Cigarette Taxes and Smoking Among Sexual Minority Adults♠

Christopher S. Carpenter[1]

Dario Sansone[2]

This version: June 2021

Abstract

We provide the first quasi-experimental evidence on the relationship between cigarette taxes and smoking among sexual minority adults, a group that has been understudied in past research. We use large samples of individuals in same-sex households (a large share of whom are sexual minorities in same-sex romantic relationships) from the 1996-2018 Behavioral Risk Factor Surveillance System. We find that cigarette taxes significantly reduced smoking among men and women in same-sex households, and the effects we find for men in same-sex households are very robust and significantly larger than the associated effects for men in different-sex households (the vast majority of whom are heterosexual married or partnered men). For men in same-sex households, we find no unintended consequences of higher cigarette taxes on other risky behaviors, and in fact we find that cigarette taxes are associated with significant *improvements* in self-rated health for men in same-sex households. These results suggest that the sizable disparities in adult smoking rates between heterosexual and sexual minority men would have been even larger in the absence of stricter tobacco control policy. However, in line with previous research indicating that cigarette taxes have 'lost their bite', we find no significant relationship between cigarette taxes and sexual minority smoking in more recent years.

Keywords: LGBT, cigarette tax, smoking, health disparities

JEL: H20, H71, I12, I18

♠ We gratefully acknowledge financial support from the Robert Wood Johnson Foundation Grant #76601. Results do not imply the endorsement of the Robert Wood Johnson Foundation or any other organization. We are grateful to the Editor Ana Balsa, two anonymous referees, Garance Genicot, Joseph Sabia, and participants at the 2020 AEA/ASSA Meetings and the 2020 EALE/SOLE/AASLE Annual Conference for helpful comments. The usual caveats apply.

[1] Vanderbilt University, NBER and IZA. E-mail: christopher.s.carpenter@vanderbilt.edu

[2] University of Exeter and IZA. E-mail: d.sansone@exeter.ac.uk

1. Introduction

A very large literature in health economics demonstrates that higher excise taxes on cigarettes are associated with lower rates of smoking (DeCicca et al., 2021). While the earliest studies demonstrated this finding using cross-state variation in taxes (see, for example, Chaloupka and Wechsler, 1997), numerous studies have also applied more internally valid difference-in-difference approaches that leverage within-state changes in cigarette taxes (for recent examples, see Cotti et al., 2016 and Pesko et al., 2020). The majority of published studies in economics have demonstrated that cigarette tax hikes significantly reduce adult smoking, although the estimates differ substantially in magnitude (Gallet and List, 2003).

Moreover, several studies also examine the effects of cigarette taxes on smoking behaviors for various demographic sub-groups. For example, youths are of particular interest given that most adult smokers started smoking before age 18 and given concerns that youths may not fully understand the consequences of their decisions about risky behaviors. Studies of youths have returned mixed effects: while DeCicca et al. (2002) find no effects of cigarette taxes on youth smoking in the National Education Longitudinal Study, Carpenter and Cook (2008) use data from the 1991-2005 Youth Risk Behavior Surveys (YRBS) and find that youth smoking was significantly negatively related to state excise taxes on cigarettes. More recently, Hansen et al. (2017) have documented that cigarette taxes have 'lost their bite' among youths over the period 2007-2013. In addition to youths, economists have also studied how cigarette taxes affect other sub-populations, including older-adults (DeCicca and McLeod, 2008; MacLean et al., 2016), pregnant women (Ringel and Evans, 2001; Colman et al., 2003; Lien and Evans, 2005; Simon, 2016), racial and ethnic minorities (Farrelly et al., 2001), and light and heavy smokers (Cotti et al., 2016; Nesson, 2017).

In this paper, we contribute to the literature on heterogeneity in the effects of cigarette taxes on smoking by providing the first quasi-experimental evidence on the effects of cigarette taxes and other tobacco control policies on smoking rates among sexual minority (that is, lesbian, gay, bisexual, or queer) adults. This population has historically been invisible in social science and health research due to a lack of data on sexual orientation, a lack of research funding (Coulter et al., 2014), and structural barriers such as discrimination (Klawitter, 1998). Based on recent statistics, gay men, lesbian women, and bisexual individuals are a particularly important sub-population to study because their smoking rates (20.3 percent) are much higher than the smoking rates of heterosexual adults (13.7 percent) (CDC, 2018a). This difference in smoking rates by sexual minority status (6.6 percentage points) is larger than the difference in smoking rates between men and women (3.6 percentage points); younger adults age 18-24 and older adults age 65+ (2.2 percentage points); white and black adults (0.3 percentage points); adults in the regions of the US with the highest (Midwest) and lowest (West) smoking rates (5.9 percentage points); and unmarried versus married adults (2 percentage points). Although public health scholars have long documented differences in smoking rates by sexual minority status (Hoffman et al., 2018),

we are not aware of any research that has related changes in state excise taxes on cigarettes to changes in rates of smoking for sexual minorities.[3] Our study is the first to fill this gap.[4]

One important challenge in credibly estimating the effects of cigarette taxes on smoking behaviors among sexual minorities is the relative absence of good data on sexual orientation, particularly in surveys that also include information on smoking. While several large surveys have begun to add questions about sexual orientation, credible estimation in difference-in-differences models for this setting requires substantial data before and after cigarette tax hikes. This is a problem because few datasets include direct information on sexual orientation prior to the mid-2010s. As multiple studies have found that cigarette tax effects on cigarette smoking were larger in the 1990s and 2000s than in the more recent decade (Hansen et al., 2017; Callison and Kaestner, 2014), the lack of historical data on smoking behaviors among sexual minority adults is a particularly serious challenge.

A key contribution of this paper is to overcome this data challenge by using information on household structure and sex composition from the Centers for Disease Control's Behavioral Risk Factor Surveillance System (BRFSS) for the period 1996-2018. This approach has been used previously to study household income among sexual minorities (Carpenter, 2004) and the effects of LGBT public policies on these sub-populations (Carpenter et al., 2021a). Specifically, in the BRFSS individuals are asked about the total number of adults in the household as well as how many of the adults are men and how many of the adults are women. The intuition is very simple: households with exactly two adult men or exactly two adult women are disproportionately likely to contain sexual minorities in same-sex romantic relationships. Of course, same-sex adults may cohabit for other, non-romantic reasons as well; we examine results from several subsamples where misclassification is likely to be less severe. Moreover, we use independent data with direct measures of sexual orientation available from 2014 to validate our approach. While this household-based method of identifying likely sexual minorities is indirect and has a few other important limitations (e.g., it cannot be used to identify single sexual minorities), it does have many advantages as well, including the fact that it returns meaningfully large samples of individuals in same-sex households over a long period (approximately 200,000 individuals in same-sex households in the BRFSS from 1996 to 2018), and that it does not rely on individuals truthfully self-reporting their sexual orientation.

To preview our results, in our difference-in-differences models with controls for individual demographic characteristics, other state-by-year contextual and policy variables, and fixed effects

[3] As we conceptualize sexual orientation as a demographic characteristic similar to age or race/ethnicity, our paper has more in common with studies addressing cigarette tax effects and disparities in such effects for older adults and for racial and ethnic minorities as opposed to studies focusing on youths and pregnant women for whom paternalism and externality arguments are stronger.

[4] For this reason, we do not provide a detailed literature review here. Hatzenbuehler et al. (2014) study 577 adults in the 2004 wave of the National Epidemiologic Survey of Alcohol Related Conditions and find that sexual minority adults in areas with more restrictive tobacco environments had lower smoking rates than otherwise similar sexual minority adults in areas with more permissive tobacco environments. The authors do not separately examine cigarette taxes per se, however, and they only examine one cross section of data.

for state and time, we find clear evidence that cigarette taxes were significantly related to lower rates of smoking among individuals in same-sex households. Over the period 1996-2018 we estimate that a one dollar increase in the state excise tax on cigarettes was associated with a 1.8 percentage point reduction in the likelihood of being a daily smoker among men in same-sex households. We find similar estimates when looking at the probability of being a current smoker. The associated estimates for women in same-sex households are smaller and marginally statistically significant when analyzing daily smoking. The results for men in same-sex households are robust to the inclusion of state-specific time trends, the exclusion of states with numerous local cigarette tax jurisdictions, the further restriction on subsets of same-sex households more likely to contain sexual minorities, and a range of other robustness tests. We then replicate several patterns from the published literature, including that cigarette taxes have been less effective at reducing smoking rates in the more recent period (2011-2018).

We also show that cigarette taxes are estimated to have reduced smoking among individuals in different-sex households – the vast majority of whom are married or partnered heterosexual individuals. Nevertheless, we find that cigarette taxes were significantly more effective at reducing smoking among men in same-sex households than among men in different-sex households. These results are in line with the previous literature suggesting that certain tobacco policies - including cigarette taxes - may benefit more disadvantaged groups and even reduce inequalities in smoking (Thomas et al., 2008), and we explore several possible explanations for this differential sensitivity.

Finally, we examine the effects of cigarette taxes on a range of other employment and health outcomes for individuals in same-sex households. Apart from the robust effect of cigarette taxes at reducing smoking, we do not find economically or statistically significant effects on employment or a range of preventive and risky health behaviors that are particularly relevant for sexual minorities, including HIV testing, alcohol consumption, and body weight. We do, however, find that state excise taxes on cigarettes significantly improved self-rated health among men in same-sex households.

Overall, our results are the first to credibly document that cigarette taxes reduced smoking among sexual minority adults and indicate that the large sexual orientation-related gap in adult smoking rates would have been even larger in the absence of cigarette excise tax increases from 1996 to 2018.

2. Why might there be heterogeneity in the effects of cigarette taxes on smoking by sexual orientation?

There are many reasons why we might expect cigarette taxes to have differential effects on smoking behavior by sexual orientation.[5] First, as noted above, smoking rates among sexual

[5] We focus this section on those explanations for which we can provide some evidence from our data, though there are others that we cannot adjudicate. For example, research has found that tobacco industry marketing specifically targets sexual minorities (Dilley et al., 2008), which may also undermine the effectiveness of stricter tobacco controls for this group. We are unable to provide evidence on this directly, however, as we do not observe the types and quantities of tobacco marketing to which individuals are exposed.

minorities are much higher than among heterosexual individuals. This may indicate that sexual minorities smoke for different reasons than heterosexuals. For example, public health research has suggested that higher smoking rates among sexual minorities may be due to 'minority stress' (Meyer, 1995): i.e., high levels of chronic stress due to stigmatization, internalized homophobia, harassment, and discrimination that could lead sexual minority individuals to use smoking as a coping mechanism (Friedman, 2020). As sexual minorities face the same broad policy environment as heterosexual individuals, the higher smoking rates in sexual minority communities may indicate that tobacco control policies such as cigarette taxes could be less effective at reducing smoking among sexual minorities than among heterosexual individuals. Minority stress theory also suggests that any tax-induced reductions in smoking by sexual minorities might induce substitution to other substances or activities to cope with minority stress. We will provide direct evidence on this question in our empirical analysis.

Second, research has shown that, compared to heterosexual individuals, sexual minority individuals have lower rates of health insurance coverage (Badgett et al., 2021; Gonzales and Blewett, 2014), are less likely to have routine access to care, and are less likely to have regular check-ups (Buchmueller and Carpenter, 2010). Unique health profiles of sexual minorities also make them differentially likely to access certain types of specialty care (e.g., obstetrics-related care). If these differences translate to lower rates of insurance-related smoking cessation treatment, worse access to information on the benefits of quitting smoking, or fewer recommendations to quit smoking from health care professionals, then tobacco control policies that work through insurance or access to care mechanisms may also show less efficacy among sexual minorities. We address this below by estimating models that control for health insurance coverage, as well as by stratifying by individual insurance status, and we also include in our main empirical specification controls for state public insurance program coverage of smoking cessation treatment.

In addition to reasons why smoking rates of sexual minorities might be less responsive to cigarette taxes than heterosexuals, it is also possible that sexual minorities might be more responsive to cigarette taxes than heterosexuals. For example, it is possible that sexual minorities use smoking as a socialization tool more so than heterosexual individuals, which is suggested by public health research (Jannat-Khah et al., 2018; Remafedi, 2007). If so, then once an individual stops smoking because of the tax, that will induce others to stop smoking as well since it is not a socializing tool anymore. Since smoking rates are higher among gay men, this network or spillover effect may be larger for gay men than for heterosexual men. While our data do not contain direct measures of socialization, we will address this indirectly through other variables correlated with sociability, such as alcohol consumption.

Finally, there are large literatures documenting income and earnings differences for sexual minorities relative to heterosexual sub-populations (see for instance Plug and Berkhout, 2004; Carpenter, 2007; Drydakis, 2009; Tilcsik, 2011; Geijtenbeek and Plug, 2018; Aksoy et al., 2019). With some exceptions (Weichselbaumer, 2003), the main findings are that gay men earn less than similarly situated heterosexual men, while lesbians earn more than similarly situated heterosexual women. Differentials in high school graduation and college completion rates have also been found

by Black et al. (2007), Carpenter (2009), and Sansone (2019a), with most of these studies finding higher human capital accumulation for sexual minority adults as compared to heterosexual individuals. Since human capital and the availability of economic resources can play a key role in determining the effects of the excise taxes on cigarettes (Remler, 2004; Franks et al., 2007; Harding et al., 2012; Goldin and Homonoff, 2013), it is possible that the effects of these policies would differ by sexual minority status. For example, lower earnings may make sexual minorities more responsive to cigarette tax hikes than heterosexual individuals if taxes constitute a larger share of income for sexual minorities. Furthermore, higher education could help sexual minorities better understand the adverse health consequences of smoking that are signaled by higher taxes. We address these hypotheses below by estimating models that control for as well as stratify by these potentially endogenous variables (e.g., education, income). In addition, we directly address whether differences in the prevalence of the characteristics across groups mainly contributes to differential cigarette tax responsiveness or whether there is meaningful substantive heterogeneity specifically related to sexual orientation.

Thus, while there is no credible quasi-experimental evidence on cigarette tax hikes and smoking among sexual minorities, there are several reasons why we might expect the effects of cigarette taxes to differ by sexual orientation. Furthermore, the direction of any differential tax impact is unclear *ex-ante*. If sexual minorities are using smoking to cope more so than heterosexuals, or if sexual minorities have less access to anti-smoking assistance, we might expect cigarette taxes to be less effective at reducing smoking among sexual minorities as compared to heterosexual individuals. In contrast, if sexual minority smokers have lower earnings and greater education compared to heterosexual individuals, or if sexual minorities use smoking as a socialization tool more so than heterosexuals, we might expect that cigarette tax hikes might be more effective at reducing smoking among sexual minorities as compared to heterosexual individuals. Ultimately, this is an empirical question, and one we address in this paper.

3. Data

3.1 Behavioral Risk Factor Surveillance System (BRFSS)

The Behavioral Risk Factor Surveillance System (BRFSS) is a nationally representative health survey conducted by the Center for Disease Control and Prevention (CDC). The first BRFSS survey was conducted in 1984 in 15 states and has been extended to all 50 states (plus the District of Columbia) since 1993. More than 400,000 noninstitutionalized adults (18 years or older) are interviewed each year by phone, making it the largest ongoing telephone health survey in the world. Phone calls are made 7 days per week, during both daytime and evening hours. Participants do not receive monetary compensation for taking part in this survey.

BRFSS collects state data about U.S. residents regarding their health-related risk behaviors and events, chronic health conditions, and use of preventive services. The BRFSS survey also includes standard demographic questions such as age, race, ethnicity, education, and marital status. This dataset has previously been used to analyze the impact of cigarette taxes by, among others, DeCicca and McLeod (2008) and Pesko et al. (2020).

Beginning with the 2011 dataset, the CDC started making survey calls to cell-phone numbers in addition to landlines in order to keep the data representative of the U.S. population. Furthermore, it changed the statistical method used to compute sampling weights, moving from post-stratification to iterative proportional fitting (Pierannunzi et al., 2012). Given these methodological changes, we consider models using the full sample, as well as separately from 1996-2010 and 2011-2018. The 2018 wave includes also individuals interviewed in the first three months of 2019. In addition, we follow prior research that pools all BRFSS waves and adjusts weights accordingly (Simon et al., 2017).

3.2 Identifying same-sex couples in the BRFSS

As already mentioned in the introduction, we identify same-sex couples in the BRFSS using the same procedure implemented by Carpenter et al. (2021a). Specifically, we use the fact that in the BRFSS one randomly selected adult in the household is asked to state the number of adult men and the number of adult women in the household.[6] Combined with information on the sex of the respondent, this permits the identification of households containing exactly two adult men and exactly zero adult women; henceforth, men in same-sex households (SSH). Similarly, households that contain exactly two adult women and no adult men are defined as women in same-sex households, while households with exactly one man and one woman are recorded as individuals in different-sex households (DSH).

The underlying idea is that sexual minority individuals are much more likely than heterosexual respondents to live in a household composed of exactly two same-sex adults, and thus these data can be used as an indirect way of identifying meaningfully large samples of sexual minority adults in same-sex relationships. One incidental advantage of this indirect approach is that individuals do not have to explicitly self-identify as a sexual minority to the interviewer, somewhat reducing concerns about selective disclosure. A more important advantage is that this approach can be used to identify same sex household throughout the history of the BRFSS: we go back to 1996 because the smoking questions are comparable since that year. Because the relationship between sexual orientation and our household structure measure may be weaker for younger adults who are more likely to be students and/or co-residing with a same-sex adult for reasons other than a romantic relationship, we restrict the sample to individuals aged 25 and older.

Starting from 2014, the BRFSS has also offered an identically-worded sexual orientation and gender identity (SOGI) optional module to states, and 35 states have used the module at least once and permit the BRFSS to release their data on the public use file. We use these more recent data to provide direct evidence on whether individuals in same-sex households are indeed more likely

[6] Because the household screener with questions about the number of adult men and adult women in the household was not administered to the cellphone sample, all cellphone interviews are excluded from the analysis. This may have the effect of making the samples in those years older and less educated, as individuals who only use cellphones are younger and more highly educated. As noted above, the cellphone sample was not added until 2011, so we can separately estimate results for the period 1996-2010 using all BRFSS respondents.

to self-identify as non-heterosexual.[7] As demonstrated in Carpenter et al. (2021a) using the same kind of comparisons, but based on a more limited number of years, only 1% of individuals in different-sex households identified as non-heterosexual. On the other hand, 11% of women and 28% of men in same-sex households identified as non-heterosexual.

In addition, in line with the fact that gay and bisexual men are the groups most affected by HIV in the U.S. (CDC, 2018b), Carpenter et al. (2021a) find that men in same-sex households were more likely to report having ever been tested for HIV than men in different-sex households. We confirm this to be true in the 1996-2018 BRFSS data: while 40.5 percent of men in different-sex households report ever having an HIV test, the corresponding rate for men in same-sex households was 57.1 percent (i.e., 41 percent higher). Furthermore, Carpenter (2004) shows that a high percentage of male respondents in same-sex households reported condom use for disease prevention as opposed to contraception. These individuals were also more likely to report anal sex without condoms than other male respondents. Similarly, women in same-sex households were much less likely to report birth control use than women in different-sex households. Finally, as further evidence on reliability of the BRFSS data, Carpenter et al. (2021a) show that the likelihood an individual reported being married was systematically related to legal access to same-sex marriage for individuals in same-sex households but not for individuals in different-sex households in the BRFSS.[8]

[7] We considered using demographic characteristics and the SOGI information from 2014-2018 to predict minority sexual orientation for individuals in the earlier BRFSS sample but decided against it for multiple reasons. First, there are ethical concerns associated with predicting minority sexual orientation given ongoing reports of discrimination and persecution against gay, lesbian, bisexual, and queer people. Second, in practice the observed demographic characteristics available to us are very poor predictors of minority sexual orientation, a pattern that has also been found in the prior literature even when rich genetic data are available (Ganna et al., 2019). For example, the sensitivity or recall rates (that is, the share of all sexual minorities that the prediction model correctly identifies) using demographics are below 20 percent using logit models or more advanced machine learning algorithms such as LASSO. In contrast, our approach of using household sex composition has both high accuracy (i.e., well over 90 percent of all individuals identified using this method are correctly identified as heterosexual or non-heterosexual) as well as good recall: 54.1 percent of non-heterosexual men in the 2014-2018 BRFSS sample of two adult households are correctly identified using our approach.

[8] Using a different dataset (the American Community Survey), Sansone (2019b) provides further evidence on the reliability of the same-sex household measure to identify sexual minorities. An important advantage of the ACS relative to the BRFSS for identifying same-sex couples is that the ACS contains information on the relationship of all household members to the household head (the person in whose name the house or apartment is owned or rented). In contrast, the BRFSS does not include information on relationship of the adults in the household to each other. Given the structure of the ACS, Sansone (2019b) divides same-sex households into same-sex roommates and same-sex married/unmarried couples. While the proportion of same-sex roommates was similar to that of same-sex married/unmarried couples in less tolerant states, it was smaller in more LGBT-friendly states. Moreover, the proportion of same-sex roommates had remained stable over time in more tolerant states, but it had declined in less tolerant states. At the same time, the proportion of unmarried and married same-sex couples had increased. These patterns support the hypothesis that individuals in same-sex relationships were more likely to report being roommates when they preferred not to disclose their sexual orientation, thus emphasizing an advantage of using same-sex households as the main criterion to identify LGB individuals.

4. Econometric framework

4.1 Difference-in-difference model

Formally, the estimated difference-in-difference model is the following linear model:

$$y_{ist} = \alpha + \beta(cigarette\ tax)_{st} + \ \delta_s + \mu_t + x'_{st}\gamma_1 + x'_{ist}\gamma_2 + \varepsilon_{ist}$$

where y_{ist} is the relevant smoking behavior for individual *i* living in state *s* at time *t*. The coefficient of interest is β. The specification includes state fixed effects (δ_s), month-by-year fixed effects (μ_t), time-varying state-level controls (x'_{st}, described in detail below), as well as individual-level controls (x'_{ist}). Individual controls x'_{ist} are: age (in five-year age groups), race, ethnicity, and education.[9] This specification is estimated using only the sample of same-sex households. Standard errors are clustered at the state level (Bertrand et al., 2004). All models are weighted using the BRFSS sampling weights rescaled following Simon et al. (2017).

4.2 Policy exogeneity

Taxes on cigarettes are levied at the federal, state, and municipal levels. In line with most of the previous literature, and since the public-use BRFSS data do not contain detailed sub-state geographic information over the entire sample period, this analysis focuses on state excise taxes.

In order for the coefficient β in the difference-in-difference specification described in the previous section to estimate the causal impact of cigarette taxes on health outcomes, it has to be the case that there are no time-varying factors correlated with the state decision to increase tobacco taxes and influencing health indicators among individuals in same-sex households. All state time-invariant factors, such as location, are already controlled for by the state fixed effects δ_s, while the time fixed effects μ_t account for macroeconomic shocks or federal policies affecting all U.S. states at the same time.

Historically, tobacco taxes have been enacted to increase state funding, often motivated by budgetary shortfalls, without any tax revenues being earmarked for helping smokers quit (Dewan, 2009). State financial conditions are unlikely to be directly correlated with LGBTQ health outcomes. Nevertheless, as the knowledge of the adverse health effects of smoking has spread, states have started using taxes to reduce cigarette consumption. As a result, states where the tobacco industry is stronger, or in which the population is more resistant to taxation, might have been less likely to increase their cigarette taxes. Despite this, Midwest and Southern states were just as likely to raise tobacco taxes in the early 2000s (Simon, 2016). In addition, tobacco tax increases have been implemented in almost all states: in the time period considered in our main empirical analysis, only two states – Missouri and North Dakota – did not increase their tobacco tax. The remaining forty-eight states, plus the District of Columbia, passed 160 cigarette tax

[9] While running a state-level regression with weights for population would give the same point estimates, the inclusion of individual-level controls may increase precision (Angrist and Pischke, 2009). See Section A in the Online Appendix for detailed descriptions of how the control variables have been created.

changes since 1996. Most states increased their taxes more than once.[10] Moreover, since it can take several years for a tax law to go into effect after the initiation of a campaign for a tobacco tax increase (Gruber and Köszegi, 2001), Simon (2016) argues that such a delay implies that, once enacted, the tobacco tax is unlikely to be correlated with short-term changes in antismoking sentiment.

To address any further endogeneity concern, we additionally control for the state time-varying population (which may be related to the size of the state budget) and employment rate. The main specification also includes controls for LGBT policies: same-sex marriage legalization; constitutional or statutory bans on same-sex marriage; introduction of same-sex civil unions and domestic partnerships; anti-discrimination laws; sodomy laws; and hate-crime legislation that includes sexual orientation-motivated bias. Finally, we also include controls for other relevant state health and tobacco-related policies: bans on smoking in restaurants, private workplaces or bars; Tobacco 21 laws; state funding for smoking reduction; Medicaid coverage for smoking cessation; the presence of any e-cigarette tax in the state; bans on vaping in bars, restaurants, and private worksites; Affordable Care Act Medicaid expansions; and Medicaid private options.

4.3 Triple-difference model

The main econometric specification can be extended by estimating a triple-difference model, i.e., by comparing changes in smoking for individuals in same-sex households to the associated changes in smoking for individuals in different-sex households coincident with cigarette tax hikes within the same states over time. As we will demonstrate that taxes are also effective at significantly reducing smoking among individuals in different-sex households, it is worth noting that we do not conceptualize of individuals in different-sex households in this specification as constituting pure controls who are not treated by cigarette taxes. Instead, the goal of this fully interacted model is to provide novel evidence on whether cigarette taxes were differentially effective at reducing smoking among individuals in same-sex households as compared to individuals in different-sex households.

More formally, the equation of interest can be written as follows:

$$y_{igst} = \alpha + \beta(cigarette\ tax)_{st} * Same - Sex\ Household_{ist} + \mu_{st} + \pi_{gt} + \rho_{gs} + x'_{igst}\gamma + \varepsilon_{ist}$$

where y_{igst} indicates smoking behavior for individual *i* living in state *s* at time *t*. The subscript g indicates whether the respondent was in a same-sex household or a different-sex household. The coefficient of interest is β. The specification includes state-specific time effects that are common across same-sex and different-sex households (μ_{st}), time-varying effects specific to same-sex households (π_{gt}), state-specific shocks among same-sex households (ρ_{gs}), and individual controls (x'_{igst}). As with the aforementioned difference-in-difference specification, this model includes month-by-year fixed effects as well.

[10] We show this tax variation visually in Online Appendix Figure C1.

5. Results

5.1 Trends and descriptive patterns

Figures 1 and 2 show trends in rates of daily smoking and current (daily or occasional) smoking respectively, separately for men in same-sex households, men in different-sex households, women in same-sex households, and women in different-sex households. For the sake of completeness, although not exactly comparable, we include data from 1993 to 1995 in these trend graphs.[11] Notably, all the series exhibit general reductions in smoking rates over this time period. In both figures men in same-sex households have the highest smoking rates, followed by women in same-sex households, men in different-sex households, and women in different-sex households.

We present descriptive statistics in Table 1 separately for women in same-sex households, women in different-sex households, men in same-sex households, and men in different-sex households. In line with the previous literature, Table 1 indicates that men and women in same-sex households had substantially higher smoking rates than men and women in different-sex households between 1996 and 2018. Women in same-sex households were more likely to be age 40 or older, less likely to be white, much less likely to be married (as expected since same-sex marriage was not legal in most states in the time period considered), less likely to have a bachelor's degree, and more likely to have low household income than women in different-sex households. Men in same-sex households were less likely to be age 40 or older, less likely to be white, less likely to be married, and more likely to have low household income than men in different-sex households.

As already mentioned in Section 3.2, we present evidence in Table 2 on the relationship between household structure and self-reported sexual orientation using data from individuals in states that released their SOGI module to the public use file in the 2014-2018 BRFSS. For individuals interviewed by landline in this sample, we observe both the household sex composition as well as the individual's self-reported sexual orientation. This allows us to directly examine whether our measure of individuals living in same-sex households has purchase for identifying samples that likely contain non-heterosexual adults, as demonstrated previously by Carpenter et al. (2021a). Indeed, Table 2 indicates that very small shares of individuals in different-sex households – less than two percent – identify as non-heterosexual in the SOGI data. This percentages are even smaller when focusing on the share identifying as gay or lesbian (and thus excluding bisexual individuals, who are more likely to be in a different-sex relationship, see Badgett et al., 2021): 0.1 percent of women in different-sex households identify as lesbians, while 0.9 percent of men in different-sex households identify as gay.

In contrast, fully 13.7 percent of women in same-sex households and 24.5 percent of men in same-sex households identify as non-heterosexual, consistent with the idea that a substantial share of individuals in same-sex households are sexual minority adults. The lower rows of Table 2 further show the share identifying as heterosexual or non-heterosexual for individuals in same-sex

[11] As described in Section A.1 in the Online Appendix, some of the questions on smoking in the BRFSS changed multiple times between 1993 and 1996, thus making it difficult to harmonize responses over this time period.

households, separately by the respondent's marital status. A very interesting pattern emerges: among individuals in same-sex households who describe their marital status as 'a member of an unmarried couple', fully 89.6 percent of women and 68 percent of men identify as non-heterosexual. For individuals in same-sex households who describe themselves as married, the associated shares identifying as non-heterosexual are also very large: 58.6 percent for women and 49.2 percent for men. A considerable fraction of never married individuals in same-sex households also identify as non-heterosexual: 15.6 percent for women and 29.1 percent for men. These patterns suggest that household structure and household sex composition convey important information about sexual orientation and support our investigation into the effects of state cigarette taxes on smoking behaviors for this sample, a substantial share of which is composed of sexual minority adults.

5.2 Estimation results

5.2.1 Main difference-in-difference estimates

Table 3 presents our baseline difference-in-differences estimates of the effects of state cigarette taxes on smoking outcomes for women in same-sex households in the top panel and for men in same-sex households in the bottom panel.[12] Each column is from a separate linear probability model, and we report the coefficient on the state excise tax on cigarettes, measured in nominal U.S. dollars.[13] Columns 1-3 present results for the daily smoker outcome, while columns 4-6 present results for the current smoker outcome. Columns 1 and 4 present results from the basic difference-in-differences model including only state and month-by-year fixed effects. Columns 2 and 5 add controls for individual demographic characteristics, and columns 3 and 6 add the state/time varying contextual and policy variables.

The results in Table 3 indicate that cigarette taxes reduced smoking probability for individuals in same-sex households. In the top panel for women in same-sex households we estimate in column 3 that a one dollar increase in cigarette taxes was associated with a 0.6 percentage point reduction in the likelihood of being a daily smoker, and this estimate is statistically significant at the ten percent level. In the bottom panel, for men in same-sex households we estimate in column 3 that a one dollar increase in cigarette taxes reduced the likelihood of being a daily smoker by 1.8 percentage points, and this estimate is statistically significant at the one percent level. In columns 4-6 of Table 3 we estimate generally similar effects of cigarette taxes at reducing the likelihood of

[12] As reported in Table C1 in the Online Appendix, we do not find any meaningful relationship between state excise taxes on cigarettes and the likelihood that an individual is observed to be in a same-sex household. Furthermore, there is no significant relationship between cigarette taxes and the race, age, or educational level of individuals in same-sex households. In addition, Figure C1 in the Online Appendix shows that the share of all two-adult households in the BRFSS that is a same-sex household is quite stable over the time period considered in our empirical analysis. These patterns suggest that composition bias is unlikely to be a serious concern in our study.

[13] Our main results are also robust to adjusting cigarette taxes for inflation, an approach followed in some of the previous studies (Callison and Kaestner, 2014; Pesko et al., 2020). If anything, the estimated impact of cigarette taxes on daily smoking is even larger (Online Appendix Table C4).

current smoking among individuals in same-sex households, though the estimates for women in same-sex households in the top panel of columns 4-6 of Table 3 are not statistically significant.[14]

These estimates correspond to a smoking participation elasticity of -0.08 for men in same-sex households and are within the range of published health economics studies that use quasi-experimental methods to examine the effects of cigarette taxes on smoking of other demographic groups. For example, Callison and Kaestner (2014) find a smoking participation elasticity among 18-70 year olds of -0.026 using the 1995-2007 Tobacco Use Supplements of the Current Population Survey, while DeCicca and McLeod (2008) find an elasticity of about -0.2 for 45-64 year olds using the 2000-2005 BRFSS. Carpenter and Cook (2008) estimate a participation elasticity of -0.25 for high school youths from the 1991-2005 national YRBS, while Hansen et al. (2017) estimate the same elasticity to be 0.061 from the 2007-2013 waves of those same national YRBS data, leading them to conclude that cigarette taxes have 'lost their bite'.

5.2.2 Robustness checks

In Table 4 we present a variety of results exploring robustness and heterogeneity in the estimated effects of cigarette taxes on daily smoking among individuals in same-sex households. We again present results for women in the top panel and men in the bottom panel. Each column represents a different sample or specification change, and each entry is the coefficient on the cigarette tax from a separate regression. We reprint the baseline estimates from column 3 of Table 3 for daily smoking into column 1 of Table 4.

As described in Angrist and Pischke (2014), the common trends assumption can be modified by controlling for state-specific time trends. As shown in column 2, doing so results in smaller and statistically insignificant estimates for women in same-sex households but returns slightly larger and statistically significant estimates for men in same-sex households.[15]

In column 3 we report results for the slightly longer period 1993-2018, thus adding 1993-1995 data even though the smoking questions were slightly different. Doing so has no effect on the

[14] Online Appendix Table C2 reports estimates for the other control variables, which generally conform to expectations: for instance, older individuals smoke less than younger individuals, and more educated individuals smoke less than individuals with less education. Regarding other state tobacco control policies, we do not find evidence that policies other than state excise taxes on cigarettes are consistently statistically significant and robust predictors of lower smoking rates for individuals in same-sex households. Thus, while other more targeted policies may be more likely to affect sexual minority smoking *ex ante*, we find that cigarette taxes are uniquely effective at reducing smoking rates for individuals in same-sex households.

[15] Online Appendix Table C3 shows the results of additional robustness tests for the daily smoking outcome. For example, controlling for quadratic state time trends (in addition to linear state trends) does not change the finding that cigarette taxes significantly reduce smoking for men in same-sex households. Similarly, the main findings for men in same-sex households do not change when adding controls for income per capita at the state-year level, or when adding potentially endogenous individual level controls included in some of the previous studies (Callison and Kaestner, 2014; Pesko et al., 2020) such as employment status, health insurance status, household income, and presence of children in the household. Estimating models without sample weights returns somewhat smaller estimates of the effect of cigarette taxes on smoking among individuals in same-sex households, but the estimates remain negative and significant. Our result on daily smoking for men in same-sex households is also robust to estimating logit or probit models (as done in Carpenter and Cook, 2008, Hansen et al., 2017, and suggested in Kahn-Lang and Lang, 2020).

results for men in same-sex households but returns a slightly smaller estimate for women in same-sex households that is no longer statistically significant. In column 4 we exclude states that have large numbers of local jurisdictions that levy substantial local level taxes on cigarettes: Alaska, Illinois, Massachusetts, New York, Pennsylvania, and Virginia. Doing so does not change the finding that higher taxes significantly reduce smoking among individuals in same-sex households.

In columns 5 and 6 we show estimates from models separately estimated on the 1996-2010 period and the 2011-2018 period, respectively. This is informative both because of the change in BRFSS sampling that occurred with the 2011 wave, and because multiple recent papers have suggested that cigarette taxes have 'lost their bite' at reducing smoking in recent years. Indeed, we find evidence consistent with this hypothesis for men in same-sex households: while estimates in the earlier period suggest significant effects of cigarette taxes at reducing daily smoking among men in same-sex households, estimates for the later period are smaller and not statistically significant.[16]

Finally, in column 7 we report estimates from models that restrict attention to individuals in same-sex households who report being never married or a member of an unmarried couple over the period 1996-2010. Over this time range, legal access to same-sex marriage was extremely limited in the United States, so sexual minorities individuals were more likely to be single or cohabitating with an unmarried partner. Restricting attention to these individuals returns larger estimates of the effects of cigarette taxes on reduced daily smoking for men, while the estimate for women is not statistically significant.

Taken together, the findings in Table 4 indicate that the relationship between cigarette taxes and daily smoking is highly robust for men in same-sex households. That the estimates for women in same-sex households are somewhat more sensitive to these robustness and heterogeneity tests may result from the pattern observed in Table 2 that being an individual in a same-sex household is a stronger signal of a sexual orientation minority status for men than for women.[17]

[16] Online Appendix B and Online Appendix Table C13 report results from additional analyses where we examined cigarette taxes and smoking among those who self-identified as sexual minorities in the states that administered the sexual orientation and gender identity (SOGI) module from 2014-2018. Those analyses did not yield evidence that cigarette taxes significantly reduced smoking among self-identified sexual minorities from 2014-2018, further consistent with the idea that in recent years cigarette taxes have not been effective at reducing smoking.

[17] We do not find any evidence of nonlinearities in the impact of cigarette taxes (Table C4 of the Online Appendix). In the same table we consider an alternative outcome variable (an indicator variable for having tried to quit smoking in the past month) and find evidence that cigarette taxes significantly increased the likelihood of trying to quit smoking for women in same-sex households. We also dropped each state one at a time in Online Appendix Table C5 and found that no individual state is driving the main result for men reported in Table 3. In addition, in Figures C3 and C4 of the Online Appendix we show event study estimates of the effect of large tax changes, following Callison and Kaestner (2014) and Pesko et al. (2020). Neither event study suggests that differential pre-trends are a serious concern in this context. In line with the estimates in Table 3, the evidence for a significant effect of cigarette tax hikes at reducing smoking is stronger for men in same-sex households than for women in same-sex households also when looking at these event studies.

5.2.3 Comparing effects for individuals in same-sex households vs. different-sex households

In Table 5 we examine whether cigarette taxes reduced daily smoking among individuals in different-sex households, which are primarily composed of heterosexual married and partnered people. We present results for women in the top panel and men in the bottom panel. Each entry is from a separate regression using our preferred specification described in Section 4.1 with controls for individual demographic characteristics; state and month-by-year fixed effects; state and time varying economic, demographic, and policy controls. Column 1 reprints the estimates for individuals in same-sex households from column 3 of Table 3. Column 2 shows that higher cigarette taxes are associated with reduced rates of smoking for both women and men in different-sex households on the order of 0.6 and 0.4 percentage points for a dollar increase in cigarette taxes, respectively. This largely replicates prior research that has examined the full population (see, for example, Pesko et al., 2020).

In column 3 we present results from models described in Section 4.3 that include individuals in same-sex households and individuals in different-sex households and focuses on the interaction between the state excise taxes on cigarettes and a dummy variable for individuals in same-sex households. The patterns in the top panel of column 3 return no evidence that cigarette taxes had differential effects at reducing daily smoking among women in same-sex households as compared to the associated effect on women in different-sex households. In contrast, the results in the bottom panel of column 3 of Table 5 indicate that cigarette taxes were differentially effective at reducing daily smoking among men in same-sex households compared to the associated effects for men in different-sex households: the interaction coefficient indicates that a one dollar increase in state cigarette taxes was 0.9 percentage points more effective at reducing smoking among men in same-sex households than among men in different-sex households. This suggests that in the absence of higher cigarette taxes, the disparity in adult smoking rates between sexual minority men and heterosexual men would have been even larger.

Columns 4 and 5 repeat the same exercise as in columns 2 and 3, but instead of using individuals in different-sex households as the comparison group for individuals in same-sex households, we use all individuals regardless of household structure as the comparison group for individuals in same-sex households. It is worth remembering that the overwhelming majority of individuals identified as heterosexual (see Table 2). The patterns in columns 4 and 5 are very similar to those reported in columns 2 and 3 of Table 5 and again indicate that cigarette taxes were differentially effective at reducing daily smoking among men in same-sex households.

Columns 6 and 7 confirm that the same basic pattern also holds when we examine current smoking instead of daily smoking: a one dollar increase in cigarette taxes is associated with a statistically significant 1.1 to 1.2 percentage point reduction in the likelihood of being a current smoker for men in same-sex households compared to men in different-sex households.

5.2.4 Effects on cigarette taxes on other outcomes

Given the robust evidence that cigarette taxes significantly reduced smoking among men in same-sex households, it is natural to ask whether there were any effects on other outcomes for this same sample. For example, did these men engage in other risky behaviors at higher rates to substitute for the reduced smoking? Or were there complementary reductions in other risky behaviors that could have improved the overall health of men in same-sex households? We present evidence on these downstream effects in Table 6. The format of Table 6 is as follows: each row examines a different outcome, and every entry in the table is from a separate regression. Each entry is the coefficient on the state excise tax on cigarettes for the relevant outcome stated in each row.[18] The empirical specification mirrors the basic difference-in-differences model from our main estimates presented in Table 3.

The results in Table 6 indicate that increases in state excise taxes on cigarettes did not have economically or statistically significant effects on a broad range of employment and health outcomes for men in same-sex households. For example, cigarette taxes were unrelated to the likelihood of alcohol consumption or binge drinking for men in same-sex households. This null finding is particularly interesting in light of the strong rates of co-substance use among gay men and the possibility described in Section 2 that gay men might use smoking as a coping mechanism for societal discrimination. While this may be true, Table 6 suggests that the tax-induced smoking reductions were not replaced with increases in the use of alcohol to cope. We similarly find no meaningful relationship with exercise, body weight, or HIV testing – which is particularly relevant for this sample given the historical disproportionate burden of HIV and AIDS among gay men.

Table 6 does, however, reveal one meaningful relationship between state excise taxes on cigarettes and health: we find that higher cigarette taxes significantly improved self-rated health for men in same-sex households. Specifically, we find that higher cigarette taxes significantly increased the likelihood of reporting excellent or very good health and significantly decreased the likelihood of reporting fair or poor health. Together with the other null findings in Table 6, these patterns suggest that the tax-induced smoking reduction for men in same-sex households significantly improved their overall well-being as measured by self-rated health.[19]

6. Discussion

6.1 Why are effects stronger among men in same-sex households?

The results above consistently indicate that cigarette taxes reduced smoking among men in same-sex households more than for men in different-sex households. A natural follow-up question is: why? We explore several potential explanations and mechanisms.

[18] Results in Table 6 are presented for men in same-sex households; results for women in same-sex households are presented in Online Appendix Table C6 and return broadly similar patterns.

[19] Table C7 in the Online Appendix further shows that there is no such relationship between cigarette taxes and self-rated health for men in different-sex households and that the difference in the effects of cigarette taxes on self-rated health between men in same-sex households and men in different-sex households in a similarly specified triple differences model is statistically significant.

First, it is important to stress that the heterogeneity we observed in Table 4 – that cigarette taxes were significantly more effective at reducing smoking among men in same-sex households than among men in different-sex households – is not simply due to differences in characteristics across the two groups (though clearly such differences exist). Evidence on this point is presented in Appendix Table C8 where we estimate models stratified by household income, education, health insurance status, and presence of children. Those models returned quite different patterns of heterogeneity between men in same-sex households and men in different-sex households. For example, we estimate larger and significant cigarette tax effects at reducing smoking among men in different-sex households who are lower income, less educated, without health insurance coverage, and with children in the household, whereas for men in same-sex households the effects are larger and significant for individuals in middle income households, more highly educated men, those with health insurance coverage, and those without children. These patterns suggest that differences in the prevalence of group characteristics across men in same-sex versus different-sex households cannot explain our core findings. For instance, if we had observed that for men in different-sex households it was also highly educated men who reduced their smoking in response to cigarette taxes, we might have suspected that the differential responsiveness of men in same-sex households to cigarette taxes is associated with their higher likelihood of having a college degree. This is consistent with our earlier finding in Appendix Table C3 that our main results are robust to including direct controls for these augmented variables (e.g., household income, health insurance, and the presence of children). These patterns suggest that the heterogeneous effects of taxes on smoking is indeed substantive and more directly related to underlying sexual orientation differences.

We explore other candidate hypotheses for sexual orientation differences in Appendix Table C9 which shows results stratified by body weight and alcohol consumption. Body weight is interesting because there is ample evidence that gay men are less likely to be overweight or obese than otherwise similar heterosexual men (Carpenter, 2003; Deputy and Boehmer, 2014). One possible reason for this is that gay men might be more likely to be smoking to control weight than heterosexual men. If so, we might expect that cigarette taxes would be relatively less effective at reducing smoking among lighter gay men than among lighter heterosexual men, since there may be other reasons for gay men to continue smoking. Interestingly, we find exactly the opposite pattern: the largest effects of cigarette taxes at reducing smoking among men in same-sex households are observed for the lightest men, with smaller effects for men in same-sex households who are overweight or obese. In contrast, the bottom panel of Appendix Table C9 shows that for men in different-sex households the largest effects are for overweight and obese men. These patterns – like those for education described above – suggest that weight characteristics cannot explain the differential effects of cigarette taxes at reducing smoking among men in same-sex households compared to men in different-sex households.[20]

[20] We also explored similar mechanisms for explaining the pattern that cigarette taxes reduce smoking among men in same-sex households, with weaker and smaller effects for women in same-sex households. Regarding body weight, there is ample evidence that lesbian women are disproportionately likely to be heavier than heterosexual women

Appendix Table C9 also shows results stratified by whether the individual is a drinker. There are several reasons to examine heterogeneity by alcohol consumption, not the least of which is the well documented high correlation between smoking and drinking. This is especially true for gay men (Ostrow and Stall, 2008). The results in columns 4 and 5 of the top panel of Appendix Table C9 show that the effects of cigarette taxes at reducing smoking are larger for men in same-sex households who also report alcohol consumption. In contrast, there is far less evidence of a differential pattern of responsiveness of smoking to cigarette taxes by drinker status for men in different-sex households in columns 4 and 5 of the bottom panel of Appendix Table C9.

The finding that cigarette taxes reduce smoking primarily among men in same-sex households who are drinkers may also shed light on the role of smoking as a socialization tool as described above in Section 2. Public health research documents that gay men are more likely to be using smoking as a socializing device than heterosexual men. If so, then once somebody stops smoking because of the tax, that will induce others to stop smoking as well since it is not a socializing tool anymore. Since more gay men were smoking to begin with, this network or spillover effect may be larger for gay men than for heterosexual men. Unfortunately, we do not have any direct measures of socialization in the BRFSS. Alcohol consumption is not an unreasonable proxy for socializing, however, given its outsized prominence in social spaces that gay men are more likely to occupy (e.g., bars and clubs). That we find a strong pattern that cigarette taxes are more effective at reducing smoking among men in same-sex households who are drinkers compared to the effects for men in same-sex households who are not drinkers – and that this differential is much smaller for men in different-sex households – suggests that smoking for the purposes of socialization and social network effects may be important for explaining the empirical finding that cigarette taxes were significantly more effective at reducing smoking among men in same-sex households than among men in different-sex households.

6.2 Is mismeasurement of sexual minorities likely to be driving our results?

As noted above, our approach for studying sexual minorities uses information on households consisting of exactly two same-sex adults since we require sufficiently large samples over a time period when there were several changes in state excise taxes on cigarettes. We have argued that this approach has very good specificity and reasonable sensitivity, that is, the same-sex household proxy can identify sexual minorities with sufficient precision, both absolutely and relative to

(Carpenter, 2003; Deputy and Boehmer, 2014). Given this, we might have expected that, as per the reasoning above, cigarette taxes could be relatively *less* effective at reducing smoking among men in same-sex households than among women in same-sex households (since men in same-sex households are plausibly using smoking for weight control – i.e., for another reason than simply nicotine delivery – more than women in same-sex households). Online Appendix Table C10 shows that cigarette taxes had small and statistically insignificant effects on smoking for women in same-sex households across all weight groups. Appendix Table C10 also reports results separately by the presence of children in the household. Women in same-sex households are much more likely to have children present in the household than men in same-sex households, but we do not find that cigarette taxes significantly reduced smoking among women in same-sex households with or without children. Thus, we do not uncover evidence consistent with the hypothesis that differential rates of children present in the household are responsible for the large differences we see in the effects of cigarette taxes at reducing smoking for men in same-sex households compared to the effects for women in same-sex households or for men in different-sex households.

alternatives. A natural question, though, is whether the mismeasurement in our approach could be contributing to the patterns we document above.

Table C11 in the Online Appendix examines results when we focus on samples that are less likely to include two same-sex individuals who are cohabiting platonically. For example, if we restrict attention to 30-64 year old adults, we continue to find strong evidence that cigarette taxes reduced current and daily smoking rates for men in same-sex households. This age restriction should reduce contamination of the sample by younger people who may be cohabiting for non-romantic purposes and older adults who may be living with a same-sex adult child or caregiver. Similarly, if we restrict attention to individuals who report being never married, married, or a member of an unmarried couple, we find that cigarette taxes significantly reduced smoking for men in same-sex households. And if we combine these sample restrictions – that is, examine only individuals age 30-64 who reported being never married, married, or a member of an unmarried couple – we continue to find strong evidence that cigarette taxes significantly reduced smoking among men in same-sex households.[21] Notably, these models do not return evidence that cigarette taxes significantly reduced smoking among women in same-sex households.

We also draw on evidence from our heterogeneity analyses described in Section 6.1 immediately above where we found that our main results for men in same-sex households are driven by those with a bachelor's degree or more and by those in the middle – as opposed to the top or the bottom – part of the household income distribution. These patterns are largely inconsistent with mismeasurement driving our results, since one of the most likely sources of error is from two adult men who are not in a romantic relationship with each other but who cohabit for other reasons such as to share living expenses. In that case we would be particularly concerned if the results for men in same-sex households were driven by less educated individuals or by those with lower household incomes. That we find cigarette taxes reduced smoking mainly for men in same-sex households with high education is strongly suggestive that the miscoded households most likely to impart bias to our estimates are not driving our main results.

7. Conclusion

In this paper, we contribute to a large literature in health economics that has examined whether state excise taxes on cigarettes are significantly related to lower rates of smoking. Prior research has examined these effects for the general adult population, youths, older individuals, pregnant women, and racial and ethnic minorities. Our paper provides the first credible evidence on the effects of cigarette taxes on smoking among sexual minority adults – a group that has been missing from most prior research in health economics – and suggests that taxes were effective at reducing daily smoking among women and men in same-sex households from 1996-2018. Results for men

[21] As a point of comparison, when we impose both of these sample restrictions on data from 2014-2018 for the states that administered the BRFSS sexual orientation and gender identity module, we find that 34.8 percent of women and 42.8 percent of men in same-sex households who meet these additional criteria (30-64 years old and never married, married, or a member of an unmarried couple) do not identify as heterosexual. These shares are substantially higher than the associated shares when we do not impose these restrictions (13.7 percent of women and 24.5 percent of men in same-sex households who do not identify as heterosexual).

are particularly robust and indicate that a one dollar increase in state cigarette taxes was associated with approximately a 1.8 percentage point reduction in the likelihood of daily smoking, controlling for additional smoking policies, other LGBT-related policies, a host of individual characteristics, as well as state and month-by-year fixed effects. Moreover, we find that cigarette tax hikes over this period were differentially more effective at reducing smoking among men in same-sex households as compared to men in different-sex households. This finding suggests that the substantial sexual orientation-related disparity in smoking would have been even larger in the absence of stricter tobacco controls over the period.

When we focus on individuals in same-sex households only in more recent years (2011-2018), we do not find evidence that higher cigarette taxes reduced smoking. This is consistent with the idea that cigarette taxes are no longer an effective health policy tool, and other policies such as those targeting e-cigarettes may become relatively more important. Different underlying channels could explain these results. First, there is some evidence – although not consistent across studies and data sources (Badgett et al., 2021) – that the gay earnings differential is disappearing (Clarke and Sevak, 2013; Carpenter and Eppink, 2017), thus sexual minorities may have become less responsive to prices. Second, recent legislative reforms, such as the legalization of same-sex marriage, could have increased mental health among sexual minorities and reduced the need of smoking as a coping mechanism. However, we similarly do not find any significant impact of cigarette taxes for individuals in different-sex households in more recent years. Therefore, the more likely explanation is that in most cases the individuals who still smoke, both heterosexuals and non-heterosexuals, are hard-core smokers who are not responsive to cigarette tax hikes.

These findings are subject to some notable limitations. First, like the vast majority of prior work in this area, we rely on self-reported smoking outcomes and self-reported information to identify sexual minority status (or proxies for sexual minority status). Second, our measure of respondent sex and household sex composition is a stronger proxy for non-heterosexual identity for men than for women, which may explain why our results for women in same-sex households are smaller and less robust than the results for men in same-sex households.

Finally, we should be clear that while we have found robust evidence that state excise taxes on cigarettes were effective at reducing smoking among individuals in same-sex households, there may be other better ways to achieve the goal of reducing the disparity in smoking between sexual minorities and heterosexual individuals. For example, smoking cessation programs that explicitly target sexual minorities might be particularly effective. Our empirical models controlled for whether a state's Medicaid program covered smoking cessation, but we are not aware of systematic information on cessation programs targeting gay men, lesbian women, bisexual and queer individuals. Other tobacco control policies, including those targeting vaping in relatively recent periods, could also be effective at reducing the burden of tobacco among sexual minorities in the United States.

Despite these limitations, our work represents the first evidence using credible within-state variation in cigarette taxes to understand how smoking among sexual minority populations

responds to stricter tobacco controls. In fact, our paper represents more generally some of the first evidence on how population-targeted health policies such as tobacco control may have differential effects on sexual minority populations compared to heterosexual populations. A large literature in health economics has asked whether policies such as the Affordable Care Act or welfare reform have had measurably different effects on demographically identifiable groups such as racial and ethnic minorities as compared to whites, or women as compared to men (Bitler et al., 2003; Buchmueller et al., 2016). Our paper represents one of the first steps – together with (Carpenter et al., 2021b) – toward making the case that sexual orientation is another demographic characteristic on which we may expect differential effects of public policies. Future work should use these and other data to consider other contexts where economic theory may predict differential effects of public policies on health behaviors and health outcomes by sexual minority status.

References

Aksoy, C.G., Carpenter, C.S., Frank, J., Huffman, M.L., 2019. Gay Glass Ceilings : Sexual Orientation and Workplace Authority in the UK. J. Econ. Behav. Organ. 159, 167–180.

Angrist, J.D., Pischke, J.S., 2014. Mastering 'Metrics. Princeton University Press.

Angrist, J.D., Pischke, J.S., 2009. Mostly Harmless Econometrics: An Empiricist's Companion. Princeton University Press.

Badgett, M.V.L., Carpenter, C.S., Sansone, D., 2021. LGBTQ Economics. J. Econ. Perspect. 35, 141–170.

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How Much Should We Trust Difference-In-Difference Estimates? Q. J. Econ. 119, 249–275.

Bitler, M.P., Gelbach, J.B., Hoynes, H.W., 2003. Some evidence on race, welfare reform, and household income, in: American Economic Review. pp. 293–298.

Black, D.A., Sanders, S.G., Taylor, L.J., 2007. The economics of lesbian and gay families. J. Econ. Perspect. 21, 53–70.

Buchmueller, T., Carpenter, C.S., 2010. Disparities in health insurance coverage, access, and outcomes for individuals in same-sex versus different-sex relationships, 2000-2007. Am. J. Public Health 100, 489–495.

Buchmueller, T., Levinson, Z.M., Levy, H.G., Wolfe, B.L., 2016. Effect of the Affordable Care Act on Racial and Ethnic Disparities in Health Insurance Coverage. Am. J. Public Health 106, 1416–21.

Callison, K., Kaestner, R., 2014. Do Higher Tobacco Taxes Reduce Adult Smoking? New Evidence of the Effect of Recent Cigarette Tax Increases on Adult Smoking. Econ. Inq. 52, 155–172.

Carpenter, C.S., 2009. Sexual orientation and outcomes in college. Econ. Educ. Rev. 28, 693–703.

Carpenter, C.S., 2007. Revisiting the income penalty for behaviorally gay men: Evidence from NHANES III. Labour Econ. 14, 25–34.

Carpenter, C.S., 2004. New evidence on gay and lesbian household incomes. Contemp. Econ. Policy 22, 78–94.

Carpenter, C.S., 2003. Sexual orientation and body weight: Evidence from multiple surveys. Gender Issues 21, 60–74.

Carpenter, C.S., Cook, P.J., 2008. Cigarette taxes and youth smoking: New evidence from national, state, and local Youth Risk Behavior Surveys. J. Health Econ. 27, 287–299.

Carpenter, C.S., Eppink, S.T., 2017. Does It Get Better? Recent Estimates of Sexual Orientation and Earnings in the United States. South. Econ. J. 84, 426–441.

Carpenter, C.S., Eppink, S.T., Gonzales, G., McKay, T., 2021a. Effects of Access To Legal Same-Sex Marriage on Marriage and Health: Evidence from BRFSS. J. Policy Anal. Manag. 40, 376–411.

Carpenter, C.S., Gonzales, G., McKay, T., Sansone, D., 2021b. Effects of the Affordable Care Act Dependent Coverage Mandate on Health Insurance Coverage for Individuals in Same-Sex Couples. Demography Accepted.

CDC, 2018a. Tobacco Product Use Among Adults: United States, 2017. Morb. Mortal. Wkly. Rep. 67, 1–8.

CDC, 2018b. HIV and Gay and Bisexual Men. CDC Fact Sheet September, 1–2.

Chaloupka, F.J., Wechsler, H., 1997. Price, tobacco control policies and smoking among young adults. J. Health Econ. 16, 359–373.

Clarke, G., Sevak, P., 2013. The disappearing gay income penalty. Econ. Lett. 121, 542–545.

Colman, G., Grossman, M., Joyce, T., 2003. The effect of cigarette excise taxes on smoking before, during and after pregnancy. J. Health Econ. 22, 1053–1072.

Cotti, C., Nesson, E., Tefft, N., 2016. The effects of tobacco control policies on tobacco products, tar, and nicotine purchases among adults: Evidence from household panel data. Am. Econ. J. Econ. Policy 8, 103–123.

Coulter, R.W.S., Kenst, K.S., Bowen, D.J., Scout, 2014. Research funded by the National Institutes of Health on the health of lesbian, gay, bisexual, and transgender populations. Am. J. Public Health 104, e105–e112.

DeCicca, P., Kenkel, D., Lovenheim, M.F., 2021. The Economics of Tobacco Regulation: A Comprehensive Review. J. Econ. Lit. Accepted.

DeCicca, P., Kenkel, D., Mathios, A., 2002. Putting out the fires: Will higher taxes reduce the onset of youth smoking? J. Polit. Econ. 110, 144–169.

DeCicca, P., McLeod, L., 2008. Cigarette taxes and older adult smoking: Evidence from recent large tax increases. J. Health Econ. 27, 918–929.

Deputy, N.P., Boehmer, U., 2014. Weight status and sexual orientation: Differences by age and within racial and ethnic subgroups. Am. J. Public Health 104, 103–109.

Dewan, S., 2009. States Look at Tobacco to Balance the Budget. New York Times.

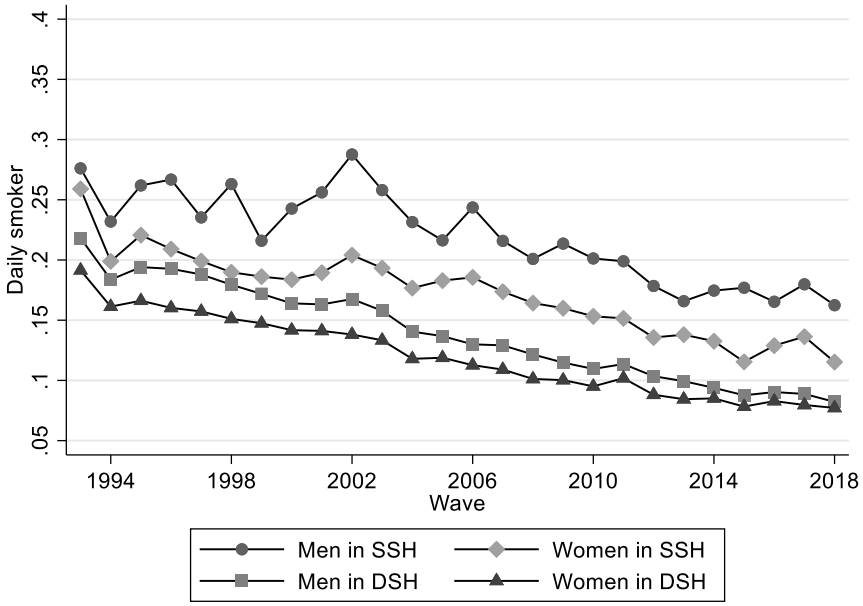
Dilley, J.A., Spigner, C., Boysun, M.J., Dent, C.W., Pizacani, B.A., 2008. Does tobacco industry marketing excessively impact lesbian, gay and bisexual communities? Tob. Control 17, 385–390.

Drydakis, N., 2009. Sexual orientation discrimination in the labour market. Labour Econ. 16, 364–372.

Farrelly, M.C., Bray, J.W., Pechacek, T., Woollery, T., 2001. Response by Adults to Increases in Cigarette Prices by Sociodemographic Characteristics. South. Econ. J. 68, 156.
Franks, P., Jerant, A.F., Leigh, J.P., Lee, D., Chiem, A., Lewis, I., Lee, S., 2007. Cigarette Prices, Smoking, and the Poor: Implications of Recent Trends. Am. J. Public Health 97, 1873–1877.
Friedman, A.S., 2020. Smoking to cope: Addictive behavior as a response to mental distress. J. Health Econ. 72, 102323.
Gallet, C.A., List, J.A., 2003. Cigarette demand: a meta-analysis of elasticities. Health Econ. 12, 821–835.
Ganna, A., Verweij, K.J.H., Nivard, M.G., Maier, R., Wedow, R., Busch, A.S., Abdellaoui, A., Guo, S., Fah Sathirapongsasuti, J., Team, R., Lichtenstein, P., Lundström, S., Långström, N., Auton, A., Harris, K.M., Beecham, G.W., Martin, E.R., Sanders, A.R., Perry, J.R.B., Neale, B.M., Zietsch, B.P., 2019. Large-scale GWAS reveals insights into the genetic architecture of same-sex sexual behavior. Science (80-.). 365.
Geijtenbeek, L., Plug, E., 2018. Is there a penalty for registered women? Is there a premium for registered men? Evidence from a sample of transsexual workers. Eur. Econ. Rev. 109, 334–347.
Goldin, J., Homonoff, T., 2013. Smoke Gets in Your Eyes: Cigarette Tax Salience and Regressivity. Am. Econ. J. Econ. Policy 5, 302–336.
Gonzales, G., Blewett, L.A., 2014. National and state-specific health insurance disparities for adults in same-sex relationships. Am. J. Public Health 104, 95–105.
Gruber, J., Köszegi, B., 2001. Is Addiction "Rational"? Theory and Evidence. Q. J. Econ. 116, 1261–1303.
Hansen, B., Sabia, J.J., Rees, D.I., 2017. Have Cigarette Taxes Lost Their Bite? New Estimates of the Relationship between Cigarette Taxes and Youth Smoking. Am. J. Heal. Econ. 3, 60–75.
Harding, M., Leibtag, E., Lovenheim, M.F., 2012. The Heterogeneous Geographic and Socioeconomic Incidence of Cigarette Taxes: Evidence from Nielsen Homescan Data. Am. Econ. J. Econ. Policy 4, 169–198.
Hatzenbuehler, M.L., Keyes, K.M., Hamilton, A., Hasin, D.S., 2014. State-level tobacco environments and sexual orientation disparities in tobacco use and dependence in the USA. Tob. Control 23, e127–e132.
Hoffman, L., Delahanty, J., Johnson, S.E., Zhao, X., 2018. Sexual and gender minority cigarette smoking disparities: An analysis of 2016 Behavioral Risk Factor Surveillance System data. Prev. Med. (Baltim). 113, 109–115.
Jannat-Khah, D.P., Dill, L.C.J., Reynolds, S.A., Joseph, M.A., 2018. Stress, Socializing, and Other Motivations for Smoking Among the Lesbian, Gay, Bisexual, Transgender, and Queer Community in New York City. Am. J. Heal. Promot. 32, 1178–1186.
Kahn-Lang, A., Lang, K., 2020. The Promise and Pitfalls of Differences-in-Differences: Reflections on '16 and Pregnant' and Other Applications. J. Bus. Econ. Stat. 38, 613–620.
Klawitter, M., 1998. Why Aren't More Economist Doing Research on Sexual Orientation? Fem. Econ. 4, 55–59.
Lien, D.S., Evans, W.N., 2005. Estimating the impact of large cigarette tax hikes: The case of maternal smoking and infant birth weight. J. Hum. Resour. 40, 373–392.
MacLean, J.C., Kessler, A.S., Kenkel, D.S., 2016. Cigarette Taxes and Older Adult Smoking: Evidence from the Health and Retirement Study. Health Econ. 25, 424–438.
Meyer, I.H., 1995. Minority Stress and Mental Health in Gay Men. J. Health Soc. Behav. 36, 38.
Nesson, E., 2017. Heterogeneity in Smokers' Responses to Tobacco Control Policies. Health Econ. 26, 206–225.
Ostrow, D.G., Stall, R., 2008. Alcohol, tobacco, and drug use among gay and bisexual men, in: Wolitski, R.J., Stall, R., Valdiserri, R.O. (Eds.), Unequal Opportunity: Health Disparities Affecting Gay and Bisexual Men in the United States. Oxford University Press, New York, NY, pp. 121–158.
Pesko, M.F., Courtemanche, C.J., Maclean, J.C., 2020. The Effects of Traditional Cigarette and E-Cigarette Taxes on Adult Tobacco Product Use. J. Risk Uncertain. 60, 229–258.
Pierannunzi, C., Town, M., Garvin, W., Shaw, F.E., Balluz, L., 2012. Methodologic Changes in the Behavioral Risk Factor Surveillance System in 2011 and Potential Effects on Prevalence Estimates. Morb. Mortal. Wkly. Rep. 61, 410–413.
Plug, E., Berkhout, P., 2004. Effects of sexual preferences on earnings in the Netherlands. J. Popul. Econ. 17, 117–131.
Remafedi, G., 2007. Lesbian, gay, bisexual, and transgender youths: Who smokes, and why? Nicotine Tob. Res. 9, S65–S71.
Remler, D.K., 2004. Poor Smokers, Poor Quitters, and Cigarette Tax Regressivity. Am. J. Public Health 94, 225–229.
Ringel, J.S., Evans, W.N., 2001. Cigarette Taxes and Smoking During Pregnancy. Am. J. Public Health 91, 1851–1856.
Sansone, D., 2019a. LGBT students: new evidence on demographics and educational outcomes. Econ. Educ. Rev. 73, 101933.
Sansone, D., 2019b. Pink work: Same-sex marriage, employment and discrimination. J. Public Econ. 180, 104086.
Simon, D., 2016. Does Early Life Exposure to Cigarette Smoke Permanently Harm Childhood Welfare? Evidence

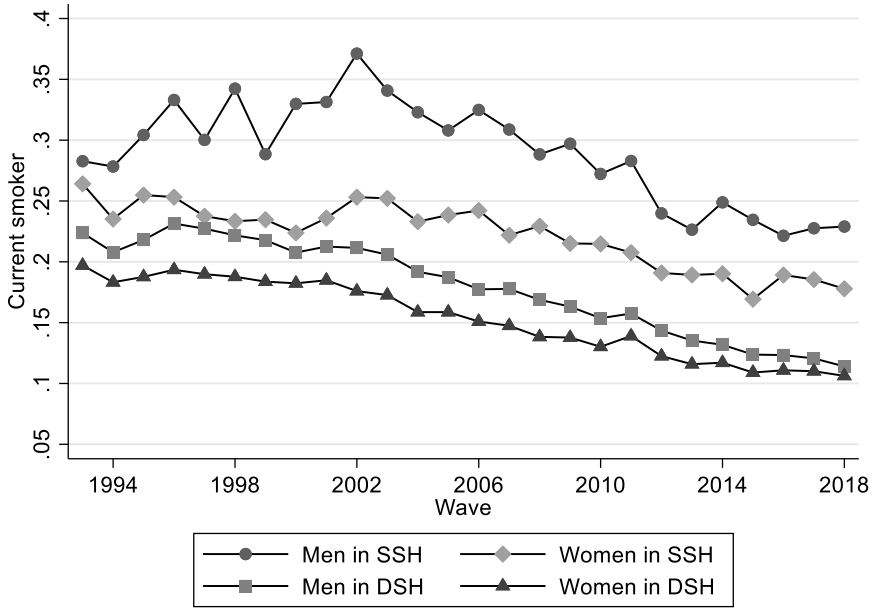
from Cigarette Tax Hikes. Am. Econ. J. Appl. Econ. 8, 128–159.
Simon, K., Soni, A., Cawley, J., 2017. Affordable Care Act. The Impact of Health Insurance on Preventive Care and Health Behaviors: Evidence From the First Two Years of the ACA Medicaid Expansions. J. Policy Anal. Manag. 36, 390–417.
Thomas, S., Fayter, D., Misso, K., Ogilvie, D., Petticrew, M., Sowden, A., Whitehead, M., Worthy, G., 2008. Population tobacco control interventions and their effects on social inequalities in smoking: Systematic review. Tob. Control 17, 230–237.
Tilcsik, A., 2011. Pride and Prejudice: Employment Discrimination against Openly Gay Men in the United States. Am. J. Sociol. 117, 586–626.
Weichselbaumer, D., 2003. Sexual orientation discrimination in hiring. Labour Econ. 10, 629–642.

Figure 1: Rates of daily smoking. By sex and whether in a same-sex household.



Source: BRFSS 1993-2018. Weighted means. Respondents younger than 25 have been excluded. "SSH" indicates same-sex households, while "DSH" indicates different-sex households.

Figure 2: Rates of current smoking. By sex and whether in a same-sex household.



Current smokers include both daily and occasional smokers. Source: BRFSS 1993-2018. Weighted means. Respondents younger than 25 have been excluded. "SSH" indicates same-sex households, while "DSH" indicates different-sex households.

Table 1: Descriptive statistics.

	Women		Men	
	Same-sex households	Different-sex households	Same-sex households	Different-sex households
	(1)	(2)	(3)	(4)
Cigarette tax (in $)	0.973	0.918	0.918	0.899
Current smoker	0.224	0.157	0.306	0.186
Daily smoker	0.172	0.120	0.228	0.142
Below age 40	0.252	0.340	0.400	0.325
White	0.687	0.853	0.759	0.835
Black	0.224	0.070	0.123	0.076
Asian	0.024	0.030	0.038	0.032
Hispanic	0.118	0.091	0.124	0.091
Married	0.061	0.869	0.140	0.881
Member of unmarried couple	0.049	0.029	0.110	0.029
Never married	0.303	0.026	0.405	0.040
High school degree or GED	0.289	0.291	0.272	0.276
Some college	0.282	0.277	0.259	0.245
Bachelor's degree or more	0.282	0.336	0.351	0.377
Total household income below $50,000	0.769	0.493	0.639	0.467
Total household income below $15,000	0.204	0.062	0.124	0.048
N	143,455	1,752,290	57,511	1,337,016

Weighted means. Sample size (N) refers to the total number of respondents in the relevant sub-group. Respondents younger than 25 have been excluded. All variables are described in Section A of the Online Appendix. In line with how variables have been coded in the main regressions (Table 3), missing values for age, race, ethnicity, marital status, and education have been imputed as zeros. Missing values for household income have instead not been considered. Source: BRFSS 1996-2018.

Table 2: Household structure, sexual orientation, and marital status.

		Women		Men	
Sample	Subgroup	Heterosexual	Non-heterosexual	Heterosexual	Non-heterosexual
All landline respondents	All	97.3% 295,254	2.7% 7,066	96.7% 174,150	3.3% 6,190
Different-sex household	All	98.5% 125,360	1.5% 1,558	98.0% 95,747	2.0% 1,098
		Of which lesbian: 0.1% (147 obs)		*Of which gay:* 0.9% (224 obs)	
Same-sex household	All	86.3% 9,772	13.7% 1,508	75.5% 3,020	24.5% 1,294
	Married	41.4% 436	58.6% 633	50.8% 401	49.2% 556
	Member of an unmarried couple	10.4% 73	89.6% 295	32.0% 34	68.0% 327
	Never Married	84.4% 2,412	15.6% 299	70.9% 875	29.1% 299
	Divorced	97.7% 3,144	2.3% 168	92.8% 872	7.2% 79
	Widowed	98.6% 3,253	1.4% 76	99.2% 690	0.8% 13
	Separated	96.2% 388	3.8% 23	96.6% 129	3.4% 8
	Refused	82.3% 66	17.7% 14	70.6% 19	29.4% 12

Weighted means and raw sample sizes. Source: BRFSS 2014-2018. This sample include all relevant respondents (age 25+) from states that administered the SOGI module at least once and released data to the BRFSS public use file. Non-heterosexual includes respondents whose reported sexual orientation was lesbian, gay, bisexual or other. Only respondents from landline (not mobile phones) interviews have been considered.

Table 3: Cigarette taxes reduced smoking among men and women in same-sex households.

	Daily smoker			Current smoker		
	(1)	(2)	(3)	(4)	(5)	(6)
Women in same-sex households						
Cigarette tax	-0.004*	-0.004*	-0.006*	-0.003	-0.002	-0.004
	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)
N	141,517	141,517	141,517	141,517	141,517	141,517
Mean of dependent variable	0.172	0.172	0.172	0.224	0.224	0.224
Adjusted R-squared	0.016	0.080	0.080	0.014	0.087	0.088
Men in same-sex households						
Cigarette tax	-0.014***	-0.012***	-0.018***	-0.017***	-0.015***	-0.020***
	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.005)
N	56,807	56,807	56,807	56,807	56,807	56,807
Mean of dependent variable	0.228	0.228	0.228	0.306	0.306	0.306
Adjusted R-squared	0.023	0.084	0.085	0.023	0.082	0.082
Controls for:						
State and month-by-year FE	X	X	X	X	X	X
Individual controls		X	X		X	X
State time-varying controls			X			X

Sample: individuals in same-sex households (respondent's age 25+). All specifications include state and month-by-year fixed effects, as well as an indicator equal to one if respondent was interviewed after 2010. Individual controls: education, age, race, and ethnicity. State policies and controls: bans on same-sex marriage; same-sex marriage legalization; domestic partnership and civil union laws; LGBT anti-discrimination laws; LGBT hate crime law; sodomy laws; smoking bans in non-hospitality workplaces, restaurants, and bars; tobacco 21 laws; e-cigarette tax indicator; e-cigarette bans for restaurants, bars, and private worksites; Medicaid coverage of cessation treatments; Medicaid pre-expansion; ACA expansion; Medicaid private option; total tobacco-related appropriation and grant funding; population; and unemployment rate. When necessary, missing indicators for control variables not available in all years, states, or for all individuals have been included. Standard errors clustered at the state level in parentheses. Since it was not possible to identify same-sex households among respondents interviewed through mobile phones, only individuals interviewed by landlines are included in these analyses. Weighted regressions and summary statistics. Source: BRFSS 1996-2018. All estimated coefficients for the controls in columns 3 and 6 are reported in Table C2 in the Online Appendix. All variables are described in Section A of the Online Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Cigarette tax effects on daily smoking. Robustness and heterogeneity.

	Baseline Table 3, Column 6	Add linear state time trends	1993-2018	Drop states with high local taxes	1996-2010	2011-2018	Only never married or member of an unmarried couple, 1996-2010
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Women in same-sex households							
Cigarette tax	-0.006*	-0.004	-0.005	-0.009*	-0.008	0.016*	-0.011
	(0.003)	(0.005)	(0.003)	(0.004)	(0.006)	(0.009)	(0.011)
N	141,517	141,517	147,414	128,322	88,988	52,529	29,765
Mean of dependent variable	0.172	0.172	0.178	0.171	0.183	0.135	0.174
Adjusted R-squared	0.080	0.081	0.082	0.085	0.079	0.077	0.074
Men in same-sex households							
Cigarette tax	-0.018***	-0.020**	-0.018***	-0.022**	-0.026***	-0.011	-0.051***
	(0.006)	(0.008)	(0.006)	(0.008)	(0.008)	(0.025)	(0.012)
N	56,807	56,807	59,924	51,183	37,779	19,028	17,926
Mean of dependent variable	0.228	0.228	0.231	0.230	0.238	0.179	0.222
Adjusted R-squared	0.085	0.086	0.086	0.089	0.084	0.093	0.078
Controls for:							
State and month-by-year FE	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X
State time-varying controls	X	X	X	X	X	X	X

Sample: individuals in same-sex households (respondent's age 25+). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Column 2 includes state-specific linear time trends. Column 4 excludes states with the highest local taxes (Alaska, Illinois, New York, Pennsylvania, and Virginia). Column 7 includes only individuals in a same-sex household who were never married or were a member of an unmarried couple. Weighted regressions and summary statistics. Source: BRFSS 1996-2018 (Columns 1-2 and 4), 1993-2018 (Column 3), 1996-2010 (Columns 5 and 7), 2011-2018 (Column 6). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Cigarette taxes were more effective at reducing smoking among men in same-sex households (SSH) than among men in different-sex households (DSH).

	Daily smoker					Current smoker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample is individuals in ➔	SSH (Table 3, Column 3)	DSH	SSH vs. DSH	All individuals	SSH vs. All individuals	SSH vs. DSH	SSH vs. All individuals
Women							
Cigarette tax	-0.006*	-0.006***	--	-0.005***	--	--	--
	(0.003)	(0.001)		(0.001)			
Cigarette tax * In a same-sex household	--	--	0.001	--	-0.00003	0.005	0.004
			(0.003)		(0.00254)	(0.003)	(0.003)
N	141,517	1,732,820	1,874,337	3,776,544	3,776,544	1,874,337	3,776,544
Mean of dependent variable	0.172	0.120	0.125	0.133	0.133	0.163	0.174
Adjusted R-squared	0.080	0.070	0.073	0.067	0.068	0.077	0.073
Men							
Cigarette tax	-0.018***	-0.004***	--	-0.003***	--	--	--
	(0.006)	(0.001)		(0.001)			
Cigarette tax * In a same-sex household	--	--	-0.009**	--	-0.008**	-0.012***	-0.011***
			(0.004)		(0.004)	(0.003)	(0.004)
N	56,807	1,321,561	1,378,368	2,320,809	2,320,809	1,378,368	2,320,809
Mean of dependent variable	0.228	0.142	0.146	0.162	0.162	0.192	0.214
Adjusted R-squared	0.085	0.077	0.080	0.072	0.074	0.088	0.081
Controls for:							
State and month-by-year FE	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X
State time-varying controls	X	X		X			
State-time, state-SSH, and time-SSH FE			X		X	X	X

Sample: individuals (age 25+) in same-sex households (Column 1); individuals in different-sex households (Column 2); individuals in same-sex and different sex households (Columns 3 and 6); all landline respondents, also those not in a 2-adult household (Columns 4-5 and 7). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. The triple-difference models in columns 3 and 5-7 include all year-by-state, month-by-state, month-by-year, year-by-SSH, month-by-SSH, and state-by-SSH double interactions, so the coefficient of cigarette tax is omitted because of perfect collinearity. Weighted regressions and summary statistics. Source: BRFSS 1996-2018. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Cigarette tax effects on other outcomes for men in same-sex households.

		Cigarette tax effect	
	Outcome	Coefficient	Standard error
1	Current smoking (N=56,807)	-0.020***	(0.005)
2	Daily smoking (N=56,807)	-0.018***	(0.006)
3	Employed (N=57,184)	0.012	(0.007)
4	Had a checkup in past year (N=48,358)	-0.008	(0.007)
5	Had a flu shot in past year (N=52,438)	-0.004	(0.007)
6	Any past month alcohol consumption (N=52,622)	0.005	(0.008)
7	Any past month binge drinking (N=51,706)	0.002	(0.006)
8	Ever had an HIV test (N=42,134)	0.001	(0.008)
9	Any exercise in past month (N=54,311)	0.019	(0.013)
10	Body mass index (N=56,219)	6.311	(9.123)
11	Obese (BMI>=30) (N=56,219)	0.008	(0.006)
12	Excellent or very good health (N=57,304)	0.024***	(0.007)
13	Fair or poor health (N=57,304)	-0.013*	(0.007)

Sample: men in same-sex households (respondent's age 25+, 25-64 for the HIV test). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Weighted regressions. Source: BRFSS 1996-2018. N indicates the sample size for the outcome in each row. All variables are described in Section A of the Online Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Cigarette Taxes and Smoking Among Sexual Minority Adults

Christopher S. Carpenter[22]

Dario Sansone[23]

Online Appendix

[22] Vanderbilt University, NBER and IZA. E-mail: christopher.s.carpenter@vanderbilt.edu
[23] University of Exeter and IZA. E-mail: d.sansone@exeter.ac.uk

Appendix A: Variable description

A.1 Dependent variables

Respondents were asked whether they had smoked at least 100 cigarettes in their entire lives. If yes, they were asked whether at the time of the interview they smoked cigarettes every day, some days, or not at all. The second question was different in the first BRFSS waves. In the 1994 and 1995 survey, respondents reported the number of days preceding the interview that they had smoked. In the 1993 survey, respondents were asked whether they were currently smoking, and the number of cigarettes a day they smoked *on average*.

Current smoker is an indicator variable equal to one for respondents who, at the time of the survey, smoked every day or some days; zero for those who did not smoke at the time of the survey, or who had never smoked 100 cigarettes in their entire lives; missing for those who answered "Don't know", who refused to answer, or with missing raw variables (as specified in the previous paragraph).

For the 1994-1995 waves, this variable has been set equal to one for respondents who had smoked in all of the 30 days preceding the interview, between 1 and 29 days, or for those who reported that they were smoking at the time of the survey, but that they had not smoked in the previous 30 days; zero for former smokers or never smokers; missing for respondents who refused to answer the questions, or who did not provide the number of days they had smoked in the previous month, or with missing raw variables.

For the 1993 waves, this variable has been set equal to one for respondents who had smoked at least 100 cigarettes in their entire lives, and were smoking at the time of the survey, zero for those who had not smoked 100 cigarettes in their entire lives, or for those who had smoked at least 100 cigarettes, but were not smoking at the time of the survey; missing for respondents who refused to answer the questions, or with missing raw variables.

Daily smoker is an indicator variable equal to one for respondents who smoked every day at the time of the survey; zero for those who smoked some days, or who did not smoke at the time of the survey, or who had never smoked 100 cigarettes in their entire lives; missing for those who answered "Don't know", who refused to answer, or with missing raw variables.

For the 1994-1995 waves, this variable has been set equal to one for respondents who had smoked in all of the 30 days preceding the interview; zero for those who had smoked between 1 and 29 days of the 30 days preceding the interview, or for those who had reported that they were smoking at the time of the survey, but that they had not smoked in the previous 30 days, for former smokers or never smokers; missing for respondents who refused to answer the questions, or who did not provide the number of days they had smoked in the previous month, or with missing raw variables.

For the 1993 waves, this variable has been set equal to one for respondents who had smoked at least 100 cigarettes in their entire lives, and were smoking at the time of the survey, and smoked at least one cigarette a day on average at the time of the survey; zero for those who had not smoked 100 cigarettes in their entire lives, or for those who had smoked at least 100 cigarettes, but were not smoking at the time of the survey, or for those who were not smoking regularly; missing for respondents who refused to answer the questions, or with missing raw variables.

Quit smoking is an indicator variable equal to one for respondents who had quit smoking for 1 day or longer in the 12 months preceding the interview; zero for those who had not tried to quit smoking; missing for those who were not smoking, who answered "Don't know", who refused to answer, or with missing raw variable.

Employed is an indicator variable equal to one for respondents who were "employed for wages" or "self-employed" at the time of the interview; zero for those who were "out of work for 1 year or more", "out of work for less than 1 year", "homemaker", "student", "retired", or "unable to work"; missing for those who refused to answer, or with missing raw variable.

Had a checkup in past year is an indicator variable equal to one for respondents who had seen a doctor for a routine checkup in the 12 months preceding the interview; zero for those who had not seen a doctor for more than a year or had never seen a doctor for a checkup; missing for those who answered "Don't know", who refused to answer, or with missing raw variable. It is worth noting that in some waves, the survey question included the words "doctor, nurse or other health professional" instead of just "doctor", or "get any kind of care for yourself" instead of "routine checkup".

Had a flu shot in past year is an indicator variable equal to one for respondents who had received a flu shot in the 12 months preceding the interview; zero for those who had not received a flu shot; missing for those who answered "Don't know", who refused to answer, or with missing raw variable. It is worth noting that in the most recent waves the survey questions included also "a seasonal flu vaccine that was sprayed in your nose" in addition to the arm injection.

Any past month alcohol consumption is an indicator variable equal to one for respondents who had reported having had at least one drink in the 30 days preceding the interview; zero for those who did not report any alcohol consumption; missing for those who answered "Don't know", who refused to answer, or with missing raw variable.

Any past month binge drinking is an indicator variable equal to one for respondents who had reported having on average five or more drinks (four or more for women) on one occasion on the days when they drunk in the 30 days preceding the interview; zero for those who did not report any alcohol consumption or who reported lower average alcohol consumption; missing for those who answered "Don't know", who refused to answer, or with missing raw variable.

Ever had an HIV test is an indicator variable equal to one for respondents who were ever tested for HIV, excluding blood donations; zero for those who were never tested; missing for those who answered "Don't know", who refused to answer, or with missing raw variable. It is worth noting that in many waves this question was not asked to respondents older than 64.

Any exercise in past month is an indicator variable equal to one for respondents who reported participating in any "physical activities or exercises such as running, calisthenics, golf, gardening, or walking for exercise" in the 30 days preceding the interview; zero for those who did not report any exercise; missing for those who answered "Don't know", who refused to answer, or with missing raw variable.

Body mass index (BMI) reports the BMI computed from each respondent using their reported weight and height. This variable is missing for those who answered "Don't know", who refused to answer, or with missing raw variable. From this variable, the variable *Obese* has been defined as having a BMI equal to or higher than 30.

Excellent or very good health is an indicator variable equal to one for respondents who reported their general health to be "excellent" or "very good"; zero for those who reported their general health to be "good", "fair", or "poor"; missing for those who answered "Don't know", who refused to answer, or with missing raw variable.

Fair or poor health is an indicator variable equal to one for respondents who reported their general health to be "fair", or "poor"; zero for those who reported their general health to be "excellent", "very good", or "good"; missing for those who answered "Don't know", who refused to answer, or with missing raw variable.

A.2 Sexual orientation and gender identity indicators

SOGI states. From 2014, states could choose to administer a Sexual Orientation and Gender Identity module to their BRFSS survey and release their data to the public use file.

- 19 states included this module in the 2014 BRFSS questionnaire and released their data to the public use file: Delaware, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Minnesota, Montana, Nevada, New York, Ohio, Pennsylvania, Vermont, Virginia, Wisconsin, Wyoming.
- 22 states included this module in the 2015 BRFSS questionnaire and released their data to the public use file: Colorado, Connecticut, Delaware, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa (only to a random subset of its sample), Kansas, Maryland, Massachusetts, Minnesota, Missouri, Nevada, New York, Ohio, Pennsylvania, Texas, Virginia, West Virginia, Wisconsin.
- 25 states included this module in the 2016 BRFSS questionnaire and released their data to the public use file: California, Connecticut, Delaware, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kentucky, Louisiana, Massachusetts, Minnesota, Mississippi, Missouri,

Nevada, New York, Ohio, Pennsylvania, Rhode Island, Texas, Vermont, Virginia, Washington, Wisconsin.

- 27 states included this module in the 2017 BRFSS questionnaire and released their data to the public use file: California, Connecticut, Delaware, Florida, Georgia, Hawaii, Illinois, Indiana, Iowa, Louisiana, Massachusetts, Minnesota, Mississippi, Montana, Nevada, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, Texas, Vermont, Virginia, Washington, Wisconsin.
- 29 states included this module in the 2018 BRFSS questionnaire and released their data to the public use file: Arizona (only to a random subset of its sample), Connecticut, Delaware, Florida, Hawaii, Idaho, Illinois, Kansas, Louisiana, Maryland, Minnesota, Mississippi, Missouri, Montana, Nevada, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, Tennessee, Texas, Vermont, Virginia, Washington, West Virginia, Wisconsin

Male/female are two indicator variables recording the sex of the respondents. In the 2018 BRFSS survey, the wording of the question could be "What is your sex?" or" What was your sex at birth?". Missing for those who answered "Don't know / Not sure", who refused to answer, or with missing raw variable.

Non-hetero is an indicator variable equal to one for respondents who identified as lesbian, gay, bisexual, or something else; zero for those who identified as straight; missing for those who answered "Don't know / Not sure", who refused to answer, or with missing raw variable. This variable has only been asked in the non-random and time-varying subset of states that included the SOGI module in their BRFSS questionnaire from 2014 onwards.

Same-sex household is an indicator variable equal to one for respondents living in a two-adult household with a same-sex household member; zero for those living in a two-adult household with a different-sex household member; missing for those whose reported number of adults in the households does not match the number of men plus number of women in the households, or with missing raw variables. In addition, households including only two adult women or only two adult men and whose respondents identified as transgender individuals have been classified as same-sex households.

A.3 Individual-level controls

Age is a series of indicators variables recording the respondent's age (in five-year age categories) at the time of the interview. An additional indicator variable has been set equal to one for the respondents who answered "Don't know", who refused to answer, or with missing raw variable; zero otherwise.

Race is a series of indicator variables recording the respondent's preferred race: white (Hispanic or not), black or African-American (Hispanic or not), Asian (including Native Hawaiian or other Pacific Islander), American Indian or Alaskan Native, other races or no preferred race or

"Multiracial but preferred race not answered". An additional indicator variable has been set equal to one for the respondents who answered "Don't know / Not sure", who refused to answer, or with missing raw variable; zero otherwise.

Ethnicity is an indicator variable equal to one for respondents who identified as Hispanic, Latino/a, or with Spanish origin; zero if they did not have Hispanic/Spanish origin. An additional indicator variable has been set equal to one for the respondents who answered "Don't know", who refused to answer, or with missing raw variable; zero otherwise.

Education is a series of indicators variables recording the respondent's highest grade or year of school completed: less than a high school degree, high school diploma or GED, some college or technical school, college degree or more. An additional indicator variable has been set equal to one for the respondents who refused to answer, or with missing raw variable; zero otherwise.

A.3 Smoking-related and other health policies

Cigarette tax records the state cigarette tax rate (in $) in each state over time. These data have been obtained from the Campaign for Tobacco-Free Kids and the Center for Disease Control and Prevention.[24]

Smoking bans. The American Nonsmokers' Rights Foundation (ANRF) lists the effective date for all 100% smoke-free state laws.[25] From these data, it is possible to create a series of indicator variables equal to one for all states and time periods covered by a law that prohibits smoking in non-hospitality workplaces, restaurants, or freestanding bars; zero otherwise.

Tobacco 21 is an indicator variable equal to one for all states and time periods in which the state set the minimum legal sale age for tobacco products at 21; zero otherwise. These data have been obtained from the Campaign for Tobacco-Free Kids.[26]

E-cigarette tax indicator is an indicator variable equal to one for all states and time periods in which the state set a tax on e-cigarettes; zero otherwise. These data have been obtained from the CDC STATE system.[27]

E-cigarette ban workplaces is an indicator variable equal to one for all states and time periods in which the state set clean indoor air policies for vaping in private worksites; zero otherwise. These data have been obtained from the CDC STATE system.[28]

---

[24] TFK source: https://www.tobaccofreekids.org/assets/factsheets/0275.pdf. Accessed Oct/1/2019.
CDC source: https://data.cdc.gov/Policy/The-Tax-Burden-on-Tobacco-1970-2018/7nwe-3aj9/data. Accessed Oct/1/2019.

[25] Source: https://no-smoke.org/wp-content/uploads/pdf/EffectivePopulationList.pdf. Accessed Oct/1/2019.

[26] Source: https://www.tobaccofreekids.org/assets/content/what_we_do/state_local_issues/sales_21/states_localities_MLSA_21.pdf. Accessed Oct/1/2019.

[27] Source: https://www.cdc.gov/STATESystem/. Accessed Apr/12/2021.

[28] Source: https://www.cdc.gov/STATESystem/. Accessed Apr/12/2021.

E-cigarette ban restaurants and bars is an indicator variable equal to one for all states and time periods in which the state set clean indoor air policies for vaping in bar and restaurants; zero otherwise. These data have been obtained from the CDC STATE system.[29] It is worth noting that restrictions for bars and restaurants are coded separately in the CDC database, but such restrictions have been introduced by states at the same time in the time period considered in our empirical analysis, so we have only included one indicator.

Medicaid coverage of cessation treatments is an indicator variable equal to one for all states and time periods in which the state had a comprehensive Medicaid coverage of smoking cessation treatments; zero otherwise. These data have been obtained from the CDC STATE system.[30] This variable has been imputed to zero for all states before December 31, 2008; a missing indicator has thus been set equal to one for all states before December 31, 2008.

State tobacco funding appropriations/grants reports the total appropriation and grant funding for each state and year. These data have been obtained from the CDC STATE system.[31] This variable has been imputed to zero when this information was not available; a missing indicator has thus been set equal to one in those cases.

ACA pre-expansion. The Affordable Care Act (ACA) provided states with the option, effective April 2010, to receive federal Medicaid matching funds to cover low-income adults in order to get an early start on the 2014 Medicaid expansion. This indicator variable is equal to one in all states and time periods covered by an early Medicaid expansion to low-income adults through this new ACA option; zero otherwise. These data have been obtained from the Kaiser Family Foundation.[32]

Medicaid expansion is an indicator variable equal to one in all states and time period covered by a 'regular' ACA Medicaid expansion (i.e., not a pre-expansion); zero otherwise. These data have been obtained from the Kaiser Family Foundation.[33]

Private option is an indicator variable equal to one in all states and time periods in which a state Medicaid program decided to buy private health insurance for its Medicaid population instead of providing coverage directly through the state's Medicaid program (or in which a private option waiver was effective); zero otherwise. These data have been obtained from Families USA.[34]

[29] Source: https://www.cdc.gov/STATESystem/. Accessed Apr/12/2021.
[30] Source: https://www.cdc.gov/STATESystem/. Accessed Apr/12/2021.
[31] Source: https://www.cdc.gov/STATESystem/. Accessed Apr/12/2021.
[32] Source: https://www.kff.org/health-reform/issue-brief/states-getting-a-jump-start-on-health/. Accessed Oct/1/2019.
[33] Source: https://www.kff.org/medicaid/issue-brief/status-of-state-medicaid-expansion-decisions-interactive-map/. Accessed Oct/1/2019.
[34] Source: https://familiesusa.org/1115-waiver-element-private-option. Accessed Oct/1/2019.

A.4 LGBT policy variables

SSM legal is an indicator variable equal to one in all states and time periods when same-sex marriage was legal; zero otherwise. The effective date has been used to code this variable. These data have been primarily obtained from the National Center for Lesbian Rights (NCLR, 2016).

SSM ban is a series of indicator variables equal to one in all states and time periods in which same-sex marriage was banned in the state constitution or state statute; zero otherwise. These indicators remain equal to one even in later years after the legalization of same-sex marriage in a given state. When more than one statutory ban was passed in a state, the oldest one has been used to code the state statute ban variable. These data have been primarily obtained from the Freedom to Marry campaign.[35]

Domestic partnership is an indicator variable equal to one in all states and time periods in which same-sex domestic partnerships were legal; zero otherwise. This indicator remains equal to one even in later years when\if a state had converted same-sex domestic partnerships into marriages. These data have been primarily obtained from the National Center for Lesbian Rights (NCLR, 2016).

Civil union is an indicator variable equal to one in all states and time periods in which same-sex civil unions were legal; zero otherwise. This indicator remains equal to one even in later years when\if a state had converted same-sex civil unions in marriages. These data have been primarily obtained from the National Center for Lesbian Rights (NCLR, 2016).

Anti-discrimination law is an indicator equal to one in all states and time periods in which employer discrimination based on sexual orientation was not allowed; zero otherwise. This variable has been set equal to one even if the law covered only sexual orientation, not gender identity, or if a law protecting trans individuals was passed at a later date. Laws protecting only public employees have not been considered. These data have been primarily obtained from the Freedom for All Americans campaign.[36]

Hate crime is a series of indicator variables equal to one in all states and time periods in which there was a law specifically addressing hate or bias crimes based on sexual orientation only, or on sexual orientation and gender identity; zero otherwise. Since some states passed these laws after 2009, these variables have not been set equal to one for all states after President Obama signed the Matthew Shepard and James Byrd, Jr. Hate Crimes Prevention Act into law on October 28, 2009. These data have been primarily obtained from the Human Rights Campaign. [37]

Sodomy law repeal is an indicator variable equal to one in all states and time periods in which sodomy laws regarding same-sex sexual activities (both oral and anal sex) had been repealed or decriminalized; zero otherwise. This variable has been set equal to one even in cases when a state

[35] Source: http://www.freedomtomarry.org/pages/winning-in-the-states. Accessed Oct/1/2019.
[36] Source: https://www.freedomforallamericans.org/states/.Accessed: Oct/21/2019.
[37] Source: https://www.hrc.org/state-maps/hate-crimes. Accessed Oct/25/2019.

or federal Supreme Court had found sodomy laws unconstitutional, although sodomy laws were still included in the state statute, since they were inapplicable. The effective date has been used to code this variable. These data have been primarily obtained from the Gay and Lesbian Archives of the Pacific Northwest.[38]

A.5 Additional state-level controls

The following variables have been derived from data downloaded from the Bureau of Labor Statistics.[39]

Population records the estimates (in log) of the civilian noninstitutional population ages 16 and older computed by the Census Bureau.

Unemployment rate records the state-month unemployment rates for the civilian noninstitutional population ages 16 and older, not seasonally adjusted.

Additional references

NCLR, 2016. Legal Recognition of LGBT Families. Natl. Cent. Lesbian Rights September.

[38] Source: https://www.glapn.org/sodomylaws/usa/usa.htm. Accessed Oct/1/2019.
[39] Source: https://www.bls.gov/lau/rdscnp16.htm. Accessed Oct/1/2019.

Appendix B: Additional analyses of data from states that administered the sexual orientation and gender identity module from 2014-2018

In this Appendix we describe additional analyses using individual-level self-reports of a non-heterosexual identity from the 2014-2018 BRFSS sample of individuals in states that administered the sexual orientation and gender identity (SOGI) module and released this microdata to the public use file. The number of states in the sample varies across years: 35 states participated at some point between 2014 to 2018. We present descriptive statistics on this pooled sample in Online Appendix Table C12 (the format of which follows Table 1). Column 1 presents means for heterosexual women, column 2 presents means for non-heterosexual women, column 3 presents means for heterosexual men, and column 4 presents means for non-heterosexual men. Notably, the patterns by self-reported sexual orientation in Online Appendix Table C11 are qualitatively very similar to those in Table 1 using the measure of same-sex households that allow us to go back much further in time (1996 compared to 2014). In particular, we continue to find that self-identified sexual minorities have higher current and daily smoking rates than heterosexual adults.

In Table C13 we present the difference-in-differences estimates of the effects of cigarette taxes on current and daily smoking probabilities for self-reported non-heterosexual and heterosexual adults for the 2014-2018 period. To match the main analysis, we restrict attention to individuals who were interviewed via landlines. We present results for women in the top panel and results for men in the bottom panel, and we present results for the daily smoker outcome in columns 1-2 and for the current smoker outcome in columns 3-4. Similarly to our main analysis in Table 3, each entry in Table C13 is from a separate regression with all the individual and state controls, plus state and time fixed effects.

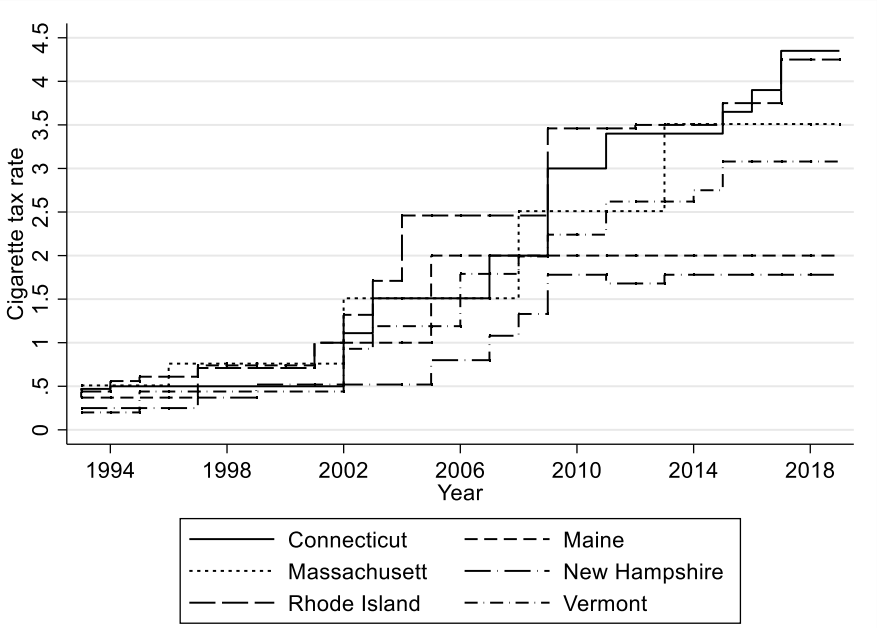
The results in Appendix Table C13 indicate that state cigarette excise taxes did not significantly affect smoking behaviors of self-identified non heterosexual individuals. Estimates for women in same-sex households indicate an inverse relationship with smoking, though they are not statistically significant. Estimates for men in same-sex households are wrong-signed and not statistically significant. Columns 2 and 4 of Appendix Table C13 show that these patterns are not appreciably different for self-identified heterosexual individuals from 2014 to 2018. Taken together, the results in Appendix Table C13 are consistent with the null findings in column 6 of Table 4 which showed that cigarette taxes were not effective at reducing smoking among men or women in same-sex households over the period from 2011 to 2018. This is consistent with prior work suggesting that cigarette taxes have become less effective in more recent periods (Hansen et al. 2017, Callison and Kaestner 2014).

Finally, it is worth noting that we are unable to provide reliable estimates of the effect of cigarette taxes for self-identified transgender individuals due to small sample sizes (there are only 4,075 self-identified transgender individuals in the BRFSS public use file from 2014 to 2018, compared to 35,756 self-identified lesbian women, gay men, bisexual and queer individuals).

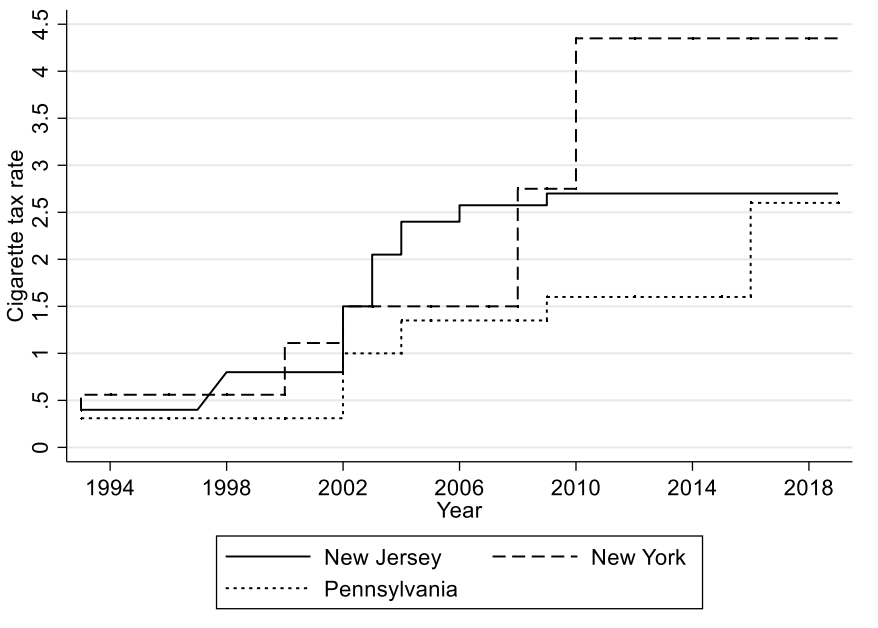
Appendix C: Additional figures and tables.

Figure C1: State cigarette taxes (in $) by state over time.

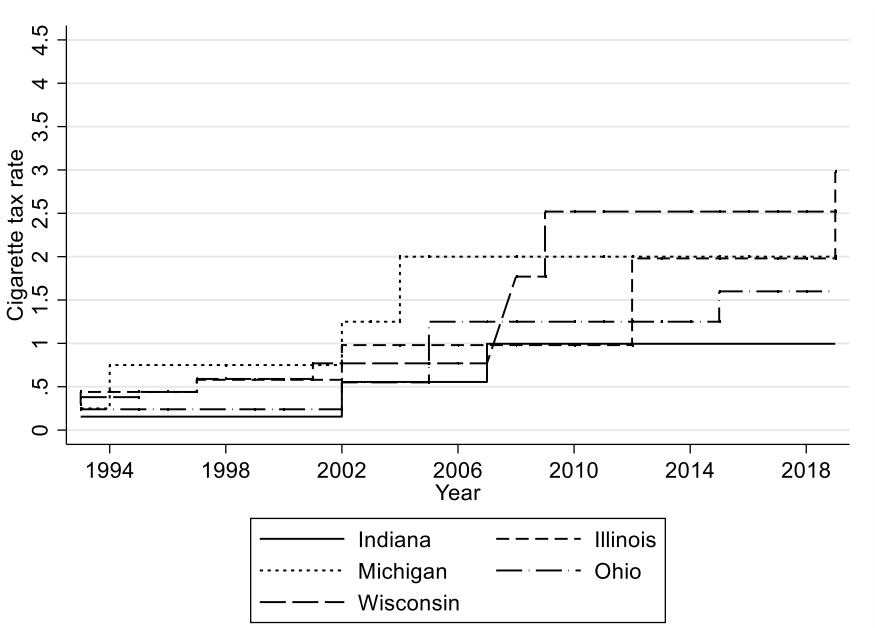
Panel A: New England.



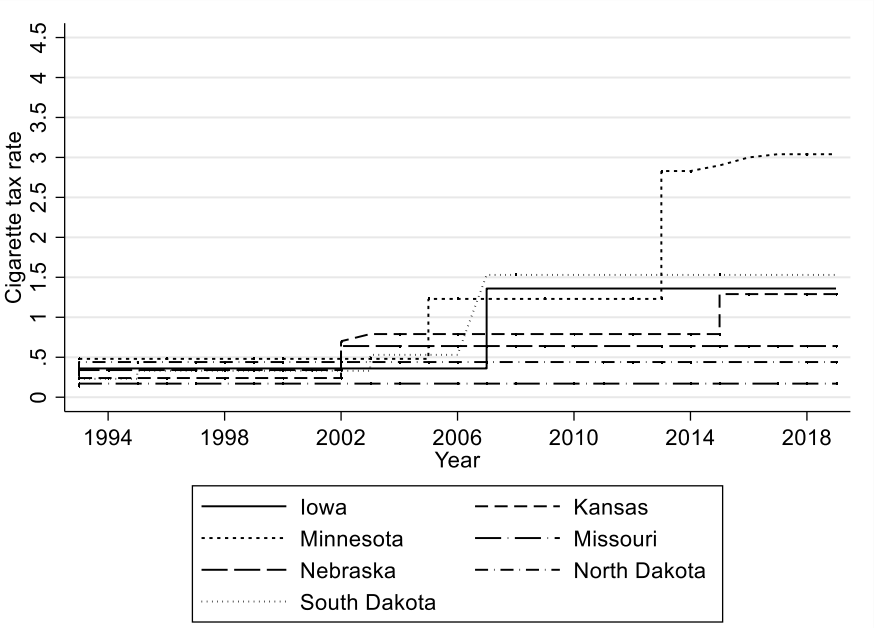
Panel B: Middle Atlantic.



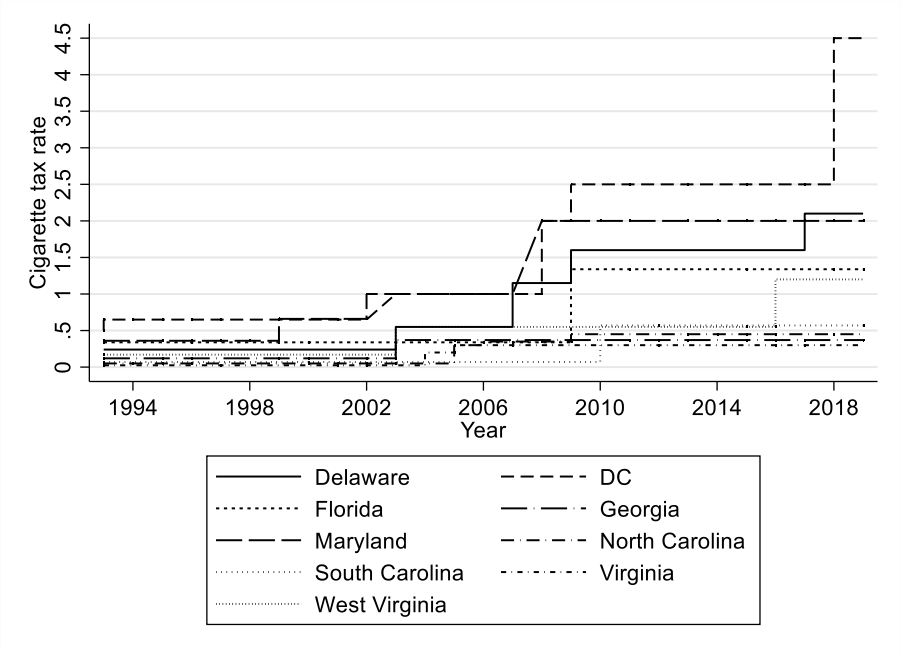
Panel C: East North Central.



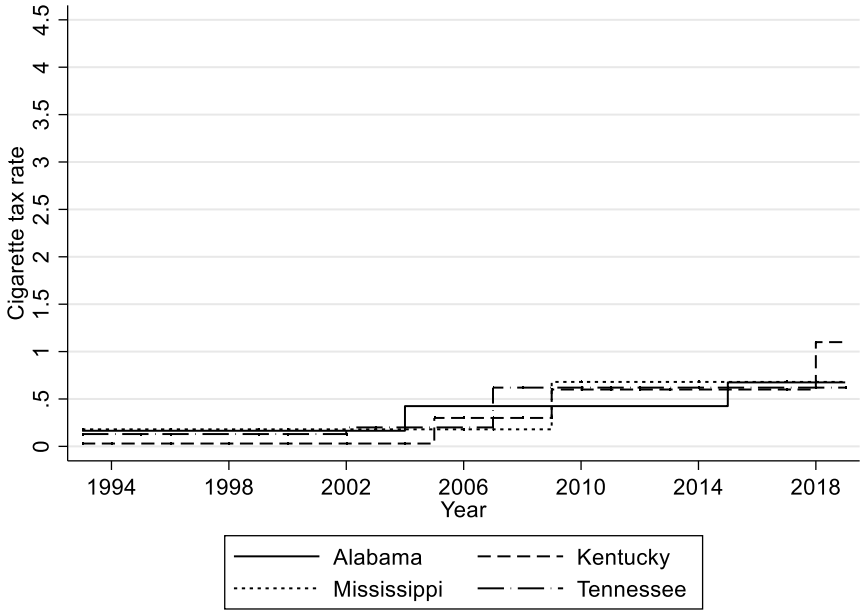
Panel D: West North Central.



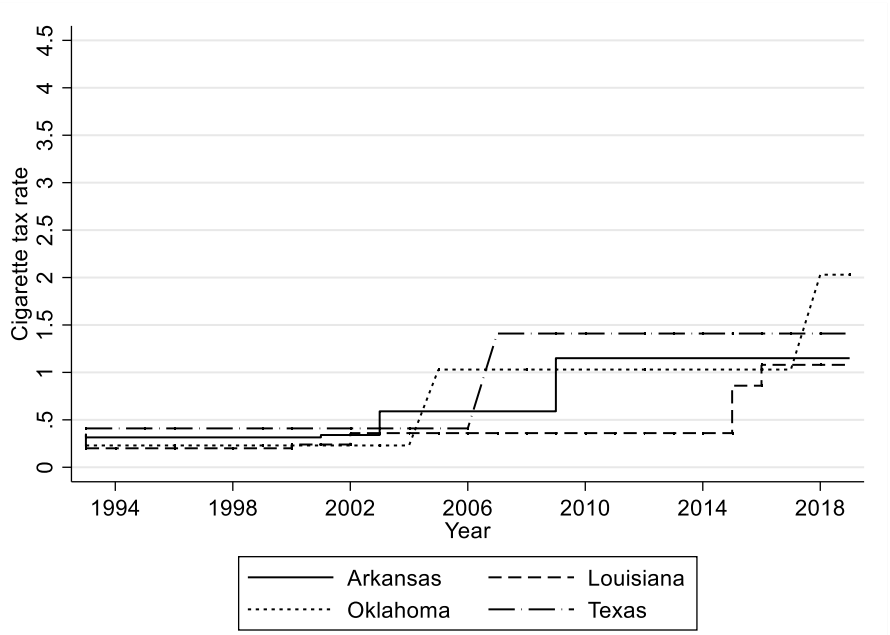
Panel E: South Atlantic.



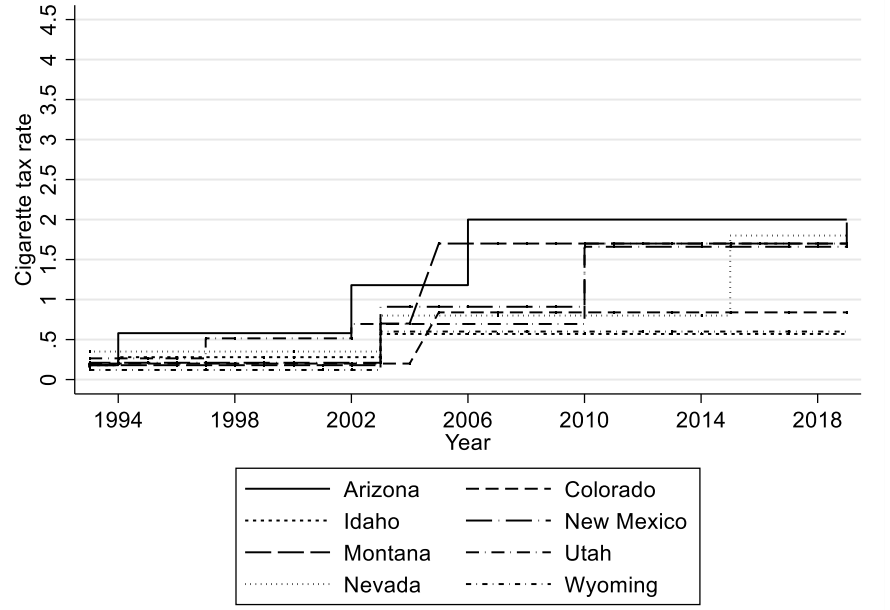
Panel F: East South Central.



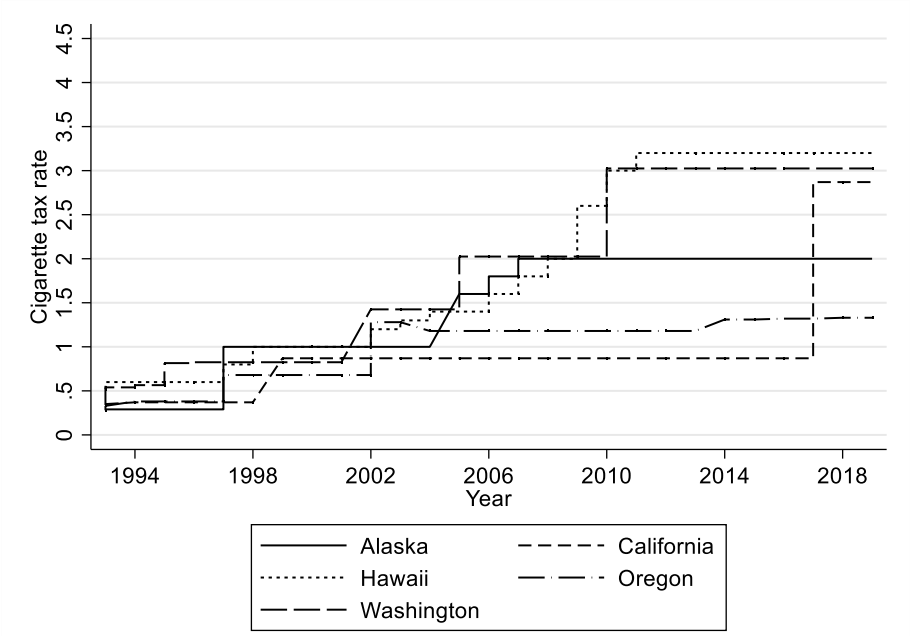
Panel G: West South Central.



Panel H: Mountain.

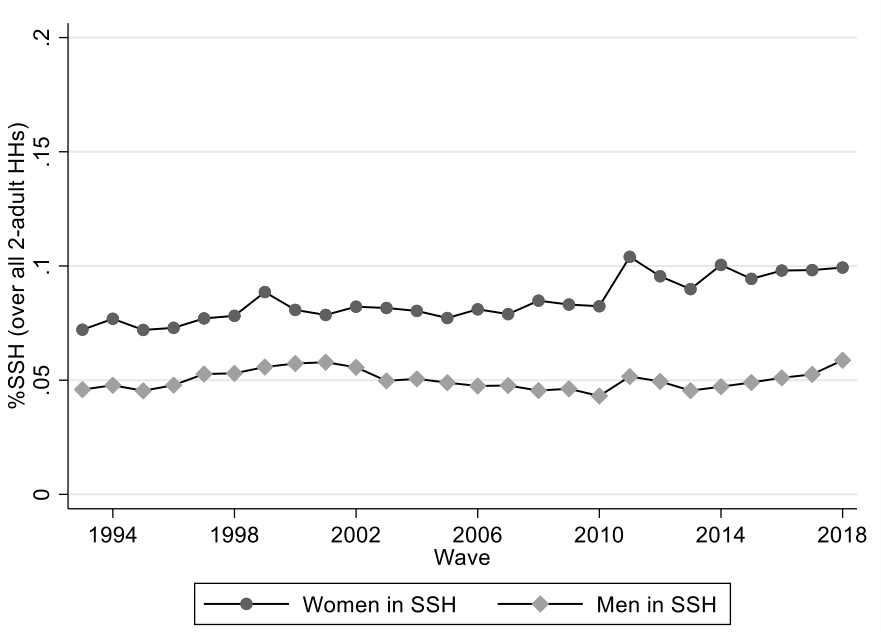


Panel I: Pacific.



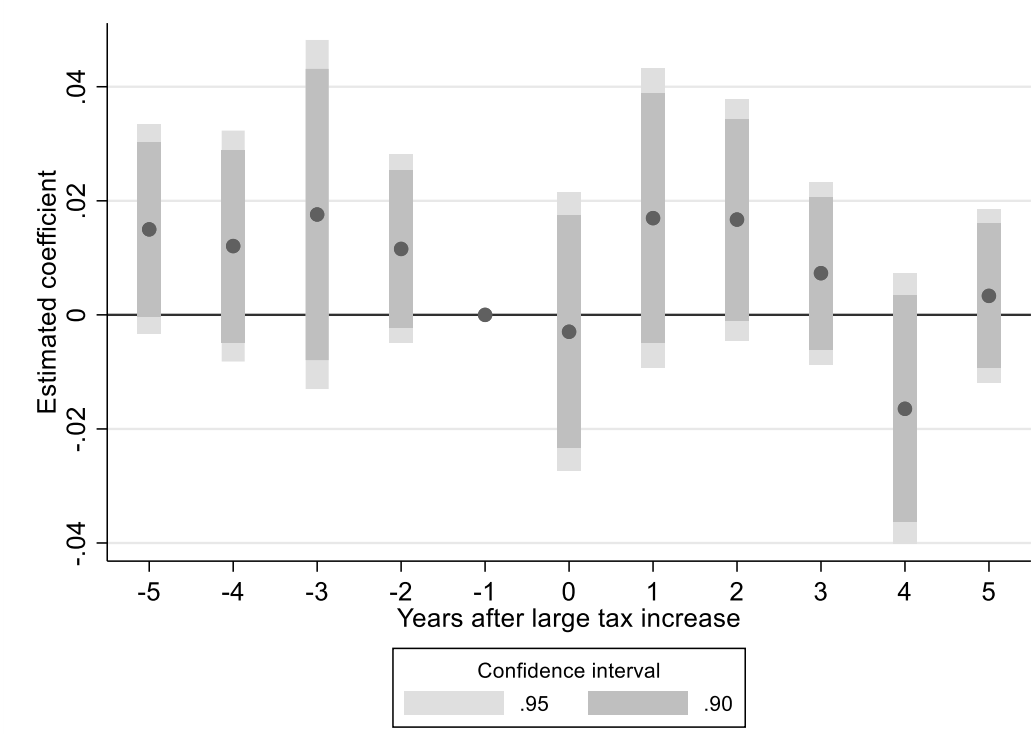
Source: Campaign for Tobacco-Free Kids and the Center for Disease Control and Prevention. See also Section A in the Online Appendix.

Figure C2: Percentage of respondent in same-sex households.



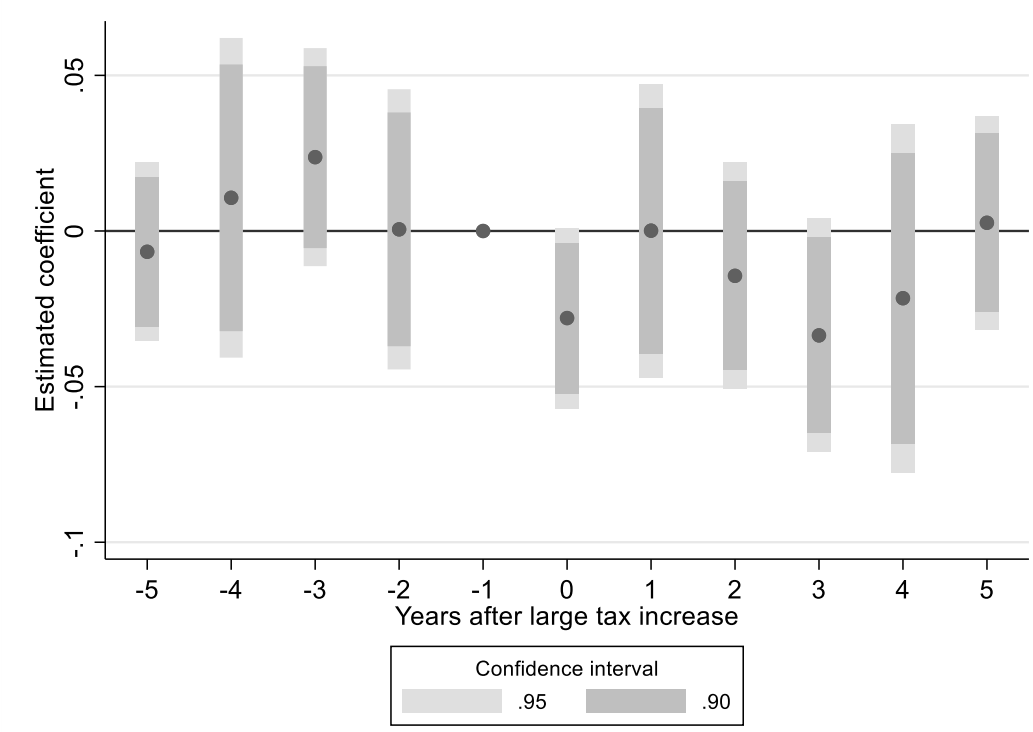
Source: BRFSS 1993-2018. Weighted statistics. Individuals with missing sex, or with inconsistent number of adults in the households (number of men plus number of women does not equal total number of adults) have not been considered. Respondents younger than 25 have been excluded. "SSH" indicates same-sex households, while "DSH" indicates different-sex households.

Figure C3: Event study for daily smoking. Women in same-sex households.



Sample: women in same-sex households (respondent's age 25+). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. A large tax increase is defined as an increase in cigarette tax equal or higher than 50 cents in a certain calendar year in a given state. If a state had implemented more than one large tax change in the time period considered, only the first large tax increase is considered. First lag (1 in event time) equal to one if a state had implemented a large tax increase in the previous year, second lag (2 in event time) equal to one if a state had implemented a large tax increase two years ago, first lead (-1 in event time) equal to one if a state was going to implement a large tax increase after one year, second lead (-2 in event time) equal to one if a state was going to implement a large tax increase after two years. Third and fourth leads and lags are similarly defined. Fifth lag (5 in event time) equal to one if a state had implemented a large tax increase five or more years ago, fifth lead (-5 in event time) equal to one if a state was going to implement a large tax increase after 5 or more years. All variables are mutually exclusive. States without large tax increases are always coded as zero. First lead normalized to zero. Source: BRFSS 1996-2018.

Figure C4: Event study for daily smoking. Men in same-sex households.



Sample: men in same-sex households (respondent's age 25+). See notes in Figure C3.

Table C1: Cigarette taxes unrelated to likelihood of being in a same-sex household and to the demographics (race, age, and education) of respondents in same-sex households.

	Same-sex vs. different-sex households	Same-sex households				
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome is ➔	Same-sex household	Race = Black	Age 35-39	Age 40-44	Age 45-49	Educ = HS
Women in same-sex households						
Cigarette tax	-0.0022	0.0058	-0.0025	0.00001	0.0013	0.0013
	(0.0014)	(0.0057)	(0.0022)	(0.00220)	(0.0019)	(0.0053)
N	1,895,745	143,455	143,455	143,455	143,455	143,455
Mean of dependent variable	0.084	0.224	0.082	0.104	0.104	0.289
Adjusted R-squared	0.037	0.102	0.011	0.009	0.007	0.016
Men in same-sex households						
Cigarette tax	-0.0003	-0.0024	0.0027	-0.0042	-0.0069	-0.0093
	(0.0012)	(0.0065)	(0.0045)	(0.0066)	(0.0050)	(0.0061)
N	1,394,527	57,511	57,511	57,511	57,511	57,511
Mean of dependent variable	0.050	0.123	0.105	0.120	0.106	0.272
Adjusted R-squared	0.012	0.072	0.018	0.015	0.014	0.022
Controls for:						
State and month-by-year FE	X	X	X	X	X	X
Individual controls	X					
State time-varying controls	X	X	X	X	X	X

Sample: column 1 includes individuals in same-sex households and individuals in different-sex households (respondent's age 25+), columns 2-6 only include individuals in same-sex households (respondent's age 25+). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3, unless otherwise specified. In contract with the estimates in Table 3, the specifications in Columns 2-6 do not include individual controls since such factors are used as dependent variables here. Weighted regressions and summary statistics. Source: BRFSS 1996-2018. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C2: Effect of cigarette taxes on smoking. Full specification.

	Women in SSH		Men in SSH	
	Daily smoker	Current smoker	Daily smoker	Current smoker
	(1)	(2)	(3)	(4)
Cigarette Tax	-0.006*	-0.004	-0.018***	-0.020***
	(0.003)	(0.004)	(0.006)	(0.005)
Individual controls:				
Education = Less than high school	0.033***	0.040**	0.066***	0.067***
	(0.012)	(0.016)	(0.011)	(0.018)
Education = Some college	-0.035***	-0.037***	-0.055***	-0.038***
	(0.005)	(0.004)	(0.009)	(0.009)
Education = College graduate or more	-0.155***	-0.176***	-0.180***	-0.180***
	(0.008)	(0.008)	(0.008)	(0.009)
Age Group: 30 to 34	-0.0004	-0.002	0.027**	0.003
	(0.0072)	(0.008)	(0.011)	(0.011)
Age Group: 35 to 39	0.020**	0.003	0.037***	-0.003
	(0.009)	(0.011)	(0.010)	(0.010)
Age Group: 40 to 44	0.005	-0.008	0.020*	-0.026*
	(0.007)	(0.008)	(0.011)	(0.013)
Age Group: 45 to 49	0.0003	-0.014	0.047***	0.001
	(0.0088)	(0.009)	(0.010)	(0.010)
Age Group: 50 to 54	-0.027***	-0.049***	0.028***	-0.032***
	(0.009)	(0.008)	(0.008)	(0.009)
Age Group: 55 to 59	-0.047***	-0.073***	-0.020	-0.074***
	(0.008)	(0.008)	(0.013)	(0.013)
Age Group: 60 to 64	-0.087***	-0.116***	-0.035***	-0.097***
	(0.008)	(0.009)	(0.013)	(0.018)
Age Group: 65 to 69	-0.103***	-0.143***	-0.086***	-0.165***
	(0.010)	(0.010)	(0.010)	(0.011)
Age Group: 70 to 74	-0.157***	-0.205***	-0.133***	-0.221***
	(0.015)	(0.015)	(0.018)	(0.013)
Age Group: 75 to 79	-0.199***	-0.258***	-0.188***	-0.281***
	(0.013)	(0.015)	(0.015)	(0.015)
Age Group: 80 or more	-0.234***	-0.306***	-0.219***	-0.318***
	(0.015)	(0.017)	(0.017)	(0.016)
Hispanic	-0.122***	-0.135***	-0.127***	-0.096***
	(0.016)	(0.020)	(0.010)	(0.012)
Race = Black (Hispanic or not)	-0.095***	-0.095***	-0.078***	-0.057***
	(0.013)	(0.015)	(0.013)	(0.012)
Race = Asian or Pacific Islander	-0.092***	-0.117***	-0.048***	-0.043***
	(0.008)	(0.011)	(0.016)	(0.011)
Race = American Indian or Alaskan Native	-0.029	0.003	0.033	0.078***
	(0.018)	(0.023)	(0.030)	(0.023)
Race = Other	-0.030**	-0.025*	-0.038	-0.043*
	(0.011)	(0.013)	(0.025)	(0.023)
LGBT policies:				
Same-sex marriage legal	0.008	0.008	-0.003	0.003
	(0.006)	(0.009)	(0.013)	(0.015)
Civil unions legal	0.010	0.007	-0.001	0.008
	(0.006)	(0.007)	(0.016)	(0.017)
Domestic partnerships legal	-0.006	-0.010	0.026	0.021
	(0.008)	(0.013)	(0.016)	(0.015)

(continues in the next page)

Constitutional ban on same-sex marriage	0.001	0.004	-0.002	0.008
	(0.007)	(0.008)	(0.011)	(0.015)
Statutory ban on same-sex marriage	-0.014*	-0.017	0.025*	0.012
	(0.008)	(0.010)	(0.013)	(0.014)
LGBT non-discrimination law	-0.009	-0.006	0.002	0.003
	(0.006)	(0.007)	(0.013)	(0.012)
Sodomy law repealed	0.014	0.020**	-0.002	-0.002
	(0.008)	(0.010)	(0.015)	(0.016)
LGB hate crime law	0.006	-0.0002	-0.018*	-0.015**
	(0.006)	(0.0071)	(0.009)	(0.007)
LGBT hate crime law	-0.011*	-0.007	0.027*	0.032***
	(0.006)	(0.009)	(0.014)	(0.010)
State policies:				
Smoke ban workplaces	-0.016**	-0.011	-0.005	-0.003
	(0.007)	(0.008)	(0.014)	(0.012)
Smoke ban restaurants	-0.006	-0.006	-0.005	0.026
	(0.010)	(0.012)	(0.017)	(0.020)
Smoke ban bars	0.023**	0.020*	-0.002	-0.044**
	(0.010)	(0.011)	(0.014)	(0.018)
Tobacco 21	0.039***	0.039***	0.053	-0.018
	(0.014)	(0.010)	(0.043)	(0.055)
E-cigarette tax indicator	0.0004	0.015**	0.017	0.016
	(0.0081)	(0.007)	(0.029)	(0.025)
E-cigarette ban workplaces	0.017	0.044***	0.029	0.024
	(0.019)	(0.014)	(0.026)	(0.027)
E-cigarette ban restaurants and bars	-0.028*	-0.043***	-0.037	-0.009
	(0.015)	(0.012)	(0.033)	(0.032)
Medicaid coverage of cessation treatments	-0.001	-0.004	0.027	0.017
	(0.007)	(0.007)	(0.018)	(0.021)
ACA pre-expansion	0.005	-0.009*	-0.032	-0.028
	(0.006)	(0.005)	(0.027)	(0.023)
ACA expansion	0.00006	-0.006	0.039**	0.028
	(0.00837)	(0.010)	(0.016)	(0.017)
Medicaid private option	-0.050***	-0.056***	-0.023	-0.014
	(0.009)	(0.011)	(0.016)	(0.034)
State tobacco funding appropriations/grants	-0.001	0.001	-0.00005	0.00032
	(0.001)	(0.001)	(0.00204)	(0.00246)
Additional controls:				
Log(population 16+)	-0.143**	-0.175***	-0.179*	-0.271**
	(0.060)	(0.053)	(0.100)	(0.107)
Unemployment rate	-0.002	-0.002	0.002	-0.005
	(0.002)	(0.002)	(0.004)	(0.004)
2011-2018 wave	0.025	0.006	0.141**	0.025
	(0.054)	(0.056)	(0.060)	(0.130)
Constant	2.411**	2.983***	3.537**	4.863***
	(0.914)	(0.802)	(1.479)	(1.597)
Month-by-year FE	X	X	X	X
State FE	X	X	X	X
Observations	141,517	141,517	56,807	56,807
Adjusted R-squared	0.080	0.088	0.085	0.082

This table replicates the results in Table 3 (Columns 3 and 6) by showing the coefficients associated with the individuals and state controls. Comparison groups: high school graduates (education), age group 25-29, white (Hispanic or not). See also notes in Table 3. Missing indicators, as well as state and month-by-year fixed effects not reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C3: Cigarette tax effects on daily smoking. Additional specifications.

	Baseline Table 3, Column 6	Add quadratic state time trends	Add income pc	Add individual controls: employment, health insurance, household income, presence of children	No weights	Logit	Probit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Women in same-sex households							
Cigarette tax	-0.006*	-0.004	-0.006*	-0.006**	-0.006**	-0.006	-0.005
	(0.003)	(0.008)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)
N	141,517	141,517	141,379	141,517	141,517	141,517	141,517
Mean of dependent variable	0.172	0.172	0.172	0.172	0.165	0.172	0.172
Adjusted R-squared	0.080	0.081	0.080	0.087	0.076		
Pseudo R-squared						0.101	0.099
Men in same-sex households							
Cigarette tax	-0.018***	-0.027***	-0.020***	-0.017***	-0.008*	-0.019***	-0.020***
	(0.006)	(0.008)	(0.006)	(0.006)	(0.004)	(0.006)	(0.006)
N	56,807	56,807	56,755	56,807	56,807	56,801	56,801
Mean of dependent variable	0.228	0.228	0.228	0.228	0.208	0.228	0.228
Adjusted R-squared	0.085	0.086	0.085	0.097	0.072		
Pseudo R-squared						0.092	0.092
Controls for:							
State and month-by-year FE	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X
State time-varying controls	X	X	X	X	X	X	X

Sample: individuals in same-sex households (respondent's age 25+). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Column 2 includes state-specific linear and quadratic time trends. Column 3 includes per capita personal income by state-year among the set of state controls (1993-2018, not available for individuals interviewed in 2019). Column 4 includes additional (potentially endogenous) individual controls: employment status, health insurance status, household income, presence of children in the household. Column 5 does not include sampling weights. Columns 6-7 estimate non-linear models instead of a linear probability model, reported estimated marginal effect (computed as average of individual marginal effects). Weighted regressions and summary statistics (except Column 5). All variables are described in Section A of the Online Appendix. Source: BRFSS 1996-2018. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C4: Cigarette tax effects. Additional extensions and robustness checks.

	Baseline Table 3, Column 6	Adjust for inflation	Test for nonlinearities	Alternative outcome variable
	(1)	(2)	(3)	(4)
Outcome is ➔	Daily smoking	Daily smoking	Daily smoking	Tried to quit smoking
Women in same-sex households				
Cigarette tax	-0.006*	-0.009*	-0.003	0.047***
	(0.003)	(0.006)	(0.010)	(0.009)
Cigarette tax^2	--	--	-0.001	--
			(0.002)	
N	141,517	141,517	141,517	30,146
Mean of dependent variable	0.172	0.172	0.172	0.555
Adjusted R-squared	0.080	0.080	0.080	0.047
Men in same-sex households				
Cigarette tax	-0.018***	-0.029***	-0.034**	-0.0004
	(0.006)	(0.009)	(0.015)	(0.0149)
Cigarette tax^2	--	--	0.004	--
			(0.003)	
N	56,807	56,807	56,807	14,985
Mean of dependent variable	0.228	0.228	0.228	0.528
Adjusted R-squared	0.085	0.085	0.085	0.069
Controls for:				
State and month-by-year FE	X	X	X	X
Individual controls	X	X	X	X
State time-varying controls	X	X	X	X

Sample: individuals in same-sex households (respondent's age 25+, 30-64 in Column 3). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Column 2 adjusts cigarette taxes for inflation using the monthly seasonally adjusted consumer price index (all urban consumers, all items). Column 3 includes both a linear and a quadratic function of cigarette taxes. Column 5 analyzes whether cigarette taxes affected the probability that a smoker tried to quit in the previous 12 months (non-smokers excluded). Weighted regressions and summary statistics. Source: BRFSS 1996-2018 (Columns 1-2 and 4), 1996-2010 (Column 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C5: Effect of cigarette taxes on daily smoking. Exclude one state at a time.

Excluded state	Women in same-sex households		Men in same-sex households	
Alabama	-0.006*	(0.003)	-0.018***	(0.006)
Alaska	-0.007*	(0.003)	-0.019***	(0.006)
Arizona	-0.006*	(0.003)	-0.018***	(0.006)
Arkansas	-0.005	(0.003)	-0.020***	(0.006)
California	-0.006*	(0.003)	-0.018***	(0.006)
Colorado	-0.007**	(0.003)	-0.017**	(0.006)
Connecticut	-0.006*	(0.003)	-0.017***	(0.005)
Delaware	-0.006*	(0.003)	-0.019***	(0.006)
DC	-0.006*	(0.003)	-0.018***	(0.006)
Florida	-0.006*	(0.003)	-0.018***	(0.006)
Georgia	-0.007**	(0.004)	-0.018***	(0.006)
Hawaii	-0.006*	(0.003)	-0.018***	(0.006)
Idaho	-0.006*	(0.003)	-0.018***	(0.006)
Illinois	-0.006*	(0.003)	-0.018***	(0.006)
Indiana	-0.008**	(0.003)	-0.021***	(0.005)
Iowa	-0.006*	(0.003)	-0.018***	(0.006)
Kansas	-0.006*	(0.003)	-0.017***	(0.006)
Kentucky	-0.006*	(0.003)	-0.018***	(0.006)
Louisiana	-0.007**	(0.003)	-0.018***	(0.006)
Maine	-0.006*	(0.003)	-0.018***	(0.006)
Maryland	-0.006*	(0.003)	-0.017***	(0.006)
Massachusetts	-0.006	(0.003)	-0.018***	(0.006)
Michigan	-0.006	(0.003)	-0.017***	(0.006)
Minnesota	-0.006*	(0.003)	-0.020***	(0.006)
Mississippi	-0.006*	(0.003)	-0.016***	(0.005)
Missouri	-0.006*	(0.003)	-0.018***	(0.006)
Montana	-0.007**	(0.003)	-0.017***	(0.006)
Nebraska	-0.006*	(0.003)	-0.018***	(0.006)
Nevada	-0.006*	(0.003)	-0.018***	(0.006)
New Hampshire	-0.006*	(0.003)	-0.018***	(0.006)
New Jersey	-0.006*	(0.003)	-0.018***	(0.006)
New Mexico	-0.007*	(0.003)	-0.019***	(0.006)
New York	-0.006*	(0.003)	-0.018***	(0.006)
North Carolina	-0.006	(0.005)	-0.020**	(0.008)
North Dakota	-0.005	(0.003)	-0.019***	(0.006)
Ohio	-0.006*	(0.003)	-0.018***	(0.006)
Oklahoma	-0.006*	(0.003)	-0.017***	(0.006)
Oregon	-0.006*	(0.003)	-0.018***	(0.006)
Pennsylvania	-0.006*	(0.003)	-0.018***	(0.006)
Rhode Island	-0.006*	(0.003)	-0.017***	(0.006)
South Carolina	-0.006*	(0.003)	-0.018***	(0.006)
South Dakota	-0.007**	(0.003)	-0.018***	(0.006)
Tennessee	-0.006*	(0.003)	-0.018***	(0.006)
Texas	-0.006	(0.003)	-0.017***	(0.005)
Utah	-0.005	(0.003)	-0.014***	(0.005)
Vermont	-0.006*	(0.003)	-0.018***	(0.006)
Virginia	-0.006*	(0.003)	-0.018***	(0.006)
Washington	-0.006*	(0.003)	-0.017***	(0.005)
West Virginia	-0.006*	(0.003)	-0.018***	(0.006)
Wisconsin	-0.006*	(0.003)	-0.018***	(0.006)
Wyoming	-0.006*	(0.003)	-0.018***	(0.006)

Reported coefficient of cigarette tax. Same structure as Column 3 Table 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C6: Cigarette tax effects on other outcomes for women in same-sex households.

	Outcome	Cigarette tax effect	
		Coefficient	Standard error
1	Current smoking (N=141,517)	-0.004	(0.004)
2	Daily smoking (N=141,517)	-0.006*	(0.003)
3	Employed (N=142,618)	0.004	(0.004)
4	Had a checkup in past year (N=123,587)	-0.008	(0.007)
5	Had a flu shot in past year (N=132,688)	0.006	(0.006)
6	Any past month alcohol consumption (N=133,265)	-0.008	(0.005)
7	Any past month binge drinking (N=132,034)	-0.001	(0.002)
8	Ever had an HIV test (N=95,450)	0.015**	(0.007)
9	Any exercise in past month (N=136,725)	0.001	(0.005)
10	Body mass index (N=132,929)	-2.202	(8.991)
11	Obese (BMI>=30) (N=132,929)	0.007	(0.007)
12	Excellent or very good health (N=142,922)	0.011*	(0.006)
13	Fair or poor health (N=142,922)	-0.001	(0.003)

Sample: women in same-sex households (respondent's age 25+, 25-64 for the HIV test). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Weighted regressions. Source: BRFSS 1996-2018. N indicates the sample size for the outcome in each row. All variables are described in Section A of the Online Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C7: Effects of cigarette taxes on improved self-rated health are unique to men in same-sex households and survive triple difference estimation.

	Excellent or very good	Good	Fair or poor	Excellent or very good	Excellent or very good	Excellent or very good	Excellent or very good
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample is men in ➔	SSH	SSH	SSH	DSH	All individuals	SSH vs. DSH	SSH vs. All individuals
Cigarette Tax	0.024***	-0.011	-0.013*	0.003	0.001	--	--
	(0.007)	(0.008)	(0.007)	(0.003)	(0.003)		
Cigarette tax * In a same-sex household	--	--	--	--	--	0.017**	0.020***
						(0.007)	(0.007)
N	57,304	57,304	57,304	1,332,792	2,341,288	1,390,096	2,341,288
Mean of dependent variable	0.527	0.295	0.179	0.563	0.533	0.561	0.533
Adjusted R-squared	0.126	0.030	0.112	0.114	0.114	0.116	0.116
Controls for:							
State and month-by-year FE	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X
State time-varying controls	X	X	X	X	X		
State-time, state-SSH, and time-SSH FE						X	X

Sample: men (age 25+) in same-sex households (Columns 1-3); men in different-sex households (Column 4); men in same-sex and different sex households (Columns 6); all landline male respondents, also those not in a two-adult household (Columns 5 and 7). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. The triple-difference models in columns 6-7 include all year-by-state, month-by-state, month-by-year, year-by-SSH, month-by-SSH, and state-by-SSH double interactions, so the coefficient of cigarette tax is omitted because of perfect collinearity. Weighted regressions and summary statistics. Source: BRFSS 1996-2018. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C8: Cigarette tax effects on daily smoking among men in same-sex and different-sex households. By income, education, health care coverage, and presence of children.

	HH income below $25,000	HH income $25,000-$50,000	HH income above $50,000	High school or less	Some college	College degree or more	Without health care coverage	With health care coverage	Without children	With children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Men in same-sex households										
Cigarette tax	0.008	-0.043***	-0.011	-0.015	-0.015	-0.021**	-0.013	-0.019***	-0.017***	-0.032
	(0.012)	(0.014)	(0.010)	(0.010)	(0.014)	(0.009)	(0.017)	(0.006)	(0.006)	(0.022)
N	16,333	15,381	19,216	21,813	14,085	20,751	9,701	46,944	49,983	6,716
Mean of dependent variable	0.284	0.250	0.161	0.301	0.251	0.129	0.339	0.199	0.226	0.239
Adjusted R-squared	0.125	0.105	0.076	0.114	0.069	0.046	0.106	0.082	0.086	0.153
Men in different-sex households										
Cigarette tax	-0.010***	-0.006**	-0.002	-0.010***	-0.003	-0.002	-0.013**	-0.003**	-0.001	-0.008**
	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.006)	(0.001)	(0.001)	(0.003)
N	182,390	337,001	669,764	462,894	315,354	540,114	103,821	1,215,353	867,402	450,659
Mean of dependent variable	0.228	0.180	0.096	0.219	0.153	0.058	0.285	0.126	0.131	0.156
Adjusted R-squared	0.110	0.072	0.053	0.076	0.031	0.013	0.097	0.066	0.076	0.091
Controls for:										
State and month-by-year FE	X	X	X	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X	X	X	X
State time-varying controls	X	X	X	X	X	X	X	X	X	X

Sample: individuals in same-sex households (respondent's age 25+). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Weighted regressions and summary statistics. Source: BRFSS 1996-2018. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C9: Cigarette tax effects on daily smoking among men in same-sex and different-sex households. By BMI and drinking status.

	BMI<25	25<=BMI<30	BMI >= 30	Non-drinker	Drinker
	(1)	(2)	(3)	(3)	(5)
Men in same-sex households					
Cigarette tax	-0.035**	-0.018**	-0.008	-0.009	-0.030***
	(0.014)	(0.009)	(0.008)	(0.010)	(0.008)
N	19,009	22,813	13,874	21,388	30,989
Mean of dependent variable	0.275	0.212	0.181	0.207	0.233
Adjusted R-squared	0.140	0.093	0.087	0.097	0.104
Men in different-sex households					
Cigarette tax	-0.003	-0.005**	-0.005**	-0.004	-0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
N	329,821	616,791	357,352	482,611	751,259
Mean of dependent variable	0.184	0.130	0.120	0.133	0.138
Adjusted R-squared	0.127	0.073	0.054	0.070	0.085
Controls for:					
State and month-by-year FE	X	X	X	X	X
Individual controls	X	X	X	X	X
State time-varying controls	X	X	X	X	X

Sample: individuals in same-sex households (respondent's age 25+). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Weighted regressions and summary statistics. Source: BRFSS 1996-2018. All variables are described in Section A of the Online Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C10: Cigarette tax effects on daily smoking among women and men in same-sex households. By BMI and presence of children.

	BMI<25	25<=BMI<30	BMI >= 30	Without children	With children
	(1)	(2)	(3)	(4)	(5)
Women in same-sex households					
Cigarette tax	-0.001	-0.008	-0.008	-0.005	-0.006
	(0.006)	(0.010)	(0.006)	(0.004)	(0.007)
N	45,982	39,595	46,142	100,469	40,745
Mean of dependent variable	0.202	0.171	0.150	0.152	0.213
Adjusted R-squared	0.118	0.090	0.079	0.073	0.104
Men in same-sex households					
Cigarette tax	-0.035**	-0.018**	-0.008	-0.017***	-0.032
	(0.014)	(0.009)	(0.008)	(0.006)	(0.022)
N	19,009	22,813	13,874	49,983	6,716
Mean of dependent variable	0.275	0.212	0.181	0.226	0.239
Adjusted R-squared	0.140	0.093	0.087	0.086	0.153
Controls for:					
State and month-by-year FE	X	X	X	X	X
Individual controls	X	X	X	X	X
State time-varying controls	X	X	X	X	X

Sample: individuals in same-sex households (respondent's age 25+). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Weighted regressions and summary statistics. Source: BRFSS 1996-2018. All variables are described in Section A of the Online Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C11: Cigarette taxes reduced smoking among men in same-sex households even when restricting to 30 to 64-year-old, married, member of unmarried couple, or never married.

	Daily smoker			Current smoker		
	30 to 64-year-old	Married, never married, or member of unmarried couple	30 to 64-year-old married, never married, or member of unmarried couple	30 to 64-year-old	Married, never married, or member of unmarried couple	30 to 64-year-old married, never married, or member of unmarried couple
	(1)	(2)	(3)	(4)	(5)	(6)
Women in same-sex households						
Cigarette tax	-0.003	-0.004	0.005	-0.0002	-0.001	0.007
	(0.005)	(0.006)	(0.008)	(0.0061)	(0.007)	(0.009)
N	92,881	54,659	40,740	92,881	54,659	40,740
Mean of dependent variable	0.201	0.161	0.169	0.260	0.216	0.223
Adjusted R-squared	0.075	0.074	0.069	0.078	0.079	0.074
Men in same-sex households						
Cigarette tax	-0.030***	-0.020***	-0.035***	-0.028***	-0.018**	-0.026***
	(0.007)	(0.006)	(0.008)	(0.007)	(0.008)	(0.009)
N	38,933	34,324	24,452	38,933	34,324	24,452
Mean of dependent variable	0.251	0.200	0.212	0.328	0.284	0.289
Adjusted R-squared	0.083	0.075	0.076	0.074	0.075	0.072
Controls for:						
State and month-by-year FE	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X
State time-varying controls	X	X	X	X	X	X

Sample: individuals in same-sex households (respondent's age 25+ in Columns 2 and 5, 30-64 in Columns 1, 3, 4, 6). Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. Columns 2-3 and 5-6 include only individuals in a same-sex household who were married, never married, or were a member of an unmarried couple. Weighted regressions and summary statistics. Source: BRFSS 1996-2018. All variables are described in Section A of the Online Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C12: Descriptive statistics. SOGI module.

	Women		Men	
	Heterosexual	Non-heterosexual	Heterosexual	Non-heterosexual
	(1)	(2)	(3)	(4)
Cigarette tax (in $)	1.808	1.933	1.813	1.952
Current smoker	0.146	0.285	0.187	0.220
Daily smoker	0.104	0.209	0.130	0.165
Under age 40	0.254	0.491	0.274	0.455
White	0.807	0.710	0.793	0.707
Black	0.093	0.098	0.082	0.134
Asian	0.035	0.053	0.040	0.055
Hispanic	0.122	0.164	0.125	0.167
Married	0.574	0.331	0.621	0.280
Member of an unmarried couple	0.031	0.160	0.040	0.111
Never married	0.114	0.286	0.159	0.477
High school degree or GED	0.260	0.232	0.280	0.226
Some college	0.348	0.331	0.305	0.303
Bachelor's degree or more	0.286	0.291	0.292	0.317
Total household income below $50,000	0.513	0.643	0.440	0.610
Total household income below $15,000	0.097	0.125	0.064	0.153
N	489,663	16,466	365,460	13,969

Weighted means. Sample size (N) refers to the total number of respondents in the relevant sub-group. Source: BRFSS 2014-2018. This sample include all relevant respondents (age 25+) from states that administered the SOGI module at least once and released data to the BRFSS public use file. Non-hetero includes respondents whose reported sexual orientation was lesbian, gay, bisexual, or other. See also notes in Table 1.

Table C13: Cigarette tax effects among self-identified heterosexual and non-heterosexual individuals, 2014-2018 (selected states).

	Daily smoker		Current smoker	
	(1)	(2)	(3)	(4)
Sample is ➔	Non-heterosexual	Heterosexual	Non-heterosexual	Heterosexual
Women				
Cigarette tax	-0.035	0.009	-0.005	-0.006
	(0.036)	(0.005)	(0.050)	(0.006)
N	6,979	292,715	6,979	292,715
Mean of dependent variable	0.272	0.092	0.334	0.127
Adjusted R-squared	0.542	0.065	0.496	0.072
Men				
Cigarette tax	0.051	0.017	0.096	0.020
	(0.043)	(0.011)	(0.076)	(0.013)
N	6,129	172,679	6,129	172,679
Mean of dependent variable	0.136	0.103	0.169	0.145
Adjusted R-squared	0.288	0.072	0.286	0.088
Controls for:				
State and month-by-year FE	X	X	X	X
Individual controls	X	X	X	X
State time-varying controls	X	X	X	X

Sample respondents (age 25+) whose reported sexual orientation is lesbian, gay, bisexual or other has been counted as non-heterosexual, while respondents whose reported sexual orientation is straight has been counted as heterosexual. Columns 1 and 3 include all non-heterosexual individuals in any state and year that released SOGI data to the public-use BRFSS. Columns 2 and 4 include all heterosexual individuals in any state and year that released SOGI data to public-use BRFSS. Same standard errors, state and month-by-year fixed effects, individual controls, state policies and controls as Table 3. As in Table 3, only respondents from landline (not mobile phones) interviews have been considered. Weighted regressions and summary statistics. Source: BRFSS 2014-2018 (selected states). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$