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E-CIGARETTE TAXATION AND QUEER YOUTH

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ABSTRACT

Electronic nicotine delivery systems (ENDS) use among lesbian, gay, bisexual, and questioning (LGBQ) teenagers is over 30 percent higher than among their heterosexual counterparts. Yet little is known about how recent efforts to curb nicotine vaping through ENDS taxes impact sexual minorities. This study explores this question using data from the 2015-2023 State Youth Behavior Surveys and a generalized difference-in-differences identification strategy. We find that a one-dollar (in 2023$) per mL of e-liquid increase in ENDS taxes reduces the likelihood of any prior-month ENDS use among heterosexual teens by about three percentage points and habitual vaping (as measured by frequent and everyday use) by about one percentage point. In sharp contrast, we find no evidence that ENDS taxes reduce any of the vaping measures for queer youths. The differences in the effects between heterosexual and LGBQ youth are statistically significant in most specifications. We also find that ENDS minimum legal sales ages affect heterosexual but not LGBQ teens, while flavor bans influence heterosexual teens more quickly. The relatively weak effects of ENDS policies on queer youth appear to be driven by those who suffer from persistent sadness, bullying, or suicidal ideation. Overall, our results are consistent with LGBQ youths exhibiting dependence on e-cigarettes to cope with unique stress-related psychological challenges.

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1. Introduction

Lesbian, gay, bisexual, and questioning (LGBQ) teenagers face unique psychological challenges.[1] Religious critiques of homosexuality (Blosnich et al. 2020; Hatzenbuehler et al. 2012), societal discrimination (Badgett et al. 2021),[2] and family conflict surrounding sexual identity (Donahue et al. 2017; Ryan et al. 2009) generate substantial unique stresses that contribute to sharp disparities in mental health between LGBQ and heterosexual youth (Eckstrand et al. 2022; Hatzenbuehler et al. 2024). LGBQ teens are more likely to experience depressive symptoms (Irish et al. 2019) and engage in suicidal behaviors (Liang et al. 2023; Rees et al. 2022; Sun et al. 2023) than their heterosexual counterparts. These disparities likely contribute to higher rates of risky health behaviors for sexual minorities, in part as a coping mechanism for psychological trauma (Earnshaw et al. 2017; Friedman 2020; Rosario et al. 2014).

Tobacco use among LGBQ-identifying teenagers is also substantially higher than among heterosexual youth. In 2023, prior-month use of electronic nicotine delivery systems (ENDS) among LGBQ teenagers was 55 percent higher (23 percent versus 14.8 percent) and combustible cigarette use was 108 percent higher (5.4 percent versus 2.6 percent) than among heterosexual teens (Centers for Disease Control and Prevention 2025). These differences may lead to disparities in mortality and morbidity later in life. Combustible tobacco use is the leading cause of preventable death in the United States, carrying with it elevated risks of cancers, heart disease, and respiratory health problems (Hall and Doran 2016; U.S. Surgeon General 2014; 2020). While ENDS are generally considered less harmful than combustible tobacco products, vaping may still carry some health risks relative to abstinence from all tobacco products (McNeill et al. 2018; Mohammadi et al. 2022; National Academies of Sciences, Engineering, and Medicine 2018; U.S. Surgeon General 2016). Moreover, nicotine consumption – regardless of administration mode – during the teenage years can impede brain development which continues through the mid-20s (Goriounova and Mansvelder 2012).

Curbing youth tobacco use remains a public health priority because (1) initiation into tobacco use is most common among teenagers and young adults (Barrington-Trimis et al. 2020;

[1] We use the Centers for Disease Control and Prevention Youth Risk Behavior Survey (YRBS) in our analysis. Over our study period, the YRBS data do not allow us to accurately measure those persons who identify as part of the larger LGBTQ+ community, for example, transgender- and asexual-identifying persons. Additionally, the "Q" in the LGBQ acronym often refers to "queer" rather than "questioning," but we label it "questioning" to match the available choices in the data.

[2] See also Badgett et al. (2024), Carpenter (2008), Drydakis (2009), Drydakis (2022), Hatzenbuehler et al. (2014), Sabia (2014), Sabia (2015), Sabia et al. (2017), Schuster et al. (2015), and Weichselbaumer (2022).

Khuder et al. 1999; U.S. Department of Health and Human Services 2014; Committee on the Public Health Implications of Raising the Minimum Age for Purchasing Tobacco Products 2015), and (2) youths may be more likely to make irrational decisions than adults with respect to addictive behaviors that adversely affect future health such as tobacco use (Crettez and Deloche 2021). Teenagers' (and young adults') prefrontal cortexes are not fully developed (Arian et al. 2013; Casey et al. 2008; Romer 2010; Spear 2000), which could cause them to hyperbolically discount the future costs of addiction (Gruber and Köszegi 2001). In other words, youths may inflate the importance of short-run utility gains from tobacco use (e.g., reducing the disutility of stress) and give insufficient weight to the longer-run health risks of tobacco use (Gentzke et al. 2022; U.S. Surgeon General 2016). Thus, successful policy interventions to curb disparities in youth tobacco use, especially of combustible products (National Academies of Sciences, Engineering and Medicine 2018), could generate disproportionate health and welfare benefits for groups with relatively high usage rates such as LGBQ individuals.

With the goal of reducing youth ENDS use, 33 states, the District of Columbia (D.C.), and several large cities and counties have adopted ENDS taxes (Centers for Disease Control and Prevention 2024a; Public Health Law Center at Mitchell Hamline School of Law 2023; Tax Foundation 2024). Twelve states have implemented an excise tax per mL of "e-liquid," 16 states and D.C. have adopted either an ad valorem tax (percent of wholesale price) or a sales tax (percent of pre-tax retail price), and five states have implemented a bifurcated tax strategy, with excise taxes on "closed" (with prefilled cartridges) systems and ad valorem taxes on "open" (with refillable cartridges) systems. While ENDS taxation is a popular policy strategy to reduce ENDS use, there is growing evidence that by increasing the price of e-cigarettes relative to combustible cigarettes (or cigars), the policy may have the unintended consequence of inducing substitution to more harmful tobacco products among both youths and adults (Abouk et al. 2023a; Allcott and Rafkin 2022; Cotti et al. 2022; Friedman and Pesko 2022; Pesko et al. 2020). However, the heterogeneous effects of ENDS taxes have been far less explored in the literature, particularly with respect to vulnerable, historically marginalized populations.

This study is, to our knowledge, the first to explore how ENDS taxes and other policies affect nicotine vaping among LGBQ-identifying youths and why these effects may differ from those among their heterosexual counterparts. LGBQ youths may be less responsive to ENDS taxes if they turn to an addictive good such as ENDS to cope with or escape from stress-related psychological trauma (Saffer and Dave 2005). Such psychological trauma may arise from (1) bullying victimization

(Liang et al. 2023), (2) homophobia-induced societal discrimination (Pachankis et al. 2021; Hatzenbuehler et al. 2012), (3) family conflict due to coming out (Donahue et al. 2017), or (4) peer ostracism (Rosario et al. 2014). Consistent with the "minority stress" hypothesis (Meyer 1995), LGBQ teens have higher pre-tax vaping and smoking rates, earlier initiation ages (Centers for Disease Control and Prevention 2023), and greater likelihood of using tobacco as a socialization tool (Carpenter and Sansone 2021).

Each of these factors suggests a stronger attachment to nicotine that can be framed using predictions from standard economic models of addiction (Becker and Murphy 1988; Cawley and Ruhm 2011). One characteristic of these models, "reinforcement," refers to the marginal utility of current consumption rising with the stock of past consumption. Higher pre-tax nicotine usage implies a greater stock of past consumption and therefore stronger reinforcement. Another feature of these models is "withdrawal," which implies a positive marginal utility of consumption in the current period. Facing unique stressors could enhance the instantaneous marginal utility of nicotine even holding past consumption constant, implying a higher level of withdrawal for LGBQ youth. Both reinforcement and withdrawal lead to a more gradual response to price increases and a lower long-run price elasticity. Therefore, the minority stress hypothesis combined with the higher rate of nicotine use among LGBQ teens suggests that they will exhibit a smaller and slower response to taxes, and potentially other policies that increase the full price of ENDS, than heterosexual teens.

Policies adopted to curb ENDS use, such as ENDS taxes, could improve or worsen health for all youth and for LGBQ youth in particular. For example, if such policies induce substitution towards riskier tobacco products (e.g., cigarettes or cigars), then health may decline for both non-LGBQ and LGBQ youths. In terms of LGBQ youth specifically, the minority stress hypothesis suggests that these youth self-medicate with nicotine to cope with homophobia-induced stress and policies that increase barriers to these products may prevent or limit LGBQ youth from self-mediating which, in turn, can lead to worse health for this group of youth specifically.

Using data from the 2015-2023 State Youth Risk Behavior Survey (YRBS), we find robust evidence that the effect of ENDS taxes on vaping is weaker for LGBQ youth than for heterosexual youth. In our main generalized difference-in-differences specification, a one-dollar (in 2023$) increase in ENDS taxes is associated with a 2.5 percentage-point (12.3 percent) reduction in prior-month ENDS use among heterosexual-identifying youth. In contrast, the magnitude of the effect of ENDS taxes on nicotine vaping among LGBQ-identifying youth is approximately 60 percent smaller and not statistically significant. An even starker pattern emerges when we examine more habitual

ENDS use, as measured by frequent and everyday vaping. A one-dollar increase in ENDS taxes leads to statistically significant reductions in these outcomes among heterosexual youth, with the magnitudes representing roughly one-third of baseline ENDS use rates. In contrast, the estimated marginal effects for LGBQ youth turn slightly *positive* but are not statistically significant. The differences in the effects between the two coefficient estimates are statistically significant at conventional levels in most specifications.

Additional analyses support the hypothesis that LGBQ youths face unique stress-related adverse psychological shocks, including from bullying victimization, that increase the risk of addictive behaviors and result in a more inelastic demand for ENDS products. First, heterogeneous effects also emerge for two other ENDS policies that have been shown to reduce youth vaping. Minimum legal sales age (MLSA) laws also reduce ENDS use among heterosexual but not LGBQ teens, while flavor bans affect both groups but influence heterosexual teens more quickly. This pattern of results helps to rule out explanations that would only apply to price-based policies, such as income effects. Additionally, we find evidence that the relatively weak response of LGBQ teens to ENDS policies is driven by null results among those LGBQ teens who report being depressed, suicidal, or bullied. In contrast, heterosexual teens who report these mental health problems exhibit broadly similar policy responsiveness to those who do not.

We also conduct auxiliary analyses of other outcomes and datasets. First, we test for substitution toward combustible cigarettes among teens. Next, we use data from the Behavioral Risk Factor Surveillance System (BRFSS) to examine adults, who are more likely than teens to use e-cigarettes as a smoking cessation tool (U.S. Surgeon General 2020) and, in conjunction with alcohol, as a socializing strategy (Carpenter and Sansone 2021). We find no evidence that ENDS taxes impact youth smoking or the vaping behavior of adults. However, we also are unable to rule out meaningfully large effects relative to the rates of those behaviors observed in our data.

2. Background

2.1 Rise of Electronic Nicotine Delivery Systems (ENDS)

The first commercially successful generation of nicotine-based e-cigarettes was produced in China in 2003. One year later, e-cigarettes were made widely available to consumers in China and often marketed as a smoking cessation tool (Demick 2009; Wang et al. 2019). In 2005, ENDS products from China began to be exported to other nations, with e-cigarettes entering the U.S. market in 2006 (CASAA 2021). Knowledge about ENDS products spread relatively quickly across

the developed world. According to the International Tobacco Control Four-Country Survey (which included respondents from Australia, Canada, the United Kingdom, and the United States), by 2011, 47 percent of adult respondents were aware of the existence of ENDS, eight percent had already tried ENDS, and three percent were current users (Adkison et al. 2013). As this new market expanded, youth ENDS use steadily rose throughout the 2010s before declining during the period following 2019, in part perhaps due to the COVID-19 pandemic, but also due to policies introduced contemporaneously to curb access to e-cigarettes. While approximately seven percent of U.S. adults ages 18 and older were ENDS users in 2023 (Centers for Disease Control and Prevention 2024), among middle and high school students, the prevalence was nearly 17 percent (Centers for Disease Control and Prevention 2025).[3]

The population-level health effects of broader access to e-cigarettes are not obvious. The preponderance of medical evidence suggests that ENDS use is substantially less harmful to respiratory-, heart-, and cancer-related health than combustible tobacco product use (McNeill et al. 2018; National Academies of Sciences, Engineering, and Medicine 2018).[4] However, nicotine vaping can still have some adverse respiratory (lung-related) and heart-related health effects (Balfour et al. 2021; McNeill et al. 2018; National Academies of Sciences and Medicine 2018; Sahu et al. 2023). For teens, e-cigarette use can also affect brain development (U.S. Surgeon General 2014).

The policy debate on the net public health effect of restricting access to ENDS therefore focuses on whether e-cigarettes and combustible tobacco are complements or substitutes in consumption (or perhaps both across heterogeneous consumers). ENDS use may serve as an effective tobacco harm reduction strategy for individuals who consume ENDS instead of combustible tobacco products when access to ENDS increases. Others may respond to increased ENDS access by consuming e-cigarettes when, in their absence, they would have abstained from all tobacco products. Still others could use ENDS products as a "gateway" into riskier combustible tobacco use (Etter 2018; Friedman et al. 2019a; Khouja et al. 2020).

[3] The prevalence of ENDS use declined among both youths and young adults in 2020-2021 (Centers for Disease Control and Prevention 2023; Pesko 2023). Though the National Survey on Drug Use and Health suggests that e-cigarette use may have risen in 2022, the most recent data from the National Youth Tobacco Survey and the authors' calculations using YRBS (see Figure 1) suggests continued lower youth ENDS use through 2023 (Substance Abuse and Mental Health Services Administration 2023, Birdsey et al. 2023). Adult use stayed relatively constant during the 2010s, with small increases around 2022 (Truth Initiative 2024a).

[4] ENDS use by the user and effect on others is estimated not to exceed five percent of the harm of cigarettes (Nutt et al. 2014). However, consumers overestimate the health risks of e-cigarettes relative to the true risks (Viscusi 2016).

This ambiguity has led to regulatory approaches to ENDS products that sharply differ across nations. Strategies include: (1) encouraging e-cigarette use among combustible tobacco users (e.g., United Kingdom), (2) sharply reducing teenagers' and young adults' access to e-cigarettes (e.g., United States), (3) requiring a doctor's prescription to legally purchase e-cigarettes (e.g., Australia), and (4) banning the sale of all e-cigarettes (e.g., Brazil, India, and Mexico) (World Health Organization 2023). The U.S. Surgeon General's Office has lauded the sharp decline in the prevalence of combustible tobacco smoking among both teens and adults (U.S. Surgeon General 2014), which could, in part, be explained by increased access to e-cigarettes. However, the Office has also labeled high rates of ENDS use among youth an "epidemic" (U.S. Surgeon General 2018).

2.2 Sexual Minorities and Tobacco Use

While there is robust evidence from large national surveys that LGBQ youth (Gentzke et al. 2022) and LGBQ adults (Cornelius et al. 2023) are more likely to use tobacco products than their heterosexual counterparts, there is also evidence that LGBQ individuals use tobacco for different reasons than heterosexuals. One of these explanations concerns minority stress (Meyer 1995), whereby sexual minorities use tobacco products to cope with the homophobia-driven social pressures they face (Badgett et al. 2024; Blosnich et al. 2020; Donahue et al. 2017; Ryan et al. 2009). LGBQ individuals experience unique pressures, especially during their teenage years (Kosciw et al. 2016; Schuster et al. 2015), and that acute mental distress may be causally related to addictive behaviors, including nicotine consumption (Friedman 2020).

The unique stressors faced by LGBQ youth could also indirectly lead to weaker price responsiveness through specific social pressures. LGBQ young adults are more likely than others to use tobacco products as a means of socialization, with venues associated with smoking, like bars and clubs, being safe havens for LGBQ individuals and an important part of LGBQ culture historically. This pattern of socialization could lead to more encounters with tobacco product users and greater social pressure to smoke as compared to heterosexual individuals (Jannat-Khah et al. 2018; Remafedi 2007). Whether such socialization pressures extend to nicotine vaping remains an open question. Also, LGBQ youths' unique social support networks with peers in similar stressful circumstances (McPherson et al. 2001) could increase illicit market access, thereby blunting the impact of ENDS taxes. As with combustible tobacco product companies, ENDS producers actively target LGBQ people – for example, by sponsoring events tailored to the LGBQ community such as pride festivals (Truth Initiative, 2024b).

As discussed in the Introduction, standard economic models of addiction (whether myopic or rational) predict that addictive goods exhibit slower and weaker responsiveness to price changes than non-addictive goods. The stronger the addiction, the slower and weaker the responses of consumers. However, standard models of addiction do not incorporate the availability of a close substitute with the same addictive properties as the good subject to the price intervention. Intuitively, the availability of a close substitute could plausibly make the price responsiveness of more attached users similar to that of less attached users for the focal good, with more attached users instead being more likely to switch to the substitute rather than quit use of the substance entirely. While economic models of addiction typically focus on prices, the logic that greater attachment makes quitting harder when incentives change also applies to non-price-based interventions such as minimum legal sale ages and flavor bans as these regulations impact the "full" price of ENDS.

With that said, a potential mechanism that only applies to price-based interventions is the income effect. As discussed by Badgett et al. (2024), sexual minority men earn less than heterosexual men on average, while the reverse is true for women. However, the picture is murkier for teens who have not yet fully entered the labor force. If LGBQ teens face reduced disposable income due to discrimination in the labor market for teen workers (Sabia 2014) or estranged relationships with their families (Ryan et al. 2009), these experiences could strengthen price responsiveness. There is evidence of meaningful income effects from cigarette taxes. For instance, Darden et al. (2025) show that cigarette taxes influence smokers' expenditures on entertainment, groceries, clothing, transportation, healthcare, and housing. Rozema and Ziebarth (2017) document that the financial strain from cigarette taxes increase the take-up rate of the Supplemental Nutrition Assistance Program – a program that provides financial support for food purchases for people with low income – among smokers. We are unaware of direct evidence that these income effects extend to ENDS taxes. However, Courtemanche et al. (2025) argue that income effects could help explain their finding that ENDS taxes reduce teens' body weight. The authors note that income effects could be more pronounced for teens than adults because teens only have access to a small portion of the household's total income.

There are also other – arguably more speculative – reasons why LGBQ individuals might be differentially affected by ENDS taxes. A lengthy literature documents that gay and lesbian individuals, on average, attain higher levels of education than straight individuals (Badgett et al. 2021; Badgett et al. 2024; Carpenter and Eppink, 2017), which could lead to better knowledge about the

relative harms of vaping versus smoking and reduce the odds of switching to smoking when an ENDS tax is implemented. However, recent evidence suggests that gay and lesbian people no longer obtain more education than straight people, and other sexual minority groups have never exceeded heterosexual people in terms of educational attainment (Badgett et al. 2024). Additionally, LGBQ individuals may be less likely to be exposed to information due to their lower interaction with the healthcare system and reduced likelihood of being insured (Badgett et al. 2021; Buchmueller and Carpenter 2010; Gonzales and Blewett 2014). For the same reasons, LGBQ individuals might also have fewer opportunities for participation in programs to transition from combustible tobacco products to e-cigarettes and/or from e-cigarettes to abstinence from tobacco products.

There is very little empirical research examining the causal effects of tobacco control policies on LGBQ-identifying persons. One notable exception is Carpenter and Sansone (2021), who study the effect of cigarette taxes on cigarette smoking for LGBQ-coupled individuals. Using data from the 1996-2018 BRFSS, these authors use the survey's household roster to identify adults living in likely same-sex and opposite-sex relationships. Using a two-way fixed effects (TWFE) approach, they find that a one-dollar increase in cigarette excise taxes per pack leads to a 0.9 (1.2) percentage point larger reduction in the likelihood of being a current (every day) smoker for men in same-sex households as compared to men in different sex-households, with negligible differences among women. Carpenter and Sansone document evidence for a socialization mechanism, as cigarette taxes reduce smoking more among LGBQ men who drink (their proxy for socialization) as compared to LGBQ men who do not drink, whereas there is little difference among heterosexual men by their drinking status. Notably, these results run counter to the minority stress hypothesis. However, Carpenter and Sansone study a specific subgroup of LGBQ individuals – adult gay men who live with their partner – who likely experience this stress very differently from teenagers.

2.3 ENDS Taxes

One of the most popular policy strategies to restrict access to e-cigarettes is through ENDS taxation (Public Health Law Center at Mitchell Hamline School of Law 2021). At least 54 countries have adopted a nationwide tax (Dauchy and Fuss 2023). Although the U.S. has not adopted a federal tax, state and local ENDS taxes appear to have had both intended and unintended effects on tobacco product use among Americans.

First, there is evidence that adult tobacco use responds to changes in ENDS taxes. Price effects are the likely channel, as evidence from Nielsen Scanner Data suggests that approximately 90

percent of ENDS tax increases are passed through to retail prices (Cotti et al. 2022). Using data from the National Health Interview Survey and the Behavioral Risk Factor Surveillance System (BRFSS), Pesko et al. (2020) find that a one-dollar increase in ENDS tax (rate per fluid milliliter of nicotine) is associated with a 0.52 percentage point (15.3 percent) decline in nicotine vaping among adults aged 21 and older. However, the authors also show that daily cigarette use increases by around 0.59 percentage points (5.3 percent) following a one-dollar increase in the tax. Friedman and Pesko (2022) use data from the Current Population Survey's Tobacco Use Supplement and find that a one-dollar increase in ENDS taxes is associated with a 2-3 percentage-point decline in ENDS use among young adults ages 18-25. They also document that a one-dollar increase in ENDS taxes increases past-month cigarette use by 3.7 percentage points. Using data from the BRFSS, Dave et al. (2022) show that a one-dollar increase in ENDS taxes is associated with a 4.6 percent decline in ENDS use among adults ages 21-39. Abouk et al. (2023a) study the effects of ENDS taxes on pregnant women. They find that a one-dollar increase in ENDS taxes decreases pre-pregnancy vaping by 1.8 percentage points but increases pre-pregnancy smoking by 0.5 percentage points.

There is also evidence that youth tobacco use is sensitive to changes in both ENDS taxes (Abouk et al. 2023b; Dave et al. 2022) and ENDS prices (Pesko and Warman 2022). Abouk et al. (2023b) use data from Monitoring the Future (MTF) survey and the YRBS and find that a one-dollar increase in ENDS taxes is associated with an approximately 15 to 30 percent decline in ENDS use among youth. The authors also find evidence for ENDS tax-induced substitution to combustible cigarettes, especially in the MTF data; Abouk et al. (2023b) find that a one-dollar increase in ENDS taxes increases current cigarette use by 20 percent and heavier use (smoking at least a half a pack of cigarettes per day) by 50 percent. Pesko and Warman's (2022) results using the National Youth Tobacco Survey for prices imply that a 100 percent increase in the ENDS tax would increase youth cigarette smoking by about five cigarettes per 30 days. Together, the above studies provide compelling evidence that the spillover effects of ENDS taxes on youth and adult combustible tobacco use undermine the tobacco-related public health gains that policymakers wish to achieve.

2.4 Other ENDS Policies

Policymakers have also undertaken several other strategies to restrict ENDS access. Many state and local jurisdictions adopted a minimum legal purchasing age (MLPA) for e-cigarettes to match the MLPA for combustible tobacco products, which at the time was 18. While there is evidence that ENDS-specific MLPAs are effective at curbing youth ENDS use (Abouk and Adams

2017; Pesko 2023), the preponderance of the evidence suggests that such ENDS-specific MLPAs induce substitution to combustible cigarettes (Dave et al. 2019; Friedman 2015; Pesko 2023).

Subsequently, many jurisdictions adopted a Tobacco-21 (T-21) law, which raised the MLPA for *all* tobacco products to age 21. On December 20, 2019, a federal T-21 law was adopted, though its enforcement was delayed (Campaign for Tobacco Free Kids n.d.). Evidence, based on time periods prior to the federal law, suggests that state T-21 laws reduce nicotine vaping *and* cigarette smoking among youths (Abouk et al. 2024; Friedman et al. 2019b; Friedman and Wu 2020; Hansen et al. 2023). However, a recent study suggests that these laws may have reduced *reported* tobacco product use among those under 21 years of age, but *actual* tobacco product use – based on biometric measurements – did not decline (Cotti et al. 2024b).

Policymakers have also pursued supply-side channels to restrict e-cigarette access. As of June 2024, 36 states and the District of Columbia have enacted laws that require retailers to have a state-issued license to sell ENDS products over the counter (Centers for Disease Control and Prevention 2024a). License fees are used to enforce state ENDS sales regulations, including the MLPA; however, Courtemanche et al. (2024) find no evidence that these laws are effective at curbing youth or young adult ENDS use.

Public health advocates have expressed concern that flavored ENDS may attract youths (Campaign for Tobacco-Free Kids 2023).[5] As of June 2024, seven states and over 380 local jurisdictions (i.e., counties, cities, and towns) have adopted ENDS flavor restrictions (Campaign for Tobacco-Free Kids 2024). While ENDS flavor restrictions reduce ENDS use, particularly among youths, such laws also appear to induce substitution to combustible cigarettes (Cotti et al. 2025; Saffer et al. 2024).

In the light of concerns that tobacco users may use online purchases to bypass state and local tobacco restrictions (Cullen et al. 2018; Office of Surgeon General 2018; Williams et al. 2012, 2016, 2017), over 14 states have adopted internet sales shipping bans for tobacco products, and five include restrictions on shipping ENDS (Public Health Law Center at Mitchell Hamline School of Law 2022). While there is very little causal evidence on the impact of these laws, a descriptive study on Massachusetts' ENDS sales ban suggests that many online vendors do not comply with the statute or even sell to underage consumers (Nali et al. 2021).

[5] While flavored cigarettes were banned by the 2009 Family Smoking Prevention and Tobacco Control Act, ENDS products may legally come in a variety of flavors like fruit, mint, cream, and candy themed flavors (e.g., watermelon, lemon mint, vanilla, cotton candy). According to the National Youth Tobacco Survey, 86 percent of high school students who currently use ENDS report using flavored ENDS (Gentzke et al. 2022).

As of June 2024, 20 states and D.C. have extended clean indoor air laws to include ENDS aerosols (Centers for Disease Control and Prevention 2024a). There is some evidence that these laws have been effective at curbing ENDS and combustible tobacco use. In particular, bans on ENDS use in indoor restaurants and workplace environments are effective in decreasing the use of ENDS and cigarettes for both young and prime-age adults (Friedman et al. 2021). However, to the best of our knowledge, there is no evidence that adoption of these laws leads to changes in youth tobacco product use.

2.5 Contributions

This study builds on the work of Carpenter and Sansone (2021) by being the first to estimate the impact of ENDS taxes and other related policies on LGBQ youth and young adults. Given the potentially important differences in motivations for nicotine vaping among LGBQ youth as compared to heterosexual youth and young adults, and potentially substantial long-run health costs of addiction to tobacco products, examining how ENDS taxes differentially impact sexual minority teens is important. Second, we explore possible explanations for heterogeneity in ENDS tax effects across sexual minorities and majority teens – particularly the role of mental health and bullying. Third, this study is the first to explore intersectionality in the effects of ENDS taxes by gender-race/ethnicity and sexual identity. As there are unique challenges faced by those who face added discrimination by being sexual-, racial-, and gender-minorities, the effects of ENDS taxes differ based on intersectional demographic characteristics.

3. Data

3.1 State YRBS Data

Our primary analysis uses data from the State YRBS, a biennial, school-based survey coordinated by the Centers for Disease Control and Prevention and administered by state Departments of Education and Health and Human Services. When weighted, each state YRBS survey — which consists of hundreds, often thousands of observations (even for smaller population states) — is designed to be representative of each state's population of high school students. The representativeness of these surveys at the state level is particularly important when examining differential effects of a state policy on health between majority and minority populations. Moreover, using state-by-year population data from the Surveillance of Epidemiology and Ends Research

(SEER), pooled State YRBS surveys can be made approximately representative of U.S. teenagers ages 14 to 18 years old.

As this study focuses on differences in the effects of ENDS taxes on tobacco use among LGBQ youth, we first measure youths' self-reported sexual identity using responses to the following questionnaire item:

> "Which of the following best describes you?"
> *[Possible answers: Heterosexual (straight), Gay or Lesbian, Bisexual, or Not Sure]*

If the respondent answers "heterosexual (straight)," we classify the youth as identifying as heterosexual; if a respondent answers "gay or lesbian," "bisexual," or "not sure," we classify them as identifying as lesbian, gay, bisexual, or questioning (LGBQ).[6] Appendix Figure 1 shows that from 2011-2023 there was a sizeable increase in the proportion of youth identifying as a sexual minority, especially those who identify as bisexual or questioning.

Summary statistics are reported in Table 1. Sexual identity is missing for 14.3 percent of the sample, and most of the missing values (84 percent) are due to states not including the question in their YRBS survey. Among youths with non-missing sexual identify, across the entire 2015-2023 sample period, 19.1 percent identify as LGBQ. The majority of self-reported sexual minorities identify as bisexual (9.1 percent), followed by questioning (6.6 percent), and then gay or lesbian (3.0 percent). While large, these percentages are consistent with recent estimates of the share of the younger-aged U.S. population who identify as LGBTQ+. For instance, a recent Gallup poll found that 22.3 percent of Generation Z identified as sexual minorities (Gallup 2024a).

The second column of Table 1 shows summary statistics excluding observations with missing information on sexual identity. Alaska, Georgia, Idaho, Louisiana, Montana, South Dakota, and Tennessee do not include sexual orientation in their state-administered YRBS surveys over the 2015-2023 period. The variable means are generally very similar to those for the unrestricted sample in the first column. Moreover, as we show below, estimated effects of ENDS taxes on youth smoking are very similar in the overall YRBS sample as compared to the sample that is restricted to include non-missing information on sexual identity.

[6] In 2021, the YRBS added responses to the sexual identity question, including "I describe my sexual identity some other way," "I am not sure about my sexual identity (questioning)," or "I do not know what this question is asking." Those responding in this way are also classified as LGBQ for our main analysis, though the findings are robust to their exclusion.

Our primary outcome is ENDS use among youths. This measure is available in the 2015, 2017, 2019, 2021, and 2023 surveys. In these survey waves, respondents to the State YRBS are asked:

> "During the past 30 days, on how many days did you use an electronic vapor product?" [Examples include: "e-cigarettes, vapes, vape pens, e-cigars, e-hookahs, hookah pens, and mods [such as JUUL, SMOK, Suorin, Vuse, and blu]"]

Current ENDS Use is set equal to one if the respondent reports ENDS use on a positive number of days in the last 30 days, and zero otherwise. In the sample in which sexual identity is reported, 18.7 percent of respondents (when weighted) are prior-month ENDS users. This is exactly the same as the percent of respondents in the full sample, suggesting that over the 2015-2023 period, the selection of states that reported information on youth sexual identity was similar to the full sample with respect to ENDS use.

When we examine ENDS participation rates separately by sexual identity, we find different rates of use. Figure 1 documents trends in ENDS use among LGBQ- and heterosexual-identifying youth. We find that both groups follow a similar trend in ENDS use over the 2015-2023 period, with participation in ENDS use declining between 2015 and 2017, increasing in 2019, then decreasing slightly again in 2021 and 2023. However, we find that ENDS participation rates are consistently higher for LGBQ-identifying youth relative to heterosexuals, with a participation differential that remains fairly constant over the period.

In addition to current use, we examine more habitual e-cigarette use, measured by whether the youth reports ENDS use on at least 20 of the prior 30 days (*Frequent ENDS Use*) or on all 30 of the prior 30 days (*Everyday ENDS Use*). We find that 5.3 percent of teenagers in our analytic sample report frequently using ENDS in the prior 30 days and 3.8 percent report daily ENDS use. Rates of frequent and everyday ENDS use are generally higher for LGBQ as compared to heterosexual youth, particularly in 2021 and 2023.

Next, we measure cigarette smoking among youths. We use the same 2015-2023 period to examine spillover effects to cigarette smoking, though we are also able to expand the analysis window back to 2011 (when we have consistent measures of cigarette smoking and sexual identity). We measure cigarette smoking using responses to the following survey item:

"During the past 30 days, on how many days did you smoke cigarettes?"

Current Cigarette Smoking is then set equal to one if an individual responds that they had smoked cigarettes on a positive number of days, zero otherwise. *Frequent Cigarette Smoking* and *Everyday Cigarette Smoking* are created in a similar manner to frequent and everyday ENDS use (using 20 and 30 day prior-month cutoffs). Over the 2011-2023 period, 9.6 percent of LGBQ-identifying teens and 5.8 percent of heterosexual-identifying teens report prior-30-day cigarette smoking. Rates of frequent (everyday) cigarette smoking are 2.6 (1.9) percent for LGBQ-identifying teens and 1.4 (1.0) percent for heterosexual youth. As shown in panel (a) of Appendix Figure 2, rates of current teen cigarette smoking have declined since 2011, though significant differences by sexual identity exist throughout the sample period. Intensive measures of frequent and everyday smoking follow similar patterns, as indicated by panels (b) and (c).

Additionally, we measure *Cigarette or Cigar Smoking* among youths. Respondents are asked:

"During the past 30 days, on how many days did you smoke cigars, cigarillos, or little cigars?"

Current Cigarette or Cigar Smoking is then set equal to one if an individual responds that they had smoked cigarettes or cigars on a positive number of days; and is set equal to zero if an individual responds with zero days to both questions.[7] *Frequent Cigarette or Cigar Smoking* (*Everyday Cigarette or Cigar Smoking)* is again created similarly, with the variable turning on for responses of 20 or more days (30 days) to either question. Over the 2011-2023 period, 16.8 percent of LGBQ-identifying teens and 11.0 percent of heterosexual-identifying teens report prior-30-day cigarette or cigar smoking (Table 1). Rates of frequent (everyday) cigarette or cigar smoking were 4.4 (3.2) percent for LGBQ-identifying teens and 2.4 (1.7) percent for heterosexual youth. Appendix Figure 3 shows similar results for rates of smoking either cigarettes or cigars.

3.2 ENDS Taxes

To generate a standardized measure of ENDS taxes, we follow Cotti et al. (2023, 2024), using NielsenIQ retail scanner data and assuming a 35 percent retailer markup (based on e-cigarette

[7] The variable is coded as missing if the response to both questions is missing, or if the response to one question is missing and the other is zero days.

company purchasing forms), to convert ad valorem and sales taxes to their excise tax equivalents, allowing for comparison across states that may implement different tax schema. Our tax variable is the value for a mL of fluid nicotine. Figure 2 shows geographic, temporal and intensity of variation in real ENDS taxes (in 2023$) from 2010 through 2023. In 2023, Minnesota had the highest state ENDS tax ($2.89 per mL of liquid nicotine), while Delaware, Georgia, Kansas, North Carolina, and Wisconsin had the smallest ENDS tax ($0.05 per mL of liquid nicotine).

Our ENDS tax variable is state level because that is the narrowest geographic identifier available in our data. However, we incorporate city and county taxes (e.g., Cook County, Illinois has implemented an ENDS tax that applies to retailers in that county) into the state-level tax variable by population weighting following earlier studies examining ENDS taxes (e.g., Abouk et al., 2023). For instance, if a state has ten million people, one million of whom live in a jurisdiction with a $1.00 tax rate per fluid mL of nicotine, $0.10 is added to the state tax rate when creating our tax variable.

3.3 Sexual Orientation Policy Index

State policies towards queer individuals have been shown to influence labor market- and family-related outcomes (Badgett et al. 2024) and likely also proxy for changing attitudes towards LGBQ youth across states and time. Since all these factors could influence tobacco product use, we control for the Movement Advancement Project's state-level sexual orientation policy tally (https://www.lgbtmap.org/equality-maps). This score accounts for over 50 LGBTQ-related laws and policies, some of which are protective and increase the score, and some of which are harmful and decrease the score. The data are only available contemporaneously on the Movement Advancement Project website. To build a time series, we use the Wayback Machine to scrape the site as closely as possible to January 1st of each year 2015-2023. As the Movement Advancement Project (2025) explains, laws covering sexual orientation impact lesbian, gay, bisexual, queer, and other non-heterosexual individuals, while laws covering gender identity affect transgender people – but there is substantial overlap.

4. Empirical Strategy

We begin by estimating a fixed effects logistic regression model in which the probability that respondent *i* residing in state *s* in year *t* in semester (fall or spring) *p* vaped nicotine in the prior 30 days:

$$Pr(Y_{istp} = 1 \mid X) = \frac{e^{\beta_0 + \beta_1 \text{ENDS Tax}_{stp} + \mathbf{X}_{istp}\beta_2 + \gamma_s + \tau_t + \eta_p}}{1 + e^{\beta_0 + \beta_1 \text{ENDS Tax}_{stp} + \mathbf{X}_{istp}\beta_2 + \gamma_s + \tau_t + \eta_p}} \quad . \quad (1)$$

ENDS Tax_{stp} is a state-by-year-semester measure of ENDS tax per mL of liquid nicotine in 2023 dollars. $\mathbf{X}_{istp}$ is a vector of control variables, γ_s is a state fixed effect, τ_t is a year fixed effect, and η_p is a semester (fall or spring) fixed effect. We utilize sample weights to adjust for the State YRBS sample sizes across states not being proportional to state teen populations.[8] Standard errors are clustered at the state level in all specifications. We present average marginal effects in all tables and will also report coefficient estimates from linear probability models.

We gradually build up the set of controls across three specifications. The first, which we call "baseline controls," includes only (1) individual demographic variables (age, grade in school, race/ethnicity, gender), (2) state unemployment rate, (3) COVID-19 cumulative death rate and percent of persons who are fully vaccinated, and (4) "essential" tobacco policies. The tobacco policies that we deem as "essential" controls are cigarette tax rate (the counterpart to ENDS tax rate for the most obvious possible substitute) and the ENDS policies besides taxes with the clearest empirical support for reducing ENDS use (MLPAs, T-21 laws, and flavor restrictions). For the second specification, we use a least absolute shrinkage and selection operator ("LASSO") approach to select additional controls out of a group of candidates including indoor vaping bans (at bars, restaurants, or workplaces), ENDS licensure laws for retailers, ENDS online sales delivery bans, indoor cigarette smoking bans, combustible tobacco licensure laws, recreational marijuana laws, medical marijuana laws, and beer taxes in 2023 dollars. The third specification further adds a control for a state's sexual identify policy index described in Section 3.3.

We estimate equation (1) on the samples of heterosexual-identifying youths and LGBQ-identifying youths, with attention to differences in the parameter of interest β_1 across these two groups. One concern with estimating the equation separately by sexual identity is that identification

[8] Weighting in this manner is standard in the literature using the YRBS to identify effects of tobacco policies. Solon et al. (2015) describe different circumstances in which utilizing sample weights might be important. One of these is when treatment effects are heterogeneous across states, state sample shares are not proportional to population, and the objective is to produce estimates that can interpreted as average treatment effects among the national population. Our setting likely fits this description. Additionally, weights may also be important in our case to correct for possible endogenous sampling even if treatment effects are homogenous. Our stratification by heterosexual and LGBQ identification may be correlated with both the treatment (ENDS taxes) and outcome (vaping) variables. Thus, the source of sample selection cannot be controlled for, which in turn could make weights appropriate. Specifically, based on earlier work, we know that LGBQ youth are more likely to vape than non-LGBQ youth (Gentzke et al. 2022). Also, several states did not include information on LGBQ in the YRBS over our study period (i.e., Alaska, Georgia, Idaho, Louisiana, Montana, South Dakota, and Tennessee), and these states (other than South Dakota) all have ENDS taxes.

as a sexual minority could plausibly be related to changes in ENDS taxes if they proxy for progressive "pro-health" policies or sentiment. Previous research suggests that genetics (Ganna et al. 2019; Pillard and Bailey 1998; Song and Zhang 2024), epigenetic responses to androgen exposures in-utero (Breedlove 2017; Rice et al. 2012), and fraternal birth order (Blanchard 2018) affect sexual orientation, although the decision to reveal one's sexual identity — to oneself, to one's peers, and on a survey — is endogenous (see Anderson et al. 2021). In Appendix Table 1, we present regression results showing no evidence that ENDS taxes are related to related to the probability of identifying as a sexual minority. Estimated coefficients are very small across all models and not statistically significant, supporting the hypothesis that sample selection bias is a relatively unimportant concern in this policy context.

We exploit within-state changes in ENDS taxes to identify β_1. Over the sample period 2015-2023, there are 76 (non-inflation driven) year-by-quarter changes in ENDS taxes throughout the U.S. To generate an unbiased estimate of the effect of ENDS taxes on youth tobacco use, there must be no (1) time-varying unobservables associated with ENDS tax changes and with the outcome under study, (2) reverse causality, whereby youth tobacco use drives changes in ENDS taxes, and (3) parallel trends between adopting and non-adopting states.

With respect to the former two concerns, we implement several approaches. First, we include the aforementioned tobacco control policies as covariates to reduce the likelihood that ENDS taxes are simply proxying for a bundle of tobacco control policies or changes in anti-tobacco sentiment that accompany ENDS tax changes. In addition, we examine the sensitivity of the estimate of β_1 across subsets of observable controls. Relative stability in the estimated treatment effect could signal that ENDS taxes are enacted exogenously to youth ENDS use.

Second, we explore the sensitivity of the estimated treatment effect to augmented controls for spatial heterogeneity. First, we add controls to the right-hand side of equation (1) for census region-specific year effects. This specification controls for unmeasured shocks that commonly affect states within census regions (and could be incidentally correlated with ENDS taxes and youth tobacco use) by forcing "close controls." Second, we control for a common-treatment-state linear time trend, meaning that an indicator for treatment is interacted with time.[9]

With respect to parallel trends, our primary approach to descriptively test for this concern is through estimation of an event-study model that tests for whether youth ENDS use was trending

[9] These approaches are not without criticism and could, in fact, introduce bias in estimated treatment effects (see, for example, Neumark et al. [2014] and Burkhauser et al. [2023]).

differently in treatment and control states prior to changes in ENDS taxes. Using the approach of Scmidheiney and Siegloch (2023), we interact the time-to-treatment indicators with first differences of the continuous real ENDS tax. Given our relatively short event panel, we adopt the approach of Miller (2023) and Serrato and Zidar (2016), reporting average event-study coefficient estimates using the delta method.[10]

Finally, recent innovations in the difference-in-differences literature suggest that in the context of staggered policy adoption and heterogeneous and dynamic treatment effects, two-way fixed effects estimators such as those described in equation (1) could result in biased estimates of β_1 (Goodman-Bacon 2021). To address this concern, we use a stacked difference-in-differences approach (Cengiz et al. 2019). This approach has been used previously in the ENDS tax literature (Abouk et al. 2023b; Dave et al. 2022; Dave et al. 2024) and expunges bias due to heterogeneous and dynamic treatment effects by relying on "never adopters" (states that never adopt an ENDS tax) or "not yet adopters" (states that have not yet adopted an ENDS tax) as counterfactuals and choosing a common event window.[11]

5. Results

Our main findings appear in Tables 2 through 9 and Figures 3 through 6. Supplemental analyses appear in the appendix figures and tables.

5.1 ENDS Taxes and ENDS Use among LGBQ and Heterosexual-Identifying Teens

In panel I of Table 2, we present estimates of the effect of ENDS taxes on youth ENDS use for the full State YRBS sample. To facilitate comparison with subsequent analyses that stratify by sexual identity, panel II excludes from the sample observations with missing information on sexual identity. All regressions control for state, year, and semester fixed effects as well as excise taxes on cigarettes (in 2023$). The results in panels I and II are very similar, indicating that excluding the handful of states that do not report sexual identity information does not meaningfully influence results.

[10] We define j as event time. Per the delta method, coefficients reported at event time j=(-2,-1), the reference period, and (0,1) are standard. For event time j* < -2 (leads), the marginal effect in period j* that we report is the average marginal effect for all j in the interval j = [j*,0). For event time j^ > 1 (lags), the marginal effect in period j^ that we report is the average marginal effect for all j in the interval [0,j*].

[11] We construct separate datasets for "stacks" for each adoption year of ENDS taxes during our analysis period (2015, 2017, 2019, 2021), excluding states that adopt in 2023 due to an inability to observe dynamic effects. The event window for each dataset spans six years prior to two years post adoption. Treatment effects are then obtained in a pooled logistic regression similar to equation (1) that includes stack-by-state and stack-by-year fixed effects.

The first three columns of the table show the results using the indicator for current ENDS use as the outcome, gradually adding control variables as described above. In panel II, in our specification with baseline controls (column 1), we find that a one-dollar increase in ENDS taxes is associated with a 2.9 percentage-point decline in youth ENDS use. The magnitude of this effect corresponds to a 13.7 percent decline relative to the pre-treatment mean. The addition of controls chosen by the LASSO method – indoor vaping bans, ENDS online sales bans, indoor smoking bans, and tobacco licensure laws – slightly attenuates the estimate to 2.5 percentage points (11.8 percent; column 2). Further adding the sexual identity policy index leads to an estimate of 2.2 percentage points (10.4 percent). This estimate translates to a tax elasticity of about -0.037.[12] ENDS tax is statistically significant at the five percent level or better in all three specifications. Our find that ENDS taxes reduce the youth vaping rate is consistent with results in several prior studies (Abouk et al. 2023b; Dave et al. 2022; Dave et al. 2024).

In the other two panels of Table 2, we explore the effect of ENDS taxes on ENDS use among heterosexual-identifying youth (panel III) and LGBQ-identifying youth (panel IV). The findings reveal a stark pattern. A one-dollar increase in ENDS taxes is associated with a 2.5 to 3.1 percentage-point (12 to 15 percent) decline in ENDS use among heterosexual-identifying teens. However, for LGBQ-identifying teens, for whom pre-treatment ENDS use in ENDS tax-adopting states was almost 30 percent higher than for heterosexuals (26 percent vs. 20 percent), the effect of ENDS taxes is smaller (in absolute magnitude) and is not statistically significant. In our most fully controlled specification (column 3), we find that a one-dollar increase in ENDS taxes is associated with a non-statistically significant one percentage-point (3.9 percent) decline in ENDS use among LGBQ teens. This coefficient estimate is 60 percent smaller than the corresponding estimate for heterosexual teens. In the final row, we find that the estimated coefficients for heterosexual and LGBQ youths are statistically different at the five percent significance level in the two models with the most controls. These findings are consistent with a less tax-elastic demand for e-cigarettes among LGBQ youth than heterosexual youth.

The remaining six columns of Table 2 explore the effects of ENDS taxes on more intensive margins of e-cigarette use. Frequent ENDS use is the outcome in columns (4) through (6) while everyday ENDS use is the outcome in columns (7) through (9). Our results in Panel III show that a

[12] The average ENDS tax across 2015-2023 State YRBS observations with sexual identity information is $0.36 (in $2023). Thus, a one-dollar tax increase represents a 278 percent increase. Dividing our estimated 10.4 percent reduction by 278 yields the tax elasticity.

one-dollar increase in ENDS taxes leads to a 1.0 to 1.2 percentage-point (25 to 30 percent) reduction in frequent use and a 0.9 to 1.1 percentage-point (32 to 39 percent) decline in everyday use among heterosexual-identifying teens. All six coefficient estimates are statistically significant at conventional levels. For LGBQ teens (panel IV), however, we find no evidence that ENDS taxes lead to statistically significant changes in frequent or everyday ENDS use. In fact, the estimated coefficient is slightly *positive* in all specifications. The estimates for heterosexual and LGBQ teens are statistically different at the ten percent level in all three specifications for frequent use and at the five percent level in all three specifications for everyday use.[13]

5.2 Sensitivity Checks and Tests of Parallel Trends

One concern with our difference-in-differences estimates is that they could be contaminated by heterogeneous and dynamic treatment effects. Table 3 presents stacked difference-in-differences estimates of the effect of ENDS taxes on youth e-cigarette use. The findings are consistent with what we obtained using our fixed effects logit model, suggesting that heterogeneous and dynamic treatment effects are not an important source of bias.

In Table 4, we explore the sensitivity of our estimates to controls for spatial heterogeneity. Specifically, columns (1) through (3) include census region-by-year fixed effects, while columns (4) through (6) include a common-treatment-state linear time trend. The findings continue to suggest that ENDS taxes more strongly affect ENDS use for heterosexual compared to sexual minority teens.

Results from additional sensitivity checks are reported in the appendix. Appendix Table 2 show the robustness of the estimates from the model with full controls to the use of a linear probability model as opposed to logistic regression. In Appendix Figure 4, we explore whether our estimated treatment effects are sensitive to leaving out one treatment state at a time. California is the only state with any meaningful influence, which is not surprising given its large population. In Appendix Figure 5, we report separately the results for gay/lesbian, bisexual, and questioning youths, though the confidence intervals for these subcategories tend to be too wide to draw meaningful conclusions.

[13] We construct the p-values on the differences in the heterosexual youth and LGBQ youth ENDS tax coefficient estimates using a fully interacted logistic difference-in-difference-in-difference model, where an indicator for LGBQ is interacted with the ENDS tax coefficient, all other independent variables, and all fixed effects. The reported p-value results from the hypothesis test that the coefficient of LGBQ x ENDS tax is not equal to zero. Appendix Table 7, discussed later in this section, shows the "main effect" of ENDS taxes on youth along with the coefficient and standard error on the interacted effect of ENDS taxes and an LGBQ indicator.

Appendix Table 3 drops 2021 to eliminate any potential confounding from the pandemic. Notably, due to the biennial nature of the YRBS, no surveys were conducted in 2020, the year in which the impacts of the pandemic in terms of both policies targeting teens (e.g., school closures) and general behavioral responses (e.g., social distancing) were arguably most prevalent. In 2021, due to the lingering effects of the pandemic, YRBS surveys were fielded in the fall, which further suggests that the YRBS data are less likely to be impacted by pandemic effects on survey responses than other surveys with more frequent data collection (e.g., the monthly Current Population Survey). The results are similar to the main estimates with one exception: ENDS taxes now reduce any current ENDS use among LGBQ teens by a statistically significant 2.2 percentage points. However, the effect on heterosexual teens is still notably larger (3.6 percentage points), and the difference between these estimates is statistically significant (p=0.02). Moreover, the effect on LGBQ teens is within the confidence interval of the corresponding estimate from Table 2.

Appendix Table 4 addresses the possible concern that the inherent nonlinearity of the logit model might lead to misleading estimates when using the relatively large unit of $1.00 for the treatment variable. We run regressions using $0.01 as the unit for taxes, then multiply the resulting estimates by 100 to predict the effect of a $1.00 increase. The estimates are very similar to the results with full controls in Table 2.

A possible concern with our conversion of all ENDS taxes to excise taxes – which assumes a constant retailer markup – is that the true retailer markup may vary across states. Such differences in markup rates could be driven by heterogenous degrees of competition in ENDS product markets across states. If there is heterogeneity in retailer markups, this phenomenon could lead to bias in the taxes that are converted (i.e., the ad valorem taxes). Cotti et al (2021; 2023) report that markups appear to be quite similar across the U.S. and that at least some major ENDS companies use a standard markup rate across the country, these findings suggest that the assumption of a constant markup rate is reasonable. However, to explore this possibility, we separate ENDS taxes into excise and ad valorem taxes. Results, reported in Appendix Table 5, are robust and thus imply that our assumption of a constant retailer markup does not lead to bias in tax conversion.

In Appendix Table 6, we add the National YRBS to create a "combined" sample. There are still noteworthy differentials between the effects on heterosexual and LGBQ youths, though the results are somewhat smaller and less precisely estimated than the State YRBS results. However, the National YRBS is not designed to be representative at the level of the state – the level of treatment. Using such data in difference-in-differences analyses can lead to bias in coefficient estimates as

shown in simulation work by Maclean et al (2023) and Tello-Trillo et al (2025). Because the sample is not drawn to be representative at the level of the state, state-level samples can be unrepresentative of the underlying state populations, thereby providing an inaccurate estimate of the average treatment effect if there is heterogeneity across residents within the state. This issue is likely exacerbated in our setting, which stratifies the sample based on sexual orientation and therefore reduces sample sizes. The number of LGBQ-identifying respondents in many treatment states in a given year is very small (mean = 89) compared to the State YRBS (mean = 855). Additionally, the National YRBS includes Minnesota, which does not participate in the State YRBS. Minnesota is "always treated" during our study period, and always-treated units only contribute "forbidden comparisons" in TWFE regressions that tend to attenuate the estimate of the treatment effect.

Appendix Table 7 reports results from using the full sample (with sexual identify information) and estimating a difference-in-difference-in-differences (DDD) model that includes both ENDS tax and the interaction of ENDS tax with an indicator for LGBQ. The ENDS tax marginal effects, which represent the effects among heterosexual teens, are very similar to the marginal effects for heterosexual teens from Table 2. The estimated effects on LGBQ teens, which are given by the sum of the two reported coefficients, are also similar to the baseline results. The interaction term is statistically significant at the five percent level for two of the three outcomes and the ten percent level for the third, indicating discernable differences between the effects on the two groups.

Figure 3 shows results from event-study analyses for heterosexual and LGBQ teens. The figure contains three panels, one corresponding to each outcome. Since the YRBS data are biennial, we use two-year time bins of three to four years before treatment, one to two years before, and zero to one year after, along with bins for five or more years before and two or more years after. Each graph within the figure displays marginal effects and 95 percent confidence intervals from twelve different variations of the event-study model. Those labeled "TWFE" use two-way fixed effects linear regression with "one to two years before treatment" used as the reference period. "Logit" indicates logistic regression with the same reference period. The specifications labeled "constrained" use the average of the pre-treatment coefficients as the reference point, as suggested by Miller (2023). "BC" means baseline controls are included, while "LASSO" means the controls chosen by

the LASSO method are also included. For the TWFE and logit approaches, we also report results using stacked regression.[14]

Given the large number of variants of the event-study model shown and the relative imprecision of some of the estimates, we look for overall patterns rather than fixating on individual coefficient estimates. Column I shows the results for heterosexual teens. In Panel a, we see, if anything, a slight upward trend in the pre-treatment period for current ENDS use. In contrast, a sharp reduction emerges in the first post-treatment period (zero to one year after), with eleven of the twelve estimates in that period being statistically significant at the five percent level and the other one nearly being significant. The effect appears to weaken somewhat after two or more years, but all but one of the twelve estimates remains negative, and most are still statistically significant. For both frequent and everyday use (Panels b and c), there is even less evidence of any pre-trend. Across the two outcomes, all 24 estimates in the "zero to one year after treatment" time bin are negative, and 22 are statistically significant at the five percent level. The effect sizes appear to shrink slightly two or more years after treatment, but 20 of the 24 coefficient estimates are still negative and 16 of those 20 are significant. Therefore, the event-study results support the conclusion that ENDS taxes reduce ENDS use among heterosexual teens.

Column II shows that the results are quite different for LGBQ youth. For current ENDS use (Panel a), the pre-treatment trend is essentially flat. The post-treatment evidence is consistent with no effect, as there is a mix of positive and negative signs, most coefficient estimates are close to zero, and only one estimate out of 24 is statistically significant at the five percent level, which is about the false rejection rate that would be expected under a true null. For frequent use (Panel b), the pre-trend is either flat or slightly negative depending on the specification. There is a mixed pattern of signs in the post-treatment period. Only two of the 24 estimates are statistically significant, and both of those are in the opposite direction of the expected reduction. Those two positive and significant results are for the two logit stacked specifications, which are the rightmost ones shown on the graph. In both cases, those results appear to be driven by the base period being an outlier in the negative direction, rather than a counterintuitive causal effect. For everyday use (Panel c), there are more specifications with a downward pre-trend, and those tend to be accompanied by a significant positive coefficient in at least one of the post-treatment periods. In the specifications where we do not observe a downward pre-trend, there is no evidence of a post-

[14] Running a "constrained" variant of the stacked regression is not feasible due to the Stata command -cnsreg- being unable to accommodate such a large number of fixed effects.

treatment effect in either direction. Therefore, the anomalous positive results are likely driven by mean reversion rather than causality. In short, the overall pattern of results is consistent with there being no clear effect of ENDS taxes on the vaping behavior of LGBQ teens in either direction, at least during the timeframe of our study.

5.3 Spillover Effects to Combustible Tobacco Products

The above findings show consistent evidence that ENDS taxes reduce ENDS use among heterosexual but not LGBQ teens. We next explore spillover effects to combustible tobacco smoking. Given our findings above, unless there are independent channels other than price effects through which ENDS taxes could affect combustible tobacco product use — for instance, by signaling relative health effects or social acceptability of e-cigarettes versus combustible tobacco products — ENDS taxes would only be expected to affect cigarette (or cigar) smoking among heterosexual-identifying teens, as there were no "first-stage" effects on tobacco use among LGBQ-identifying students.

Table 5 presents logistic regression estimates of the effects of ENDS taxes on cigarette and cigar smoking among heterosexuals and LGBQ-identifying students for the 2015-2023 period that mirrors our ENDS analyses (Panel I) and the entire 2011-2023 period for which combustible smoking data are available (Panel II). We find no evidence that ENDS taxes induce substitution towards combustible tobacco use for either heterosexual or LGBQ teens. The coefficient estimates have a mixed pattern of signs for heterosexual youth. For LGBQ youth, ENDS taxes are statistically significantly related to decreased cigarette and/or cigar consumption at the five percent level in three of the 12 regressions, and always in the negative direction – which would imply complementarity rather than substitutability.

This pattern of findings is somewhat at odds with the earlier literature which demonstrates that teens may substitute to combustible tobacco products as ENDS taxes rise. We emphasize that not finding evidence of substitution is not the same as conclusively showing there is no substitution. Most of the smoking variables have low sample rates (as low as 1.1 percent), complicating our ability to detect plausible amounts of substitution with statistical precision. To illustrate, consider the result for everyday cigarette smoking in the 2011-2023 sample (Panel 1, column 3). The 95 percent confidence interval reaches as high as 0.6 percentage points, which sounds small but is still a sizeable 38 percent of the sample rate of 1.6 percent.

Appendix Figure 6 shows results for an event-study analysis using the longer time span of 2011-2023 and logistic regression with baseline controls. Results from other variants of the event-study are similar. The graph shows relatively flat pre-treatment trends and post-treatment effects consistent with those from the main regression results.

5.4 Intersectionality by Sexual Identity and Demographic Characteristics

In Figure 4, we explore heterogeneity in the effects of ENDS taxes, focusing on the intersection of sexual identity with both gender and race/ethnicity.[15] Panel (a) shows little evidence of differential effects of ENDS taxes across gender and race among heterosexual teens. For LGBQ teens, the most notable finding is that the estimated effect is around four percentage points for males but essentially zero for females. However, the estimate for males is still not statistically significant at the five percent level, and its confidence interval contains the estimate for females.

Panels (b) and (c) contain results for the more intensive vaping outcomes. Heterogeneity across subgroups is relatively mild for heterosexual teens, as they all exhibit significant or nearly significant reductions of around two percentage points or less. There is no evidence that ENDS taxes reduce frequent or everyday vaping among any subgroup of LGBQ teens, with most of the estimates actually being positive.

5.5 Other Tobacco Policies

Recall that, based on the minority stress faced by LGBQ teens combined with economic models of addiction summarized by Cawley and Ruhm (2011), we predict that LGBQ teens' responses to ENDS tax increases would be smaller and/or more gradual than those of heterosexual teens. The findings from Sections 5.1 and 5.2 are consistent with this hypothesis, as ENDS taxes reduce vaping among heterosexual youth soon after implementation but have no discernable effect on LGBQ youth – at least during the relatively short-run time frame that we are able to examine. Therefore, even if long-run effects on LGBQ teens emerge in subsequent years, this finding would still fit the hypothesis because the effects would be delayed relative to those on heterosexuals.

The primary focus of our study has been ENDS taxes, both because they are the most widely studied ENDS policy and because economic models of addiction tend to focus on price-based interventions. However, intuitively, the minority stress hypothesis should apply to other types

[15] Appendix Figures 7 and 8 conduct similar analyses for cigarette smoking and cigarette or cigar smoking, respectively.

of tobacco control policies as well. In contrast, other potential mechanisms such as income effects are directly related to prices and therefore would not likely apply to non-price-based interventions. Moreover, while our emphasis has been on ENDS use because that is the more acute concern regarding teen nicotine use, the same hypothesis should apply to other sources of nicotine such as cigarettes.

Therefore, an indirect test of the minority stress hypothesis is to evaluate whether other ENDS policies exhibit a similar pattern of differential impacts by sexual orientation on ENDS use and whether cigarette policies do the same for cigarette smoking. Of course, some of these policies might not affect vaping or smoking for any group, which would neither confirm nor refute the minority stress hypothesis. Consequently, in this section we examine three policies for which the evidence is particularly strong that they reduce consumption of the intended nicotine product among teens, the primary age group that we examine in this study. For ENDS, these policies are MLSAs (e.g., Dave et al. 2019; Pesko 2023) and flavor bans (e.g., Cotti et al. 2025; Friedman et al. 2024a and 2024b; Saffer et al. 2024).[16] For cigarettes, we examine cigarette taxes, as that is the policy with the most empirical support for lowering youth smoking rates (e.g., DeCicca et al. 2002; Carpenter and Cook 2008).

Importantly, though, whether we should expect to see an effect of cigarette taxes on youth smoking during our particular sample period is not obvious. While Carpenter and Cook (2008) find a strong negative impact of cigarette taxes on teen smoking using YRBS data from 1991 to 2005, Hansen et al. (2017) show that adding 2007-2013 data weakens this finding, and that there is no evidence of an effect at all when focusing on only 2007-2013. Courtemanche et al. (2020) provide evidence that this is due to a diminishing marginal effect of cigarette taxes on youth smoking; from the already high baseline tax rates common in 2007, further increases fell on the flat of the curve.

[16] State T-21 laws also have some empirical support for influencing teen ENDS use in the literature (Hansen et al. 2023), but how to analyze them as a variable of interest with data through 2023 is not obvious because of the ambiguous timing of the Federal T-21 law. The Federal law was signed in December 2019, but the Food and Drug Administration's final rule was not issued until August 2024, and retailers had until September 2024 to comply (U.S. Food and Drug Administration 2024). Therefore, how to define a clean pre and post period for a T-21 variable in our study period is not clear. We take the former approach when using the T-21 law as a control variable, though the results for ENDS taxes are not meaningfully altered if we use the latter approach. However, neither approach yields stable or sensible coefficient estimates for the T-21 variable. Moreover, a recent study by Cotti et al. 2024b) offers suggestive evidence – again using data prior to the Federal law – that state-level T-21 policies may reduce teen reported use of tobacco products, but biomarkers for exposure to tobacco products (e.g., cotinine) do not change following adoption of such a law, which suggests that the effects of T-21 laws are complex. Given these factors, we do not include T-21 laws as a focal variable in this section.

Accordingly, results shown in Table 6 provide no evidence that cigarette taxes reduce youth smoking in our sample. This is true for both heterosexual and LGBQ teens, and using both the 2011-2023 and 2015-2023 sample periods, and all three cigarette smoking outcomes (current, frequent, and everyday smoking). All twelve estimates are very small, and the only two that are statistically significant are in the *positive* direction. Given the absence of an effect on heterosexual youth, the lack of an effect on LGBQ youth is uninformative in terms of testing the minority stress hypothesis.

Table 7 turns to the ENDS use outcomes and reports the marginal effects for cigarette tax, ENDS flavor bans, and ENDS MLSA. The estimated effects of cigarette tax on current, frequent, and everyday ENDS use are all small and not statistically significant. This is unsurprising given the absence of reductions in smoking.

The pattern of results for MLSAs is the same as for ENDS taxes, providing further support for the minority stress hypothesis. For heterosexual teens, MLSAs have negative, statistically significant, and meaningfully large effects of around three percentage points on current ENDS use and around two percentage points on frequent and everyday use. In contrast, the marginal effects for LGBQ teens are slightly positive and not statistically significant. The differences in the effects on the heterosexual and LGBQ samples are all statistically significant at the five percent level.

For flavor bans, the picture is murkier. All marginal effects are negative for both the heterosexual and LGBQ samples, with magnitudes ranging from 0.5 to 2.3 percentage points. The magnitudes are larger for LGBQ youth, but the differences in estimates between the two samples are never close to statistically significant. None of the individual estimates are statistically significant at the five percent level, and only one – a 1.9 percentage point reduction in everyday ENDS use among LGBQ teens – is significant at the ten percent level.

Figure 5 shows event study graphs for ENDS flavor bans, which help clarify the above results. Column I shows the results for heterosexual youths. In multiple specifications, flavor bans lead to statistically significant and sizeable reductions in habitual vaping in the first post-treatment period that disappear after two or more years. In contrast, for LGBQ youth, in the specifications where significant post-treatment effects emerge, it is always in the *latter* post-treatment period. There is no evidence in any specification that LGBQ teens are responsive to flavor bans until two or more years later. In other words, the effect on LGBQ teens is more delayed than that on heterosexual teens – which supports the minority stress hypothesis. With that said, wide confidence intervals and

sensitivity of the results to different specifications limit our ability to draw more definitive conclusions.

By comparison, the event-study graphs for MLSAs, shown in Figure 6, are quite clear. Pre-treatment trends are relatively flat for both heterosexual and LGBQ youths. Meaningfully large impacts on all three vaping outcomes emerge for heterosexual youths in the first post-treatment period and persist in the second period. In contrast, post-treatment estimates for LGBQ teens center tightly around zero. This pattern of results mirrors that for ENDS taxes and is consistent with the minority stress hypothesis.

5.6 Heterogeneity by Markers of Mental Stress

This section provides a more direct test of the minority stress hypothesis. Homophobia-driven social pressures and the unique psychological trauma of being a sexual minority likely affects some LGBQ teens more strongly than others. If sexual minorities are statistically unresponsive to ENDS taxes as a group because they face disproportionate mental health challenges, then LGBQ youths who *do not* face these challenges should exhibit a more similar response to heterosexual youths. Following a similar approach to Carpenter and Sansone's (2021) analysis of cigarette taxes, we explore whether the effects of ENDS taxes and the other policy variables explored in Section 5.5 vary with persistent sadness/depression, bullying victimization, or suicidal ideation. The first and third are direct indicators of mental health, while bullying victimization has been linked to depressive symptomatology and suicidal behaviors among LGBQ youth (Liang et al. 2023; Rees et al. 2022). For simplicity, we categorize individuals as facing mental-health-related challenges if they answer affirmatively to any of these questions. While splitting the sample based on mental-health-related variables could be concerning given that mental health could be endogenously affected by ENDS taxes, perhaps through nicotine effects (Kutlu and Gould 2015a; Kutlu et al. 2015b), auxiliary analysis results shown in Appendix Table 8 do not provide evidence that ENDS taxes influence the probability of facing any of these challenges.

Table 8 reports the results for current ENDS use. The sample rates alone are instructive. Seventy three percent of LGBQ youths have at least one of the mental-health-related conditions, compared to only 43 percent of heterosexual youths.[17] Regardless of sexual orientation, vaping rates

[17] More specifically, in the State YRBS data, we find that LGBQ teens are 100 percent more likely to report depression (60 percent vs. 30 percent), 186 percent more likely to report having considered suicide (40 percent vs. 14 percent), and 81 percent more likely to report bullying victimization (38 percent vs. 21 percent) than their heterosexual peers.

are much higher (by around 12 percentage points) among those who report any conditions. After stratifying by mental health, the vaping rates for LGBQ and heterosexual youth are similar, implying that mental health explains nearly the entire discrepancy in ENDS use by sexual orientation.

Columns (1) and (2) contain the regression results for LGBQ teens, while (3) and (4) do the same for heterosexual teens. Among LGBQ individuals who report persistent sadness, bullying victimization, or suicidal ideation, ENDS taxes are associated with no observable effect (marginal effect that rounds to -0.000) on the probability of any current ENDS use. In contrast, when we examine LGBQ individuals who do not report any of these conditions, a one-dollar increase in ENDS tax is associated with a large and statistically significant (at the ten percent level) 4.6 percentage point (28.4 percent) decline in ENDS use. The difference between the estimates by mental health status is statistically significant at the five percent level). In contrast, ENDS taxes lead to sizeable and statistically significant (at the five percent level or better) reductions in the likelihood of any ENDS use for both subsamples of heterosexual youth, with the difference between these estimates being far from statistically significant.

The results for the remaining policies are shown in the next three rows. For cigarette taxes, there continues to be no evidence of an effect on any subsamples.[18] For ENDS flavor bans, the estimates are again more strongly negative for LGBQ teens with no mental-health-related problems than for LGBQ teens with such problems. The effect on those without such problems is large, 5.2 percentage points, and statistically significant at the one percent level. The difference between the coefficient estimates is almost significant at the five percent level (p=0.051). For heterosexual teens, the estimated effect is also negative for both subsamples and larger in magnitude for those without mental health problems, though neither individual estimate nor the difference between the estimates is significant. For MLSAs, there are large and significant estimated effects on both subgroups of heterosexual teens but small and insignificant estimated effects on both subgroups of LGBQ teens. Nonetheless, the coefficient estimate for LGBQ teens without mental health problems is more negative than that for LGBQ teens with such problems, which is consistent with the results for the other ENDS policies.

In short, for LGBQ teens, the overall null effects observed in previous sections appear to be driven by those who report mental health problems. A large enough percentage of these teens have such problems to drive null results for the entire LGBQ sample, despite those without these

[18] Also, there is also no evidence that cigarette taxes reduce the probability of being a cigarette smoker for any subsamples. These results are available in Appendix Table 9.

problems exhibiting policy responsiveness. In contrast, heterosexual teens with mental health problems exhibit roughly the same responsiveness to ENDS policies as those without these problems. This pattern of results is consistent with the unique stressors facing queer teens leading to greater dependence on vaping.

5.7 Auxiliary Analysis of Adults in BRFSS

Finally, we explore whether the differential effects of ENDS taxes by sexual identity exist for adults as well as teens. Given that reasons for use of e-cigarettes may differ between adults and youths (e.g., smoking cessation tool vs. first nicotine product), patterns of differential tax responsiveness could also differ. In particular, LGBQ adults and teens likely face different types of stressors, which could explain why Carpenter and Sansone (2021) find an opposite pattern of heterogeneity in the effect of cigarette taxes on adult partnered male smoking than we did for the effect of ENDS taxes on vaping for youth. We separately analyze younger (18-30 years old) and older (31+ years) adults given the literature suggesting that younger adults vape more and are more tax sensitive (Dave et al. 2022; Dave et al. 2024; Friedman and Pesko 2022).

Our data source for adults is the 2016-2023 BRFSS. Table 9 reports the results. Following Carpenter and Sansone (2021), we use sampling weights and cluster our standard errors at the state level. We first observe that pre-tax vaping rates are much lower for young adults than youth (e.g., 13 percent current use rate for LGBQ 18–30-year-olds compared to 26 percent for LGBQ youth) and even lower for adults over 30 years (e.g., 5.2 percent for LGBQ adults). These lower baseline rates limit the potential impacts of taxes. Perhaps for this reason, we observe no statistically significant evidence that ENDS taxes reduce ENDS use for any of the combinations of outcomes, sexual orientations, and ages. Nonetheless, the marginal effects are negative for heterosexual adults in seven of the eight columns and positive for LGBQ adults in all eight, which is somewhat consistent with the pattern observed for teens. We cannot rule out meaningfully large effect sizes relative to the pre-tax sample rates, particularly for heterosexuals. For instance, in the specification with full controls and any current use as the outcome (column 2), the 95 percent confidence interval includes reductions up to 2.3 percentage points, which is 23 percent of the sample usage rate of 10.2 percent.

Event-study estimates for 18–30-year-olds are presented in Appendix Figure 9, while those for adults over 30 years are in Appendix Figure 10. There is some evidence of an upward pre-treatment trend for heterosexual young adults. After adjusting for this pattern of results via the event-study framework, ENDS taxes lead to statistically significant reductions in the vaping

outcomes of around 1.5-2 percentage points in four out of six specifications. There is no discernable evidence of either pre-treatment trends or post-treatment effects for heterosexual adults over 30 years or for LGBQ adults in either age range in any of the event-study models. The post-treatment estimates for LGBQ adults are all *positive* in both figures, but the confidence intervals are quite wide and none of these estimates are statistically significant.

6. Conclusions

Despite dramatic social progress and greater social acceptance over the last two decades (Gallup 2024b), LGBQ youth and young adults continue to face many societal, familial, and schooling challenges that may increase stressors and adversely affect their mental health. There is strong evidence that minority stress as well as barriers to effective psychological treatment and support services for LGBQ youth may cause a turn to risky addictive behaviors as a means to escape or cope with psychological trauma, as well as to socialize and build queer community. Tobacco use is one such addictive behavior.

The prevalence of ENDS (cigarette) use is 55 (108) percent higher among LGBQ-identifying teens relative to their heterosexual-identifying counterparts. This study provides, to our knowledge, the first empirical investigation of whether the impact of ENDS taxes on ENDS use is unique for sexual minorities. Using data from the State YRBS over the period 2015-2023, our findings show that a one-dollar increase in ENDS taxes is associated with around a three percentage-point decline in current ENDS use and one percentage-point declines in frequent and everyday use among heterosexual-identifying youths. In contrast, there is no evidence of effects on any of these outcomes among LGBQ-identifying teens.

We also conduct a number of auxiliary analyses. First, we find that responsiveness to two other important ENDS policies – MLSA laws and flavor bans – is also weaker or slower among LGBQ youth than heterosexual youth. This analysis helps to rule out mechanisms that would only apply to taxes, such as income effects. Next, evidence suggests that the lack of clear effects of ENDS policies on LGBQ youth is driven by null results among those who report mental-health-related problems, whereas heterosexual youth reporting the same challenges respond more similarly to those without those challenges. Additionally, we find no evidence that ENDS taxes affect either combustible smoking or the vaping behavior or adults – though the results are not precise enough to rule out meaningful impacts.

Of course, our analyses have limitations. First, the evidence being consistent with the minority stress hypothesis does not mean that our analysis conclusively confirms the hypothesis. Next, subsamples of LGBQ individuals in survey-based dataset are relatively small, and statistical power is at a premium when quasi-experimental methods are used with state-level treatments – particularly those that correlate with other related state-level policy changes. These factors combine to lead to results that can be, at times inconclusive. Additionally, since ENDS taxes are relatively recent, our estimates can only be interpreted as short-to-medium-run effects. Long-run effects could differ, as economic models of addiction predict that the effects of price changes will evolve over time. Finally, YRBS surveys are conducted during the school year. Impacts of ENDS taxes could be different during the summer when LGBQ teens do not face in-school bullying. Much therefore remains to be learned about how sexual orientation and ENDS policies such as taxes interact to influence patterns of tobacco use.

Together, our findings provide suggestive evidence to support the minority stress hypothesis. ENDS use may be a stress-induced coping strategy utilized by LGBQ teens in response to barriers to psychological services or unique trauma, such as from bullying. To the extent that this is true, improving mental health of LGBQ youth or reducing bullying may increase tax sensitivity. This pattern may point towards practical instruction on healthy tools to manage the consequences of minority stress, as well as effective outreach for and provision of LGBQ-inclusive psychological services.

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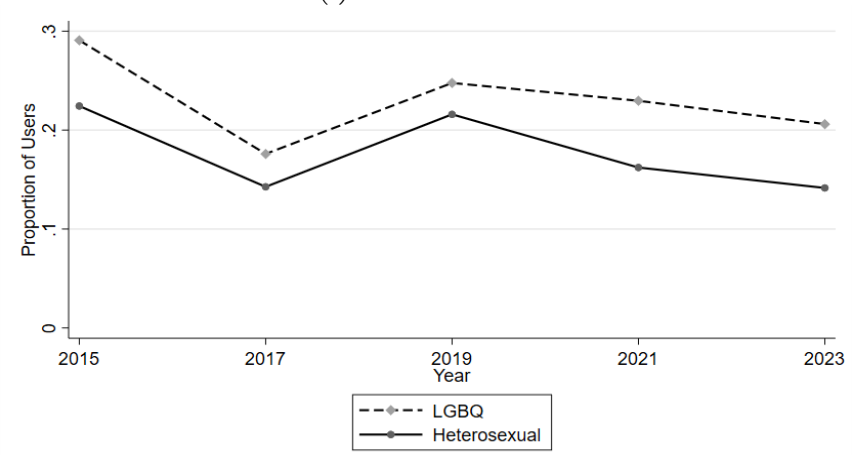
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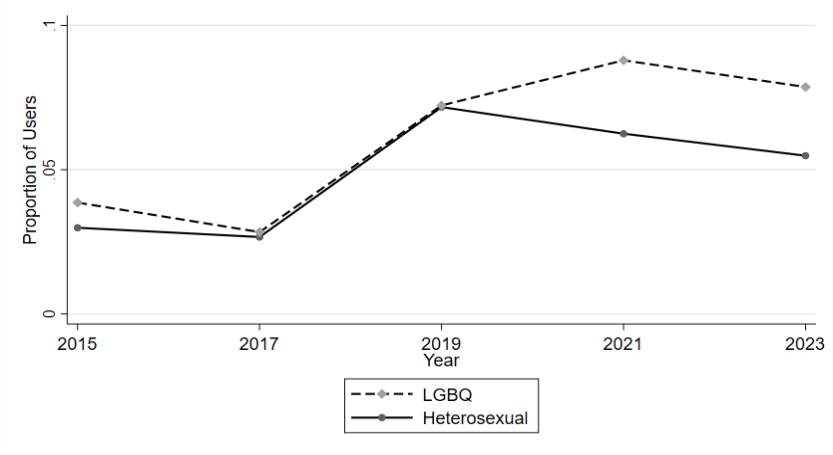
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Figure 1. Trends in Prior-Month Youth ENDS Use, 2015-2023, by Sexual Identity

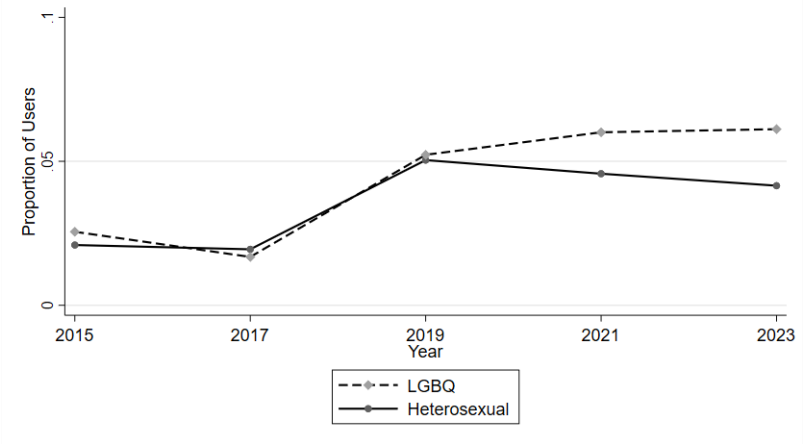
Panel (a): Current ENDS Use



Panel (b): Frequent ENDS Use



Panel (c): Everyday ENDS Use



Notes: Weighted estimates are drawn from the State Youth Risk Behavior Surveys from 2015-2023.

Figure 2. Geographic and Temporal Variation in ENDS Taxes



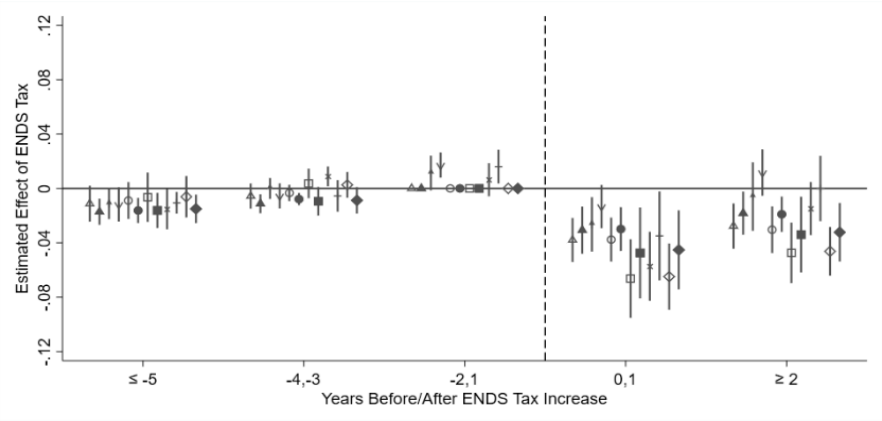
Notes: ENDS taxes are from Cotti et al. (2023). The annual average ENDS tax is calculated for each state and reported above.

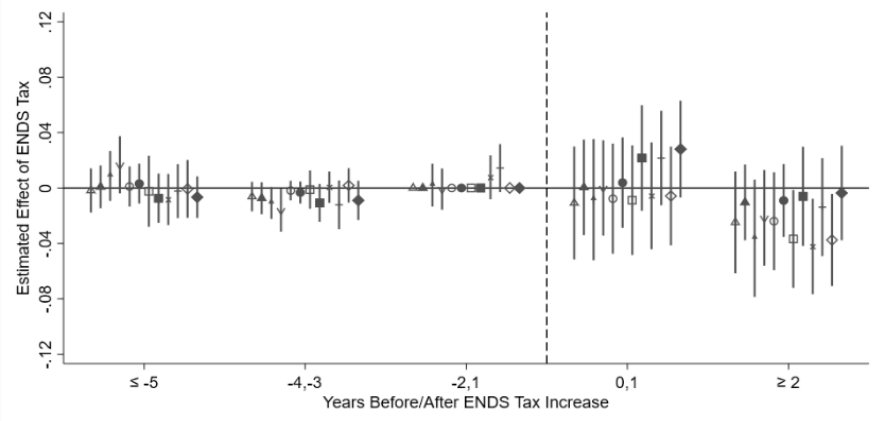
Figure 3. Event-Study Analyses of the Effect of ENDS Taxes on ENDS Use, by Sexual Identity, Various Model Specifications, 2015-2023

(I) Heterosexual-Identifying Youth

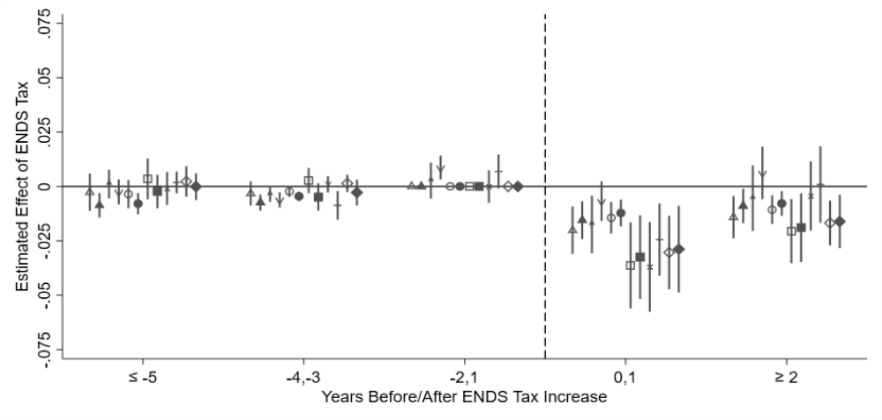
(II) LGBQ-Identifying Youth

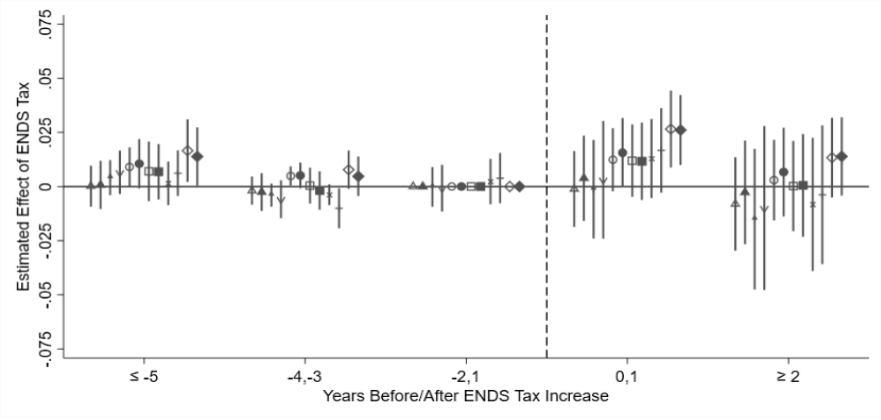
Panel (a): Current ENDS Use





Panel (b): Frequent ENDS Use





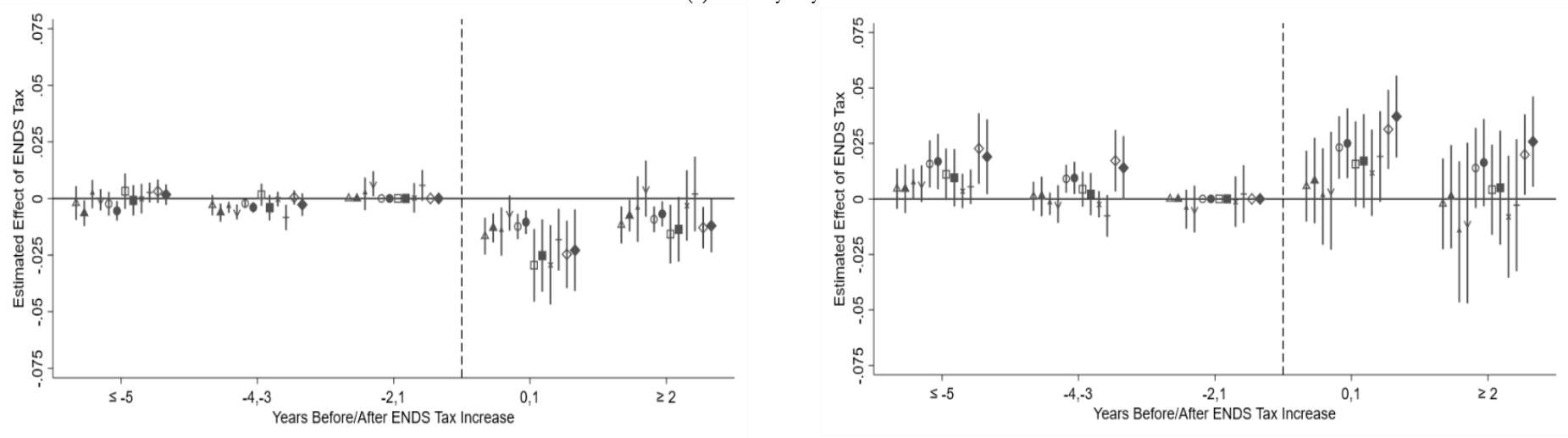
Legend

△ TWFE, BC ▲ TWFE, LASSO □ TWFE Stacked, BC ■ TWFE Stacked, LASSO
+ Constrained, BC ∨ Constrained, LASSO × Constrained Stacked, BC + Constrained Stacked, LASSO
○ Logit, BC ● Logit, LASSO ◇ Logit Stacked, BC ◆ Logit Stacked, LASSO

Figure 3, Continued

(I) Heterosexual-Identifying Youth (II) LGBQ-Identifying Youth

Panel (c): Everyday ENDS Use



Legend

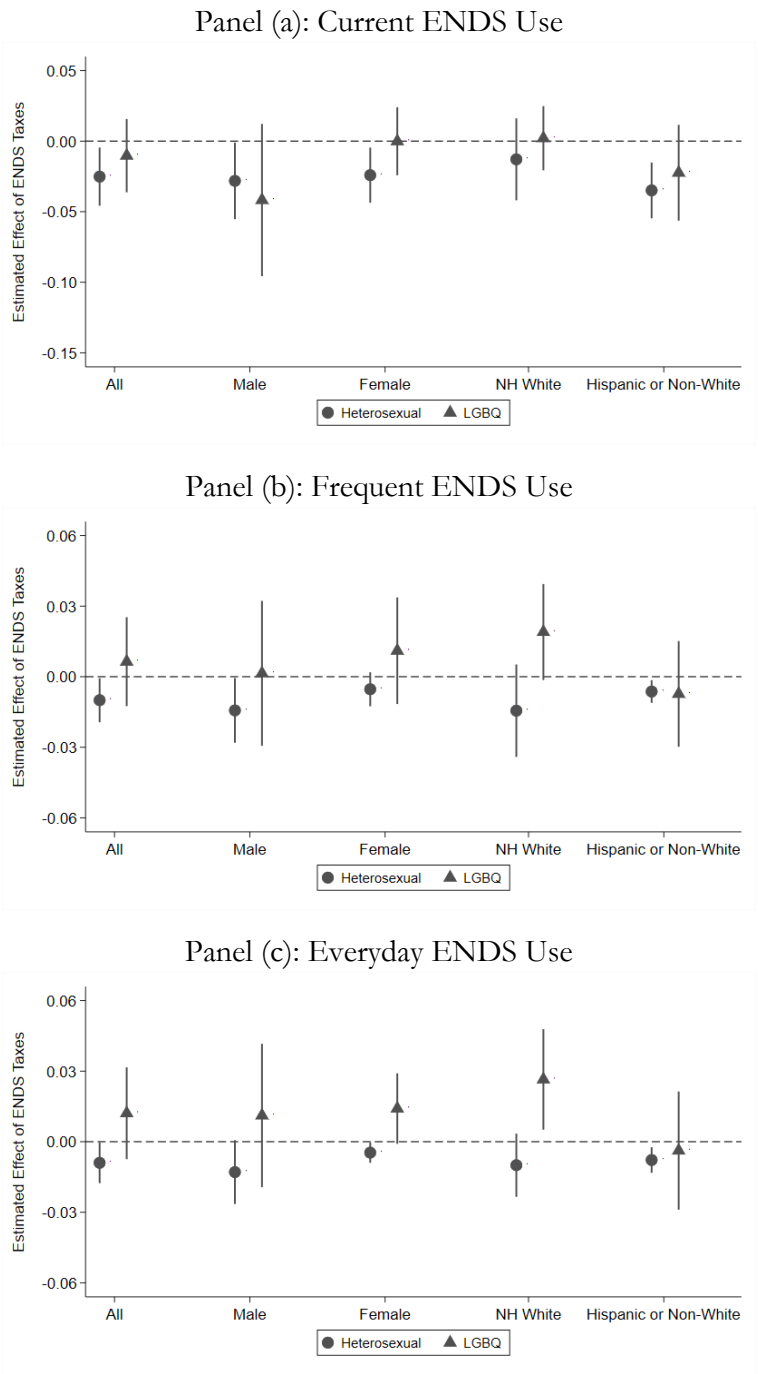
△ TWFE, BC ▲ TWFE, LASSO □ TWFE Stacked, BC ■ TWFE Stacked, LASSO

+ Constrained, BC ∨ Constrained, LASSO × Constrained Stacked, BC + Constrained Stacked, LASSO

○ Logit, BC ● Logit, LASSO ◇ Logit Stacked, BC ◆ Logit Stacked, LASSO

Notes: These data are drawn from the State Youth Risk Behavior Survey from 2015-2023. Standard errors are clustered at the state level. Except for "Constrained" regressions, the reference period is 1-2 years prior to an ENDS tax increase. "TWFE" denotes two-way-fixed effects estimates obtained using the approach outlined by Miller (2023) in Serrato and Zidar (2016), "BC" denotes baseline controls, "LASSO" denotes controls for LASSO-selected covariates and the LGBT policy index, "Stacked" denotes stacked difference-in-differences estimates, "Logit" denotes logistic regression estimates, and "Constrained" denotes event study estimates obtained using the average of the pre-treatment periods as the reference period. 95% confidence intervals are shown as vertical lines.

Figure 4. Exploring Intersectionality in Treatment Effects, by Sexual Identity & Gender and Sexual Identity & Race, State YRBS, 2015-2023

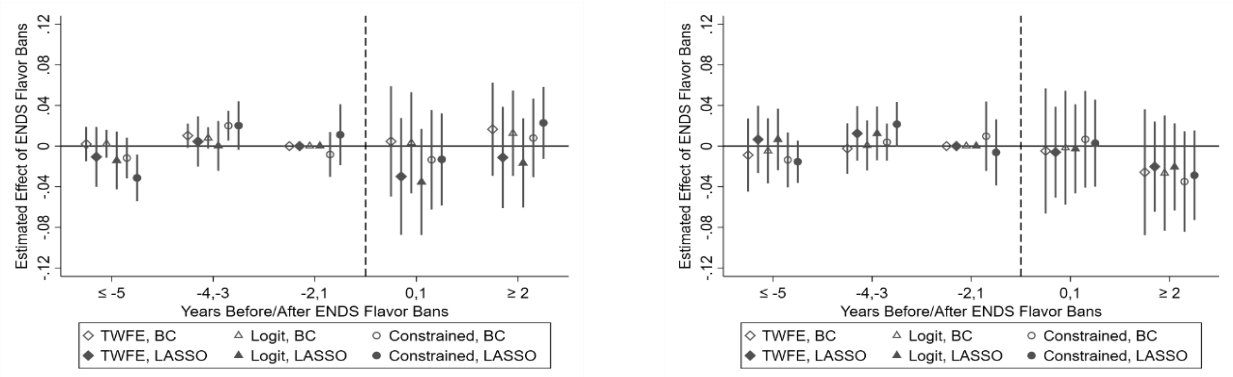


Notes: These data are drawn from the State Youth Risk Behavior Survey from 2015-2023. Average marginal effects are estimated using weighted logistic regression (with 95% CIs), and standard errors are clustered at the state level. Controls include state, year, and semester fixed effects, demographic controls, macroeconomic and COVID-19 controls, flavored ENDS restrictions, ENDS minimum legal sales age laws, Tobacco-21 Laws, and cigarette taxes (in 2023$). The x-axis label "NH White" refers to Non-Hispanic White individuals.

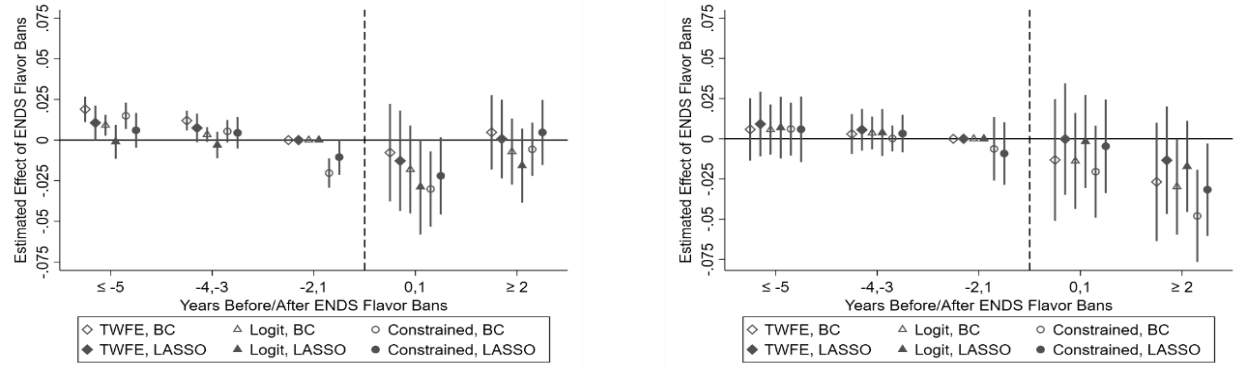
Figure 5. Event-Study Analysis of ENDS Flavor Restrictions and ENDS Use, by Sexual Identity, 2015-2023

(I) Heterosexual-Identifying Youths (II) LGBQ-Identifying Youths

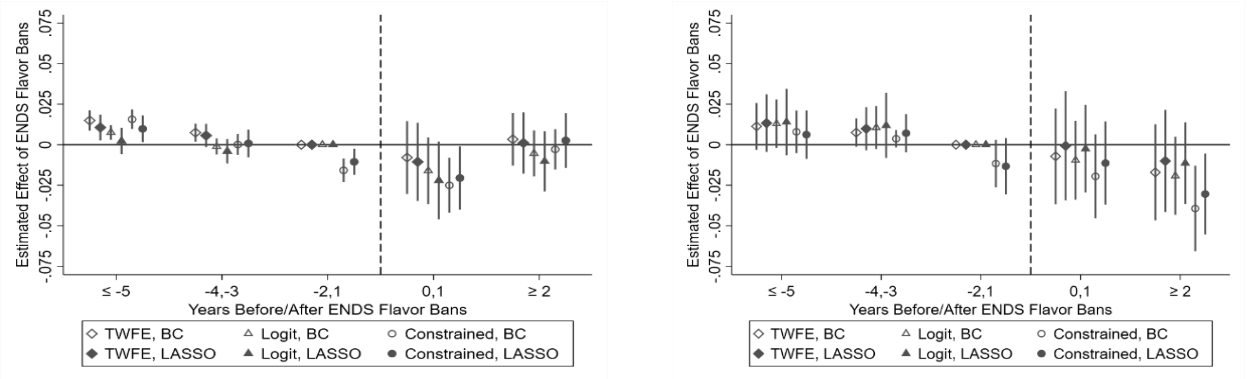
Panel (a): Current ENDS Use



Panel (b): Frequent ENDS Use



Panel (c): Everyday ENDS Use

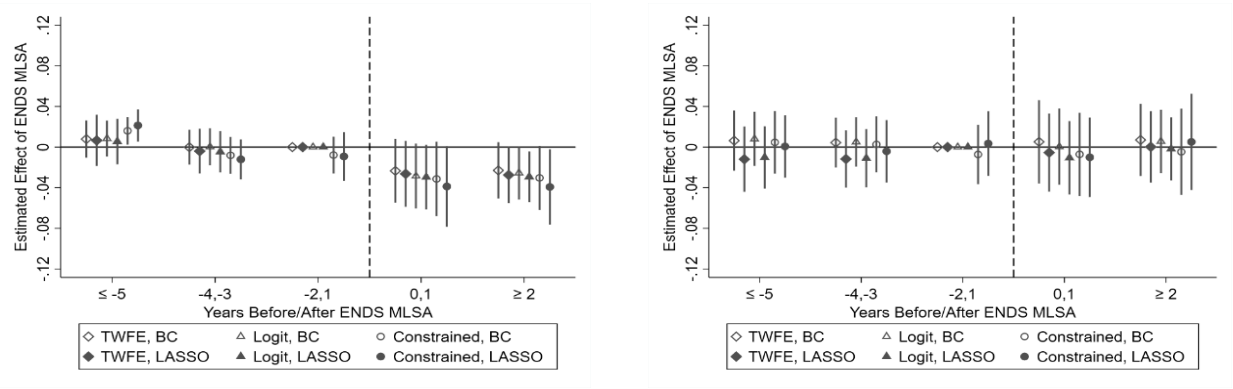


Notes: These data are drawn from the State Youth Risk Behavior Survey from 2015-2023. Standard errors are clustered at the state level. Except for "Constrained" regressions, the reference period is 1-2 years prior to the adoption of ENDS flavor restrictions. "TWFE" denotes two-way-fixed effects estimates obtained using approach outlined by Miller (2023) in Serrato and Zidar (2016), "BC" denotes baseline controls, "LASSO" denotes controls for LASSO-selected covariates and the LGBT policy index, "Logit" denotes logistic regression estimates, and "Constrained" denotes event study estimates obtained using the average of the pre-treatment period as the reference period. 95% confidence intervals are shown as vertical lines.

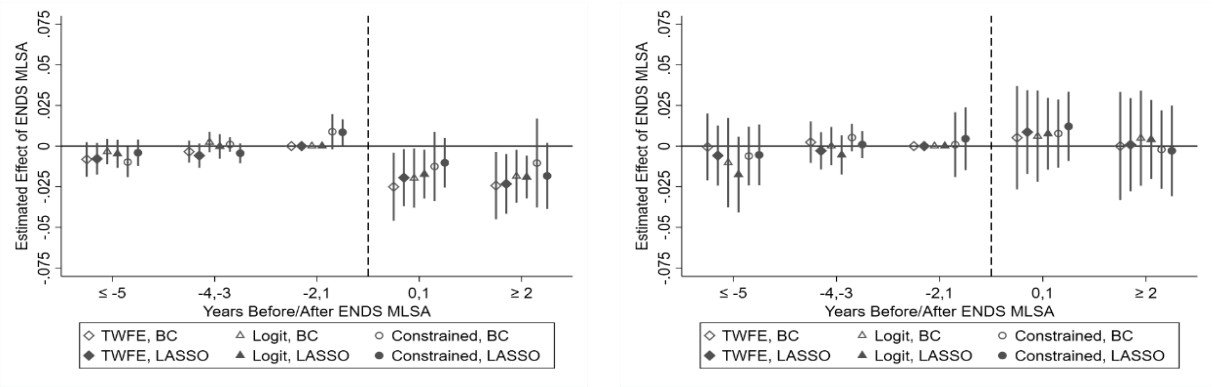
Figure 6. Event-Study Analysis of MLSA Adoption and ENDS Use, by Sexual Identity, State YRBS, 2015-2023

(I) Heterosexual-Identifying Youths (II) LGBQ-Identifying Youths

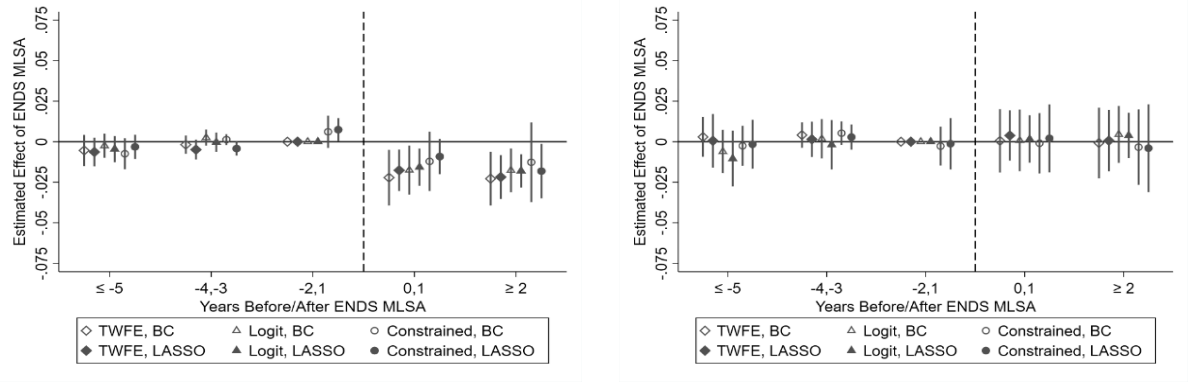
Panel (a): Current ENDS Use



Panel (b): Frequent ENDS Use



Panel (c): Everyday ENDS Use



Notes: These data are drawn from the State Youth Risk Behavior Survey from 2015-2023. Standard errors are clustered at the state level. Except for "Constrained" regressions, the reference period is 1-2 years prior to the adoption of ENDS MLSA laws. "TWFE" denotes two-way-fixed effects estimates obtained using approach outlined by Miller (2023) in Serrato and Zidar (2016), "BC" denotes baseline controls, "LASSO" denotes controls for LASSO-selected covariates and the LGBT policy index, "Logit" denotes logistic regression estimates, and "Constrained" denotes event study estimates obtained using the average of the pre-treatment period as the reference period. 95% confidence intervals are shown as vertical lines.

Table 1. Descriptive Statistics

	All (2015-2023)	All* (2015-2023)	LGBQ (2015-2023)	Hetero (2015-2023)	LGBQ (2011-2023)	Hetero (2011-2023)
Dependent Variables						
Current ENDS Use	0.187	0.187	0.226	0.179	0.226	0.179
Frequent ENDS Use	0.053	0.053	0.068	0.050	0.068	0.050
Everyday ENDS Use	0.038	0.038	0.048	0.036	0.048	0.036
Current Cigarette Smoking	0.060	0.057	0.086	0.051	0.096	0.058
Frequent Cigarette Smoking	0.014	0.013	0.021	0.011	0.026	0.014
Everyday Cigarette Smoking	0.011	0.010	0.016	0.008	0.019	0.010
Current Cigarette/Cigar Use	0.110	0.107	0.152	0.096	0.168	0.110
Frequent Cigarette/Cigar Use	0.024	0.023	0.037	0.019	0.044	0.024
Everyday Cigarette/Cigar Use	0.018	0.017	0.027	0.014	0.032	0.017
Core Baseline Controls						
ENDS Tax (2023$)	0.307	0.357	0.398	0.348	0.377	0.311
	(0.619)	(0.659)	(0.681)	(0.653)	(0.668)	(0.626)
Flavored ENDS Restriction	0.074	0.087	0.117	0.080	0.111	0.071
ENDS MLSA	0.937	0.927	0.948	0.922	0.904	0.837
Tobacco-21 Law	0.454	0.480	0.594	0.453	0.562	0.405
Cigarette Tax (2023$)	2.062	2.258	2.283	2.252	2.290	2.265
	(1.312)	(1.295)	(1.325)	(1.288)	(1.310)	(1.263)
Unemployment Rate	4.440	4.495	4.411	4.514	4.620	4.924
	(1.024)	(1.016)	(0.978)	(1.023)	(1.326)	(1.580)
Cumulative COVID Death Rate	0.102	0.104	0.143	0.096	0.136	0.085
	(0.143)	(0.143)	(0.151)	(0.140)	(0.150)	(0.136)
Percentage Population with COVID Vaccine	20.878	21.874	30.095	19.985	28.495	17.846
	(28.791)	(29.576)	(31.046)	(28.899)	(30.955)	(27.999)
Available Additional LASSO Controls						
ENDS Licensure Law	0.355	0.362	0.414	0.350	0.392	0.312
Indoor Vaping Restriction	0.240	0.259	0.273	0.255	0.259	0.229
ENDS Online Sales Ban	0.058	0.060	0.076	0.056	0.072	0.050
Indoor Smoking Restriction	0.682	0.704	0.703	0.705	0.715	0.729
Tobacco Licensure Law	0.738	0.767	0.771	0.766	0.756	0.738
Beer Tax (2023$)	0.350	0.282	0.271	0.284	0.277	0.294
	(0.323)	(0.203)	(0.199)	(0.204)	(0.204)	(0.211)
Recreational Marijuana Law	0.228	0.261	0.299	0.252	0.283	0.225
Medical Marijuana Law	0.607	0.673	0.679	0.672	0.659	0.631
Sexual Identity Policy Index	8.328	9.305	9.392	9.285	N/A	N/A
	(6.006)	(5.885)	(6.030)	(5.851)		
Observations	813069	697139	132956	564183	146763	682476

Note: State Youth Risk Behavior Surveys data is used in each column. Weighted means are shown for dichotomous variables, while weighted means and standard deviations are shown for continuous variables. All denotes the entire sample, All* denotes the sample that includes information on sexual identity, LGBQ denotes individuals who respond as lesbian or gay, bisexual, or questioning, and Hetero denotes individuals who respond as heterosexual. The Cumulative Covid Death Rate variable is scaled up by a factor of 100.

Table 2. Logit Estimates of Effect of ENDS Taxes on Youth ENDS Use

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Current ENDS Use			Frequent ENDS Use			Everyday ENDS Use		
Panel I: Pooled Sample									
ENDS Tax (2023$)	-0.027**	-0.023*	-0.020*	-0.010**	-0.009**	-0.008*	-0.009**	-0.008*	-0.007
	(0.011)	(0.012)	(0.012)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Pre-Treat Mean DV	0.205	0.205	0.205	0.042	0.042	0.042	0.029	0.029	0.029
N	735109	735109	735109	735109	735109	735109	735109	735109	735109
Panel II: Pooled Sample with Information on Sexual Identity									
ENDS Tax (2023$)	-0.029***	-0.025**	-0.022**	-0.010**	-0.008*	-0.008	-0.008*	-0.007	-0.006
	(0.010)	(0.011)	(0.011)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)
Pre-Treat Mean DV	0.211	0.211	0.211	0.042	0.042	0.042	0.029	0.029	0.029
N	630742	630742	630742	630742	630742	630742	630742	630742	630742
Panel III: Heterosexual-Identifiers									
ENDS Tax (2023$)	-0.031***	-0.028***	-0.025**	-0.012**	-0.011**	-0.010**	-0.011**	-0.010**	-0.009**
	(0.009)	(0.010)	(0.011)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Pre-Treat Mean DV	0.203	0.203	0.203	0.040	0.040	0.040	0.028	0.028	0.028
N	511618	511618	511618	511618	511618	511618	511618	511618	511618
Panel IV: LGBQ-Identifiers									
ENDS Tax (2023$)	-0.023	-0.014	-0.010	0.002	0.005	0.006	0.008	0.011	0.012
	(0.017)	(0.013)	(0.013)	(0.009)	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)
Pre-Treat Mean DV	0.259	0.259	0.259	0.056	0.056	0.056	0.037	0.037	0.037
N	119124	119124	119124	119124	119124	119124	119124	119124	119124
p-value on $\hat{\beta}_{\text{ENDS Tax}^{\text{Hetero}}} = \hat{\beta}_{\text{ENDS Tax}^{\text{LGBQ}}}$	0.190	0.015**	0.015**	0.085*	0.077*	0.073*	0.031**	0.041**	0.034**
Controls:									
Baseline Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LASSO with Extended Controls?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
LASSO w/ Sexual Identity Policy Index?	No	No	Yes	No	No	Yes	No	No	Yes

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Baseline controls include state fixed effects, year fixed effects, semester fixed effects, race/ethnicity, age, gender, grade in school, state unemployment rate, cumulative COVID-19 death rate, the percent of fully COVID-19 vaccinated persons in the state, restrictions on the sales of flavored ENDS, ENDS minimum legal sales age of 18 laws, Tobacco-21 laws, and cigarette taxes (in 2023$). For *Current ENDS Use*, among the full set of possible independent variables listed in Table 1, the LASSO approach included additional controls for indoor vaping bans, ENDS online sales bans, indoor smoking bans (clean indoor air laws), and tobacco licensure laws. For *Frequent ENDS Use* and *Everyday ENDS Use*, the LASSO approach included additional controls for indoor vaping bans, ENDS online sales bans, indoor smoking bans (clean indoor air laws), and medical marijuana laws. The p-value for the difference in LGBQ and heterosexual youth coefficients comes from a fully-interacted triple difference specification described in footnote 15.

Table 3. Robustness of Estimates in Table 2 (Columns 3, 6, and 9) to Use of a Stacked DD Estimator

	(1)	(2)	(3)
	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use
	Panel I: Heterosexual-Identifiers		
ENDS Tax (2023$)	-0.030**	-0.019**	-0.016**
	(0.012)	(0.008)	(0.008)
Pre-Treat Mean DV	0.193	0.045	0.032
N	3101475	3101475	3101475
	Panel II: LGBQ-Identifiers		
ENDS Tax (2023$)	0.003	0.008	0.017
	(0.015)	(0.010)	(0.011)
Pre-Treat Mean DV	0.258	0.062	0.040
N	599528	599528	599277
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{LGBQ}}$	0.0001***	0.0130**	0.0088***

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

Notes: Average marginal effects, using a stacked difference in differences design and weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Controls include those used in columns (3), (6), and (9) of Table 2. Standard errors are in parentheses and clustered at the state level.

Table 4. Sensitivity of Estimates in Table 2 (Columns 3, 6, and 9) to Controls for Region or State-Specific Linear Time Trends

	(1)	(2)	(3)	(4)	(5)	(6)
	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use
	Census Region-Specific Linear Time Trend			Common Treated State-Specific Linear Time Trend		
	Panel I: Heterosexual-Identifiers					
ENDS Tax (2023$)	-0.018*	-0.009*	-0.008**	-0.026**	-0.010**	-0.009**
	(0.010)	(0.005)	(0.004)	(0.011)	(0.005)	(0.004)
Pre-Treat Mean DV	0.203	0.040	0.028	0.203	0.040	0.028
N	511618	511618	511618	511618	511618	511618
	Panel II: LGBQ-Identifiers					
ENDS Tax (2023$)	-0.003	0.007	0.012	-0.012	0.007	0.013
	(0.012)	(0.010)	(0.010)	(0.013)	(0.010)	(0.010)
Pre-Treat Mean DV	0.259	0.056	0.037	0.259	0.056	0.037
N	119124	119124	119124	119124	119124	119124
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{LGBQ}}$	0.034**	0.088*	0.027**	0.030**	0.070*	0.029**

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2.

Table 5. Logit Estimates of Effect of ENDS Taxes on Combustible Tobacco Use

	(1)	(2)	(3)	(4)	(5)	(6)
	Current Cigarette Smoking	Frequent Cigarette Smoking	Everyday Cigarette Smoking	Current Cigarette or Cigar Smoking	Frequent Cigarette or Cigar Smoking	Everyday Cigarette or Cigar Smoking
			Panel I: 2011-2023			
			(a) Heterosexual-Identifiers			
ENDS Tax (2023$)	0.0020	0.0001	0.0004	0.0012	0.0001	0.0007
	(0.0039)	(0.0014)	(0.0010)	(0.0106)	(0.0040)	(0.0031)
Pre-Treat Mean DV	0.082	0.023	0.016	0.137	0.033	0.023
N	656185	656185	656185	556291	550554	550208
			(b) LGBQ-Identifiers			
ENDS Tax (2023$)	-0.0011	-0.0065	-0.0061**	0.0040	-0.0027	-0.0039
	(0.0060)	(0.0040)	(0.0030)	(0.0197)	(0.0092)	(0.0077)
Pre-Treat Mean DV	0.153	0.047	0.034	0.238	0.071	0.052
N	136672	136672	136672	116587	113674	113468
			Panel II: 2015-2023			
			(a) Heterosexual-Identifiers			
ENDS Tax (2023$)	-0.0012	-0.0007	-0.0006	-0.0062	-0.0028	-0.0021
	(0.0032)	(0.0032)	(0.0009)	(0.0091)	(0.0040)	(0.0029)
Pre-Treat Mean DV	0.065	0.016	0.011	0.110	0.024	0.017
N	542742	542742	542742	454442	451368	451215
			(b) LGBQ-Identifiers			
ENDS Tax (2023$)	-0.0001	-0.0071**	-0.0069**	0.0039	-0.0039	-0.0053
	(0.0052)	(0.0032)	(0.0027)	(0.0202)	(0.0087)	(0.0067)
Pre-Treat Mean DV	0.126	0.035	0.025	0.201	0.055	0.040
N	124287	124287	124287	104545	102744	102617

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the periods 2011-2023 (Panel I) and 2015-2023 (Panel II). Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Standard errors are in parentheses and clustered at the state level. All regressions control for flavored ENDS restrictions, ENDS minimum legal sales age of 18 laws, Tobacco-21 laws, and cigarette taxes (in 2023$). Additional controls for *Current Cigarette Smoking* include indoor vaping bans, ENDS online sales bans, indoor smoking bans, and tobacco licensure laws; additional controls for *Frequent Cigarette Smoking* and *Everyday Cigarette Smoking* include ENDS online sales bans, indoor smoking bans and tobacco licensure laws; additional controls for *Current Cigarette or Cigar Smoking* include indoor vaping bans, ENDS online sales bans, indoor smoking bans, tobacco licensure laws, and medical marijuana laws; additional controls for *Frequent Cigarette or Cigar Smoking* and *Everyday Cigarette or Cigar Smoking* include ENDS online sales bans, indoor smoking bans, tobacco licensure laws, and medical marijuana laws.

Table 6. Logit Estimates of Effect of Cigarette Taxes on Combustible Tobacco Use, State and Combined YRBS, 2011-2023

	(1)	(2)	(3)
	Current Cigarette Smoking	Frequent Cigarette Smoking	Everyday Cigarette Smoking
		Panel I: 2011-2023	
		(a) Heterosexual-Identifiers	
Cigarette Tax (2023$)	-0.0033	-0.0009	-0.0007
	(0.0032)	(0.0012)	(0.0010)
Pre-Treat Mean DV	0.082	0.023	0.016
N	656185	656185	656185
		(b) LGBQ-Identifiers	
Cigarette Tax (2023$)	-0.0025	0.0022	-0.0013
	(0.0062)	(0.0033)	(0.0032)
Pre-Treat Mean DV	0.153	0.047	0.034
N	136672	136672	136672
		Panel II: 2015-2023	
		(a) Heterosexual-Identifiers	
Cigarette Tax (2023$)	0.0030	0.0020**	0.0015**
	(0.0031)	(0.0009)	(0.0008)
Pre-Treat Mean DV	0.065	0.016	0.011
N	542742	542742	542742
		(b) LGBQ-Identifiers	
Cigarette Tax (2023$)	0.0002	0.0056	0.0019
	(0.0069)	(0.0038)	(0.0037)
Pre-Treat Mean DV	0.126	0.035	0.025
N	124287	124287	124287
p-value on $\hat{\beta}_{\text{Cig Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{Cig Tax}}^{\text{LGBQ}}$	0.4518	0.3417	0.4013

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2011-2023 (Panel A) and 2015-2023 (Panel B). Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Standard errors are in parentheses and clustered at the state level. All regressions control for flavored ENDS restrictions, ENDS minimum legal sales age of 18 laws, Tobacco-21 laws, and ENDS taxes (in 2023$). Additional controls for *Current Cigarette Smoking* include indoor vaping bans, ENDS online sales bans, indoor smoking bans, and tobacco licensure laws; additional controls for *Frequent Cigarette Smoking* and *Everyday Cigarette Smoking* include ENDS online sales bans, indoor smoking bans and tobacco licensure laws.

Table 7. Logit Estimates of the Effect of Cigarette Taxes, Flavor Restrictions, and Minimum Legal Sales Ages on Youth ENDS Use

	(1)	(2)	(3)
	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use
	Panel I: Heterosexual-Identifiers		
Cigarette Tax (2023$)	0.003	0.000	0.002
	(0.011)	(0.005)	(0.004)
ENDS Flavor Ban	-0.005	-0.015	-0.012
	(0.018)	(0.010)	(0.009)
ENDS MLSA	-0.031*	-0.018**	-0.017***
	(0.016)	(0.007)	(0.006)
Pre-Treat Mean DV	0.203	0.040	0.028
N	511618	511618	511618
	Panel II: LGBQ-Identifiers		
Cigarette Tax (2023$)	0.008	0.002	-0.003
	(0.017)	(0.010)	(0.009)
ENDS Flavor Ban	-0.023	-0.020	-0.019*
	(0.019)	(0.014)	(0.011)
ENDS MLSA	0.003	0.010	0.007
	(0.016)	(0.011)	(0.007)
Pre-Treat Mean DV	0.259	0.056	0.037
N	119124	119124	119124
p-value on $\hat{\beta}_{\text{Other ENDS Policy}}^{\text{Hetero}} = \hat{\beta}_{\text{Other ENDS Policy}}^{\text{LGBQ}}$			
Cigarette Tax (2023$)	0.753	0.884	0.521
Flavor Ban	0.513	0.966	0.810
ENDS MLSA	0.028**	0.005***	0.000***

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2. For ENDS flavor ban regressions, we require 2 years of post-period data and exclude areas with simultaneous introductions of menthol cigarette ban, which drops California and Massachusetts from our sample.

Table 8. Logit Estimates of the Effects of ENDS Taxes on Current ENDS Use, Separate Analyses for Those With vs. Without Persistent Sadness, Bullying Victimization, or Suicidal Ideation

	(1)	(2)	(3)	(4)
	Sadness, Bullying, or Suicidal	None of Those	Sadness, Bullying, or Suicidal	None of Those
	LGBQ		Heterosexual	
ENDS Tax (2023$)	-0.000	-0.046*	-0.038***	-0.023**
	(0.012)	(0.025)	(0.012)	(0.012)
Cigarette Tax ($2023)	0.000	0.019	0.002	-0.001
	(0.020)	(0.018)	(0.013)	(0.009)
ENDS Flavor Ban	-0.026	-0.052***	-0.007	-0.025
	(0.020)	(0.018)	(0.022)	(0.021)
ENDS MLSA	0.007	-0.004	-0.047*	-0.035**
	(0.020)	(0.021)	(0.025)	(0.014)
Pre-Treat Vape Mean	0.298	0.162	0.279	0.158
N	77759	30895	180724	268415
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{DBS=1}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{DBS=0}}$	0.0426**		0.8398	
p-value on $\hat{\beta}_{\text{Cig Tax}}^{\text{DBS=1}} = \hat{\beta}_{\text{Cig Tax}}^{\text{DBS=0}}$	0.2634		0.7357	
p-value on $\hat{\beta}_{\text{Flavor Ban}}^{\text{DBS=1}} = \hat{\beta}_{\text{Flavor Ban}}^{\text{DBS=0}}$	0.0514*		0.2154	
p-value on $\hat{\beta}_{\text{MLSA}}^{\text{DBS=1}} = \hat{\beta}_{\text{MLSA}}^{\text{DBS=0}}$	0.7791		0.6034	

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Persistent sadness is defined by responding "yes" to the survey question, "During the past 12 months, did you ever feel so sad or hopeless almost every day for two weeks or more in a row that you stopped doing some usual activities?" Bullying victimization status is coded using the question, "During the past 12 months, have you ever been bullied on school property." To be consistent with Rees et al. (2022) and Liang et al. (2023), we do not include online bullying in that definition. Suicidal ideation is based on the question, "During the past 12 months, did you ever seriously consider attempting suicide?" Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in column (3) of Table 2. For ENDS flavor ban regressions, we require 2 years of post-period data and exclude areas with simultaneous introductions of menthol cigarette ban, which drops California and Massachusetts from our sample.

Table 9. TWFE Estimates of Effects of ENDS Tax on ENDS Use Among Adults, BRFSS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Current ENDS Use		Everyday ENDS Use		Current ENDS Use		Everyday ENDS Use	
	Ages 18-30				Ages 31 and Older			
	Panel I: Heterosexual-Identifiers							
ENDS Tax (2023$)	-0.0094	-0.0097	-0.0037	-0.0039	0.0001	-0.0000	-0.0011	-0.0011
	(0.0076)	(0.0069)	(0.0043)	(0.0043)	(0.0015)	(0.0014)	(0.0015)	(0.0015)
Pre-Treat Mean DV	0.102	0.102	0.033	0.033	0.033	0.033	0.012	0.012
N	120876	120876	120876	120876	1061392	1061392	1061392	1061392
	Panel II: LGBQ-Identifiers							
ENDS Tax (2023$)	0.0105	0.0106	0.0119	0.0119	0.0182**	0.0181**	0.0092	0.0093
	(0.0197)	(0.0197)	(0.0150)	(0.0149)	(0.0085)	(0.0082)	(0.0067)	(0.0062)
Pre-Treat Mean DV	0.133	0.133	0.041	0.041	0.052	0.052	0.018	0.018
N	23547	23547	23547	23547	59545	59545	59545	59545
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{LGBQ}}$	0.2532	0.2571	0.2257	0.2232	0.0459**	0.0349**	0.1285	0.0993*
Controls:								
Baseline Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LASSO with Extended Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LASSO w/ Sexual Identity Policy Index?	No	Yes	No	Yes	No	Yes	No	Yes

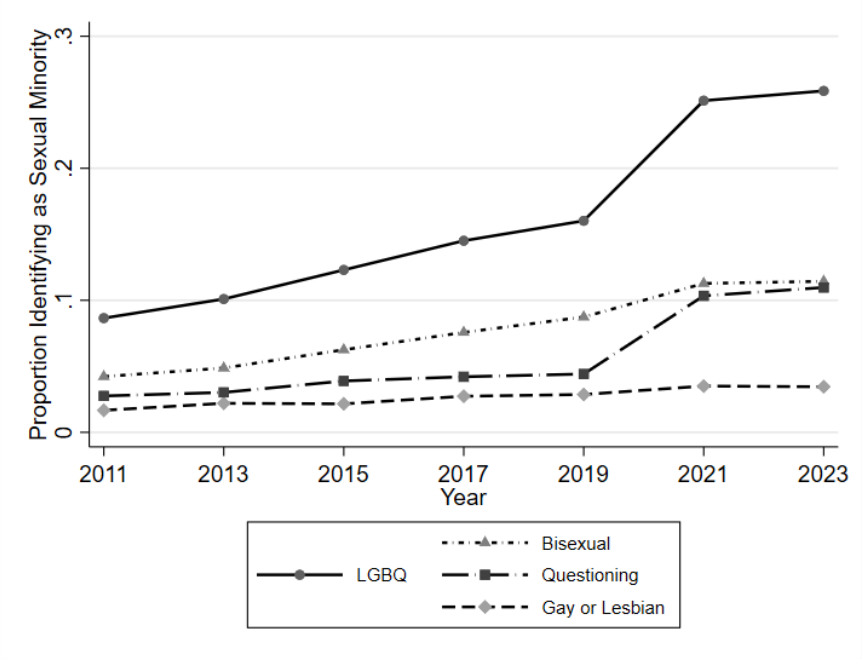
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from the individual-level Behavioral Risk Factor Surveillance Survey (BRFSS) collected over the period 2016-2023. ENDS use is based on the survey question, "Do you now use e-cigarettes or other electronic vaping products every day, some days, or not at all?" Those who respond with "every day" or "some days" are coded as current ENDS users. To define sexual orientation, we use the question, "Which of the following best represents how you think of yourself?" LGBQ individuals are coded as those who respond with "gay," "bisexual," "something else," or "I don't know the answer." Heterosexual individuals are then defined as those who respond "straight, that is, not gay." Sample weights are generated using the individual BRFSS-provided sample weights. Standard errors are in parentheses and clustered at the state level. Baseline controls include state fixed effects, year fixed effects, quarter fixed effects, race/ethnicity, age, gender, marital status, educational attainment state unemployment rate, cumulative COVID-19 death rate, the percent of fully COVID-19 vaccinated persons in the state, restrictions on the sales of flavored ENDS, ENDS minimum legal sales age of 18 laws, and Tobacco-21 laws. For *Current ENDS Use* among the full set of possible independent variables listed in Table 1, the LASSO approach included additional controls for ENDS online sales bans and indoor smoking bans; for *Everyday ENDS Use*, the LASSO approach included additional controls for ENDS online sales bans.

Online Appendix

Figures and Tables

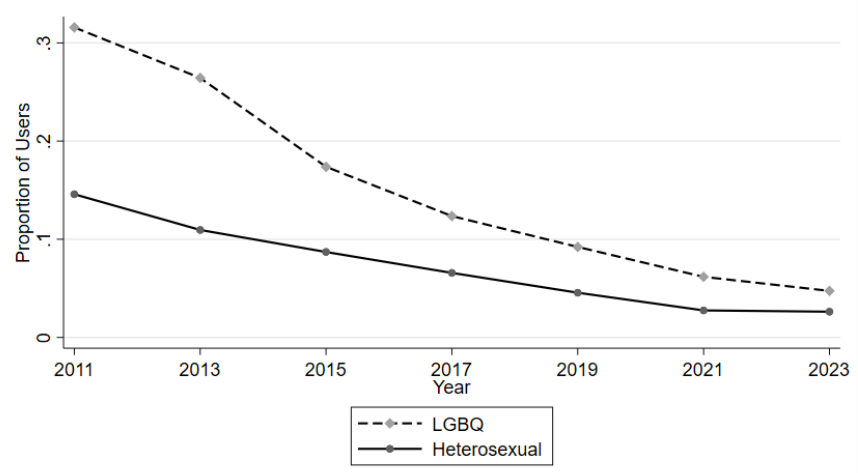
Appendix Figure 1. Trends in Identifying as Sexual Minority



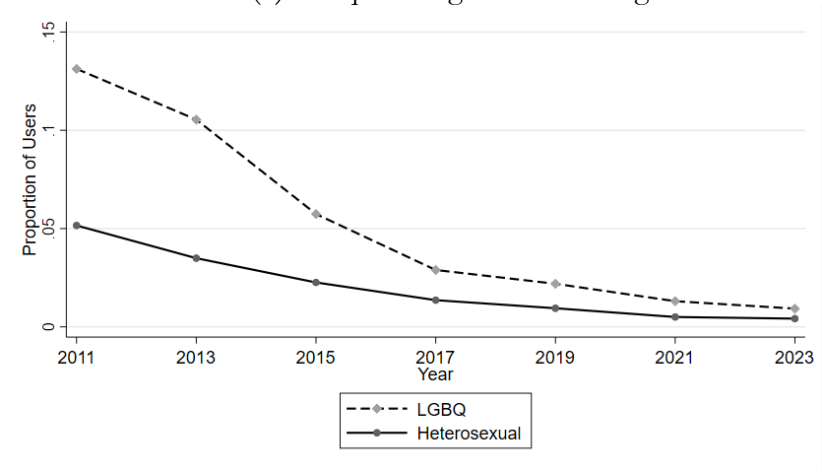
Notes: Weighted estimates from the 2011-2023 State Youth Risk Behavior Surveys are shown above.

Appendix Figure 2. Trends in Prior-Month Youth Cigarette Smoking, by Sexual Identity

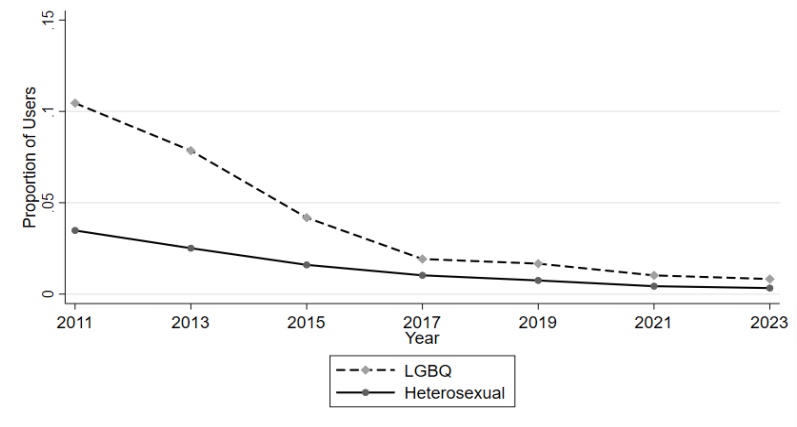
Panel (a): Current Cigarette Smoking



Panel (b): Frequent Cigarette Smoking



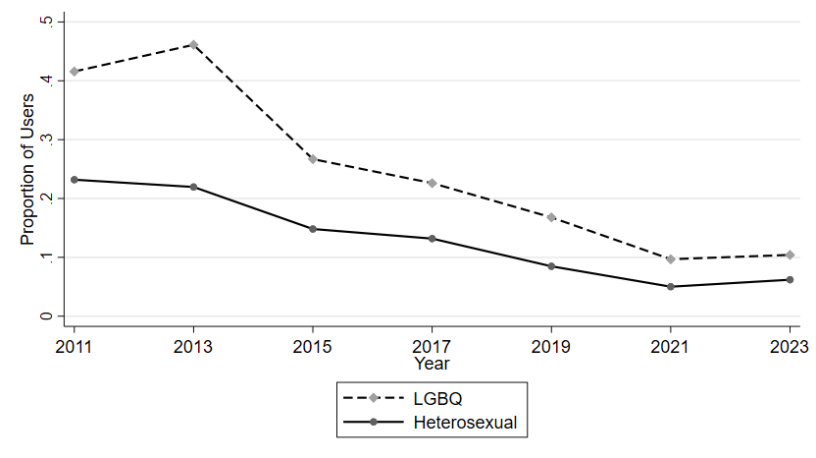
Panel (c): Everyday Cigarette Smoking



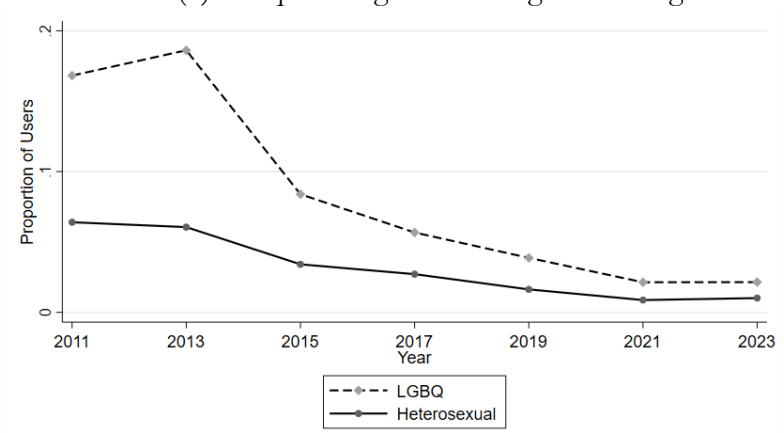
Notes: Weighted estimates from the 2011-2023 State Youth Risk Behavior Surveys are shown above.

Appendix Figure 3. Trends in Prior-Month Youth Cigarette or Cigar Smoking, by Sexual Identity

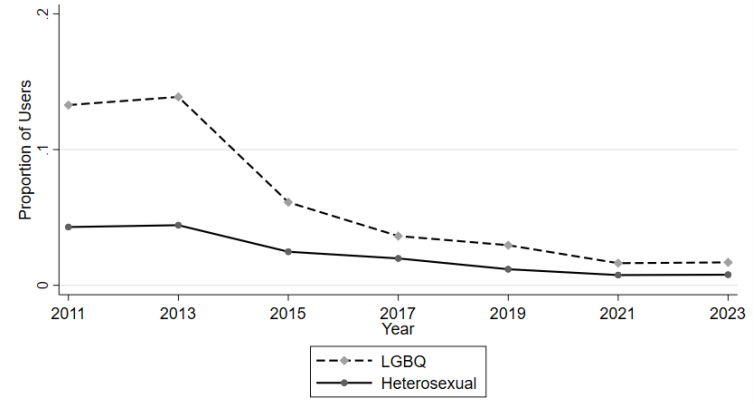
Panel (a): Current Cigarette or Cigar Smoking



Panel (b): Frequent Cigarette or Cigar Smoking



Panel (c): Everyday Cigarette or Cigar Smoking

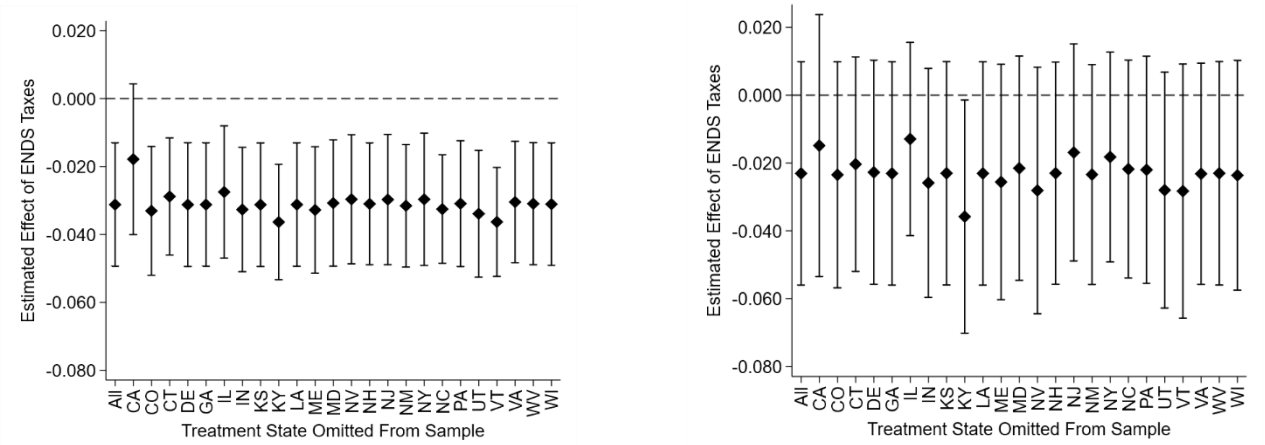


Notes: Weighted estimates from the 2011-2023 State Youth Risk Behavior Surveys are shown above.

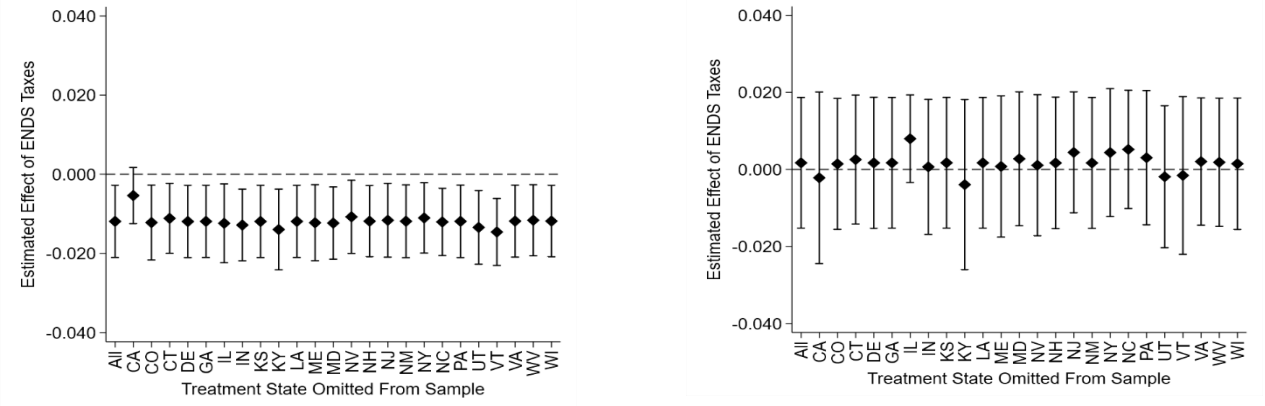
Appendix Figure 4. Robustness to Leaving Out One Treatment State at a Time

(I) Heterosexual-Identifiers (II) LGBQ-Identifiers

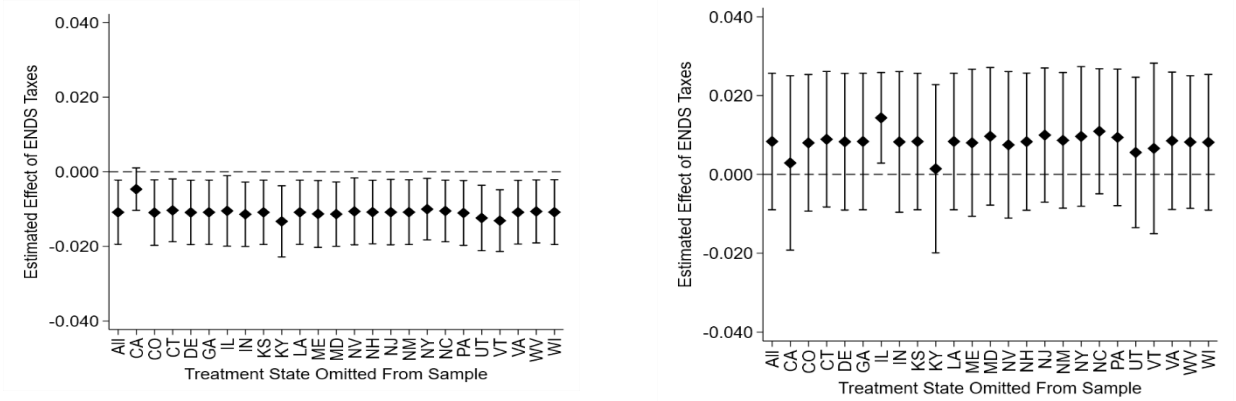
Panel (a): Current ENDS Use



Panel (b): Frequent ENDS Use



Panel (c): Everyday ENDS Use



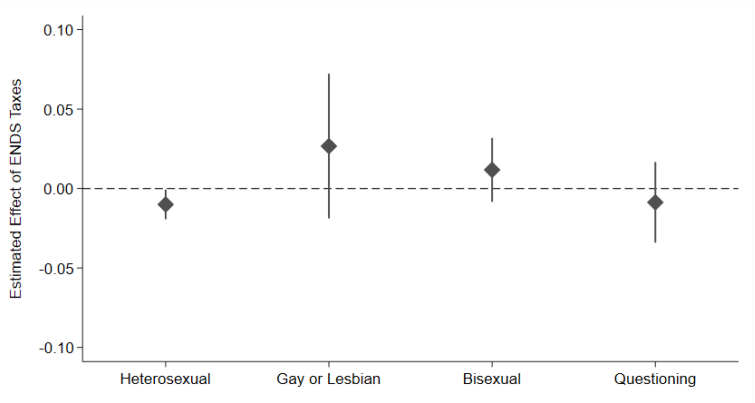
Notes: These data are drawn from the State Youth Risk Behavior Survey from 2015-2023. Average marginal effects are estimated using weighted logistic regression (with 95% CIs), and standard errors are clustered at the state level. Controls include state, year, and semester fixed effects, demographic controls, macroeconomic and COVID-19 controls, flavored ENDS restrictions, ENDS minimum legal sales age laws, Tobacco-21 Laws, and cigarette taxes (in 2023$).

Appendix Figure 5. Exploring Heterogeneity in Estimated Treatment Effect on ENDS Use Among Sexual Minority Youth

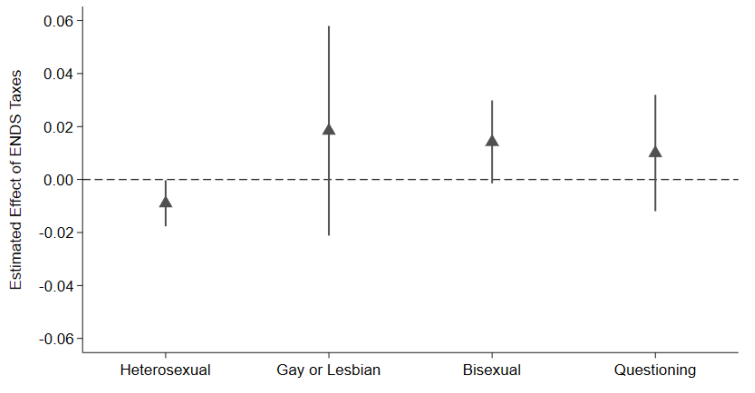
Panel (a): Current ENDS Use



Panel (b): Frequent ENDS Use



Panel (c): Everyday ENDS Use



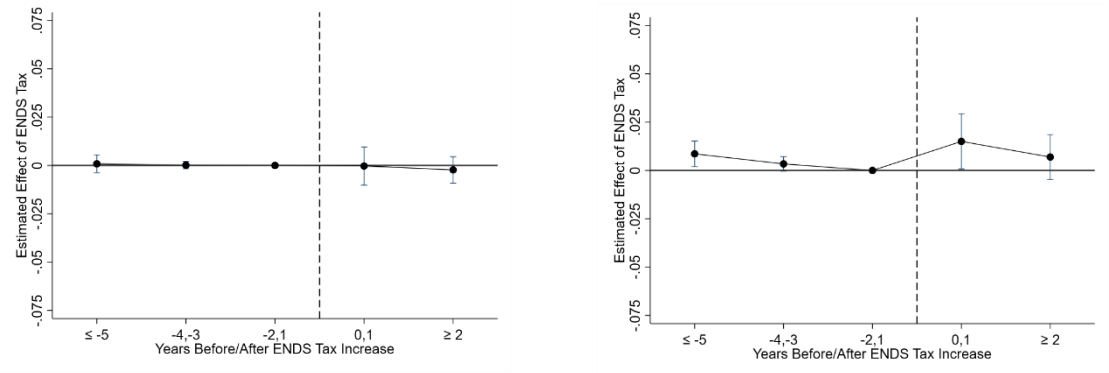
Notes: These data are drawn from the State Youth Risk Behavior Survey from 2015-2023. Average marginal effects are estimated using weighted logistic regression (with 95% CIs), and standard errors are clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2.

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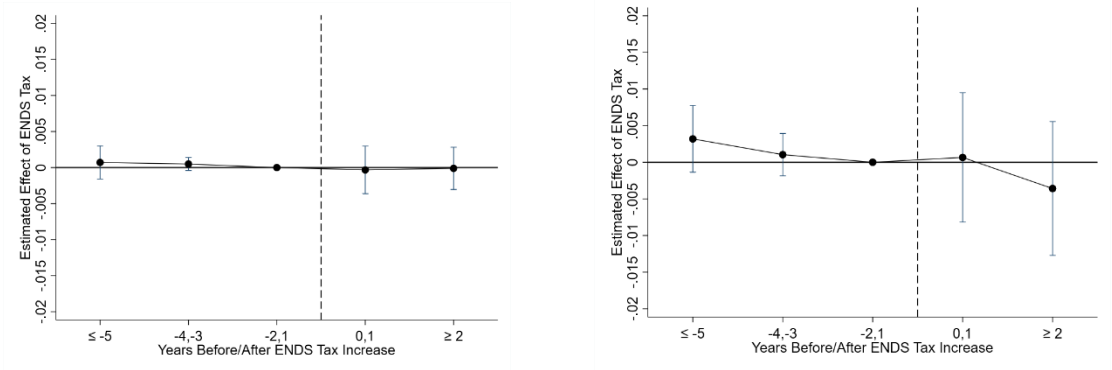
Appendix Figure 6. Event-Study Analysis of ENDS Taxes and Cigarette Smoking, by Sexual Identity, State YRBS 2011-2023, Using TWFE Logit Estimates

(I) Heterosexual-Identifiers (II) LGBQ-Identifiers

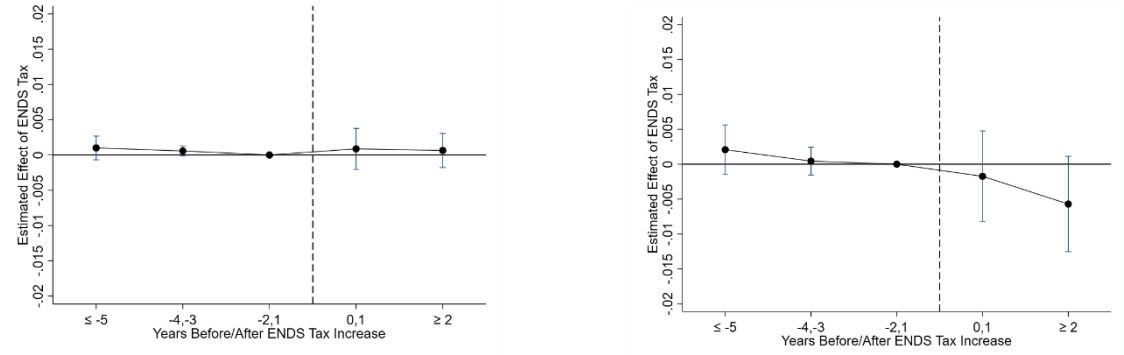
Panel (a): Current Cigarette Smoking



Panel (b): Frequent Cigarette Smoking



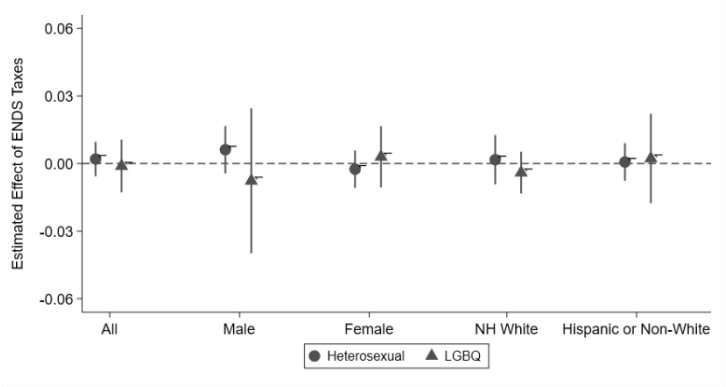
Panel (c): Everyday Cigarette Smoking



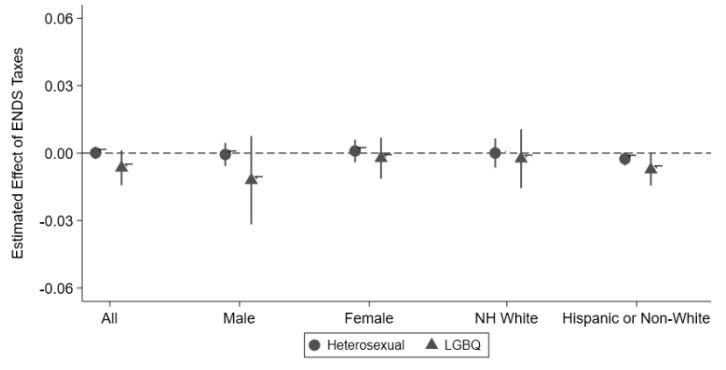
Notes: These data are drawn from the State Youth Risk Behavior Survey from 2011-2023. Average marginal effects are estimated using weighted logistic regression (with 95% CIs), and standard errors are clustered at the state level. Controls include state, year, and semester fixed effects, demographic controls, macroeconomic and COVID-19 controls, flavored ENDS restrictions, ENDS minimum legal sales age laws, Tobacco-21 Laws, and cigarette taxes (in 2023$). The reference period is 1-2 years prior to an ENDS tax increase.

Appendix Figure 7. Further Exploring the Role of Intersectionality on Spillovers to Cigarette Smoking

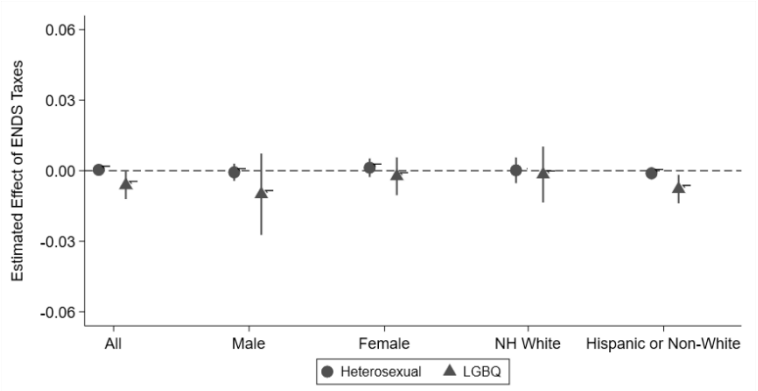
Panel (a): Current Cigarette Smoking



Panel (b): Frequent Cigarette Smoking



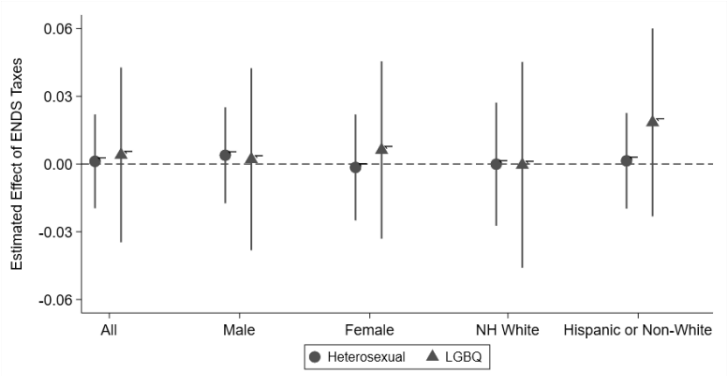
Panel (c): Everyday Cigarette Smoking



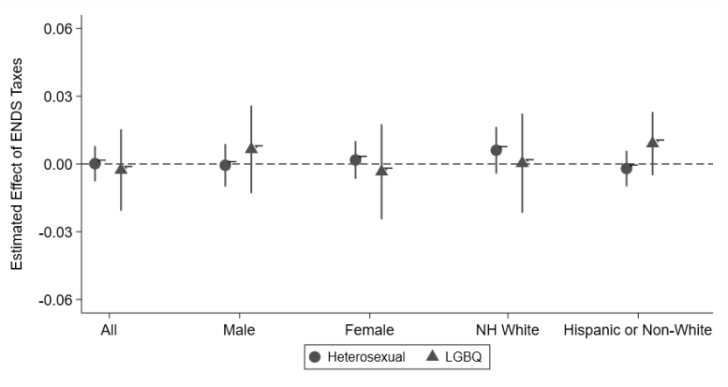
Notes: These data are drawn from the State Youth Risk Behavior Survey from 2011-2023. Average marginal effects are estimated using weighted logistic regression (with 95% CIs), and standard errors are clustered at the state level. Controls include those used Table 5. The x-axis label "NH White" refers to Non-Hispanic White individuals. Due to non-convergence, logistic estimates are replaced by OLS in panel (b) for the Non-Hispanic White, Heterosexual subgroup, and in panel (c) for the Non-Hispanic White, Heterosexual subgroup.

Appendix Figure 8. Exploring the Role of Intersectionality on Spillovers to Cigarette or Cigar Smoking

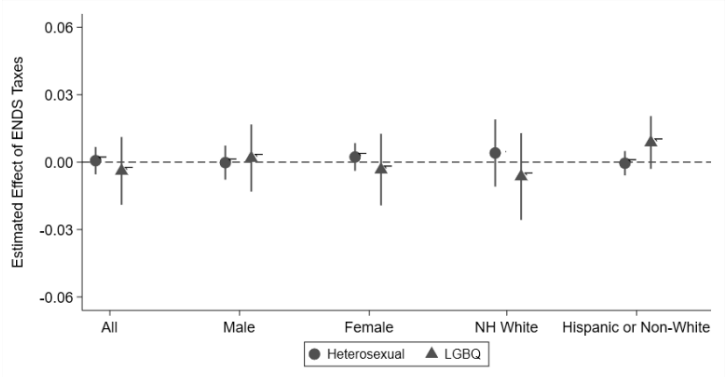
Panel (a): Current Cigarette or Cigar Smoking



Panel (b): Frequent Cigarette or Cigar Smoking



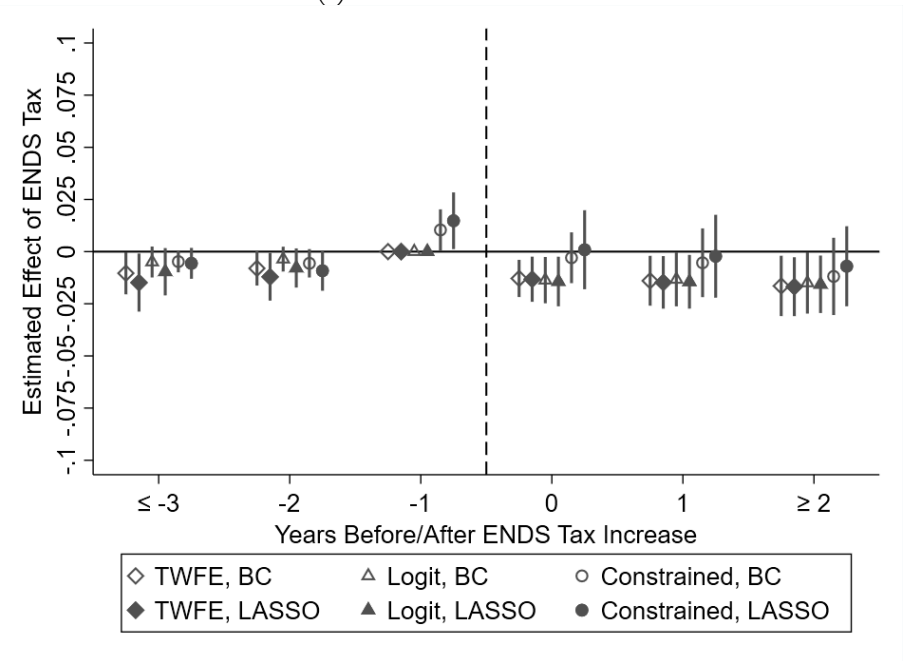
Panel (c): Everyday Cigarette or Cigar Smoking



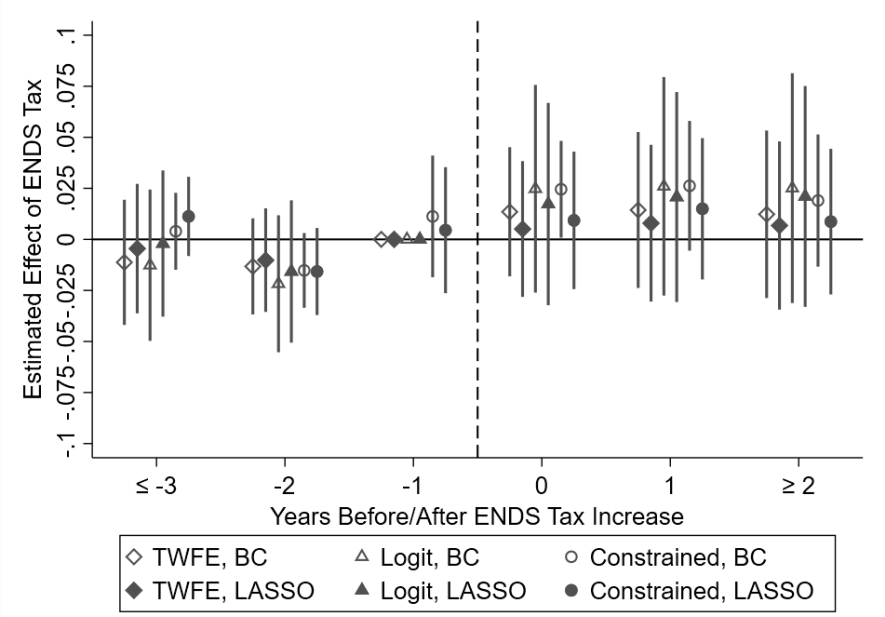
Notes: These data are drawn from the State Youth Risk Behavior Survey from 2011-2023. Average marginal effects are estimated using weighted logistic regression (with 95% CIs) from our fully specified model, and standard errors are clustered at the state level. Controls include those used in Table 5. The x-axis label "NH White" refers to Non-Hispanic White individuals. Due to non-convergence, logistic estimates are replaced by OLS in panel (c) for the Non-Hispanic White, Heterosexual subgroup.

Appendix Figure 9. Event-Study Analyses of ENDS Taxes and Current ENDS Use Among Young Adults Aged 18-30, BRFSS, 2016-2023, by Sexual Identity

Panel (a): Heterosexual-Identifiers



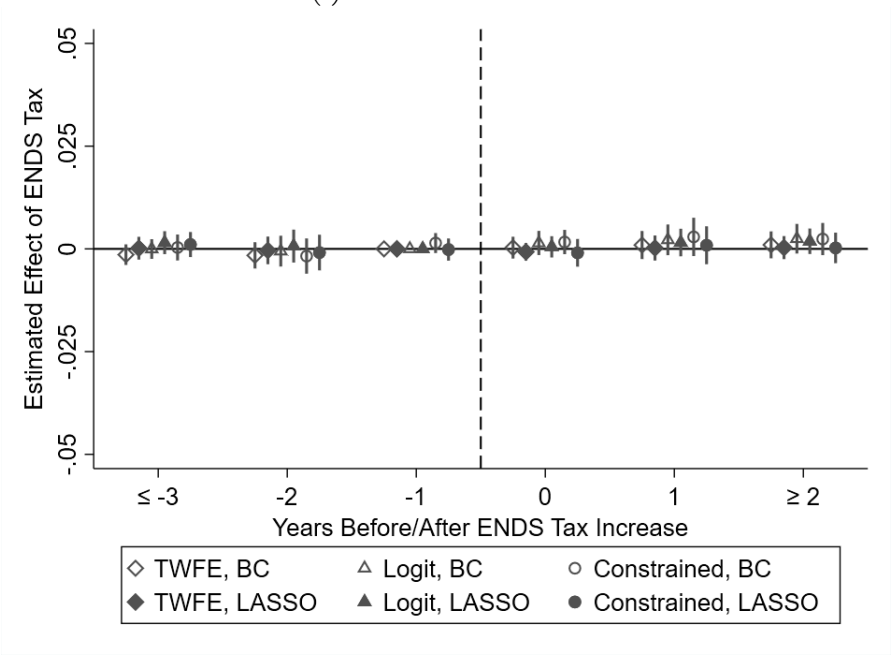
Panel (b): LGBQ-Identifiers



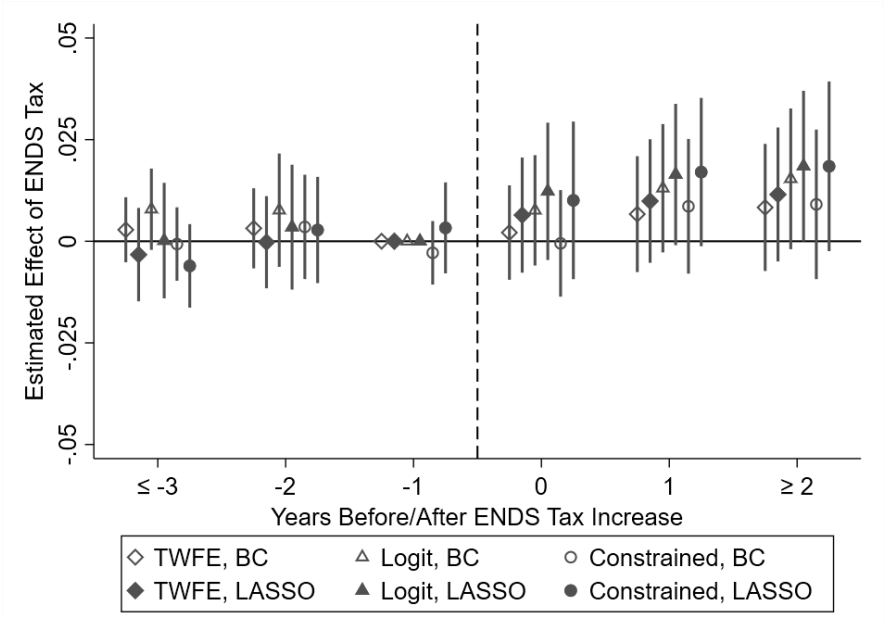
Notes: Data come from the Behavioral Risk Factor Surveillance Survey from 2016-2023. ENDS use is based on the question, "Do you now use e-cigarettes or other electronic vaping products every day, some days, or not at all?" "Every day" or "some days" are coded as current ENDS use. Sexual orientation is based on the question, "Which of the following best represents how you think of yourself?" LGBQ individuals are coded as responding with "gay," "bisexual," "something else," or "I don't know the answer." Heterosexual individuals are defined as those who respond "straight, that is, not gay." Except for "Constrained" regressions, the reference period is 1-2 years prior to an ENDS tax increase. "TWFE" denotes two-way-fixed effects estimates obtained using approach outlined by Miller (2023) in Serrato and Zidar (2016), "BC" denotes baseline controls, "LASSO" denotes controls for LASSO-selected covariates and the LGBT policy index, "Stacked" denotes stacked difference-in-differences estimates, "Logit" denotes logistic regression estimates, and "Constrained" denotes event study estimates obtained using the average of the pre-treatment period as the reference period. 95% confidence intervals are shown as vertical lines.

Appendix Figure 10. Event-Study Analyses of ENDS Taxes and Current ENDS Use Among Young Adults Over 30 Years Old, BRFSS, 2016-2023, by Sexual Identity

Panel (a): Heterosexual-Identifiers



Panel (b): LGBQ-Identifiers



Notes: Data come from the Behavioral Risk Factor Surveillance Survey from 2016-2023. ENDS use is based on the question, "Do you now use e-cigarettes or other electronic vaping products every day, some days, or not at all?" "Every day" or "some days" are coded as current ENDS use. Sexual orientation is based on the question, "Which of the following best represents how you think of yourself?" LGBQ individuals are coded as responding with "gay," "bisexual," "something else," or "I don't know the answer." Heterosexual individuals are defined as those who respond "straight, that is, not gay." Except for "Constrained" regressions, the reference period is 1-2 years prior to an ENDS tax increase. "TWFE" denotes two-way-fixed effects estimates obtained using approach outlined by Miller (2023) in Serrato and Zidar (2016), "BC" denotes baseline controls, "LASSO" denotes controls for LASSO-selected covariates and the LGBT policy index, "Stacked" denotes stacked difference-in-differences estimates, "Logit" denotes logistic regression estimates, and "Constrained" denotes event study estimates obtained using the average of the pre-treatment period as the reference period. 95% confidence intervals are shown as vertical lines.

Appendix Table 1. Test of Sample Selection Bias with LGBQ Identification as an Outcome Variable, YRBS, 2015-2023

	(1)	(2)	(3)
ENDS Tax (2023$)	0.004	0.005	0.008
	(0.007)	(0.006)	(0.006)
Pre-Treat Mean DV	0.149	0.149	0.149
N	697139	697139	697139
Controls:			
Baseline Controls?	Yes	Yes	Yes
LASSO with Extended Controls?	No	Yes	Yes
LASSO w/ Sexual Identity Policy Index?	No	No	Yes

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Baseline controls include state fixed effects, year fixed effects, semester fixed effects, race/ethnicity, age, gender, grade in school, state unemployment rate, cumulative COVID-19 death rate, the percent of fully COVID-19 vaccinated persons in the state, restrictions on the sales of flavored ENDS, ENDS minimum legal sales age of 18 laws, Tobacco-21 laws, and cigarette taxes (in 2023$). The LASSO approach includes additional controls for indoor vaping bans, ENDS online sales bans, indoor smoking bans, and medical marijuana laws.

Appendix Table 2. Robustness of Estimates to Use of OLS

	(1)	(2)	(3)	(4)	(5)	(6)
	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use
		OLS			OLS, Common Treated State Linear Time Trend	
			Panel I: Heterosexual-Identifiers			
ENDS Tax (2023$)	-0.023**	-0.013**	-0.011**	-0.024**	-0.014**	-0.012**
	(0.011)	(0.006)	(0.005)	(0.012)	(0.006)	(0.005)
Pre-Treat Mean DV	0.203	0.040	0.028	0.203	0.040	0.028
N	511618	511618	511618	511618	511618	511618
			Panel II: LGBQ-Identifiers			
ENDS Tax (2023$)	-0.013	-0.003	0.002	-0.015	-0.004	0.002
	(0.014)	(0.010)	(0.010)	(0.014)	(0.010)	(0.009)
Pre-Treat Mean DV	0.259	0.056	0.037	0.259	0.056	0.037
N	119124	119124	119124	119124	119124	119124
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{LGBQ}}$	0.331	0.264	0.124	0.382	0.274	0.116

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

Notes: Estimates, using weighted least squares, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2.

Appendix Table 3. Robustness of Estimates to Exclusion of 2021 Survey

	(1)	(2)	(3)
	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use
	Panel I: Heterosexual-Identifiers		
ENDS Tax (2023$)	-0.036***	-0.016***	-0.014***
	(0.010)	(0.005)	(0.005)
N	426084	426084	426084
	Panel II: LGBQ-Identifiers		
ENDS Tax (2023$)	-0.022**	0.002	0.007
	(0.010)	(0.013)	(0.015)
N	88911	88911	88911
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{LGBQ}}$	0.020**	0.163	0.627

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023, excluding the 2021 survey wave. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2.

Appendix Table 4. Robustness of Estimates to Using a $0.01 ENDS Tax Increase as Treatment Variable Then Multiplying Marginal Effect by 100

	(1)	(2)	(3)
	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use
	Panel I: Heterosexual-Identifiers		
ENDS Tax (2023$)	-0.025**	-0.010**	-0.009**
	(0.011)	(0.005)	(0.004)
N	511618	511618	511618
	Panel II: LGBQ-Identifiers		
ENDS Tax (2023$)	-0.010	0.006	0.012
	(0.013)	(0.010)	(0.010)
N	119124	119124	119124
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{LGBQ}}$	0.015**	0.073*	0.034**

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2.

Appendix Table 5. Robustness of Estimates to Modeling Excise and Ad Valorem Taxes Separately

	(1) Current ENDS Use	(2) Frequent ENDS Use	(3) Everyday ENDS Use
	Panel I: Heterosexual-Identifiers		
ENDS Tax: Excise ($2023)	-0.008	-0.005	-0.003
	(0.014)	(0.005)	(0.003)
ENDS Tax: Ad Valorem ($2023)	-0.030***	-0.012**	-0.012**
	(0.011)	(0.006)	(0.005)
p-value on $\hat{\beta}_{\text{Excise}}^{\text{Hetero}} = \hat{\beta}_{\text{Ad Valorem}}^{\text{Hetero}}$	0.219	0.345	0.107
N	511618	511618	511618
	Panel II: LGBQ-Identifiers		
ENDS Tax: Excise ($2023)	0.008	0.011*	0.024***
	(0.015)	(0.006)	(0.006)
ENDS Tax: Ad Valorem ($2023)	-0.017	0.004	0.007
	(0.013)	(0.013)	(0.013)
p-value on $\hat{\beta}_{\text{Excise}}^{\text{LGBQ}} = \hat{\beta}_{\text{Ad Valorem}}^{\text{LGBQ}}$	0.149	0.613	0.235
N	119124	119124	119124
p-value on $\hat{\beta}_{\text{Excise}}^{\text{Hetero}} = \hat{\beta}_{\text{Excise}}^{\text{LGBQ}}$	0.087*	0.003***	0.000***
p-value on $\hat{\beta}_{\text{Ad Valorem}}^{\text{Hetero}} = \hat{\beta}_{\text{Ad Valorem}}^{\text{LGBQ}}$	0.088*	0.202	0.142

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2.

Appendix Table 6. Robustness of Estimates to Use of Combined State and National YRBS Sample

	(1)	(2)	(3)
	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use
	Panel I: Heterosexual-Identifiers		
ENDS Tax (2023$)	-0.012	-0.004	-0.006*
	(0.008)	(0.004)	(0.003)
Pre-Treat Mean DV	0.231	0.045	0.030
N	567461	567461	567461
	Panel II: LGBQ-Identifiers		
ENDS Tax (2023$)	0.016	0.003	0.012
	(0.013)	(0.013)	(0.013)
	0.262	0.065	0.042
N	132123	132123	132123
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{LGBQ}}$	0.068*	0.537	0.146

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State and National Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated following Courtemanche et al. (2025). In both panels, standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2 as well as an indicator for whether the observation was drawn from the state vs national YRBS.

Appendix Table 7. DDD Estimates of the Effect of ENDS Taxes on Youth ENDS Use, State YRBS

	(1)	(2)	(3)
	Current ENDS Use	Frequent ENDS Use	Everyday ENDS Use
ENDS Tax ($2023)	-0.026**	-0.011**	-0.009**
	(0.011)	(0.005)	(0.005)
ENDS Tax X LGBQ	0.017**	0.016*	0.019**
	(0.007)	(0.009)	(0.009)
N	630742	630742	630742
p-value on $\hat{\beta}_{\text{ENDS Tax}}^{\text{Hetero}} = \hat{\beta}_{\text{ENDS Tax}}^{\text{LGBQ}}$	0.016**	0.073*	0.034**

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

Notes: Estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2 that present average marginal effects, along with an indicator for LGBQ status and its interaction with ENDS taxes.

Appendix Table 8. Effect of ENDS Taxes on the Probability of Reporting Persistent Sadness, Bullying Victimization, or Suicidal Ideation

	(1)	(2)
	LGBQ	Heterosexual
ENDS Tax (2023$)	-0.005	0.016
	(0.016)	(0.023)
Pre-Treat Outcome Mean	0.687	0.380
N	112331	480639

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

Notes: Estimates, using weighted least squares, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2015-2023. Persistent sadness is defined by responding "yes" to the survey question, "During the past 12 months, did you ever feel so sad or hopeless almost every day for two weeks or more in a row that you stopped doing some usual activities?" Bullying victimization status is coded using the question, "During the past 12 months, have you ever been bullied on school property." To be consistent with Rees et al. (2022) and Liang et al. (2023), we do not include online bullying in that definition. Suicidal ideation is based on the question, "During the past 12 months, did you ever seriously consider attempting suicide?" Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Controls include those used in columns (3), (6), and (9) of Table 2.

Appendix Table 9. Logit Estimates of the Effects of Cigarette Taxes on Current Cigarette Smoking

	(1)	(2)	(3)	(4)
	Sadness, Bullying, or Suicidal	None of Those	Sadness, Bullying, or Suicidal	None of Those
	LGBQ		Heterosexual	
Cigarette Tax ($2023)	-0.008	0.016**	-0.003	-0.003
	(0.008)	(0.008)	(0.006)	(0.002)
Pre-Treat Smoking Mean	0.178	0.082	0.121	0.055
N	92729	33801	252452	335304
p-value on $\hat{\beta}_{\text{Cig Tax}}^{\text{DBS=1}} = \hat{\beta}_{\text{Cig Tax}}^{\text{DBS=0}}$	0.0027***		0.4868	

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

Notes: Average marginal effect estimates, using weighted logistic regression, are generated from individual-level State Youth Risk Behavior Surveys collected over the period 2011-2023. Persistent sadness is defined by responding "yes" to the survey question, "During the past 12 months, did you ever feel so sad or hopeless almost every day for two weeks or more in a row that you stopped doing some usual activities?" Bullying victimization status is coded using the question, "During the past 12 months, have you ever been bullied on school property." To be consistent with Rees et al. (2022) and Liang et al. (2023), we do not include online bullying in that definition. Suicidal ideation is based on the question, "During the past 12 months, did you ever seriously consider attempting suicide?" Sample weights are generated using the individual State YRBS-provided weights and the Surveillance, Epidemiology, and End Results Program (SEER) data. Standard errors are in parentheses and clustered at the state level. Standard errors are in parentheses and clustered at the state level. All regressions control for flavored ENDS restrictions, ENDS minimum legal sales age of 18 laws, Tobacco-21 laws, and ENDS taxes (in 2023$). Additional controls for *Current Cigarette Smoking* include indoor vaping bans, ENDS online sales bans, indoor smoking bans, and tobacco licensure laws.