008). The closest example we can find is Hublet et. al (2009), who use the 2006 HBSC wave (thus, cross-sectional data) to show a borderline significant effect of prices on youth smoking.

For adults, Yeh et al. (2017) conduct a study in the years 2005-2014 on all EU member states using a threshold regression model and panel data. They estimate cigarette price elasticities for three different income regimes and find that they range from -1.227 (the regime with the lowest income) and -0.057 (the regime with the highest income), that is, low-income countries are more sensitive to cigarette prices than high-income countries. Only the third estimate, that of the effect in high-income countries, is in the range of credible estimates as described by DeCicca (2022).

There are some studies at the national level on youth smoking and taxation. One example is Stoklosa et al. (2022) which employs a survival analysis technique to study cigarette-smoking initiation for youths and find that prices have a negative effect on the probability of picking up smoking for Polish youths.

The lack of credible and modern research on how youths respond to tobacco taxation in Europe leaves a gap in the literature, one of high relevance to policymakers when considering the trend of decreasing effects over time found in an American setting and the high faith put into tobacco taxes by public heath policy makers. In order to fill this research gap, we will use a two-way fixed effects approach similar to Hansen et al. (2017) to study whether the smoking behavior of the youths that responded to the Health Behaviour in School-aged Children survey during 2001-2018 were affected by increased tax rates on cigarettes. This analysis will be carried out in the following parts of this thesis.

3 Data

This thesis will combine data on youth smoking from the WHO with data on tobacco taxation from the European Commission to study whether tax increases have casually contributed to the declining youth smoking rates in the EU15 depicted in Figure 1. In addition, data on country-level economic variables used as controls are retrieved from Eurostat.

The Health Behaviour in School-aged Children (HBSC) survey is organized by the WHO every four years and asks questions about various topics to 11-, 13- and 15-year olds. The study is conducted in Europe and North America by researchers from academia and public health institutions. Each country employs probability sampling to obtain a sample representative of the target population (i.e. 11-, 13- and 15-year olds in each respective country). Sampling processes differ between countries, which is why no response rates may be summarized (Currie et al. 2014), (Currie et al. 2010).

We use data from the 2001, 2006, 2010, 2014, and 2018 waves, which are the ones available to us. The sample size for each country and wave varies between 1,000 and 15,000, although most countries and waves consist of approximately 5,000 children. In total, we are able to use information on the smoking habits of roughly 440,000 children.

Information on tobacco taxation is sourced from the European Commissions' archive of excise duty tables on CIRCABC (2001-2015) and the database Taxes in Europe (2016-2020). Since we have found no standard way of handling the fact that our group of countries have several different exchange rates, we have recalculated the tax rates using a fixed exchange rate to real 2010 euros in order to isolate variation from tax changes. By using a fixed exchange rate, variation in exchange rates over time does not confound our results. We want to avoid the case, for example, of a country's strong currency being confused for a tax increase in euros. Statistics on inflation and exchange rates are gathered from Eurostat.

The tax rate is expressed as the excise yield per 20 cigarettes in euros, calculated using either the most popular price category (MPCC) (2001-2010) or the weighted average price (WAP) (2011-2020). The shift between MPPC and WAP is unfortunate because the latter is, on average, 26 cents lower than the prior (calculated for 2011-2015, when both measures are available), potentially creating some bias in our estimates. Some of this will be captured in the time-specific effect, however, there is variation in the differences between WAP and MPCC prices, creating undesired noise in estimates. The distribution of the average differences for each country, where both measures are available, can be viewed in Appendix A1.

Since our tax data is sourced from EU databases and not all EU countries were members in 2001, when our first HBSC data is from, we would have an unbalanced data set if we included all current EU member countries. There was a large wave of entry in 2004 when ten east- and southern European countries became members. Then, there were two entries in 2007 and one in 2013.

Many of these new countries were given exemptions in regard to the EU legislation surrounding tobacco sales and use. For example, seven countries that entered in 2004 and after did not have to fulfill the requirements for minimum taxation in the European Council directive 2011/64/EU according to article 10:2 until 2017. That is, the legally required taxation in these countries has not been harmonized with the earlier member states for the majority of the time period studied. At the same time, Blecher et al. (2013) report that excise taxes and prices were raised aggressively in the new member states, more so compared to previous members.

Given the risk of heterogeneity in the effect of tobacco taxation raises on youth smoking depending on the actual level of the excise tax, one can suspect there will be different effects of tax changes for pre-2004 member states and post-2004 member states. Because of an unbalanced data and risk of heterogeneous effects, we restrict our study to the countries that were members in 2001, the EU15[2].

In our main specification, 440,000 observations are included, evenly distributed across the five survey waves and fifteen countries. In Table 1, the available demographic statistics for the sample are described.

In our analysis, there are two outcome variables: smoking participation, i.e. if the person

[2]Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden, United Kingdom

TABLE 1: Descriptive statistics for demographic, outcome and control variables.

Variable	Mean	Std. dev.	N
Demographic variables			
Girl	0.507	.500	450,555
Age			
11	0.338	.471	446,379
13	0.345	.476	446,379
15	0.322	.467	446,379
Outcome variables			
Smoking participation	0.234	.423	442,795
Frequent smoking	0.068	.251	445,883
Country control variables			
Unemployment	0.081	.044	450,555
GDP per capita	33,445	11,851	450,555

has ever smoked (extensive margin), and frequent smoking, i.e. how often the individual smokes (intensive margin). These are summarized in Table 1. For the extensive margin, the question "Have you ever smoked tobacco? (At least one cigarette, cigar or pipe)" from HBSC waves 2001, 2006, and 2010 and the question "On how many days (if any) have you smoked cigarettes in the past 30 days?" from waves 2014 and 2018 was used. If this is 0, it has also been coded as 0, while if it is larger than 0, it has been coded as 1.

For the intensive margin, the question "How often do you smoke tobacco at present?" with 4 different answer options was used for the first four waves. However, this question was not asked in 2018, instead the question "On how many days (if any) have you smoked cigarettes in the past 30 days?" with 7 different answer options included. These questions measure the same thing but on different scales. The data from 2018 has been recoded into the 2001-2014 format, as described in Appendix A3.

Once all individuals had data in the 2001-2014 format, the variable was recoded to a dummy such that *At least once a week* and *Every day* were assigned the value 1, while *Less than* *once a week* and *I do not smoke* were assigned 0. This is such that meaningful regression estimates may be computed.

When dealing with survey data it is important to consider there will always be some survey error. This may come from the sampling procedure or from the questionnaire itself. To start, there may be measurement errors from incorrect answers. Two examples are that individuals do not remember past events or habits or that they give more socially acceptable answers (Lohr 2022, p. 10-12). In this setting, children may not remember exactly how often they smoke or do not wish to disclose this information due to stigma. Lohr (2022, p. 311-312) goes on to discuss the issue of non-response. This only becomes an issue if the non-responders differ from the responders in some way that is relevant to the research question at hand. In our setting, non-response may occur at the class level or the student level. For example, schools with many troublesome students who smoke also may not answer the survey as often due to a lack of time and resources.

Still, since we look at changes over time and have no strong reason to believe these errors would change over time, the errors should be captured in controls and fixed effects. The possible exception is if students' probability to lie in their answers correlates with anti-smoking sentiment such that it changes over time, which is a problem that will be discussed in section 4.2.

4 Method

4.1 Model

If the world was a laboratory where we could design perfect experiments, we would want to randomly assign tobacco taxes to countries. If we were to do so, the expected potential outcomes for the untreated units and treated units would be equal, implying the only difference in their outcomes could be attributed to the tax increases. However, we are not able to randomly assign cigarette excise tax rates to EU member countries. Instead, countries choose their tax rates themselves, introducing some endogeneity. If we were to run a simple regression of youth smoking on tax increases, we could for example attribute a decline in smoking to tax increases, while in reality it was a price shock or some new information on the dangers of smoking that was published.

To work around these endogeneity problems, this thesis will employ two-way fixed effects (TWFE) logistic regression to model an extensive and another intensive margin smoking variable after the level of cigarette excise tax, similar to Hansen et al. (2017). The fixed effects framework is a way to deal with effects that vary over time but not unit, or vice versa. The time-fixed effect accounts for all variation that is common across all units and a specific time period. The unit fixed effect accounts for all variation that is time-independent but unit dependent. This removes effects that should not be attributed to the variable of interest without explicitly controlling for everything that can be included in the time and unit fixed effects (Allison 2009, p.7).

In this setting, the time-fixed effect may account for the effects of general price shocks that strike all EU15 countries equally or some new information on smoking available to all individuals. The unit fixed effect is not at the individual level since the data is of type repeated cross-section, rather than panel, but at the country level. This addresses, for example, cultural aspects or general price levels which are specific to a country but do not change over the period studied.

In line with Hansen et. al (2017) we will use a logistic regression ensure the predicted values are between 0 and 1. Thus, they can be interpreted as the probability of an individual ever having smoked, or smoking at least once a week. Logistic regression, or logit models, have the same setup as regular linear regression, but the outcome variable is transformed into log-odds. All parameter estimates should be interpreted as linear changes in the log-odds[3] of smoking, or if exponentiating the estimates, linear changes in the odds of smoking (Allison 2009, p.28-31).

Assembling these pieces, we end up with the following regression model:

$$\log \frac{p(S_{ist})}{1 - p(S_{ist})} = \alpha_0 + \beta \text{Tax}_{st} + \gamma \mathbf{X}_{ist} + \eta_s + \delta_t + \epsilon_{ist} \tag{1}$$

where S_{ist} is a binary variable indicating whether a person i in state s at time t smokes or not (either on the extensive or intensive margin). Country-fixed effects are represented by η_s while wave-fixed effects are represented by δ_t. $\boldsymbol{X}_{ist}$ is a vector of time-variant control variables, including gender, age, country unemployment rate, and GDP/capita. ϵ_{ist} is the error term and α_0 is some intercept. The variable of interest is Tax_{st}, expressed as the excise tax yield per pack in real 2010 euros. The time-fixed effect variable and the age variable are, in fact, a set of dummies, indicating which HBSC survey wave the data is from and which age category the individual belongs to, respectively.

All standard errors reported have been clustered at the country level. Our inference is based on asymptotic distributions, and when clustering is used MacKinnon et al. (2023) explain that either the number of clusters or the number of observations within clusters has to tend to infinity to make reliable inference. If this fails, they recommend validating the standard errors with bootstrap methods. Since we only have 18 countries (EU15, where the UK and Belgium are reported as three and two separate domains, respectively) and thus 18 clusters, bootstrapped standard errors will be reported as a robustness check in Section 5.4.4.

[3] The probability of the event (smoking) divided by the probability of the complement (not smoking), logarithmized.

4.2 Identification

The model rests on two assumptions that are needed to make sure that the estimated effects are causal: parallel trends and the absence of unobserved shocks correlated with tobacco tax changes. An event study is the most common method to show that the parallel trends assumption holds in a standard DiD framework, which is very similar to TWFE. However, the frequent tobacco tax changes many countries have seen make this procedure impractical (DeCicca 2022).

The most common objection to the parallel trends assumption in this field of literature is the presence of country-specific time trends (CSTT). If tax increases are endogenous (for example, if countries that already have a declining smoking rate are more likely to increase taxes), then estimates will be positively biased in a TWFE model. However, if the effect of tax changes is dynamic (the effect varies over time), including CSTT will understate the effect. DeCicca (2022) recommends that researchers report estimates with and without CSTT, given the uncertainty of the appropriate method. These results will be found in Section 5.4.1.

Unobserved contemporary shocks are the second possible source of endogeneity. One example is if countries pair the introduction of other tobacco regulations with tax increases. If this is the case, the estimated effect will contain an upward bias from the paired regulation rather than just the causal effect of the tax increase. DeCicca (2022) argues that if states have changed the tax rate many times, the chance of bias stemming from contemporaneous shocks decreases. Since the EU coordinates tobacco regulation among member states and taxes have been raised continually throughout the whole period we study, we believe this further decreases the risk of bias from this source. There is no currently systematic research regarding whether this bias is a problem when studying tobacco taxation.

Overall, the two-way fixed effects approach has both strengths and weaknesses when it comes to identifying causal effects. Limitations in data make this approach one of the most common in this field of research. We believe that the combination of the EU coordination of tobacco policies and the long time period between each HBSC survey wave lowers the risk of unobserved contemporary shocks, at least in the case of policy pairing. The lack of clarity regarding country-specific time trends is unfortunate, which we address by including CSTT as a robustness check.

4.3 Heterogeneous and dynamic treatment effects

There is also the issue of heterogeneous and dynamic effects. Heterogeneity in treatment effects arises when effects differ across time and countries, e.g. if a tax increase in Greece in 2004 has a larger impact than the same tax increase in Denmark in 2017. The TWFE estimator β_{FE} is a weighted average of the estimates from all possible combinations of two countries and two consecutive time periods, and it is possible for some of these weights to be negative. De Chaisemartin and D'Haultfœuille (2020) discuss how these negative weights become an issue in the presence of treatment effect heterogeneity and show that β_{FE} can take the opposite sign of the true average treatment effect (ATE). They also propose a measure of treatment heterogeneity that will be presented in section 5.4.4.

The dynamic treatment effects are a similar but slightly different problem. Dynamic treatment effects can be seen as a form of effect heterogeneity but occur when it takes time for the treatment to take full effect. Goodman-Bacon (2021) states that this implies lagged treatment effects will be used as controls in parts of the regression, yielding biased estimates. Several developments in this area have been produced in the last few years, and with that estimators have been proposed to resolve this issue, see e.g. Baker et al. (2022). However, we have found no method that allows for continuous treatment coupled with no never treated individuals, which means we have to proceed with standard logit regressions in line with for example Hansen et al. (2017).

We argue that while cigarettes are addictive and thus policy changes take time to take full effect, children should not have had the time to form smoking addictions, making them adapt their behaviors more swiftly, as discussed in Section 2. We also have four years between each survey wave, providing a generous amount of time for policy changes to take full effect. All in all, we judge the risk of bias stemming from disregarding dynamic treatment effects to be small.

4.4 Control variables

We look at Hansen et. al (2017) for inspiration when choosing what control variables to include since they do a similar regression with similar data. They use standard controls such as gender, age, and grade, as well as the unemployment rate and some tobacco regulations. Including an indicator of the economy is common since it is well-known that health behaviors are affected by the business cycle. American adults are said to smoke less during economic downturns (see, for example, Ruhm, 2000). However, a recent German study suggests the relationship might be more diverse in a European context, with economic downturns associated with higher smoking participation but lower intensity (Kaiser et al., 2018). We will control for both the unemployment rate and GDP per capita to differences in wealth levels between countries that may affect smoking as well as business cycle effects.

It would be ideal to control for other tobacco regulations that affect youth smoking. However, we have found no dataset or other compilation regarding tobacco control regulations for the EU countries similar to the data on tobacco taxation. Since all countries in our studies had been members of the EU for several years before our first observation and tobacco regulations have, in part, been coordinated by the EU we believe that this lessens the risk of omitted variable bias from not controlling for other types of regulation. Hansen et al. (2017) also found that the inclusion of other control measures had no major effect on their estimates, indicating that this is no major problem.

5 Results

In this section, we present the results from our main specification and what happens when we split the sample based on gender and into two different time periods. In summary, we find small but significant effects on smoking participation but no significant results for frequent smoking. We conclude by reporting results from several robustness checks showing that our results are relatively stable. To put our estimates into context we will compare them with Hansen et al (2017), since their methodology is similar to ours.

5.1 Main results

Table 2 presents the tax variable's coefficient and the outcome variable's mean value for smoking participation and frequent smoking. For full results, see Appendix A4. To transform the coefficients from the logistic regression into something more intuitive, we calculate what these would imply in terms of reduced smoking participation and frequent smoking using the means presented in Table 2[4].

A one euro tax increase would decrease smoking participation from 23.3 % to 21.3 %, a decrease of 2 percentage points. On the intensive margin, a one euro tax increase would decrease frequent smoking from 6.7 % to 6.3 %, a (statistically insignificant) decrease of 0.4 percentage points.

This corresponds to an elasticity of -0.10 for smoking participation and -0.02 for frequent smoking[5]. Our results are somewhat lower than Hansen et al., who find smoking participation elasticities in the size of -0.2 for 1991-2013 high schoolers.

[4]See Appendix A2 for the formula used.

[5]Assuming a passthrough rate of 1 (see section 2) and an average price per pack of 4,76 euros (which was the case in the EU15 in 2010 (Blecher et al., 2013)), the elasticity is calculated by dividing the percent change in smoking participation by the percent change in price.

TABLE 2: Regression results from main specification

	Smoking participation	**Frequent smoking**
Excise Tax	-0.113***	-0.0669*
	(0.0325)	(0.0407)
Mean	0.233	0.0672
Country and survey wave fixed effects	Yes	Yes
Observations	438,86	441,901

Both regression include controls for age, gender, country GPD per capita and unemployment.

Robust standard errors clustered at the country level in parentheses.

**** p<0.01, ** p<0.05, * p<0.1*

These lower estimates are expected and could be explained by several factors: Cigarette taxes are higher in Europe than in the US, and the marginal effect of increasing from 3 to 4 euros may be smaller than the jump from 1 to 2 euros[6]. Since we study a later time period, it can also be for the reason that will be discussed in section 5.2; if the effects of tobacco taxation are diminishing over time, it is natural that our estimates are lower. We also study younger children than Hansen et al. (2017) and in section 5.3 we show that the youngest children in our sample are the most price sensitive, a pattern that perhaps also holds when comparing 11-15-year olds to high school students.

Lillard et al. (2013) showed that a lack of significant effects could be due to focusing on a time period when too few people have begun to smoke. At 15, only 19 % of European smokers have debuted and even fewer smoke regularly (Marcon et al., 2018). Thus, the lack of significance for frequent smoking could be due to this fact.

[6]This can also affect through how elasticities are calculated: Since tobacco taxation is significantly higher in Europe compared to the US to begin with, the percent change when increasing with one euro is smaller compared to increasing with one dollar in an American context.

5.2 Splitting the sample time periods

As discussed in section 2, DeCicca et al. (2022) find in their review that many studies find a declining effect of tobacco taxation over time. When we split our sample time period into two separate ones, we see the same pattern. Figure 2 shows the estimated effects, that are significantly different for smoking participation but not for frequent smoking[7]. Similar to Hansen et al. (2017), we find stronger negative effects for the first half of the sample and positive but insignificant effects for the later time period.

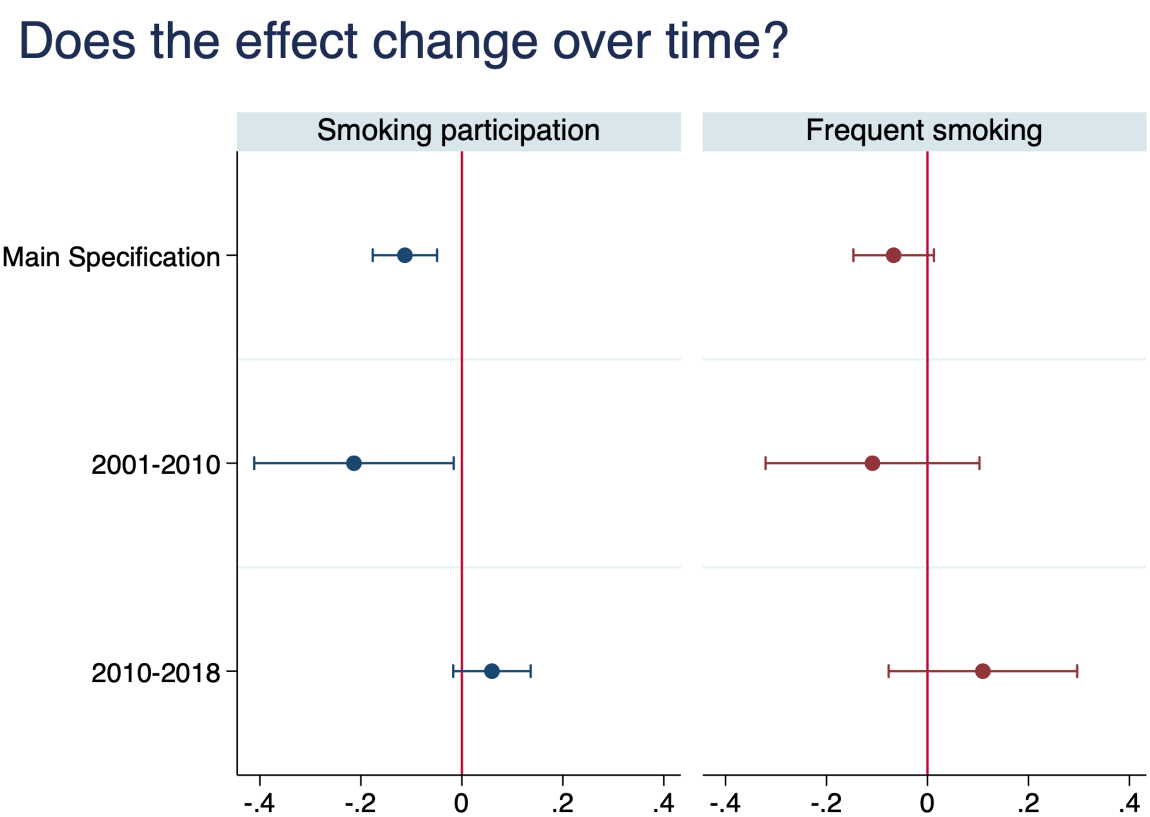

FIGURE 2: Regression results when splitting the time periods. Confidence intervals correspond to a level of 95 % certainty.

The change in measurement between MPPC and WAP takes first takes effect for the 2014 wave, making it look like countries lowered their tax rates by on average 26 cents. We are

[7]A chi2-test shows that the probability that the effect is constant over time is 0.8 % for smoking participation and 10.5 % for frequent smoking.

unable to test how this affects our results, but it could be an explanation for the positive effects for the later periods.

5.3 Heterogeneity analysis

We also present estimates when stratifying the sample based on gender and age in Figure 3. For smoking participation, tax increases have a significantly larger effect on girls compared to boys[8]. This could indicate that social acceptance is a driving factor behind youths' low price elasticity (we see no reason they would have different disposable incomes, which would be the other plausible explanation out of the ones listed in section 2).

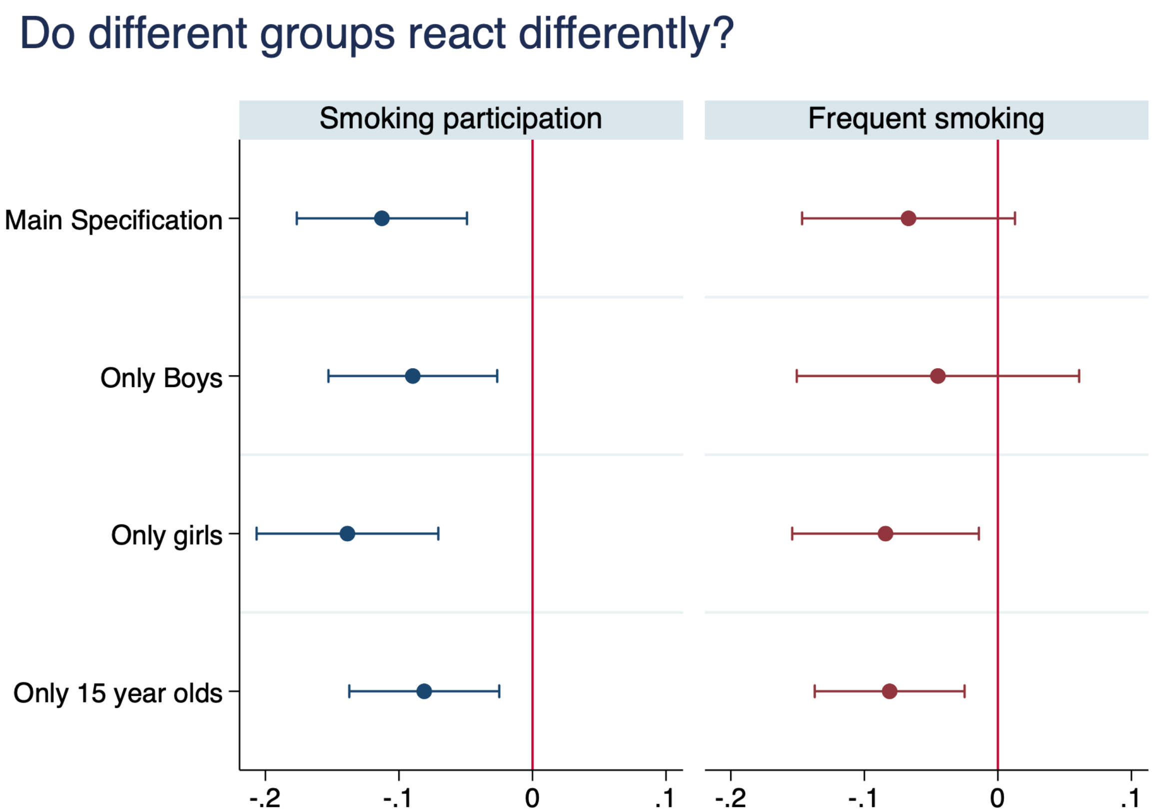

FIGURE 3: Regression results for boys, girls, and 15-year-olds separately. Confidence intervals correspond to a level of 95 % certainty.

[8]A chi2-test shows that the probability that the effect is the same for both genders is 0.5 % for smoking participation and 31.2 % for frequent smoking.

Since smoking participation is very low for 11 & 13-year-olds, we estimated the effects for 15-year-olds separately and find that the effects of tax increases are borderline significantly smaller for 15-year-olds[9]. Our data offer no possible explanation for this fact, but we speculate that this may be explained by the fact that 15 year olds have more disposable income or that smoking generates more social acceptance for older kids.

5.4 Robustness checks

In order to test the robustness of the estimates reported, we present the results of additional model specifications. The coefficient and confidence interval for the tax variable is presented in Figure 4 together with the results for the baseline specification from section 5 for comparison. For a table of all point estimates, see Appendix A5 & A6. We also investigate the possibility of heterogeneous treatment effects in section 5.4.4. Overall, the results of these additional checks align with similar studies and show that our model is sufficiently stable, although the reported standard errors are probably not as precise as they seem.

5.4.1 Country-specific time trends

In their review, DeCicca et al. (2022) recommend that studies of tax elasticity report estimate both with and without country-specific time trends since it has not been established which way is the correct one. When we include CSTT, our estimates become insignificant for both smoking participation and frequent smoking. This aligns with similar research in this field, which we discussed in more detail in Section 2. However, we judge these estimates as difficult to trust. Since every single country has increased their tax between observation years (except for Denmark, which lowered it from 2.02 to 1.96 euros per 20 cigarettes between 2001 and 2006), it is difficult to tell which effect stems from tax increases and which stems from anti-smoking sentiment in a regression setting. Assuming some degree of collinearity between these two sources, the point estimates may be volatile.

[9] A chi2-test shows that the probability that the effect is the same for 11-13 and 15-year-olds is 5.56 % for smoking participation and 42.2 % for frequent smoking.

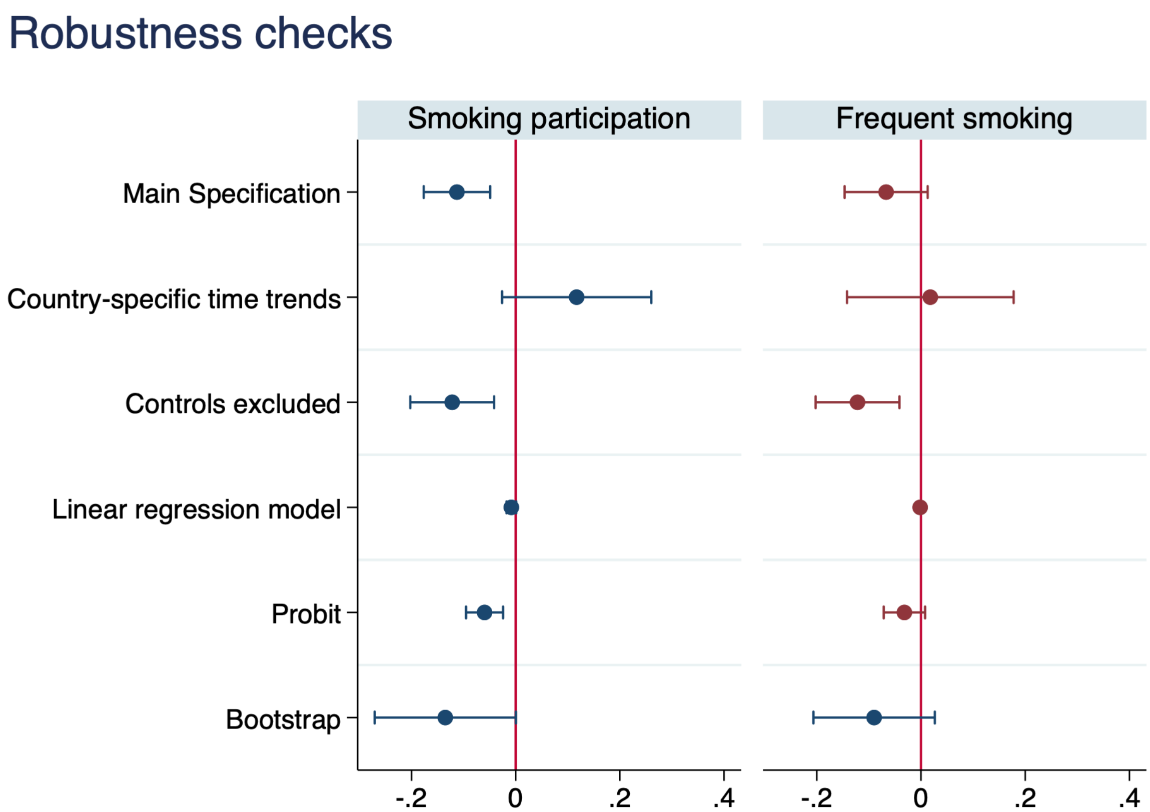

FIGURE 4: Regression results for various robustness checks. Confidence intervals correspond to a level of 95 % certainty.

5.4.2 Excluding control variables

To show that our estimation is robust regarding the choice of control variables, we report results when excluding the time-variant control variables (country unemployment and GDP per capita). As seen in Figure 4, this only has a minor effect on the estimated coefficient and slightly widens the confidence interval for smoking participation. This is in line with Hansen et. al (2017), who shows that the choice of control variables (in that case, other tobacco regulations) does not significantly affect the estimated coefficients. For frequent smoking, the estimated effect differs quite a bit, but that could be explained by the small number of frequent smokers in the sample making the model rely more on the control variables or that the estimated effect is so small in the main specification.

5.4.3 Alternative regression methods

To show that our estimates are insensitive to the choice of regression method, we present the coefficients for when the main specification is estimated with probit and a linear regression model.

For smoking participation, substituting logit for probit gives somewhat higher estimates but smaller confidence intervals. The estimate from the linear regression model cannot be compared directly with the other two. However, comparing the LRM estimate with the change in participation rates shows they are kind off similar (-0.8 % compared to 2.0 %). Both are significant, although LRM is only so at the 10 % confidence level.

For frequent smoking, the model is insignificant for all three different estimation methods. This could be due to the small number of frequent smokers in the sample, either providing too little variation for the model to detect an effect or the fact that this small group is, in fact, completely insensitive to price.

5.4.4 Clustering and bootstrapped standard errors

Since 18 is a small number of clusters, we also estimate standard errors using bootstrap methods. MacKinnon et al. (2023) and Cameron and Miller (2015) recommend using wild bootstrap methods for this case, however, this is not available when applying a logit model. Instead, Cameron and Miller (2015) recommend using the score wild bootstrap, first presented by Kline and Santos (2012). These standard errors are reported in Figure 4. Now the effect of raising the excise tax on cigarettes is insignificant both for the smoking participation and frequent smoking variables at the 5% level, although only very slightly so. This indicates the standard errors, confidence intervals and p-value presented using regular clustered standard errors are too precise.

5.4.5 Country-level effect heterogeneity

In order to show that no single countries skew the results, we present results from running the main specification while excluding one country at a time. Although some countries seem to have a somewhat larger impact on the results than others, the outliers seem to affect the results in both directions. This, combined with the fact that all coefficients are negative and significant for smoking participation, adds another argument for our results being robust.

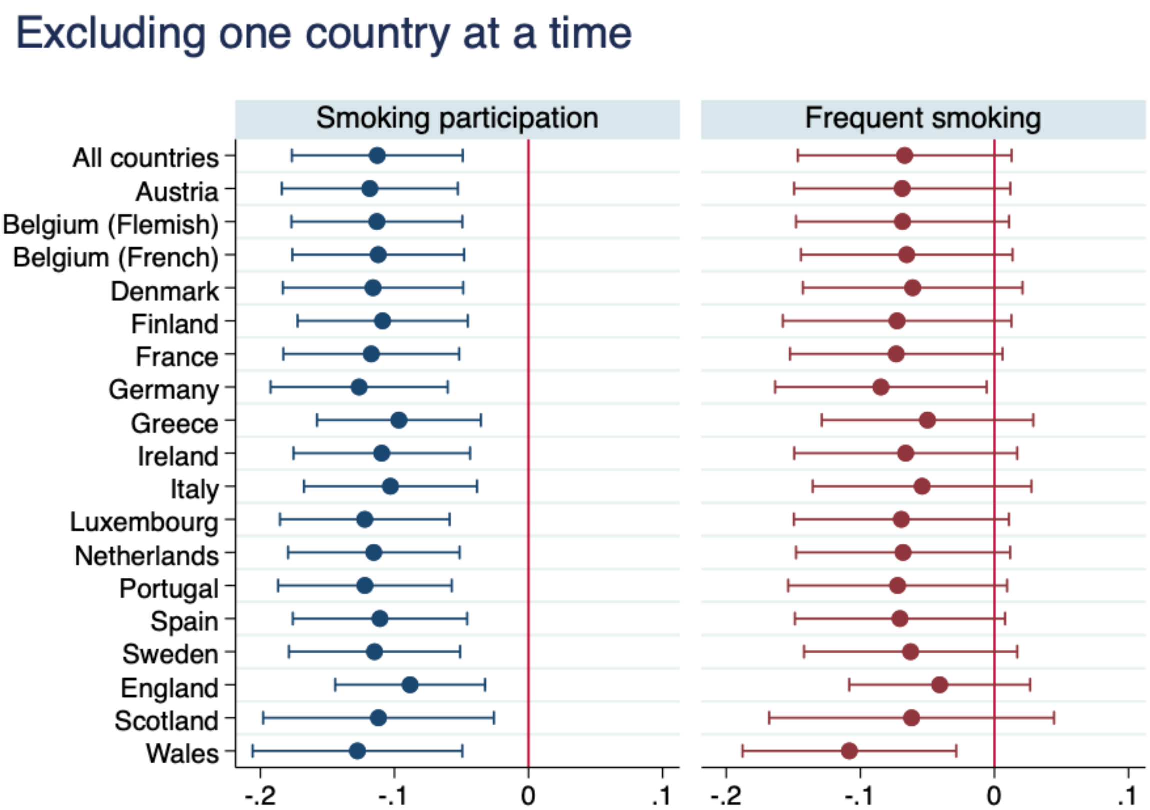

FIGURE 5: Regression coefficients for the tax variable when excluding one country at a time

When we expand our analysis by running the heterogeneity measurements proposed by De Chaisemartin and D'Haultfœuille (2020), we find results in line with those in Figure 5. These can be seen in Table 3. The Stata command does not allow individual-level controls such as gender and age, but all other aspects of our main specifications are accounted for. First reported is the number of weights attached to individual two-country-two-period ATE estimates which are negative and positive, respectively. A large number of negative weights indicates a risk of treatment effect heterogeneity, which we seem to have.

However, the standard deviation reported gives a more promising outlook. It is calculated

TABLE 3: Heterogeneity measurements as proposed by De Chaisemartin and D'Haultfœuille (2020)

Outcome variable	**Smoking participation**	**Frequent smoking**
Number of negative weights (share)	42 (0.48)	42 (0.52)
Number of positive weights (share)	46 (0.52)	46 (0.48)
Minimum std. dev.		
(s.t. average ATE opposite sign of β)	0.0015	0.0005
threshold value (uniform)	0.0026	0.0009
threshold value (normal)	0.0029	0.001
β (OLS, no individual controls)	-0.0134	-0.0047

such that it is the minimum level of variation of the 2-by-2 ATEs that would be required for the true ATE to be of the opposite sign of our estimated β. A large level of this standard deviation means much heterogeneity is required for biased results. Two different threshold values, one based on the uniform and one based on the normal distribution, are reported to judge what is "large" and not. These are computed using the standard deviation reported and should be compared to the estimated β. If the absolute value of β is larger than the threshold value, we should have a plausibly low level of treatment effect heterogeneity (de Chaisemartin et al., 2022). As seen in Table 3, we have estimates larger than the thresholds (in absolute value) with reassuring distance. These measurements support the notion that we need not be worried about treatment effect heterogeneity error, as displayed in Figure 5.

6 Conclusions

Using 2001-2018 HBSC data, we show that excise tax on manufactured cigarettes has had a small but significant effect on youth smoking for the EU15 countries, with a 1 euro increase decreasing smoking participation by 2 percentage points.

When looking at frequent smoking, our results are insignificantly different from zero for almost all specifications and subgroups. This could be because there is no causal effect of prices on youth frequent smoking, but it could also be that our sample contains to few frequent smokers for there to be enough identifying variation for the model to find a significant effect, in line with the findings by Lillard et al. (2013). In line with the argument DeCicca (2022) makes which we summarized in section 2, we will center our discussion on smoking participation.

Our results align with the emerging view that youths are becoming less responsive to tobacco tax increases. This has important implications for public health policymakers as tobacco taxation is regarded by the WHO as the most efficient policy to reduce youth smoking (2022). Also, the European Union's "Europe's Beating Cancer Plan" gives tobacco taxation a crucial role, particularly in reducing youth smoking (n.d.). In order to continue to reduce youth smoking, policymakers may need to consider other types of regulation. Further research into the impact of alternative tobacco regulations on youth smoking in Europe could help guide policymakers in which interventions to prioritize.

Similarly to Hansen et al. (2017), we also show that the effect of tobacco taxation seems to decline over time in Europe as well[10]. Restricting our sample to 2010-2018, our estimates become insignificant even for smoking participation. We have not studies the potential explanations for why youths are less price sensitive than before. Hansen et al. proposed an explanation that when smoking participation decreases substantially, the remaining group will be more price insensitive, which we find plausible.

It is important to take economic and cultural differences into account when comparing our results to similar studies. Our estimated price elasticity of -0.1 is in the lower bound of US-

[10]When we drop one survey wave at a time, the same pattern holds.

dominated literature on the price sensitivity of youth smoking in recent years. One possible explanation is that the marginal effect of taxation is decreasing and that the EU15's high level of taxation compared to the US has deterred everyone but the most price-insensitive youth smokers.

Another possible explanation is that the different treatments of manufactured and roll-your-own (RYO) cigarettes lessen the effect of raising the taxes on manufactured cigarettes in Europe. López-Nicolas et al. (2013) showed that tax increases on manufactured cigarettes primarily induce substitution to RYO cigarettes when both taxes are not raised at the same time. During the period we study, there has been a significant price gap between the two types of cigarettes(López-Nicolas and Stoklosa, 2017).

Thus, the tax raises on manufactured cigarettes may have reduced demand for that type of tobacco in favor of RYO cigarettes, which we are not able to see in our data as the HBSC only asks about smoking cigarettes in general, not manufactured cigarettes specifically. Since we believe these kinds of cultural, political and economic differences are important to take into account when interpreting the results, we also emphasize that we cannot directly generalize our results to other times or parts of the world.

The low estimates could also be due to the nature of the study. The HBSC survey younger children than typical in the literature on youth smoking in the US. The fact that the youngest children in our sample are the most price sensitive can indicate that this is the case. Another possible source of error is the low number of clusters in the standard error estimation. The bootstrap results show the standard errors and following inference are too precise, while still showing significant effects on smoking participation at the 10% level.

One possible source of endogeneity is omitted variable bias from not including a measure of anti-smoking sentiment in our model. We have tried to control for this in the country-specific time trends robustness check, but these results in themselves are difficult to trust given that we have had tax increases in all observation periods and countries. As such, we cannot evaluate the gravity of omitting anti-smoking sentiment in our model. If present, our assumption is that we have overestimated the effect of tax increases.

The other robustness checks (excluding control variables, other regression methods, country-level effect heterogeneity) show adequate results with insensitive estimates of cigarette excise tax effects. One could have suspected that heterogenous effect between countries would be a bigger problem, for example due to the different availability of alternative tobacco products like e-cigarettes or snus or proximity to countries with lower cigarette prices, since it has been shown that smuggling increases significantly when cigarette taxes are increased (Preiger and Kulick, 2018).

However, as previously discussed we do have evidence of effect heterogeneity over time. This implies our estimates are some form of average of the different magnitudes of effect over the period studied. There is also the worry of dynamic effects, which we have been unable to account for, but which could bias our results. In line with the discussion in Section 4.3, we believe the importance of such errors to be limited.

There are several topics we have been unable to investigate in this thesis. First and foremost, the rapid development of the framework surrounding dynamic effects and two-way fixed effects and difference in difference methods has not yet been applied to cigarette excise tax studies. It is plausible that tax increases take time to have an effect on smoking. Measuring this dynamic effect could give further insight into the consequences of cigarette excise tax. Perhaps, this would require data measured more often than every four years.

Due to the lack of data on tobacco taxation, we restricted our analysis to the EU15. However, it would be interesting to conduct the analysis for all the EU countries. Collecting this data is most likely possible, but time-consuming. With more data on all EU countries, it would be possible to estimate the effect for the entire EU and look at differences between different regions of Europe. Smoking culture and economic conditions vary across the continent, so investigating this possible effect heterogeneity could give a better understanding of when and where cigarette excise taxes are effective.

Our final suggestion for future research is how to handle the fact that EU countries have different exchange rates. We chose to convert all tax rates using a fixed exchange rate in order to remove noise from exchange rate fluctuations since we could find no standard approach to handle this problem. More research would help in order to make it possible to compare countries with different currencies without introducing noise or bias.

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A Appendix

A.1 MPCC and WAP deviations distribution

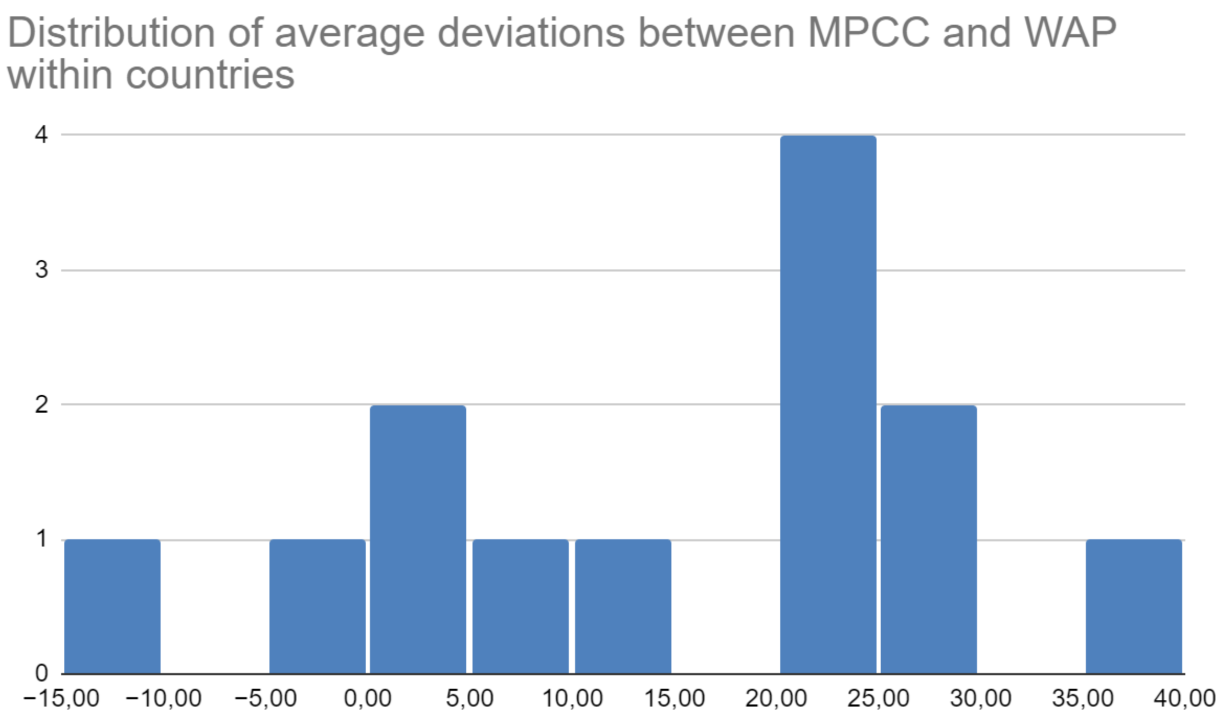

FIGURE 6

A.2 Conversion formula from log-odds

The formula for computing change in participation rates, where β is the estimated coefficient and is the sample mean assumed to be the starting point:

$$\frac{e^{\beta}\left(\frac{\alpha}{1-\alpha}\right)}{1+e^{\beta}\left(\frac{\alpha}{1-\alpha}\right)}$$

A.3 Frequent smoking questions distribution

For the intensive margin, the question “How often do you smoke tobacco at present?” with 4 different answer options was used for the first four waves. However, this question was not asked in 2018, instead the question “On how many days (if any) have you smoked cigarettes in the past 30 days?” with 7 different answer options included. These questions measure the same thing but on different scales. Both questions were asked in 2014. The distribution of the answers is seen in Appendix A3. To make the questions comparable, the 2018 data was recoded such that individuals were randomly assigned an answer from the previous question, conditional on the answer on the 2018 question. For example, those who answered 6-9 days were assigned the answer Every day with probability 17.77, according to the distribution in Appendix A3. This makes the most use of the data, is unbiased conditional on the 2014 distribution, and preserves an adequate amount of variation. 2014 - 2018 variable 2001 - 2014 variable

TABLE 4: 2014 distribution of 2001-2014 (top row) and 2014-2018 (first column) questions measuring intensive margin/frequent smoking variable. *Count (row percent).*

	Every day	At least once a week	Less than once a week	I do not smoke	Total
Never	697 (0.37)	272 (0.14)	1,339 (0.71)	186,019 (98.77)	188,327 (100.00)
1-2 days	158 (3.05)	477 (9.20)	2,229 (43.00)	2,32 (44.75)	5,184 (100.00)
3-5 days	168 (8.27)	608 (29.94)	893 (43.97)	362 (17.82)	2,031 (100.00)
6-9 days	280 (17.77)	688 (43.65)	429 (27.22)	179 (11.36)	1,576 (100.00)
10-19 days	412 (24.98)	878 (53.24)	223 (13.52)	136 (8.25)	1,649 (100.00)
20-29 days	1,061 (69.21)	336 (21.92)	57 (3.72)	79 (5.15)	1,533 (100.00)
30 days or more	3,948 (86.88)	277 (6.10)	83 (1.83)	236 (5.19)	4,544 (100.00)
Total	6,724 (3.28)	3,536 (1.73)	5,253 (2.56)	189,331 (92.43)	204,844 (100.00)

A.4 Full regression results for main specification

TABLE 5: Full regression results for main specification

VARIABLES	Extensive	Intensive
Excise Tax	-0.113*** (0.0325)	-0.0669* (0.0407)
Country: Belgium (Flemish)	-0.674*** (0.0466)	-0.494*** (0.0499)
Country: Belgium (French)	-0.381*** (0.0463)	-0.457*** (0.0493)
Country: Denmark	0.0301 (0.222)	-0.277 (0.227)
Country: Finland	0.0630 (0.0401)	-0.0860* (0.0463)
Country: France	-0.142 (0.0891)	-0.228** (0.0926)
Country: Germany	-0.127* (0.0659)	-0.217*** (0.0699)
Country: Greece	-1.277*** (0.337)	-1.007*** (0.339)
Country: Ireland	-0.155 (0.147)	-0.181 (0.156)
Country: Italy	-0.464*** (0.165)	-0.268 (0.164)
Country: Luxembourg	1.464 (1.015)	1.256 (1.035)
Country: Netherlands	-0.310*** (0.0607)	-0.313*** (0.0638)
Country: Portugal	-1.003** (0.393)	-1.088*** (0.396)
Country: Spain	-0.916*** (0.262)	-0.706*** (0.268)
Country: Sweden	-0.244*** (0.0867)	-0.687*** (0.0888)
Country: England	-0.104 (0.135)	-0.295* (0.153)
Country: Scotland	-0.234 (0.144)	-0.323** (0.161)
Country: Wales	-0.125 (0.148)	-0.210 (0.169)
Survey Wave: 2006	-0.334*** (0.0941)	-0.316*** (0.0954)
Survey Wave: 2010	-0.507*** (0.116)	-0.392*** (0.129)
Survey Wave: 2014	-1.019*** (0.171)	-0.737*** (0.173)
Survey Wave: 2018	-1.136*** (0.205)	-1.020*** (0.221)
Gender	-0.0103 (0.0393)	0.121*** (0.0397)
Age: 13	1.548*** (0.0302)	1.900*** (0.0918)
Age: 15	2.679*** (0.0490)	3.283*** (0.112)
GPD per capita	-3.33e-05 (2.15e-05)	-2.98e-05 (2.19e-05)
Unemployment	0.765 (0.957)	-0.202 (1.078)
Constant	-0.632 (0.695)	-3.187*** (0.726)
Observations	438,86	441,901

*Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

A.5 All regression results, smoking participation

TABLE 6: All regression results, smoking participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Main_ex	CSTT_ex	No_controls_ex	2001-2010_ex	2010-2018_ex	Boys_ex	Girls_ex	15_ex	LRM_ex	Probit_ex
Excise Tax	-0.113	0.117	-0.122	-0.214	0.0596	-0.0896	-0.139	-0.0811	-0.00824*	-0.0597
	***		***	**		***	***	***		***
S.E.	(0.0325)	(0.0731)	(0.0410)	(0.101)	(0.0391)	(0.0322)	(0.0347)	(0.0286)	(0.00442)	(0.0182)
N	438,86	438,86	438,86	260,933	177,927	215,637	223,223	140,999	438,86	438,86

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

A.6 All regression results, frequent smoking

TABLE 7: All regression results, frequent smoking

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Main_in	CSTT_in	No_controls_in	2001-2010_in	2010-2018_in	Boys_in	Girls_in	15_in	LRM_in	Probit_in
Excise Tax	-0.0669*	0.0179	-0.122 ***	-0.109	0.110	-0.0449	-0.0841 **	-0.0811 ***	-0.00162	-0.0319
SE	(0.0407)	(0.0816)	(0.0410)	(0.108)	(0.0953)	(0.0539)	(0.0357)	(0.0286)	(0.00201)	(0.0203)
N	441,901	441,901	438,86	260,904	180,997	217,232	224,669	140,999	441,901	441,901

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1