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CIGARETTE TAXES AND SMOKING AMONG SEXUAL MINORITY ADULTS

Christopher Carpenter
Dario Sansone

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ABSTRACT

We provide the first quasi-experimental evidence on the relationship between cigarette taxes and sexual minority adult smoking by studying individuals in same-sex households (a large share of whom are 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 significantly larger than the associated effects for men in different-sex households (the vast majority of whom are heterosexual married/partnered men). This result suggests 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. 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.

Christopher Carpenter
Department of Economics
Vanderbilt University
VU Station B, Box #351819
2301 Vanderbilt Place
Nashville, TN 37235
and NBER
christopher.s.carpenter@vanderbilt.edu

Dario Sansone
Vanderbilt University
dario.sansone@vanderbilt.edu

An online appendix is available at http://www.nber.org/data-appendix/w26692

1. Introduction

A very large literature in health economics demonstrates that higher excise taxes on cigarettes are associated with lower rates of smoking. While the earliest studies demonstrated this finding using across-states 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 (see, for example, Cotti et al., 2016, as well as Pesko et al., 2019). 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 at-risk 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 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 2007-2015. 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 for sexual minority adults. Based on CDC (2018a) 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). 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, 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. Our study is the first to fill this gap.[3]

[3] 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.

There are many reasons why cigarette taxes and related tobacco control policies might be expected to have different effects for sexual minorities compared to heterosexuals. First, as noted above, smoking rates among sexual minorities are much higher than among heterosexual individuals. Public health research has suggested that this 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. 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. It could be, for example, that the average sexual minority smoker is more addicted to smoking than the average heterosexual smoker; if so, this would lead to less responsiveness of sexual minority smokers to taxes than the associated responsiveness of heterosexual individuals. Second, previous 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.

Third, research has shown that, compared to heterosexual individuals, sexual minority individuals have lower rates of health insurance coverage (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 or worse access to information on the benefits of quitting 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.

Fourth, 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), Carpenter et al. (2019), 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.

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 the tax impact is unclear *ex-ante*. If sexual minority smokers are more addicted to smoking than heterosexual smokers, are disproportionately targeted by tobacco marketing, or 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, 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.

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 behaviors. 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., 2018). 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 (excluding those younger than 25) are disproportionately likely to contain sexual minorities in same-sex romantic relationships. Moreover, we can use independent data with more direct measures of sexual orientation available from 2014 to confirm that this is the case. While this method of identifying sexual minorities is indirect and has a few other important limitations (e.g., it cannot be used to identify most 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).

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 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 but still statistically significant when analyzing daily smoking. The results for men in same-sex households are particularly 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 also 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). Consistent with this finding, we show that between 2014 and 2018 – when we directly observe individual level self-reports of sexual orientation for adults in 35 states that administered a unified sexual orientation and gender identity module and released their data for public use – we do not find any meaningful relationship between cigarette taxes and smoking among self-identified gay men, lesbian women, bisexual or queer individuals.[4]

Finally, we show that cigarette taxes are estimated to have reduced smoking among individuals in different-sex households – the vast majority of whom were 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). 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. Data

2.1 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.

[4] We are unable to provide reliable estimates 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).

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 already been used to analyzed the impact of cigarette taxes by, among others, DeCicca and McLeod (2008), and Pesko et al. (2019).

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).

2.2 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. (2018). 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. 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. Another 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.

Two data restrictions are necessary in order to use the aforementioned technique to identify same-sex households. First, the analysis is restricted to individuals aged 25 and older. Because the relationship between sexual orientation and our household structure measure should be weaker for younger adults (who are more likely to be students and/or co-residing with another same-sex adult for reasons other than a romantic relationship), individuals aged 18-24 are excluded from the main sample. Second, 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. 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.

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 in two important ways. First, we use the 2014-2018 BRFSS data to estimate similar models of the effects of cigarette taxes on self-reported smoking rates of self-identified gay men, lesbian women, bisexual and queer individuals. Second, we use these data – which also include information on household sex composition for individuals not in the cellphone sample – to provide direct evidence on whether individuals in same-sex households are indeed more likely to self-identify as non-heterosexual. As demonstrated in Carpenter et al. (2018) 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), the authors find that men in same-sex households were more likely to report having ever been tested for HIV than men in different-sex households.

The same approach to identify same-sex households in the BRFSS has also been used before by Carpenter (2004). The author provides further evidence consistent with the idea that many individuals in same-sex households were indeed individuals in same-sex couples. For instance, he shows that a high percentage of male respondents in same-sex households reported condom use for disease prevention, not 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.[5]

3. Econometric framework

3.1 Difference-in-difference model

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

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

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 and year fixed

[5] 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. Given the structure of this dataset, the author was able to divide 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 the advantage of using same-sex households as the main criteria to identify LGB individuals.

effects (μ_t), time-varying state-level controls (x'_{st}), as well as individual-level controls (x'_{ist}). Individual controls x'_{ist} are: age (in five year age groups), race, ethnicity, and education.[6] As described in Angrist and Pischke (2014), the common trends assumption can be modified by controlling for state-specific time trends (τ_{ts}). 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).

3.2 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 (1996-2019), 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 changes since 1996. Most states increased their taxes more than once. 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 and employment rate. The main specification also includes controls for LGBT policies: same-sex marriage legalization, constitutional or statutory bans on same-sex marriage,

[6] 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.

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 policies: bans on smoking in restaurants, private workplaces or bars, Tobacco 21 laws, Affordable Care Act Medicaid expansions, and Medicaid private options.

3.3 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 is 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 $\left(x'_{igst}\right)$.

4. Results

4.1 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.[7] 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.

[7] 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.

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 have substantially higher smoking rates than men and women in different-sex households. Women in same-sex households are younger, less likely to be white, less likely to be married, and less likely to have a bachelor's degree than women in different-sex households. Men in same-sex households are older, less likely to be white, and less likely to be married than men in different-sex households.

As already mentioned in Section 2.2, 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. (2019). 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 gay men and lesbian women: 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 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.

4.2 Estimation results

4.2.1 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.[8] Each column is from a separate model, and we report the coefficient on the state excise tax on cigarettes, measured in nominal U.S. dollars.[9] 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, month, and 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. In the bottom panel, for men in same-sex households we estimate in column 3 that a one dollar increase in cigarette taxes was associated with a 1.8 percentage point reduction in the likelihood of being a daily smoker. In columns 4-6 of Table 3 we estimate generally similar effects of cigarette taxes at reducing the likelihood of 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.[10]

4.2.2 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. In column 2 we add state-specific linear time trends. 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.[11]

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[8] As reported in Table B1 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 B1 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.

[9] 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., 2019). If anything, the estimated impact of cigarette taxes on daily smoking is even larger (Online Appendix Table B4).

[10] Online Appendix Table B2 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.

[11] Online Appendix Table B3 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 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., 2019) such as employment status, health insurance status, and household income. Estimating models without sample weights returns somewhat smaller estimates of the effect of cigarette taxes on smoking among individuals

In column 3 we report results for the slightly longer period 1993-2018, thus adding 1993-1995 data even if the smoking questions were slightly different. Doing so has no effect on the 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 column 5 we restrict attention to individuals age 30-64 to try to narrow in on the sample for whom the 'individuals in same-sex households' measure is likely to be most strongly related to a sexual orientation minority status. This age restriction further eliminates young people who may be cohabiting for non-romantic purposes and older adults who may be living with a same-sex adult child or caregiver. The results are similar to the baseline estimates, though again the estimate for women in same-sex households is not statistically significant.

In columns 6 and 7 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: 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. For women, we also estimate substantial reductions in smoking associated with cigarette taxes in the 1996-2010 period in column 6, though the estimate is not statistically significant.

Finally, in column 8 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.[12]

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

in same-sex households, but the estimates remain negative and significant. Our results on daily smoking for both men and women in same-sex households are 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, 2019).

[12] As shown in Table B4 in the Online Appendix, similar conclusions are obtained when focusing in on a sample even more likely to contain sexual minorities (30 to 64-year-old respondents who report being never married or a member of an unmarried couple between 1996 and 2010).

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.[13]

4.2.3 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 with controls for individual demographic characteristics; state, year, and month 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., 2019).

In column 3 we present results from models 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 household. 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

[13] We do not find any evidence of nonlinearities in the impact of cigarette taxes (Table B4 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 B5 and found that no individual state is driving the main results reported in Table 3. In addition, in Figures B2 and B3 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. (2019). 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.

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.

4.2.3 Self-identified sexual minorities (2014-2018)

Finally, we turn to evidence 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 B6 (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 B6 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 6 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 earlier analyses, 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. Each entry in Table 6 is from a separate regression with all the individual and state controls, plus state and time fixed effects.

The results in Table 6 do not consistently indicate that cigarette taxes significantly reduced smoking among self-identified non heterosexual individuals. Although we find that cigarette taxes are estimated to have significantly reduced smoking among self-identified non-heterosexual women in the top panel of column 3 of Table 6, its magnitude is implausibly large. Moreover, we do not find a similar significant reduction in daily smoking for self-identified non-heterosexual women in column 2.[14] The bottom panel of Table 6 indicates that the relationship between cigarette

[14] In Online Appendix Table B7 we also show that the significant association between cigarette taxes and smoking among self-identified non-heterosexual women is not robust to focusing only on women in states that provided SOGI data in three, four or all five years from 2014-2018. Online Appendix Table B8 also shows that this result is not robust to adding the cellphone interviews back into the sample or to restricting attention to self-identified non-heterosexual women who also reported they were in a same-sex household (to match the sample restriction in the analyses from 1996-2018). Additionally, none of the robustness exercises in Online Appendix Tables B7 or B8 indicate that cigarette taxes significantly reduced smoking among self-identified non-heterosexual men.

taxes and smoking among self-identified non-heterosexual men is very small and not statistically significant for both the daily smoker outcome and the current smoker outcome. Finally, columns 2 and 4 Table 6 shows that these patterns are not appreciably different for self-identified heterosexual individuals from 2014 to 2018. Taken together, the results in Table 6 are consistent with the null findings in column 7 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. As already mentioned, 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).

5. Discussion and 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 and suggests that taxes were effective at reducing daily smoking among women and men in same-sex households from 1996-2018. Results for men 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, year, and month 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. This finding is particularly novel and is consistent with a large body of prior work that gay men have lower earnings and higher educational attainment than similarly situated heterosexual men. Their lower earnings may indicate that taxes constitute a larger share of income for gay men compared to heterosexual men, which may contribute to the increased responsiveness of smoking to cigarette taxes for men in same-sex households as compared to men in different-sex households. Similarly, more education may indicate that sexual minorities are able to better understand the adverse health consequences of smoking that are signaled by higher taxes, again contributing to their increased responsiveness to cigarette taxes. In addition, given the higher smoking rates among sexual minorities, it is possible that compliers – i.e., individuals who would reduce or stop smoking following an increase in cigarette taxes – represented a larger share of non-heterosexual respondents.

When we focus on individuals in same-sex households only in more recent years (2011-2018), as well as when we examine individual level data on sexual orientation from 2014 to 2018 for a select sample of states, we do not find evidence that higher cigarette taxes reduced smoking. These results are consistent with the idea that cigarette taxes are no longer an effective health policy tool. Different underlying channels could explain these results. First, there is evidence 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 do not find any significant impact of cigarette taxes for heterosexual respondents. Therefore, the 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.

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 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.

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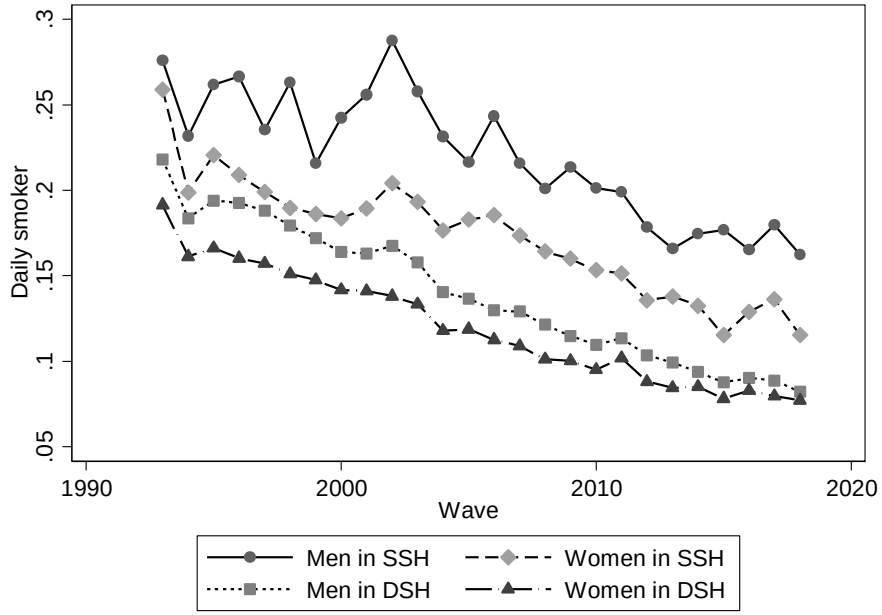
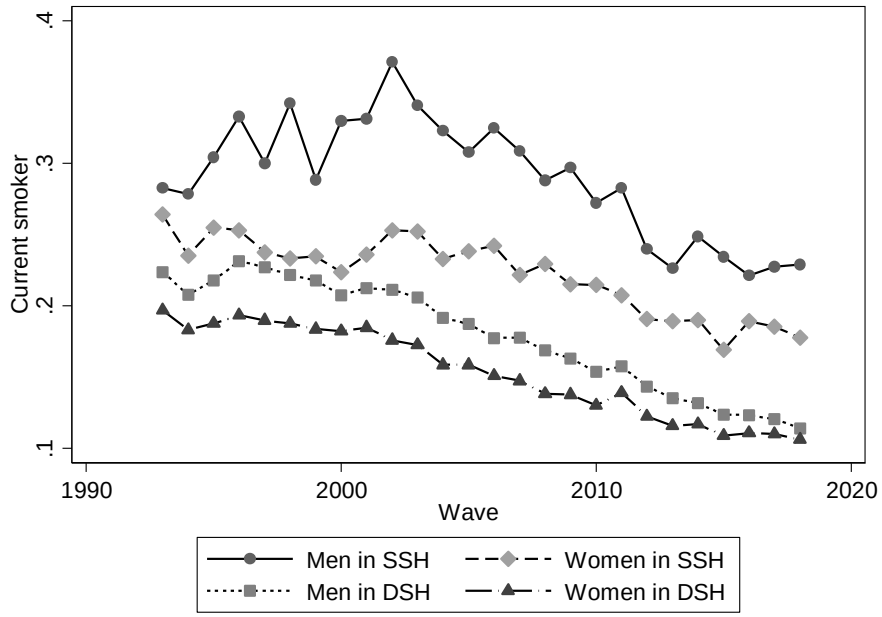
Figure 1: Rates of daily smoking, by sex and whether in a same-sex household.



Source: BRFSS 1993-2018. Weighted means. Respondents younger than 25 have been excluded.

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.

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
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
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. 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**	-0.002	-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	0.165	0.165	0.165	0.218	0.218	0.218
Adjusted R-squared	0.013	0.077	0.077	0.010	0.084	0.084
Men in same-sex households						
Cigarette tax	-0.014***	-0.012**	-0.018***	-0.016***	-0.014***	-0.019***
	(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	0.208	0.208	0.208	0.274	0.274	0.274
Adjusted R-squared	0.015	0.077	0.078	0.015	0.074	0.075
Controls for:						
State, month, and 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, year, and month 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, civil union, LGBT anti-discrimination laws, LGBT hate crime laws, sodomy laws, smoking bans in non-hospitality workplaces, restaurants, and bars, tobacco 21 laws, Medicaid pre-expansion, ACA expansion, Medicaid private option, population, and unemployment rate. 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. Source: BRFSS 1996-2018. All estimated coefficients for the controls in columns 3 and 6 are reported in Table B2 in 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	Only 30 to 64-year-old	1996-2010	2011-2018	Only never married or member of an unmarried couple, 1996-2010
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Women in same-sex households								
Cigarette tax	-0.006**	-0.004	-0.005	-0.010***	-0.005	-0.007	0.007	-0.009
	(0.003)	(0.005)	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)	(0.012)
N	141,517	141,517	147,414	128,322	92,881	88,988	52,529	29,765
Mean of dependent variable	0.165	0.165	0.168	0.165	0.197	0.185	0.132	0.169
Adjusted R-squared	0.077	0.078	0.078	0.081	0.071	0.076	0.073	0.065
Men in same-sex households								
Cigarette tax	-0.018***	-0.021***	-0.017***	-0.020***	-0.028***	-0.023***	-0.013	-0.044***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.009)	(0.022)	(0.015)
N	56,807	56,807	59,924	51,183	38,933	37,779	19,028	17,926
Mean of dependent variable	0.208	0.208	0.211	0.208	0.236	0.226	0.170	0.215
Adjusted R-squared	0.078	0.079	0.078	0.079	0.074	0.077	0.082	0.064
Controls for:								
State, month, and year FE	X	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X	X
State time-varying controls	X	X	X	X	X	X	X	X

Sample: individuals in same-sex households (respondent's age 25+, 30-64 in Column 5). Same standard errors, state, year, and month fixed effects, individual controls, state policies and controls as Table 3. Column 2 includes state-specific linear time trends. Column 4 exclude states with the highest local taxes (Alaska, Illinois, New York, Pennsylvania, and Virginia). Column 8 includes only individuals in a same-sex household who were never married or were a member of an unmarried couple. Weighted regressions. Source: BRFSS 1996-2018 (Columns 1-2, 4-5), 1993-2018 (Column 3), 1996-2010 (Columns 6 and 8), 2011-2018 (Column 7). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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***	--	-0.006***	--	--	--
	(0.003)	(0.001)		(0.001)			
Cigarette tax * In a same-sex household	--	--	0.001	--	-0.0001	0.005	0.004
			(0.003)		(0.0026)	(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	0.165	0.108	0.112	0.123	0.123	0.148	0.163
Adjusted R-squared	0.077	0.070	0.072	0.067	0.068	0.077	0.072
Men							
Cigarette tax	-0.018***	-0.004**	--	-0.004***	--	--	--
	(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	0.208	0.117	0.121	0.142	0.142	0.158	0.185
Adjusted R-squared	0.078	0.076	0.079	0.072	0.073	0.087	0.081
Controls for:							
State, month, and year FE	X	X		X			
Individual controls	X	X	X	X	X	X	X
State time-varying controls	X	X		X			
State-year, state-SSH, and year-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, year, and month fixed effects, individual controls, state policies and controls as Table 3. The triple-difference models in columns 3 and 5-7 include all time-state, time-SSH, and state-SSH double interactions, so the coefficient of cigarette tax is omitted because of perfect collinearity. Weighted regressions. Source: BRFSS 1996-2018. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Cigarette tax effects among 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	-0.059	0.001	-0.081**	-0.008*
	(0.043)	(0.005)	(0.037)	(0.005)
N	6,979	292,715	6,979	292,715
Mean of dependent variable	0.136	0.088	0.184	0.121
Adjusted R-squared	0.487	0.058	0.453	0.063
Men				
Cigarette tax	0.00008	0.007	-0.011	0.007
	(0.01967)	(0.005)	(0.027)	(0.006)
N	6,129	172,679	6,129	172,679
Mean of dependent variable	0.139	0.098	0.190	0.131
Adjusted R-squared	0.242	0.065	0.233	0.077
Controls for:				
State, month, and 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, year, and month 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. Source: BRFSS 2014-2018 (selected states). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$