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Cigarette Taxes and the Household Budget*

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Abstract

We study the effects of cigarette excise taxes on smokers' household budgets. In a randomized survey experiment, smokers respond to tax increases by adjusting cigarette shopping behaviors, substituting towards other tobacco products, and reducing both discretionary and human capital-related expenditures. Using Consumer Expenditure Survey data and a quasi-experimental design, we find cigarette taxes reduce smoking prevalence but increase cigarette expenditures among continuing smokers. Additionally, a $1 increase in cigarette taxes causes a 2.12% decline in human capital-related expenditures among below median income smokers. Our work uncovers important unintended consequences of cigarette taxes, particularly for low-income individuals.

Keywords: Cigarette Taxes; Household Budgets; Human Capital.
JEL Classification: I10; I12; I14; I18

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1 Introduction

Cigarette smoking remains the greatest cause of preventable mortality in the United States, and cigarette excise taxes are a commonly utilized tobacco control policy (Le et al., 2024). Despite a voluminous literature on the effects of cigarette taxes on smoking prevalence, very little is known about how smokers reallocate their household budgets when cigarette taxes increase. Cigarette taxes are estimated to be highly regressive (Allcott et al., 2019), particularly in light of significant socioeconomic gradients in smoking prevalence that exist by education and poverty status, especially in rural areas (Garrett et al., 2019; Darden, 2021). Standard consumer theory suggests that other types of tobacco consumption may increase or decrease when cigarette prices increase, depending on the extensive margin participation elasticity of cigarettes (i.e., the effect of a 1% increase in taxes on smoking prevalence), the direction of income effects, and the degree of substitution between cigarettes and other tobacco products.[1] Estimates of the participation elasticity range between -0.1 and -0.3, which suggests that significant fractions of smokers continue to smoke following cigarette tax increases.[2] To the extent that smokers accommodate tax increases by reducing expenditures on human capital investments, the long-run welfare implications of cigarette taxes may not be fully understood.

In this paper, we study the effects of cigarette taxes on household budgets in two ways. First, we present results from a randomized experiment within a novel survey. In a sample of 2,005 cigarette smokers, we asked a series of questions on expectations regarding expenditures across a variety of spending categories. For some respondents, we frame these questions in the context of a hypothetical cigarette tax increase as a proportion of their current expenditures on cigarettes. We ask respondents about their intentions to keep smoking; their shopping behavior with respect to cigarettes; their use of alternative nicotine delivery products, including e-cigarettes; and their expenditures on more discretionary categories, such as entertainment, and less discretionary categories, such as housing and medical care. We also ask participants an open-ended question on their expectations regarding expenditures over the following year. Our sample is balanced on demographic, socioeconomic, and tobacco use characteristics across treatment and control arms.

From our survey experiment, we report four main results. First, conditional on baseline characteristics and behaviors, the intention to quit cigarettes over the next year is 4.2 percentage points (21.9%) higher in the group randomly exposed to a hypothetical cigarette tax. The implied extensive margin participation elasticity from these results is -0.21. Second, smokers exposed to the cigarette tax were significantly more likely to compensate through their tobacco shopping behaviors, including buying cigarettes in bulk (17.5%); buying cheaper cigarettes (63.2%); buying cigarettes in lower tax states (133.4%); and buying e-cigarettes

[1] Pass-through of cigarette taxes to smokers is between 80% and 120% (Hanson and Sullivan, 2009). See Kenkel et al. (2014) for evidence that cigarettes are a normal good among low income smokers.

[2] See DeCicca et al. (2022) for a comprehensive review of the effects of cigarette taxes.

(19.0%). Third, we use ChatGPT (model o3-mini-high) to classify open-ended responses to the question about expected expenditures, and we estimate that those in the tax treatment group were significantly more likely to write about tobacco substitution (93.9%) and cigarette cessation (52.1%). Finally, we find that the treatment effect of the hypothetical cigarette tax on other expenditures depends on baseline behavior. For example, for those smoking heavily at baseline, the tax causes significant increases in the expectation of spending more on cigarettes and e-cigarettes, and larger reductions in expected spending on entertainment, groceries, clothing, and transportation. For all respondents, we find that the tax causes large expected reductions in clothing, healthcare, and housing. These results provide suggestive evidence that adjustments to the household budget may span discretionary and non-discretionary spending categories.

Our second approach pairs detailed, nationally representative expenditure diary data from the Consumer Expenditure Survey with a quasi-experimental design. Specifically, we study state-by-quarter variation in expenditures around variation in cigarette taxes from 1996 through 2022. We find that a $1 tax increase leads to a one percentage point reduction (5.5%) in the fraction of households purchasing cigarettes and an overall increase in household quarterly cigarette expenditures by approximately $7.76/quarter (11%). In the subsample who have positive cigarette expenditures (i.e., those who continue to smoke despite a cigarette tax increase), our point estimate on cigarette expenditures more than quadruples to a $33/quarter (8.7%). Focusing on this group, we analyze spending across a broad set of expenditure classes, including both discretionary and non-discretionary spending. We find that a $1 increase in the cigarette tax causes a decrease in gas station purchases of $10.95/quarter (1.47%). We define "human capital expenditures" as those relating to shelter, clothing, education, and health. We show that this category falls significantly ($47.93/quarter or 2.12%) for below-median income smokers.

Results from our two approaches contribute to the literature on compensatory behavior with respect to tobacco control policies. For example, Adda and Cornaglia (2006) show that smokers respond to cigarette taxes by smoking each cigarette more intensely.[3] Similarly, Adda and Cornaglia (2010) show that an unintended consequence of smoke-free laws is an increase in secondhand-smoke exposure at home. There is also a significant literature on tax avoidance through both cross-border and illicit market cigarette shopping (Lovenheim, 2008; Goolsbee et al., 2010; DeCicca et al., 2013, 2022), with evidence that up to 21% of aggregate cigarette sales in the United States may be from cross-border purchases (National Research Council and Institute of Medicine, 2015). Smokers may also respond to changes in *relative* prices of cigarettes due to tax increases by substituting towards other tobacco products. For example, Pesko et al. (2020) find significant evidence of economic substitution between traditional cigarettes and e-cigarettes when cigarette taxes increase. Our contribution to this literature is to measure how smokers reallocate their budgets across a

[3]See Abrevaya and Puzzello (2012) and Adda and Cornaglia (2013) for a debate over these findings.

wide variety of other expenditure categories. An important implication of our findings is that smokers who continue smoking following a tax increase do not economize only on discretionary spending items. Indeed, we find consistent evidence of both stated and revealed expenditure cuts in areas such as education, housing, and healthcare.

These results also contribute to the larger literature on optimal sin taxes by examining how varying tax rates influence consumption patterns across different socioeconomic groups. While the optimal cigarette tax is decreasing in the concentration of prevalence among the poor, higher cigarette taxes may be justified on equity grounds to the extent that lower socioeconomic status (SES) groups exhibit greater relative behavioral biases or greater elasticity of demand (Gruber and Kőszegi, 2004; Allcott et al., 2019). Less is known about how smokers, and lower SES smokers in particular, reallocate their budgets when cigarette taxes increase. For those smokers who continue smoking, we find larger human capital expenditure reductions for those with household incomes below the sample median.

Our paper proceeds as follows. Section 2 provides a conceptual framework for our work by applying standard consumer theory to the trade-off between cigarette and all other categories of expenditures. Section 3 presents our randomized experimental design and the associated findings from our survey work. Section 4 describes the Consumer Expenditure Survey, our quasi-experimental research design, and the associated findings. Section 5 summarizes what is learned from the combination of our survey and observational data evidence, and Section 6 concludes.

2 Conceptual Framework

We study the ways in which cigarette taxes lead households to reallocate their budgets. In this section, we sketch a model of general and tobacco product consumption consistent with standard consumer theory. When cigarette taxes increase, some smokers may be induced to quit. In such cases, resources previously devoted to cigarettes are freed for general consumption. For those individuals who continue smoking following a tax increase, general consumption may increase or decrease, and the model highlights both the determinants of general consumption patterns and areas of heterogeneity in the responsiveness to cigarettes taxes that we can take to data.

Consider an individual with utility over cigarettes C, e-cigarettes E, and a composite consumption good Y. In a standard static model without borrowing or saving, the budget constraint equates consumer income I with the sum of the dollar expenditures on each good. Normalizing the price of composite consumption to \$1, the budget constraint is:

$$Y + P_c C + P_e E = I$$

Given estimated pass-through rates of cigarette taxes to consumers of between 80% and 120% (Hanson and Sullivan, 2009), our analysis assumes that a $t = \$1$ increase in the cigarette tax is entirely passed on to consumers in the form of a \$1 increase in cigarette prices. Differentiating the budget constraint with respect to the cigarette tax yields a sufficient condition for $\frac{\partial Y}{\partial t} < 0$:

$$|\frac{\partial C}{\partial t}| < \frac{C + P_e \frac{\partial E}{\partial t}}{P_c}.$$

In words, this says that an increase in the cigarette tax decreases general consumption Y when the magnitude of its effect on cigarette consumption is less than the right-hand side, which includes the baseline level of cigarette consumption C and the degree of substitution between cigarettes and e-cigarettes. Here, for a given cigarette tax increase, general consumption will decrease more when the price elasticity of demand for cigarettes is smaller. Similarly, those with greater baseline cigarette consumption C must decrease general consumption more when cigarette taxes increase. Both the baseline level of cigarette consumption and the price elasticity of demand for cigarettes relate to nicotine addiction. Smokers who are more heavily addicted to nicotine face a greater degree reinforcement and withdrawal effects Becker and Murphy (1988), and, all else equal, these smokers will have smaller price elasticities and greater levels of consumption.

The framework also highlights the substitutability between cigarettes and e-cigarettes, an alternative nicotine delivery device in e-cigarettes. In general, for a given cigarette tax increase, consumption should decrease more in the degree of substitution between cigarettes and e-cigarettes. Studies using variation in cigarette and e-cigarette taxes generally find that these products are economic substitutes, but the magnitude of the cross-price elasticities vary significantly (Pesko et al., 2020; Saffer et al., 2020; Pesko and Warman, 2022; Cotti et al., 2022; Allcott and Rafkin, 2022; Friedman and Pesko, 2022; Abouk et al., 2023b,a; Diaz et al., 2025; Begh et al., 2025).

The model above suggests that changes in household spending due to cigarettes taxes will depend on baseline levels of cigarette consumption, the price elasticity of demand for cigarettes, and the cross-price elasticity of demand between cigarettes and e-cigarettes. Taking this framework to data, *ex ante* predictors of the relevant elasticities include demographic, socioeconomic, and health characteristics, in addition to baseline behavioral information such as cigarette intensity, the use of non-combustible tobacco products, and the shopping behavior of cigarette smokers (e.g., pack versus carton purchases). We consider a wide set of consumption categories, differentiating between discretionary and non-discretionary spending, and we consider a wide set of alternative nicotine delivery systems, including e-cigarettes, chewing tobacco, nicotine pouches, and snus. Our survey experiment highlights the direction of anticipated changes in these consumption categories in a controlled experimental setting. Our analysis of Consumer Expenditure Survey

data adds evidence on the revealed magnitude of these effects.

3 Evidence from a Survey Experiment

We follow a burgeoning survey research literature within economics (Elías et al., 2019; Stantcheva, 2023; Haaland et al., 2024) that attempts to measure individual behavior, beliefs, expectations, and preferences. We argue that this approach - directly asking smokers about their responsiveness to cigarette excise taxes - complements quasi-experimental investigations of revealed preference data. Employing a survey also allows us to ask open-ended questions regarding respondent preferences, and recent advances in artificial intelligence have allowed economists to analyze such data more efficiently (Korinek, 2023). Furthermore, the survey environment allows us to easily measure responses to randomized information scenarios. Following our conceptual model in Section 2, we ask smokers to forecast their expenditures on cigarettes and a variety of other goods while experimentally varying a significant cigarette tax increase.

We created our survey using Qualtrics, and, on January 29th, 2025, we posted the survey on Prolific, a survey research platform. We offered $12/hour for completion of a survey that was expected to take seven minutes. We used Prolific screening tools to restrict the population of potential respondents to current smokers, with the goal of generating a sample of 2,200 respondents. Data collection was completed on January 31st, 2025. Of the 2,200, we construct a final sample of 2,005 respondents who completed the survey in its entirety. Appendix Section 1 documents our sample construction methods and provides further details on our survey.[4]

Respondents were first asked a series of baseline questions regarding socioeconomic characteristics (i.e., education and income) and tobacco habits. We asked respondents about the frequency and intensity of their cigarette smoking behavior; the frequency of their consumption of other tobacco products (i.e., e-cigarettes); their typical cigarette expenditures ($/week) and purchasing behavior (e.g., pack vs. carton purchases); and the typical locations where they purchase cigarettes (e.g., gas stations). The first column of Table 1 presents means of our baseline respondent characteristics. Our sample of 2,005 cigarette smokers is 56.9% female and 73% white; roughly 38% of the sample holds a college degree or higher, and the mean income is $68,920/year; and 49.7% of our sample have children under the age of 18 living in the home. Turning to tobacco habits, 78.5% of our sample smoke cigarettes every day. The mean number of cigarettes per day on a day when a respondent smokes is 11.86, and the mean expenditure on cigarettes per week is $42.58. 73.1% of our sample purchases cigarettes by the pack, and 16.3% of our sample use some other form of tobacco every day. The overwhelming majority of respondents (81.7%) claim to purchase cigarettes at gas stations.

[4]The full surveys are available for the control and treatment groups.

3.1 Experimental Design

To measure the effects of cigarette taxes on household spending, respondents were randomized into two groups.[5] The control group were asked a series of questions about their anticipated cigarette, non-cigarette tobacco, and general spending habits over the next 12 months. For example, the control group respondents were asked:

> Thinking about the next 12 months, relative to your current smoking habits, please indicate which option best reflects your expectations regarding your smoking behavior,

where the possible answers were:

- I will completely quit smoking.
- I will reduce my smoking but not quit.
- My smoking behavior will not change.
- I will smoke more.

In contrast, an equally sized treatment group were asked:

> For the next several questions, suppose that your state passed a new cigarette tax increase effective immediately. The new tax will cause the amount you must spend per week on cigarettes to increase by XX/week. That is, to maintain your current smoking habits under the new tax, your weekly spending on cigarettes would go from YY/week to ZZ/week.
>
> Given this tax increase, thinking about the next 12 months, relative to your current smoking habits, please indicate which option best reflects your expectations regarding your smoking behavior.

Respondents in the treatment group were presented the same set of responses regarding anticipated smoking behavior. The hypothetical tax increase was 100% of a given respondent's weekly expenditure on cigarettes. For example, if a respondent claimed to be spending \$40/week=YY on cigarettes, the hypothetical tax increase would be \$40/week such that this person would now be required to spend \$80/week=ZZ at their current smoking intensity and shopping behavior. By design, our hypothetical tax increase is substantial, and our goal was to simulate an environment in which the empirical evidence has demonstrated measurable effects.[6] For example, Callison and Kaestner (2014) frame their results relative to a 100% increase in

[5] Our experimental design was pre-registered at the AEA RCT Registry, AEARCTR-0015056.

[6] DeCicca et al. (2022) reports that cigarette taxes constitute roughly 40% of the tax-inclusive mean price per pack in the United States. Given pass-through rates between 80-120%, we interpret a \$1 increase in the cigarette tax as a \$1 increase in the cigarette price. The tax increase required to double expenditures under these assumptions would be approximately 400%.

cigarette prices specifically because their estimated effects (a 5% decline in smoking prevalence) are so small. In our context, to elicit measurable effects on household spending, we chose to simulate a large cigarette tax increase.

Table 1 shows balance in the baseline mean characteristics across the treatment and control arms, which suggests that the randomization successfully created observationally similar groups that differ only in the framing of the survey questions.

For the treatment group, each subsequent question included the sentence:

> Suppose that cigarette taxes increased as described previously.

Both treatment and control groups were asked subsequent questions regarding their anticipated behavior with respect to cigarette shopping behavior (e.g., brand preference, bulk purchasing, etc.) and other tobacco products (e.g., e-cigarettes). Next, respondents in the control and treatment group were respectively asked an open-ended question regarding their expectations:

> Thinking about the next 12 months, describe the changes, if any, you hope to make to your household's spending habits (on cigarettes or otherwise).

or

> Suppose that cigarette taxes increased as described previously. Thinking about the next 12 months, describe the changes, if any, you hope to make to your household's spending habits (on cigarettes or otherwise),

Using ChatGPT, we categorized these open-ended responses with the following prompt:

> I have attached 2005 open-ended responses to a question about household spending habits over the next 12 months. Please categorize these responses into an appropriate number of representative response types. Please provide Stata code that generates binary variables for each response type and identifies the appropriate value for these binary variables for each individual open-ended response.

ChatGPT identified seven non-mutually exclusive categories of responses: 1) spend more, 2) reduce nonessentials, 3) no change, 4) tobacco substitution, 5) reallocate, 6) reduce smoking, and 7) other. For each category, we created a binary variable that we treat as a dependent variable below.

To evaluate the impact of the hypothetical cigarette tax increase, we estimate versions of the following regression:

$$y_i = \alpha_0 + \alpha_1 1[Treatment_i] + X_i\beta + \epsilon_i, \tag{1}$$

where y_i is the expected behavior, preference, open-ended category, or outcome for individual i, and X_i is a vector of all baseline characteristics included in Table 1. Our coefficient of interest is α_1, which captures group differences across the treatment and control groups conditional on X.[7]

Our final set of questions asked respondents to rate how they expected their expenditures in a variety of areas to change over the next 12 months. For each expenditure category, respondents were asked to respond on a five-point Likert scale from "spend much less" to "spend much more". We asked about expenditures in the following areas:

- Cigarettes
- Other Tobacco
- Entertainment
- Groceries
- Dining Out
- Clothing
- Transportation (including gasoline and car payments)
- Healthcare (including prescription drugs)
- Housing (including rent and repairs)
- Education (including books and supplies).

These data lend themselves to the following multinomial logit model:

$$\frac{lnP(y_i = k)}{lnP(y_i = 0)} = \gamma_{0k} + \gamma_{1k} 1[Treatment_i] + X_i \delta_k, \tag{2}$$

where the log odds of each spending response are modeled as a function of the treatment group indicator and the vector of baseline characteristics. For simplicity we collapse these responses into spending more or less, and we estimate these relative to the "no change" in spending response. We also estimate an ordered logit model that takes advantage of the natural ordering of responses and that produces a point estimate on the direction of expenditures differences between treatment and control groups.

[7] The inclusion of X does not significantly change the estimated value of α_1, but we include X in our regressions for improved efficiency.

3.2 Results

Figure 1 presents estimates and 95% confidence intervals of α_1 from Equation 1 for several direct cigarette and other tobacco categories of spending. For each category, we report the control mean in parentheses. The hypothetical cigarette tax increases causes a statistically significant 4.22 percentage point (21.86%) increase in the expectation of quitting cigarettes in the next 12 months. The effect is larger, 10.84 percentage points (22.92%), for reducing cigarette smoking but not quitting.[8] Next, the survey asks a series of non-mutually exclusive questions regarding cigarette shopping adjustments over the next 12 months, including buying cigarettes in bulk, purchasing a cheaper brand of cigarettes, shopping in lower tax states, buying cigarettes in informal markets or online, and buying loose tobacco to roll one's own cigarettes. Relative to the control group who were asked general expectations about their behaviors, the tax treatment group were significantly more likely to buy future cigarettes in bulk (5.35 pp, 17.47%); buy cheaper cigarette brands (16.57 pp, 63.23%); buy future cigarettes in lower tax states (14.92 pp, 133.40%); buy future cigarettes online (5.06 pp, 43.67%); and buy loose tobacco for rolling individual cigarettes (6.12 pp, 91.70%). We also asked respondents about their expectations regarding their consumption of other tobacco products over the following 12 months. Respondents were asked to select all non-combustible tobacco products that they expected to use more of in the following 12 months. Treatment group respondents were 7.89 pp (19.02%) more likely to claim they will use more e-cigarettes over the next 12 months, but there were no significant differences in the expected use of nicotine pouches, snus, or chewing tobacco.[9]

Figure 2 reports the effects of the tax on the likelihood that a respondent wrote about each of the respective open-ended categories, as categorized by ChatGPT. The Figure reports each category along the X-axis (sorted by control mean prevalence) and the treatment effect of being in the hypothetical tax increase group. For example, 3% of the control mean wrote responses that indicated that they intended to spend more over the next year, and the treatment group prevalence of this type of response was 2.25 pp (73.95%) higher. Spending more may be consistent with intentions to quit smoking. There were not statistical differences in the proportion of treatment and control group respondents who wrote about "reducing nonessential" expenditures or who claimed there would be little change in expenditures. However, those in the tax treatment group were 10.42 pp (83.94%) more likely to write about tobacco substitution (e.g., to e-cigarettes) over the next year relative to the control group, and the treatment group were 9.50 pp (52.07%) more likely to write about reducing or quitting smoking. Treatment group respondents were 3.70 pp less likely (-24.01%)

[8]The cigarette smoking response includes mutually exclusive categories for quitting smoking, reducing smoking, smoking more, and no change in smoking. We report all coefficients in Appendix Tables 2 through 4.

[9]The control arm incorrectly included an additional option for "other tobacco products" that was not included in the treatment arm question. Because alternatives in this question were not mutually exclusive – respondents were asked to assess expected consumption of each tobacco product separately – this error should not bias the effects of the hypothetical tax on alternative tobacco products.

to write about reallocating to more savings, and they were 6.60 pp (12.66%) less likely to write about all other things.

Figures 1 and 2 provide clear evidence that a large hypothetical cigarette excise tax shifts stated preferences among smokers. In our survey, the hypothetical tax increase doubles the amount spent on cigarettes per week. DeCicca et al. (2013) reports a real-world participation elasticity (i.e., the percentage change in smoking prevalence due to a 1% increase in cigarettes prices) of between -0.1 and -0.3. In our case, a 100% increase in cigarette prices is associated with a stated 21% decrease in smoking prevalence on the extensive margin or a -0.21 participation elasticity. We also document considerable scope for economic substitution between cigarettes and e-cigarettes. The implied cross-price elasticity between cigarettes and e-cigarettes is 0.192. Both of these effects are confirmed in the open-ended respondent sentiment data. The Figures also demonstrate the heterogeneity in responses. For example, some respondents clearly intend to offset the expected costs by changing their shopping behavior (e.g., seeking cheaper brand cigarettes). These (unmodeled) responses may limit changes in modeled cigarette and e-cigarette consumption.

To investigate how the hypothetical tax affects other types of expenditures, Figure 3 presents the marginal effects on each potential response (increase, decrease, or no change) from our multinomial logit specification in Equation 2. Highlighting the role of heterogeneity in responses to cigarette taxes, the treatment group is both more likely to spend less (5.75 pp) and more (9.28 pp) on cigarettes relative to the control group. The treatment group is also more likely (5.21 pp) to spend more on other tobacco products. Turning to other spending categories, the treatment group is significantly less likely to spend more on entertainment (8.01 pp), groceries (14.78 pp), dining out (3.39 pp), clothing (9.54 pp), transportation (14.94 pp), healthcare (9.15 pp), housing (16.59 pp), and education (7.09 pp). These results highlight an important nuance of responsiveness: the general perception is of increasing expenditures across the board, but those smokers facing a tax increase do not expect their expenditures to increase.

To investigate heterogeneity in these effects, Table 2 presents estimates from ordered logit models of each spending category on the tax treatment indicator and the baseline controls. The ordered logit coefficient points to the direction of spending effects that can be causally attributed to the tax increase. Column 1 of Table 2 reports coefficient estimates on the binary tax variable for the full sample of 2,005 respondents. Consistent with Figure 3, where the tax causes both an increase in "spend more" and "spend less" on expected cigarette expenditures, in Table 2, the ordered logit coefficient on cigarette expenditures is economically and statistically insignificant. In the full sample, the tax causes statistically significant decreases in expenditures on groceries, clothing, transportation, healthcare, housing, and education. In each of these cases, Figure 3 shows that the estimated reductions are relative to expected increases in the control group. That is, control individuals expect expenditures in these categories to increase, and the tax causes treatment group

respondents to revise these expectations upwards.

The value of Table 2 is in demonstrating how these coefficients change when estimated on specific subgroups of interest. In column 2, we restrict the analysis to those reporting heavy smoking (20 or more cigarettes per day) at baseline. Relative to the full sample estimates, heavy smokers anticipate spending more on both cigarettes and other tobacco products. These effects likely reflect the nature of addiction and significant evidence that, for heavier smokers, the price elasticity of demand is smaller (DeCicca et al., 2022). Furthermore, because these smokers expect to spend more on tobacco, they systematically expect to spend significantly less on all other categories relative to the full sample. These results are consistent with our conceptual framework - those with smaller price elasticities of demand and larger cross price elasticities should expect to spend significantly less on other consumption. We also present ordered logit coefficient estimates for subsamples of lower SES respondents, including those with annual income less than the 25th percentile and those with a high school education or less. Relatively low income smokers are statistically similar to the full sample in their responses to the tax, with the exception of dining out, on which they are more likely to spend less. Relatively low education respondents are less likely to increase spending on other tobacco products relative to the full sample, and they expect larger reductions on dining out, transportation, and healthcare. We also estimate the ordered logit on the subsample with children living at home. For these respondents, the effect of the tax was a significant reduction in cigarette smoking. Compared to the full sample, effects on other categories were attenuated in many cases. Finally, we estimate the ordered logit on those respondents who claim to exclusively purchase their cigarettes at gas stations at baseline (n=1,639). These results mirror the full sample results, but respondents notably claim the tax will cause greater reductions in transportation spending (-0.680 vs. -0.599) relative to the full sample.

Evidence from our randomized survey experiment suggests several key results. First, the hypothetical cigarette tax generates extensive margin effects regarding cigarettes that are similar to those observed in the large cigarette tax literature. Second, we find significant evidence that the hypothetical tax induces substitution away from cigarettes and towards nicotine alternative, particularly e-cigarettes. This evidence is apparent in both quantitative measures of stated-preferences and in open-ended qualitative data. Third, with regard to relatively elective expenditure categories such as entertainment, the effects of the hypothetical cigarette tax largely depend on the first-order question of how smoking behavior changes - for heavy baseline smokers, expected expenditure reductions are larger. Finally, there is evidence that the tax reduces expenditures across a broad set of non-discretionary categories, including healthcare, housing, and education, particularly for heavy smokers.

While the survey allows us to isolate the impact of a hypothetical tax, there are two key limitations to this approach. First, stated preferences such as the intention to quit smoking may deviate significantly

from revealed preferences. Second, our view is that it unrealistic to expect survey participants to accurately gauge the size of expenditure changes over the next year, so our survey does not shed light on the magnitude of these expenditure adjustments. To address these limitations, we turn to nationally representative data from the Consumer Expenditure Interview Survey. Paired with a standard quasi-experimental design that exploits policy variation across states and time, we are able to assess the magnitude of expenditure changes across broad class of categories following cigarette tax changes.

4 Evidence from the Consumer Expenditure Survey

We utilize data from the Consumer Expenditure Survey (CE), administered by the Bureau of Labor Statistics (BLS), spanning 1996 to 2022. The CE collects nationally representative data on household expenditures from U.S. consumer units (CUs)[10]—a unit broadly equivalent to a household, although multiple CUs can reside within the same household if individuals maintain financial independence.[11]

The CE plays a central role in constructing the "basket of goods" used in the Consumer Price Index, which underpins U.S. inflation measurement. It consists of two complementary survey instruments: the Interview Survey (CE-I) and the Diary Survey (CE-D). While both aim to capture comprehensive expenditure patterns, they differ in survey design, timing, and the granularity of expenditure data.

4.1 The Interview Survey (CE-I)

The CE-I is a rotating panel survey designed to collect data on durable and recurring household expenses. Each CU participates in up to four in-person interviews spaced three months apart, covering one full year. Each interview asks about expenditures in the prior three months, and CUs exit the panel after completing all four waves.

Respondents are typically adult household members most familiar with the unit's finances—often referred to as the "reference person." In their absence, another informed individual may be selected to complete the interview.[12] Interviews average 60 minutes and use product-specific recall periods—for instance, monthly bills are referenced over the past month, irregular or large purchases over three or twelve months, and cigarette spending is framed weekly.

Importantly for this study, cigarette expenditures are directly recorded as a standalone category and

[10]Neither the CE-I nor CE-D includes CUs from five U.S. states (Arkansas, Montana, New Mexico, North Dakota, and Wyoming), and coverage in another ten (Idaho, Iowa, Maine, Mississippi, North Carolina, Oklahoma, Rhode Island, South Dakota, Vermont, and West Virginia) is irregular. Consequently, our analysis focuses on CUs from the 35 states with consistent representation throughout the study period.

[11]For example, a household with three financially independent roommates would constitute three separate CUs.

[12]See `https://www.bls.gov/respondents/cex/faqs.htm` for guidelines on respondent eligibility.

framed using the question: **"How much do you or your household usually spend each week for cigarettes?"** This wording allows comparison with short-horizon diary data while enabling quarterly panel tracking. Because we observe expenditures over three-month intervals, the CE-I allows for analysis of the extensive margin of smoking behavior: if a CU reports no cigarette purchases over an entire quarter, it provides a more credible signal of cessation than shorter observation windows.

The CE-I encompasses 14 major categories: Food, Alcohol, Housing, Apparel, Transportation, Entertainment, Personal Care, Education, Cash Contributions, Healthcare, Tobacco, Reading, Retirement and Pension Contributions, and Miscellaneous along with over 600 subcategories recording individual line items such as milk or diesel fuel. Demographic data are also collected, including CU size, pre-tax income, sex and race of the reference person (White, Black, or Other), and urban or rural status.

Expenditure responses are normalized by the BLS to reflect quarterly values. We use these to construct a household-by-quarter panel with up to four quarters per CU. Spending changes are dated to the midpoint between interviews, allowing us to aggregate outcomes by calendar quarter. Policy variables are then merged at the quarterly level, so that partial-quarter effects are included in the post-period designation (Bureau of Labor Statistics, 2024).

4.2 The Diary Survey (CE-D)

The CE-D provides a complementary snapshot of small, frequent expenditures. Each CU maintains a real-time log of all purchases made over two weeks, recording the amount, date, location, and item category. This high-frequency tracking minimizes recall error, though it imposes higher respondent burden and is conducted only once per CU (i.e., not longitudinal).

We use the CE-D to construct two distinct expenditure aggregates focused on day-to-day consumption:

1. **Human Capital Forming Expenditures**: Adapted from Kraay (2018), this measure captures investments in household health, development, and education. It includes spending on:
    - Food at home
    - Housekeeping supplies
    - Nonprescription drugs and vitamins
    - Personal care products and services
    - Baby food and formula
    - Clothing for boys, girls, and infants
    - Fuel and utility expenses

- School supplies
- Reading materials
- Health-related supplies

2. **Gas Station and Convenience Store Expenditures**: Designed to capture incidental spending during routine gas station convenience store visits, this includes:
   - Snack foods
   - Carbonated soft drinks
   - Cookies, crackers, and baked goods
   - Alcohol consumed away from home (e.g., beer)
   - Automotive fuel
   - Lottery tickets

   This bundle is constructed based sales data from convenience retail industry reports.[13]

Although CE-D's cross-sectional structure limits its use for longitudinal analysis, it offers enhanced granularity for discretionary, high-frequency expenses. However, cigarette expenditures are not disaggregated from broader tobacco-related products in the CE-D, and the absence of repeat surveys limits its utility in tracking cessation.

4.3 Empirical Design

We implement the Dynamic Difference-in-Differences (DCDH) Estimator developed by De Chaisemartin and d'Haultfoeuille (2024) to analyze the impact of excise taxes on cigarette sales.[14] The De Chaisemartin and d'Haultfoeuille (2024) methodology is designed to handle staggered policy adoption and continuous variation in treatment levels. This is particularly well suited for our setting because it accommodates non-binary, gradually increasing treatments—such as varying cigarette tax increases—while accounting for both current and past policy changes. This flexibility is critical in our context, where tax adjustments occur at different times and in different magnitudes, and standard difference-in-differences estimators may not fully capture these dynamics.

We use the state as the unit of analysis and the calendar quarter as the temporal unit. We calculate cigarette taxes by summing state and population-weighted average local cigarette tax rates by state for

[13] These six categories represent over 92% of all convenience store sales.(National Association of Convenience Stores, 2019)

[14] The Stata command for this method is `did_multiplgt_dyn`. De Chaisemartin and d'Haultfoeuille (2024) describes how the method estimates event studies.

each quarter. From Q1 1996 to Q4 2022, the United States experienced 227 state and local-level cigarette excise tax changes, with state cigarette taxes ranging from a low of \$0.17 to a high of \$5.01, and cumulative within-state changes from \$0.00 to \$4.36. Cigarette tax increases are categorized into \$1 thresholds up to \$4 and beyond (e.g., [0,1), [1,2), [2,3), [3,4), [4,∞]). This approach allows us to explore the effects of units transitioning from one tax treatment category to another. Units that previously shared the same tax treatment category serve as controls, providing a robust analysis of shifts due to policy changes. It is important to note that this categorization results in a loss of some variation. After binning, we identify 71.4% of population-weighted cigarette tax changes as treatments. This means that 28.6% of tax changes were too small to move the cumulative cigarette tax from bin k to bin $k+1$. This induces attenuation bias, rendering our treatment effect estimates more conservative than they would be under a fully continuous specification. Nonetheless, the binning strategy preserves sufficient granularity to facilitate meaningful comparisons across policy-switching units.

To address potential confounding factors, our regression specifications incorporate a range of policy and demographic controls. These include state-level cigar taxes from the CDC State System, population-weighted local and state e-cigarette taxes (Cotti et al., 2024), and the average state-level distance to the nearest lower-tax border to account for cross-border shopping behavior.

Macroeconomic conditions are captured using the unemployment rate to reflect broader business cycle effects, and all monetary values—both spending and tax variables—are inflation-adjusted to 2020 U.S. dollars. To address potential behavioral responses to policy announcements, we also control for periods in which cigarette tax increases were announced but not yet implemented.

Demographic controls include consumer unit characteristics such as sex, race, urban or rural residence, and household size, helping mitigate concerns about non-random exposure to cigarette tax changes. Standard errors are clustered at the state level to account for serial correlation within states.

All regressions are weighted using the BLS variable `finlwt21`, which represents the final post-stratified population weight for each consumer unit in the CE. This weight adjusts for survey design features, non-response, and known population totals—ensuring that estimates are representative of the national U.S. population across time and space.

4.4 Results

We begin by examining the extensive margin of cigarette spending, defined as the probability that a household reports any cigarette purchases within a given quarter. Table 4 presents our baseline estimates of the effect of cigarette tax increases on this outcome. For the full sample, a \$1 increase in cigarette taxes is associated

with a 1 percentage point reduction in the likelihood of cigarette spending—a decline of approximately 5.5% relative to the baseline mean. When we restrict the sample to baseline smoking households,[15] the tax-induced reduction is slightly larger in absolute terms—2.3 percentage points—which corresponds to a 2.7% decline.

Figure 4 complements these findings by showing that the likelihood of reporting cigarette expenditures remains stable in the pre-policy period, suggesting no evidence of anticipatory behavioral adjustments.

A heterogeneity analysis of the extensive margin reveals that the response to tax increases varies across different demographic groups. As shown in Figure 5, non-white, above median size, and below median income households are more likely to reduce or altogether cease cigarette purchases when faced with higher taxes. This suggests that these groups are particularly sensitive to tax-induced price changes, likely due to tighter budget constraints or differing consumption patterns.

Next, we examine the intensive margin by focusing on the level of cigarette spending among households. As shown in Table 4, in the second column, a $1 increase in cigarette taxes leads to an increase in household cigarette spending of $7.76 per quarter, representing a rise of just over 11% relative to the mean quarterly spending of $67.80. These estimates capture the combined effects of changes in the probability that households enter or exit the cigarette market (the extensive margin) and adjustments in spending among households that continue purchasing cigarettes (the intensive margin). When the analysis is restricted to households that report cigarette purchases (with average spending of $380.23 per quarter), a $1 tax increase is associated with an increase of $33 per quarter representing an increase of 8.6%.[16] Figure 6 provides visual evidence from event study estimates corresponding to Table 4. We find no evidence of pre-trends in cigarette spending.[17]

Heterogeneity in the level of cigarette spending further reveals that the magnitude of spending increases is not uniform across all groups. As illustrated in Figure 7, among households that continue purchasing cigarettes, the largest increases in spending are observed in non-white, larger, and below median income households. This indicates that these groups, while possibly more prone to reducing the probability of purchasing cigarettes altogether, exhibit more pronounced increases in spending when they do continue buying, highlighting a complex responsiveness to tax changes.

Using the diary data, we examine the impact of cigarette taxes on household tobacco spending. Table 5 presents our key diary-based estimates. For the full sample of households, a $1 increase in cigarette taxes is associated with a $13.36 increase in quarterly tobacco expenditures. Among households actively

[15] Baseline smoking households are defined as those that report any cigarette purchases in the first wave of their CE-I participation. This definition mitigates concerns about misclassifications due to short-term abstention.

[16] Appendix Figure 1 visualizes our model's robustness to evaluation timing, showing cigarette spending responses by number of pre- and post-periods observed.

[17] We also show how cigarette taxes affect all 13 additional non-cigarette spending categories using CE-I data in Appendix Table 7.

purchasing tobacco, the effect is substantially larger—rising to $68.35 per quarter—reflecting concentrated price sensitivity among tobacco users.

Although these estimates are sizable and statistically significant, we do not emphasize them as central findings of this study because the diary data aggregates all tobacco products—including cigarettes, cigars, chewing tobacco, and related goods—into a single category, preventing us from isolating cigarette-specific consumption behavior. Moreover, the diary survey is less well-suited to capturing longer-run patterns or identifying baseline smokers. For these reasons, we rely more heavily on the interview data for measuring cigarette use and interpret the tobacco spending results from the diary as suggestive but not definitive.

Our primary interest in analyzing the diary data lies in measuring downstream spending behaviors associated with budget reallocation, specifically, the categories of spending that are potentially crowded out. To that end, we turn to human capital expenditures, which include spending on groceries, personal care products and services, health supplies, educational items, and goods for children. These categories reflect essential investments in health and long-term well-being. Table 5 also reports the estimated effects of cigarette taxes on this composite category. For all households, a $1 tax increase reduces human capital expenditures by $4.06 per quarter. Among tobacco-purchasing households, this effect rises to a reduction of $24.72 per quarter, suggesting that increased cigarette costs may come at the expense of more productive forms of spending.

Although these estimates are economically meaningful, they fall short of conventional significance thresholds. Still, results are robust to leave-one-out tests across component categories, as reported in Appendix Table 5. Figure 8 provides corresponding event study evidence, showing no evidence of pre-trends in human capital spending, which supports the identification strategy.

Heterogeneity analyses, however, reveal important variation. Illustrated in Figure 9, below median income families experience a significantly larger and statistically significant reduction in human capital expenditures, indicating that budget constraints drive larger spending reallocations among lower-income smokers. This finding highlights the regressivity and potential welfare consequences of cigarette taxes for economically disadvantaged households.

Building upon prior research indicating that approximately 69.1% of cigarettes are purchased at gas stations or convenience stores (Kruger et al., 2017), along with our own sample evidence (Table 1) showing that 81.7% of respondents report purchasing cigarettes at gas stations, we specifically examine household expenditures at gas stations using CE-D data.

Gas stations frequently serve as points of bundled consumption, where tobacco is purchased alongside other common items such as auto fuel, snacks, beverages, and lottery tickets. These sites therefore offer a meaningful window into short-term consumption trade-offs prompted by cigarette tax increases.

Table 5 presents the estimated effects of a $1 increase in cigarette taxes on quarterly gas station expenditures. Among all households, we find a statistically significant reduction of $6.96 per quarter. The effect is even more pronounced among tobacco-purchasing households, for whom gas station spending declines by $10.95 per quarter. Results are robust to leave-one-out robustness tests across component categories, as reported in Appendix Table 6. These findings underscore the extent to which tobacco taxes affect broader retail spending patterns, particularly in locations where multiple types of consumption co-occur.

These findings are further supported by the event study presented in Figure 10, which show a clear post-treatment decline in gas station expenditures following tax increases, with no evidence of pre-trend violations.

Given the central role of auto fuel in these bundled purchases, we also isolate this subcategory and find that, as presented in Figure 10, among tobacco-purchasing households, cigarette tax increases reduce fuel spending by $7.78 per quarter. This suggests that a potential unintended benefit of cigarette taxation is less gasoline purchases.

To validate the robustness of our findings, we conduct a placebo test using the CE-I and CE-D data. We simulate tax changes by randomly assigning each observed tax increase to different states while holding the actual enactment dates and tax magnitudes constant. This process is repeated 100 times for each sample, with pseudo-treatment states varying in each iteration. Figure 11 shows that our estimates for changes in cigarette spending are more extreme than would be expected from random chance. Furthermore, this holds for our estimate for changes in human capital forming expenditures among smoking households.

Collectively, our results suggest that households may finance increased cigarette expenditures through reallocation of spending away from both discretionary and essential goods and services. While the CE-I data highlight substantial adjustments on both extensive and intensive margins, the CE-D data provide crucial complementary evidence, capturing real-time reallocation. The stronger reductions observed among lower-income households in human capital expenditures underscore the regressive nature of these budgetary shifts, whereas the consistency across demographic groups for gas station expenditures further emphasizes broad-based household adjustments.

5 Discussion

When consumers operate under a budget constraint, cigarette price increases due to taxation must lead to a change in behavior. Our survey results provide stated preferences in response to theoretical tax increases, while the CE data provide revealed preferences in response to actual tax increases.

When comparing results across the survey and CE data, we find relatively consistent evidence regarding

cigarette use. On the extensive margin, survey responses suggest that a doubling of cigarette prices, driven by a tax increase, would meaningfully increase the likelihood that smokers report quitting, based on stated preferences. In the CE data, a $1 increase in cigarette taxes is associated with a 2.3 percentage point reduction in smoking participation among baseline smoking households.

These effects correspond to similar extensive margin participation elasticities. The elasticity implied by the survey data is approximately -0.21, while a back-of-the-envelope calculation using the weighted average cigarette tax across states in our sample ($2.31 per pack) yields an elasticity of -0.127.[18]

On the intensive margin, among baseline smoking households in the CE-I, we show an increase in cigarette purchasing of $7.76 per quarter from a mean of $67.80, and $32.99 from a mean of $380.23 for smoking households. Survey responses include substitution to cheaper cigarettes.

Gasoline purchases (and purchases at gas stations more generally) account for a sizable share of reduced spending on non-cigarette goods following cigarette tax increases, but not all of it. A $1 increase in cigarette taxes reduces gasoline purchases among all respondents by $7.39 (from a mean of $559.28, or 1.3%) and by $10.96 (from a mean of $746.87, or 1.5%) for smoking households.[19] Gasoline purchases are not counted as human capital investments in our study; however, they could indirectly have positive or negative effects on human capital. Gasoline purchase reductions could have unintended benefits in terms of improving air quality and reducing global climate change, or could have unintended consequences if this transportation is making it more difficult for people to access employment and healthcare, for example.

Findings concerning substitution are more nuanced. Using the CE-D data we see evidence of spending adjustments "in the moment" in terms of purchases at gas stations, with reductions in gas station purchases ($10.95) and automotive fuel ($7.78). At the same time, we see negatively signed but statistically insignificant estimates for reductions in human capital spending of $24.72. This aligns with the basket of responses in terms of stated preferences, where respondents indicated heightened willingness to reduce spending on transportation and groceries, but also categories like housing and healthcare.

Estimates are not homogeneous across the sample in either the survey or CE. In particular, survey data shows that lower-education and lower-income consumers show an increased likelihood of reducing transportation, housing, clothing, and education expenses relative to the full sample. In the CE, non-white and below-median-income households show the largest increases in cigarette spending alongside a statistically significant decline in human capital spending. Taken together, these hint at potentially troubling responses to cigarette taxes among the most vulnerable consumers.

Our survey data is limited in the sense that it must necessarily be prospective on the part of the respon-

[18] $\frac{\%\text{ change in participation}}{\%\text{ change in tax}} = \frac{(0.01 \div 0.18)}{(1 \div 2.31)} = \frac{0.055}{0.433} \approx -0.127$.

[19] These percent change differences could be narrowed by cigarette smokers spending more in automotive fuel to travel to locations with lower cigarette taxes.

dent, who is imagining their behavior in the context of a hypothetical tax change. The CE data allows us to examine whether these stated preferences accurately reflect consumer behavior in the face of real-world tax increases, but it too has limitations. Most critically, we cannot examine a long-term longitudinal sample of consumer behavior, limiting our ability to control for individual heterogeneity. In addition, we lack detailed information on spending in terms of both cigarettes and tobacco substitutes, meaning that we cannot examine changes on either the extensive or intensive margins of cigarette smoking in an ideal manner. Finally, we lack detailed behavioral and health data on CE respondents. Despite these limitations, we believe that the wide variety of spending categories captured in the CE-I and CE-D, along with our survey data, allow us to provide a meaningful analysis of how consumers respond to cigarette tax increases in terms of budget reallocation and tobacco use.

6 Conclusion

Cigarette taxes are most commonly associated with improvements in human capital. For example, Simon (2016) finds that in-utero exposure to higher cigarette taxes reduces childhood school sick days, doctor visits, hospitalizations, and asthma. Hoehn-Velasco et al. (2023) finds that a mother's own exposure to cigarette taxes while in-utero lowers her likelihood of smoking and increases her educational attainment at the time she gives birth for the first time. Additionally, Friedson et al. (2023) finds that a $1 increase in cigarette taxes reduces mortality by 4%.

Interestingly, these observed health and human capital gains do not appear to be driven by increased positive investment behavior, such as higher health or education spending. In fact, our results suggest that cigarette taxes often *displace* such investments. Among low-income households that smoke, we find that cigarette tax increases are associated with a reduction in human capital expenditures.

Based on our estimates from Tables 4 and 5, approximately 69.2% of the increase in cigarette spending following a tax hike is offset by reductions in human capital expenditures, while the remaining 30.8% comes from reductions in other day-to-day consumption, such as gas station purchases. This back-of-the-envelope calculation implies that the majority of the adjustment to higher cigarette prices comes from reallocation away from investments that may support long-term health and socioeconomic outcomes—potentially undermining the very human capital channels through which cigarette taxes are often presumed to operate.

Taken together, these findings suggest that alternative mechanisms—such as biological improvements in health or shifts in societal norms—outweigh any negative impacts resulting from short-term reductions in human capital investments. Moreover, substantial heterogeneity likely exists regarding who benefits from and who is harmed by cigarette taxes. For instance, non-smoking households may gain from increased tax revenue

and reduced exposure to secondhand smoke, whereas low-income households may suffer disproportionately due to higher addiction rates, limited access to FDA-approved smoking cessation products, and fewer means of evading higher taxes. Cigarette taxes could possibly be more effective in improving human capital if compensating for such heterogeneity.

In theory, a revenue-neutral Pigouvian tax with lump-sum redistribution targeted at human capital expenditures could resolve this paradox. Such a "double-dividend" would internalize the externalities from cigarette consumption while also reinvesting tax revenues into human capital. Specifically, these earmarked funds should be directed toward populations—such as low income smokers—that reduce their own human capital spending in response to cigarette taxes. Opportunities could include free or heavily subsidized access to nicotine replacement therapies, counseling, and FDA-approved smoking cessation medications, and/or financial incentives conditioned upon participation in smoking cessation programs or achieving milestones like maintaining smoking abstinence, attending health check-ups, or enrolling children in preventive health program. In practice, however, earmarking funds may not increase net expenditures if legislators reduce general appropriations by the amount earmarked (Khanal et al., 2024). Funding new initiatives that otherwise would not have occurred, as was the case for many provisions funded by California's Proposition 99 in 1988 (Abadie et al., 2010), could mitigate these fungibility concerns.

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7 Tables and Figures

Table 1: Tobacco Survey: Balance Table

	Overall	Tax Treatment		p-value
		No (n=1,019)	Yes (n=986)	
Age	42.275	42.697	41.840	0.110
Female	0.569	0.559	0.578	0.398
Race/Ethnicity				
White	0.733	0.725	0.740	0.444
Asian	0.023	0.025	0.022	0.743
Black	0.148	0.149	0.146	0.844
Mixed	0.063	0.071	0.055	0.143
Other	0.033	0.030	0.037	0.448
Education				
Less than High School	0.018	0.015	0.021	0.270
High School	0.206	0.201	0.211	0.566
Some College	0.388	0.403	0.373	0.170
College Graduate	0.254	0.250	0.258	0.687
Graduate Degree	0.134	0.132	0.137	0.735
Income ($10,000)	6.89	7.01	6.77	0.303
Children in Home	0.497	0.501	0.493	0.717
Tobacco Behaviors				
Daily Smoking	0.785	0.784	0.785	0.961
Cigarettes/Day	11.857	11.509	12.216	0.058
Cigarette Spending/Week $	42.58	41.78	43.411	0.21005
Never uses Non-Combustibles	0.419	0.423	0.416	0.751
Daily Non-Combustible Use	0.163	0.168	0.157	0.507
Purchasing Behavior				
Pack	0.731	0.733	0.728	0.795
Carton	0.167	0.164	0.171	0.689
Single	0.067	0.068	0.066	0.877
Loose	0.035	0.035	0.036	0.980
Purchasing Location				
Grocery Store	0.451	0.461	0.441	0.367
Gas Station	0.817	0.810	0.826	0.356
Tobacco Shop	0.527	0.525	0.528	0.880
Friends and Acquaintances	0.099	0.106	0.092	0.305
In Another State	0.046	0.037	0.055	0.062
Native American Res.	0.062	0.060	0.065	0.641
Online	0.047	0.051	0.044	0.435

Table 1 shows the overall and treatment specific means of baseline variables from the tobacco survey conditional on nonmissing values. There are most 1.3% missing values for any given baseline variable. Appendix Table 1 presents summary statistics on missing values. The survey ran from January 29th, 2025 through January 31st, 2025 on the survey research platform Prolific. The overall sample include 2,005 current or recent cigarette smokers, as defined by Prolific screening tools. The p-value represents the two-sided t-test p-value for equality of means. Questions on cigarette purchasing behavior and location are not mutually exclusive.

Table 2: Ordered Logit Estimates of Expected Expenditure Changes

	Full Sample	Heavy Smoker	Low Income	Less Edu.	Has Children	Gas Stations
n=	2,005	414	600	448	984	1,639
Cigarettes	-0.071	0.242	0.007	-0.143	-0.297	-0.079
	(0.093)	(0.201)	(0.177)	(0.206)	(0.136)	(0.104)
Other Tobacco	0.057	0.521	0.173	-0.217	-0.070	0.111
	(0.088)	(0.203)	(0.163)	(0.194)	(0.126)	(0.097)
Entertainment	-0.142	-0.481	-0.383	-0.150	0.021	-0.140
	(0.088)	(0.203)	(0.163)	(0.192)	(0.125)	(0.098)
Groceries	-0.458	-0.821	-0.471	-0.418	-0.461	-0.477
	(0.090)	(0.213)	(0.166)	(0.194)	(0.128)	(0.100)
Dining Out	0.140	-0.328	-0.159	-0.270	0.452	0.157
	(0.086)	(0.203)	(0.163)	(0.189)	(0.124)	(0.096)
Clothing	-0.247	-0.779	-0.308	-0.471	0.046	-0.218
	(0.088)	(0.208)	(0.163)	(0.194)	(0.125)	(0.098)
Transportation	-0.599	-0.848	-0.736	-0.873	-0.529	-0.680
	(0.097)	(0.224)	(0.178)	(0.215)	(0.134)	(0.111)
Healthcare	-0.345	-0.662	-0.222	-0.767	-0.261	-0.454
	(0.100)	(0.243)	(0.184)	(0.227)	(0.137)	(0.113)
Housing	-0.699	-1.239	-0.739	-0.828	-0.445	-0.867
	(0.102)	(0.263)	(0.191)	(0.230)	(0.137)	(0.119)
Education	-0.168	-0.354	-0.358	-0.184	-0.253	-0.132
	(0.094)	(0.215)	(0.174)	(0.205)	(0.134)	(0.106)

Table 2 shows estimates of the coefficient on the tax treatment group from ordered logit models of expected expenditures. Each cell represents an estimate from a separate regression, corresponding to the respective expenditure category and subsample. Column (1) represents the full sample of 2,005 respondents. Heavy smoking is defined as smoking 20 or more cigarettes per day at baseline. Low income represents those at or below the 25th percentile of income, approximately $30,000/year, in our sample. Less education refers to those with a high school degree or less. Has children refers to those with dependent children under age 18 living in the home. Gas only refers to those respondents who only shop for cigarettes at gas stations at baseline. All estimates are conditional on the baseline characteristics in Table 1.

Table 3: Descriptive Means of BLS CE Data and Policy Merge Data

	CE Interview Data		CE Diary Data	
	Full Sample	Cig Purchasers	Full Sample	Tobacco Purchasers
	(1)	(2)	(3)	(4)
Tobacco spending	74.166	389.475	51.586	355.169
	(0.317)	(1.252)	(0.394)	(2.061)
Cigarette spending	67.802	380.234	—	—
	(0.304)	(1.222)	—	—
Other tobacco spending	6.364	9.241	—	—
	(0.083)	(0.233)	—	—
Sex of reference person (1=female)	0.506	0.497	0.523	0.487
	(0.001)	(0.002)	(0.001)	(0.003)
White (race of reference person)	0.804	0.821	0.806	0.831
	(0.001)	(0.001)	(0.001)	(0.002)
Black (race of reference person)	0.134	0.132	0.132	0.127
	(0.001)	(0.001)	(0.001)	(0.002)
1 = Urban, 0 = Rural	0.979	0.967	0.982	0.970
	(0.000)	(0.001)	(0.000)	(0.001)
Number of members in CU	2.509	2.666	2.499	2.677
	(0.002)	(0.005)	(0.003)	(0.008)
Total cigarette tax	2.063	1.774	2.113	1.767
	(0.002)	(0.004)	(0.003)	(0.007)
E-cig tax	0.110	0.067	0.114	0.060
	(0.001)	(0.001)	(0.001)	(0.002)
Cigar tax per unit	0.006	0.006	0.006	0.005
	(0.000)	(0.000)	(0.000)	(0.000)
Cigar tax percent	24.856	21.262	25.269	21.225
	(0.037)	(0.077)	(0.054)	(0.123)
Cigar tax cap (1=tax cap present)	0.178	0.173	0.177	0.173
	(0.001)	(0.001)	(0.001)	(0.002)
State-quarter unemployment rate	5.744	5.744	5.748	5.748
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	572,026	104,367	282,554	42,615

CE-I data from 1996 to 2022. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. Standard errors in parentheses.

Table 4: Average Treatment Effects of an Additional $1 of Cigarette Taxes Quarterly Cigarette Spending

Extensive Margin (Any Cigarette Purchase = 1)				
	Full Sample		Smoking Households	
	(1)	(2)	(3)	(4)
Cigarette Tax ($1)	-0.009	-0.010	-0.021	-0.023
	(0.007)	(0.007)	(0.006)	(0.006)
Dep. Var. Mean	0.180	0.180	0.842	0.842
Observations	572,026	572,026	129,868	129,868
Intensive Margin (Cigarette Spending)				
	Full Sample		Conditional On Cigarette Purchase	
	(1)	(2)	(3)	(4)
Cigarette Tax ($1)	7.092	7.757	31.032	32.998
	(2.572)	(2.334)	(6.324)	(5.262)
Dep. Var. Mean	67.802	67.802	380.234	380.234
Observations	572,026	572,026	104,367	104,367
Policy Controls	No	Yes	No	Yes
Demographic Controls	No	Yes	No	Yes
Weighted	Yes	Yes	Yes	Yes

Corresponding event studies are shown in Figure 4 and Figure 6. CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Smoking households are identified by whether they purchase any cigarettes in the first wave of their CE interview. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals, and policy and demographic controls where noted. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. State-clustered standard errors in parentheses.

Table 5: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Quarterly Expenditures

	Full Sample		Conditional On Tobacco Purchase	
	(1)	(2)	(3)	(4)
Tobacco Spending				
Cigarette Tax ($1)	13.329	13.361	67.936	68.345
	(20.900)	(19.979)	(29.932)	(27.694)
Dep. Var. Mean	66.809	66.809	357.0125	357.0125
Human Capital Spending				
Cigarette Tax ($1)	-3.932	-4.060	-21.994	-24.720
	(8.144)	(7.320)	(26.093)	(23.120)
Dep. Var. Mean	1,933.529	1,933.529	2,265.601	2,265.601
Gas Station Spending				
Cigarette Tax ($1)	-6.199	-7.390	-9.737	-10.953
	(3.233)	(2.594)	(3.723)	(3.452)
Dep. Var. Mean	559.275	559.275	746.870	746.870
Automotive Fuel Spending				
Cigarette Tax ($1)	-4.824	-5.369	-7.022	-7.782
	(3.434)	(2.859)	(3.425)	(2.662)
Dep. Var. Mean	413.374	413.374	525.772	525.772
Observations	293,366	293,366	43,272	43,272
Policy Controls	No	Yes	No	Yes
Demographic Controls	No	Yes	No	Yes
Weighted	Yes	Yes	Yes	Yes

Corresponding event studies are shown in Figure 8 and Figure 10. CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals, and policy and demographic controls where noted. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. State-clustered standard errors in parentheses.

Figure 1: Treatment Effects on Tobacco Behaviors.

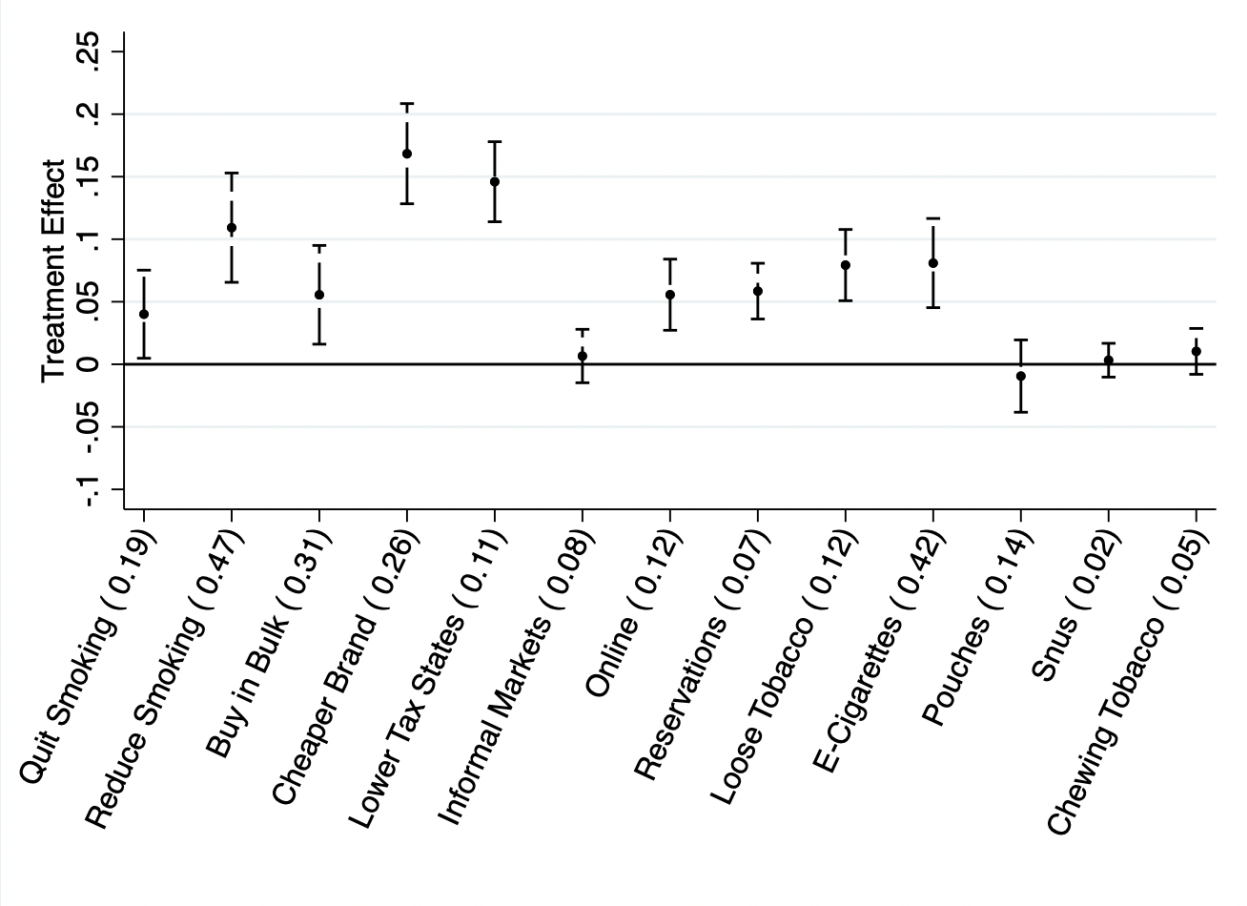


Figure 1 presents treatment effect estimates conditional on the baseline characteristics in Table 1. Each estimate comes from a separate linear probability model of the corresponding behavior. Each behavior is listed with its control group mean. Brackets indicate the 95 % confidence interval. n=2,005

Figure 2: Treatment Effects on Open-Ended Response Categories.

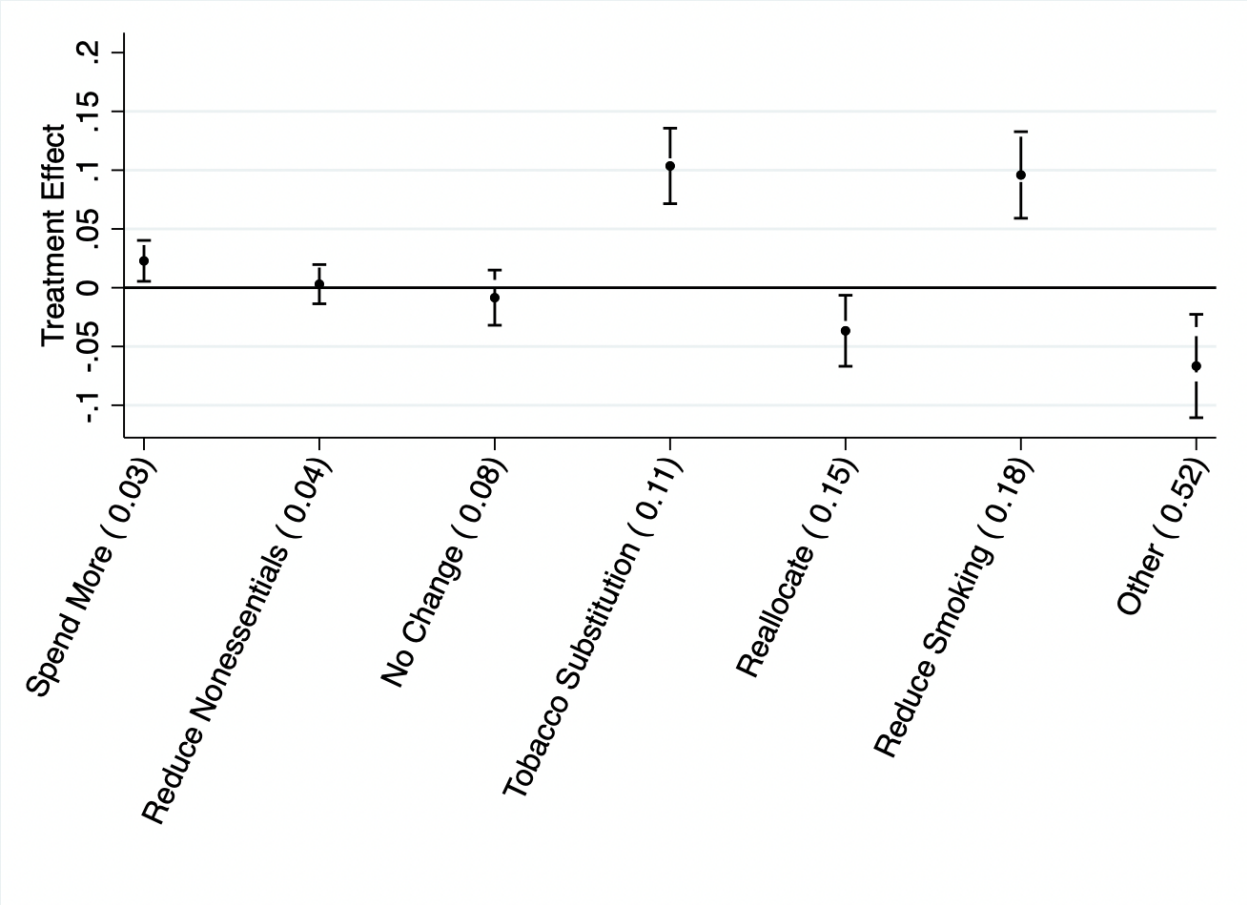


Figure 2 presents treatment effect estimates conditional on the baseline characteristics in Table 1. Each dependent variable is a binary variable that represents a category of open-ended responses to the expectations question. The categories were determined using ChatGPT with the prompt included in the main text. Each estimate comes from a separate linear probability model of the corresponding category. Each category is listed with its control group mean, which reflects it's prevalence in the control group. Brackets indicate the 95 % confidence interval. n=2,005

Figure 3: Marginal Treatment Effects on Expenditures.

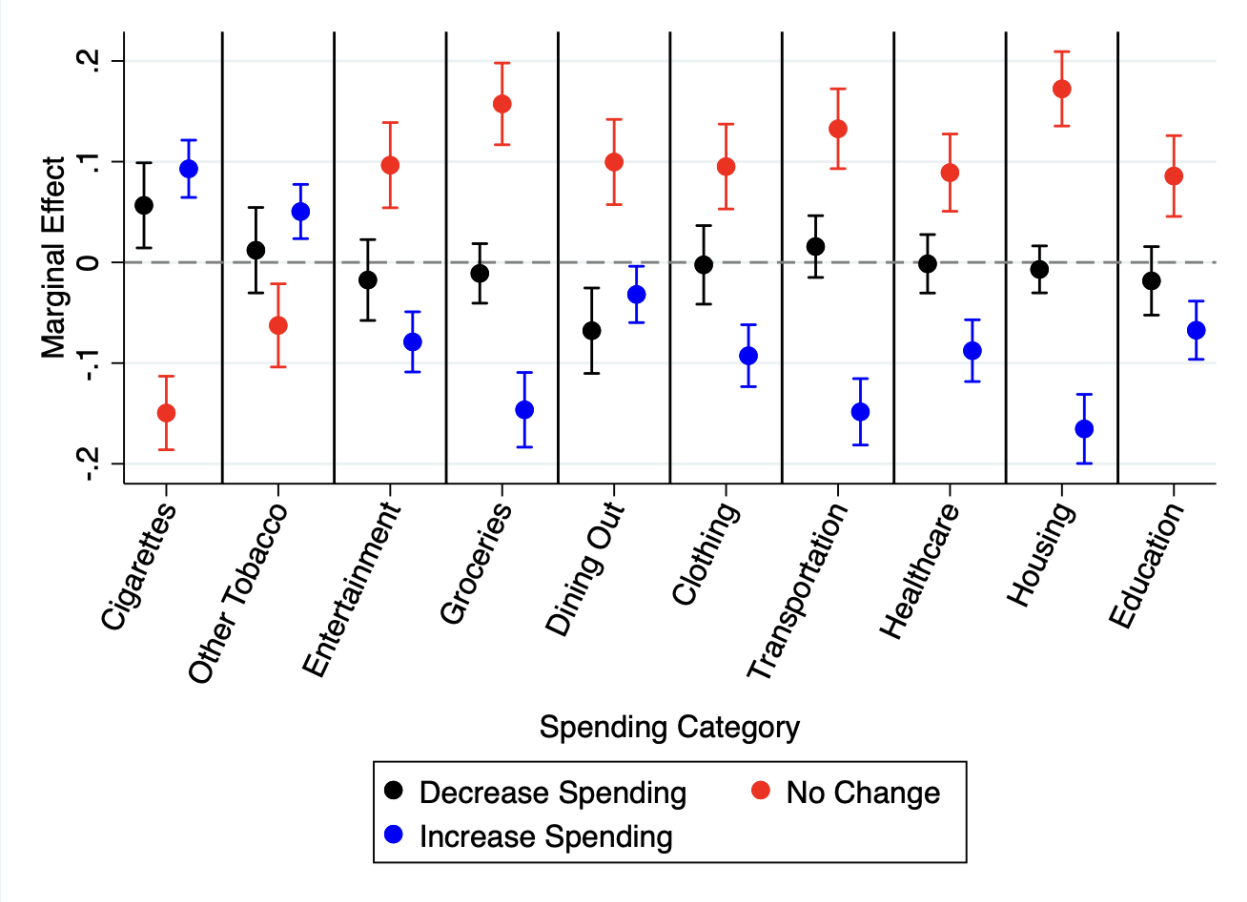
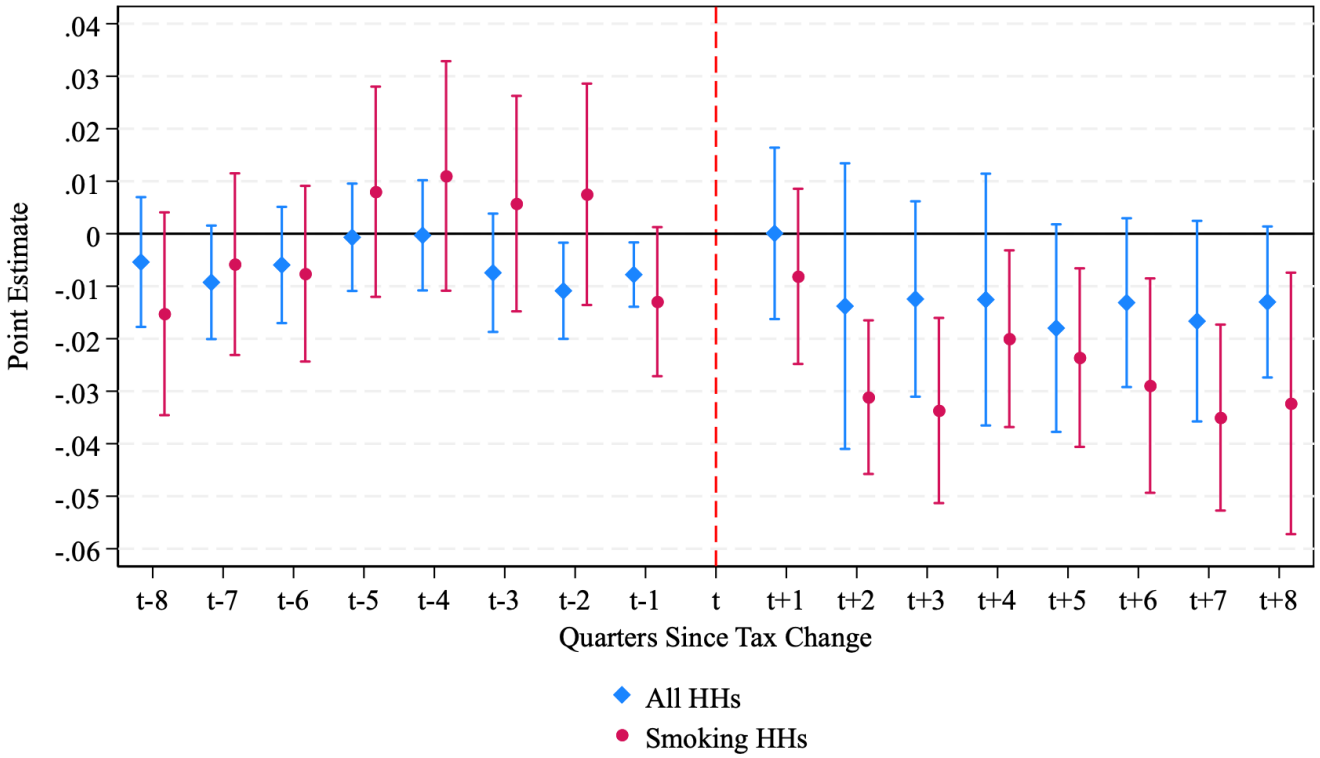


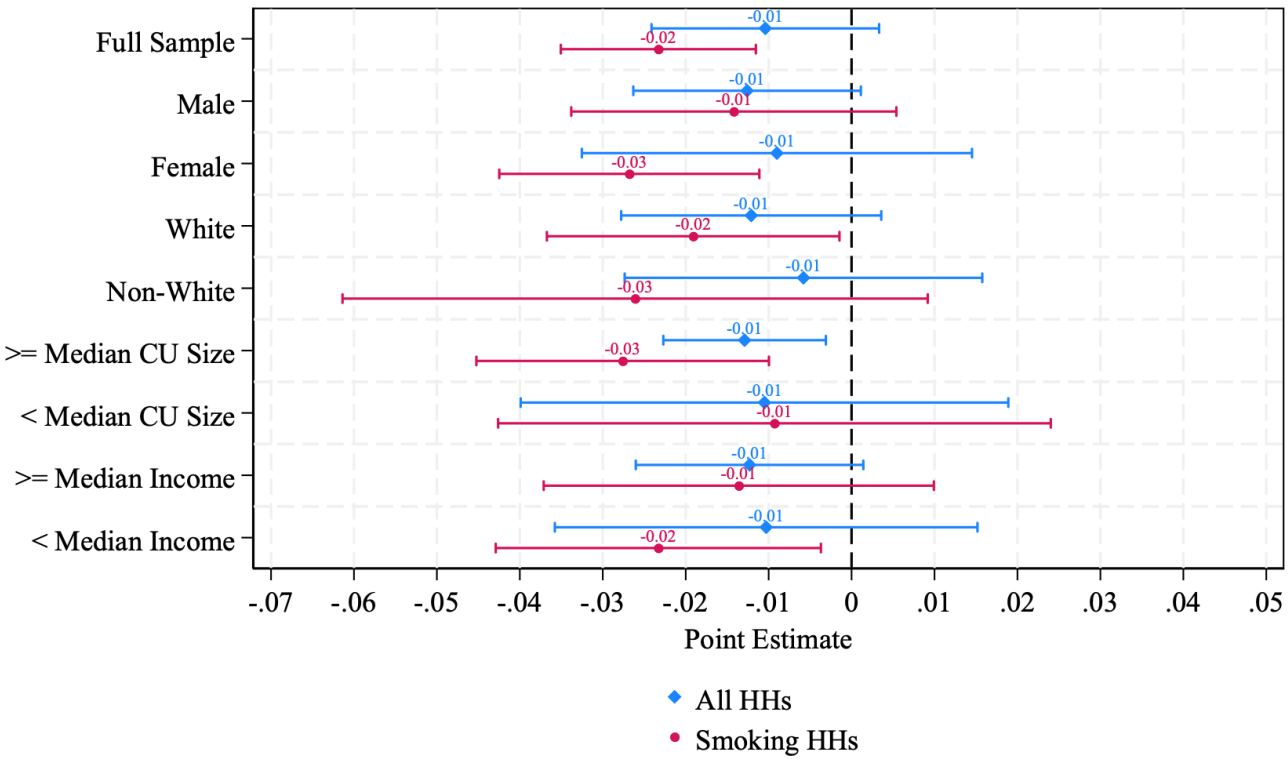
Figure 3 presents marginal treatment effect estimates conditional on the baseline characteristics in Table 1. For each expenditure category, we estimate a multinomial logit model for expecting to spend less or more, relative to no expected change. The marginal effects reflect the change in the probability of selecting a given response for the treatment group relative to the control group. Brackets indicate the 95 % confidence interval. n=2,005

Figure 4: Event Studies For Average Treatment Effects of an Additional $1 of Cigarette Taxes on Any Quarterly Cigarette Spending



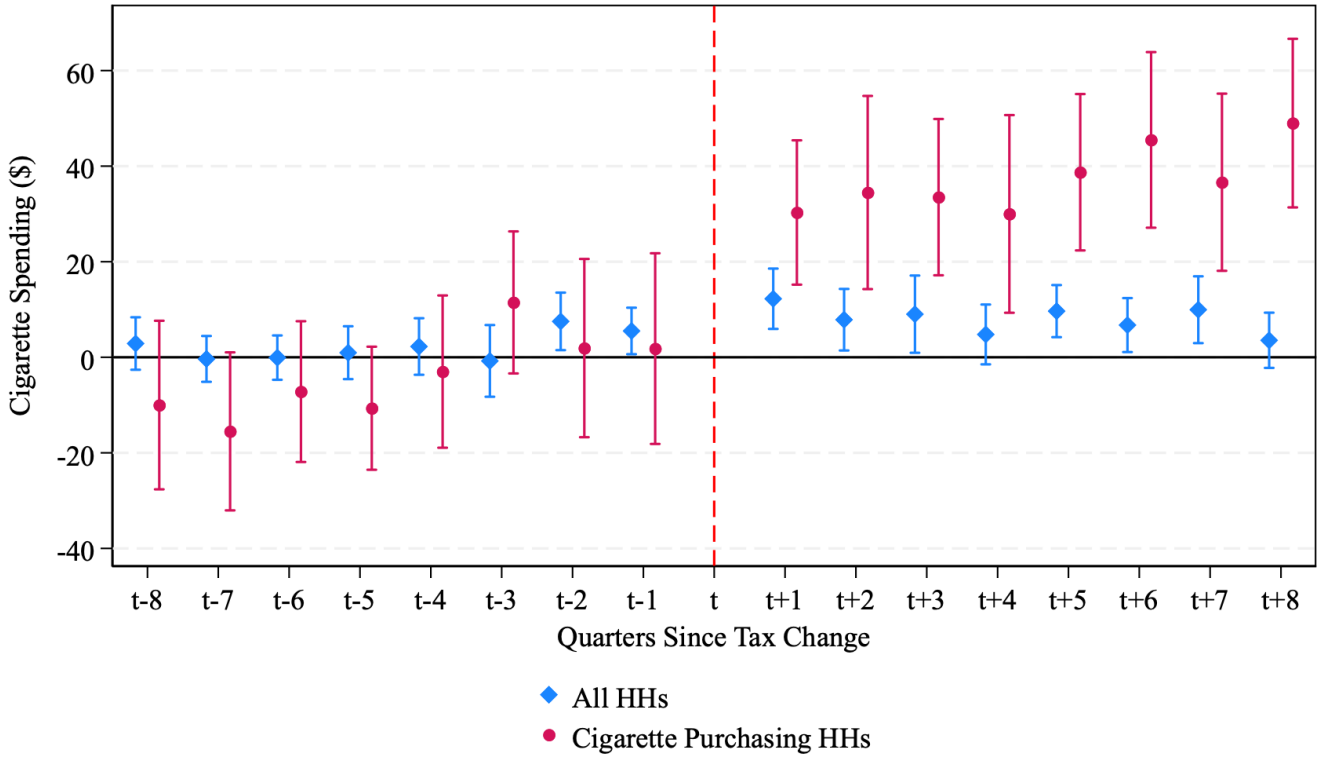
Corresponding ATEs are shown in Table 4. CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Cigarette purchasing households are identified by whether they purchase in the quarter of reference. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. Vertical lines indicate 95% confidence intervals, using state-clustered standard errors.

Figure 5: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Any Quarterly Cigarette Spending, Heterogeneity.



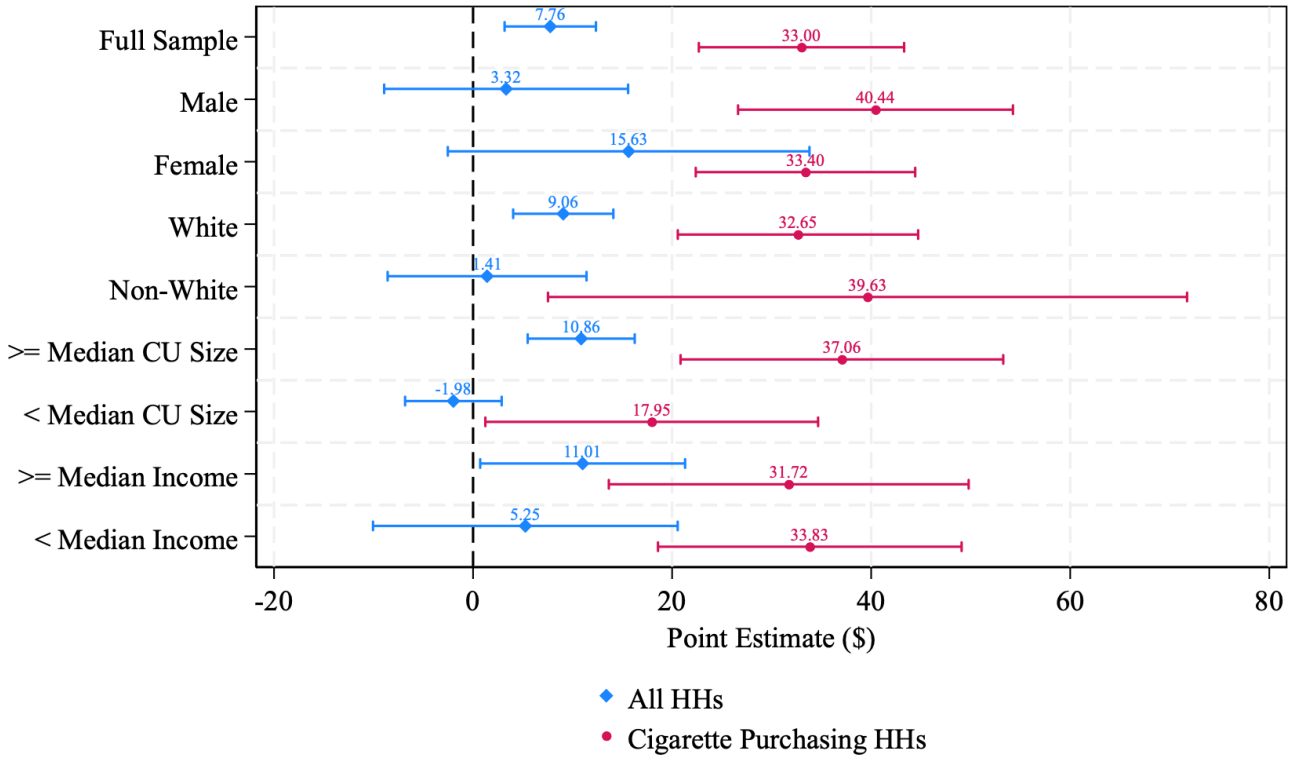
CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Smoking households are identified by whether they purchase any cigarettes in the first wave of their CE interview. ">= Median CU Size" indicates that the CU had greater than or equal to the median number of members in their household. "< Median CU Size" indicates that the CU had less than the median number of members in their household. ">= Median Income" indicates that the CU had a household income level greater than or equal to the median of the sample. "< Median Income" indicates that the CU had a household income level less than the median of the sample. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals, and policy and demographic controls are used for all estimates. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. Horizontal lines indicate 95% confidence intervals, using state-clustered standard errors.

Figure 6: Event Studies For Average Treatment Effects of an Additional $1 of Cigarette Taxes on Quarterly Cigarette Spending



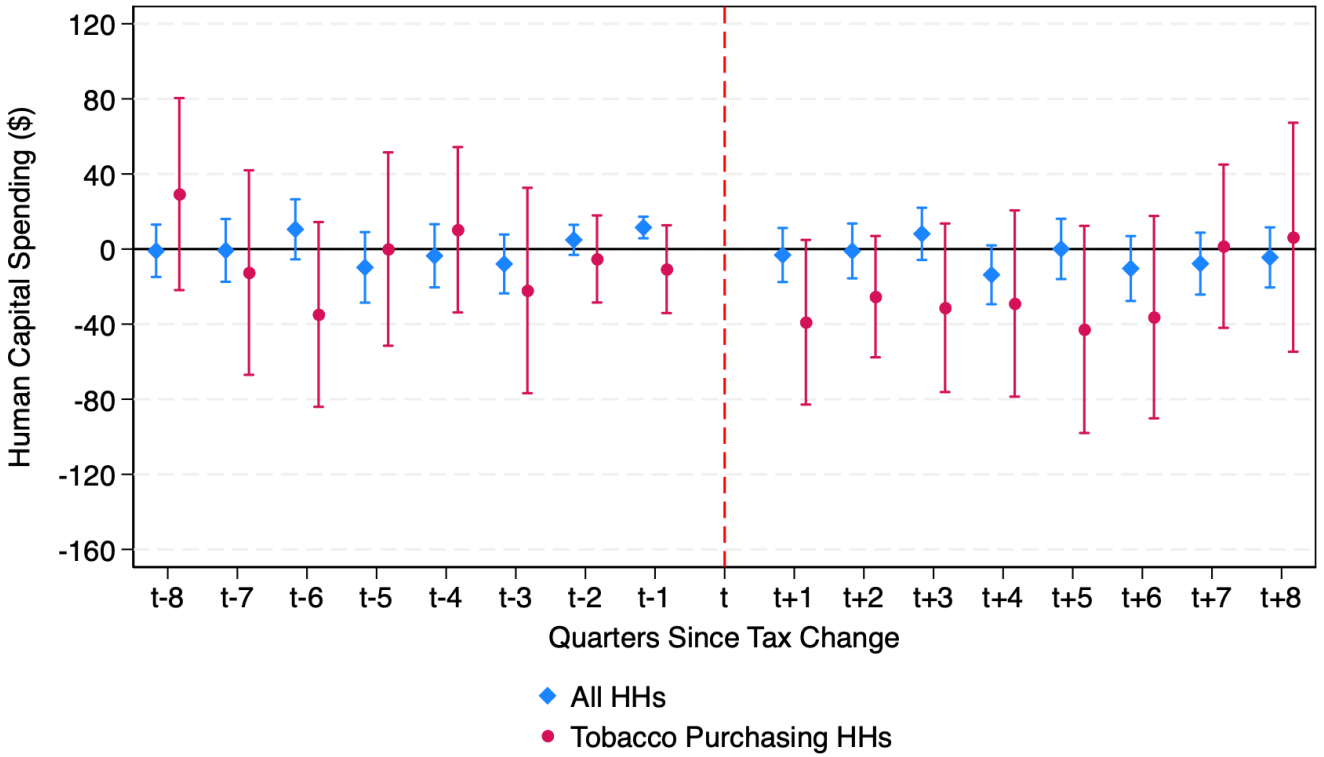
Corresponding ATEs are shown in Table 4. CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Cigarette purchasing households are identified by whether they purchase in the quarter of reference. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals, and policy and demographic controls where noted. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. Vertical lines indicate 95% confidence intervals, using state-clustered standard errors.

Figure 7: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Quarterly Cigarette Spending, Heterogeneity.



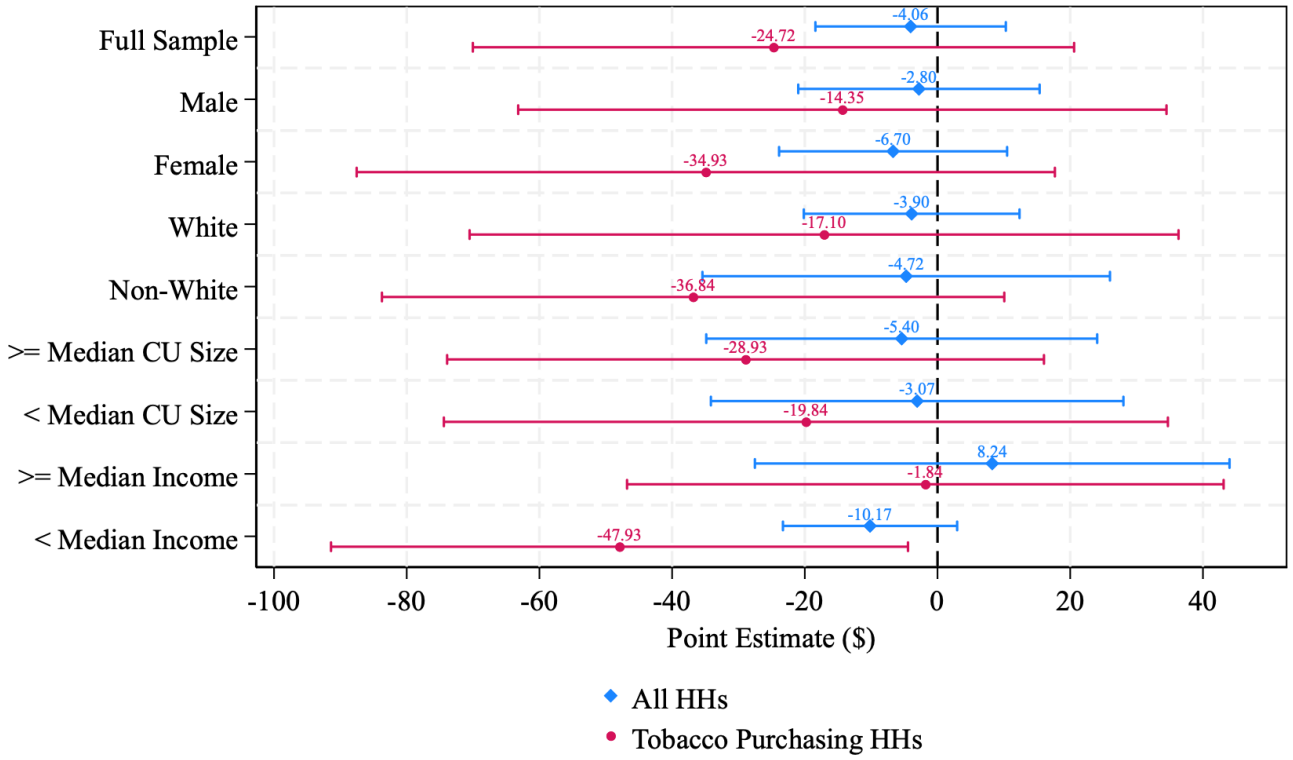
CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Cigarette purchasing households are identified by whether they purchase in the quarter of reference. ">= Median CU Size" indicates that the CU had greater than or equal to the median number of members in their household. "< Median CU Size" indicates that the CU had less than the median number of members in their household. ">= Median Income" indicates that the CU had a household income level greater than or equal to the median of the sample. "< Median Income" indicates that the CU had a household income level less than the median of the sample. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals, and policy and demographic controls are used for all estimates. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. Horizontal lines indicate 95% confidence intervals, using state-clustered standard errors.

Figure 8: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Human Capital Expenditures, Diary Data.



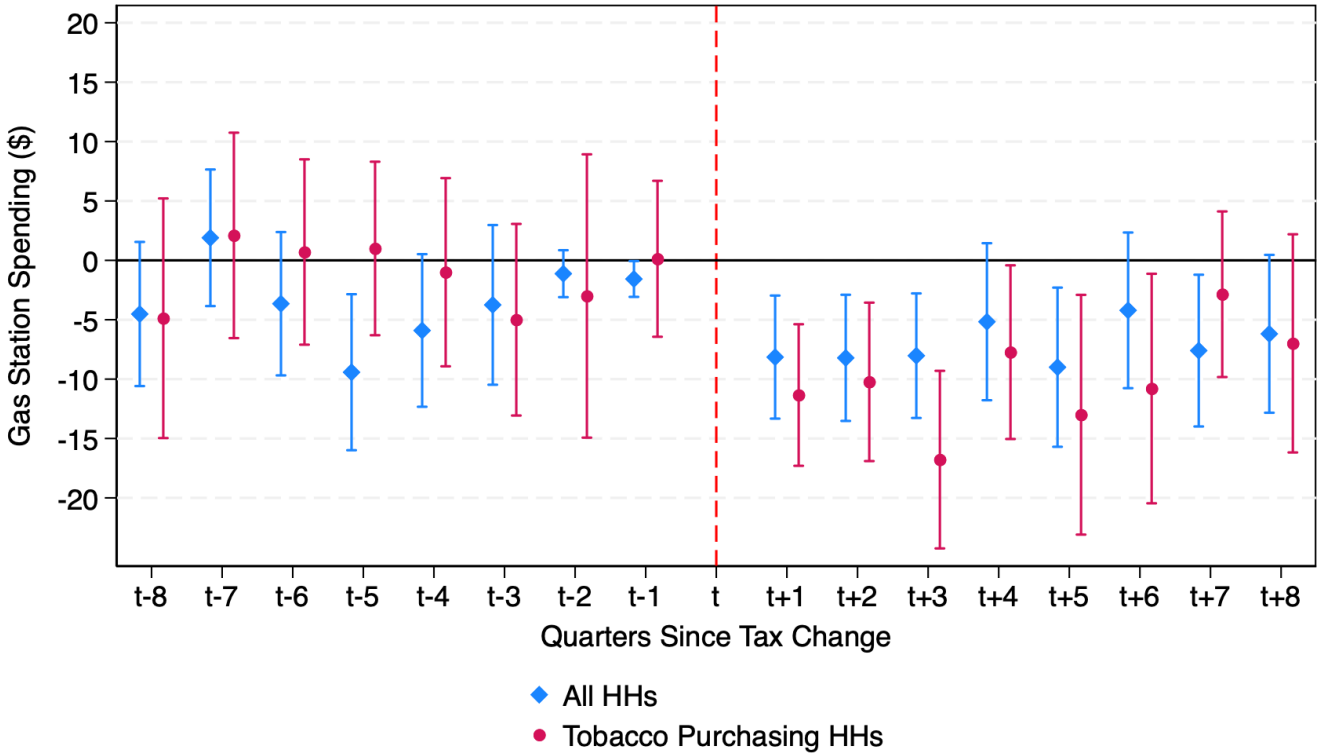
CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. "Human Capital Forming Expenditures" is an aggregate spending category based on the work in Kraay (2018). Following this work, we examine four components of human capital development: shelter, clothing, education, and health. We map these to the CE-D spending variables: foodhome, houskeep, drugsupp, persprod, persserv, babyfood, boy_exp, girl_exp, infant_exp, fuel_util, school_supp, reading_supp, health_supp. Tobacco purchasing households are identified by whether they purchase in the week of reference. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals, and policy and demographic controls are used for all estimates. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. Vertical lines indicate 95% confidence intervals, using state-clustered standard errors.

Figure 9: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Human Capital Expenditures, Diary Data, Heterogeneity.



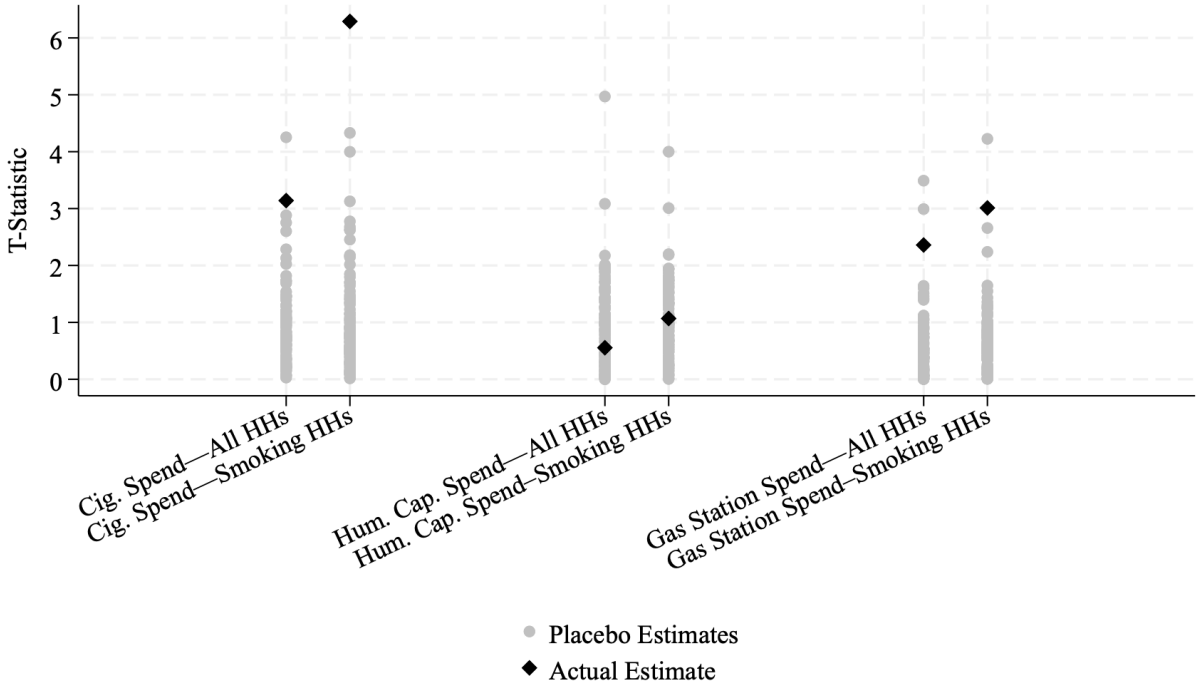
CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Tobacco purchasing households are identified by whether they purchase in the quarter of reference. ">= Median CU Size" indicates that the CU had greater than or equal to the median number of members in their household. "< Median CU Size" indicates that the CU had less than the median number of members in their household. ">= Median Income" indicates that the CU had a household income level greater than or equal to the median of the sample. "< Median Income" indicates that the CU had a household income level less than the median of the sample. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals, and policy and demographic controls are used for all estimates. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. Horizontal lines indicate 95% confidence intervals, using state-clustered standard errors.

Figure 10: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Gas Station Expenditures, Diary Data.



CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. "Gas Station Expenditures" is an aggregate spending category of the CE-D spending variables: food_snacks, food_cola, food_cookcrac, alc_away_beer, auto_fuel. Tobacco purchasing households are identified by whether they purchase in the week of reference. Regressions are estimated using the De Chaisemartin and d'Haultfoeuille (2024) estimator (did_multiplgt_dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: eight periods are chosen for pre and post-period estimation, cigarette taxes are categorized in $1 intervals, and policy and demographic controls are used for all estimates. Data weighted with the BLS CE sampling weight, FINLWT21, which is the number of similar households that an observed household represents in any given quarter. Vertical lines indicate 95% confidence intervals, using state-clustered standard errors.

Figure 11: Placebo Test – Effect of an Additional $1 of Cigarette Taxes on Spending, Randomization



CE-I and CE-D data from 1996 to 2022. Actual estimates denoted by the black diamonds, further discussion of these can be found in Tables 4 and 5. Placebo estimates denoted by circles and were derived from a simulation where states were randomly assigned each observed tax change, holding enactment dates and tax magnitudes constant. The process was repeated 100 times for each sample, changing the pseudo-treatment states in each iteration. Standard errors are clustered at the state level.