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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 hypothetical tax increases by adjusting cigarette shopping behaviors, substituting toward 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. A $1 increase in cigarette taxes causes a modest reduction in human capital-related expenditures among below-median-income smokers ($48 less per quarter). Our work uncovers important unintended consequences of cigarette taxes, particularly for low-income individuals.

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A data appendix is available at
http://www.nber.org/data-appendix/w33746
A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/15056

1 Introduction

Cigarette smoking remains the leading cause of preventable mortality in the United States, and cigarette excise taxes are among the most widely used tobacco control policies (Le et al., 2024). While a large literature examines how taxes affect smoking prevalence, far less is known about how smokers adjust their household spending when cigarette taxes rise. Because smoking rates are disproportionately higher among those with lower education and income—especially in rural areas—cigarette taxes are thought to be highly regressive (Allcott et al., 2019; Garrett et al., 2019; Darden, 2021). Standard consumer theory implies that higher cigarette prices can displace other consumption, depending on the participation elasticity of smoking (i.e., the effect of a 1% tax increase on prevalence), the sign of income effects, and the degree of substitution with other tobacco products.[1] Estimates of participation elasticities between –0.1 and –0.3 suggest that most smokers continue smoking after tax hikes (DeCicca et al., 2022). If smokers absorb these higher costs by cutting back on human capital investments, the long-run welfare consequences of cigarette taxes may be more complex than typically appreciated.

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 large, 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. The sample is balanced on demographic, socioeconomic, and tobacco use characteristics across treatment and control arms.[2]

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 the hypothetical cigarette tax. From this result, the implied extensive margin participation elasticity 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 (19.0%). Third, the tax is associated with broad reallocation, including expected expenditures on both discretionary and non-discretionary categories. For example, treated smokers were 4.1 percentage points (30.8%) more

[1] Tax pass-through to consumers typically ranges from 80% to 120% (Hanson and Sullivan, 2009). See Kenkel et al. (2014) for evidence that cigarettes are a normal good among low-income smokers.

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

likely to indicate that they would spend less on healthcare-related expenditures relative to control smokers. 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, transportation, healthcare, housing, and education. We find similar gaps for baseline smokers with lower incomes and lower levels of education. These results provide important evidence that adjustments to the household budget may be more severe for lower socioeconomic status smokers.

Our second approach pairs detailed, nationally representative expenditure 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 (1.5%). When we focus on below-median income smokers, we find significant reductions ($47.93/quarter or 1.2%) in "human capital expenditures," a category we define as those relating to shelter, clothing, education, and health. Consistent with our survey evidence, these findings suggest an important SES-gradient in responsiveness to cigarette taxes that may have significant long-run welfare implications.

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

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

increase. Our contribution to this literature is to measure how smokers reallocate their budgets across a 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, particularly among lower-SES groups.

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. That is, Y falls when smokers do not cut back on cigarette use enough to offset the higher price. 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 of 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. 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; 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

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

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. However, we allowed respondents to select multiple purchasing locations, and respondents also purchase cigarettes at tobacco shops and grocery stores. Demographically, the mean age of respondents is 42.28 years of age; our sample skews slightly female (56.9%); and 73.3% of our respondents are white.[5] Our sample is relatively well-educated, with over 38% holding a college or graduate degree, and the mean total household annual income is $68,900.

3.1 Experimental Design

To measure the effects of cigarette taxes on household spending, respondents were randomized into two groups. 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.

[5] Our race and ethnicity data come directly from Prolific, so we do not observe Hispanic ethnicity.

Respondents in the treatment group were presented the same set of responses regarding anticipated smoking behavior. The hypothetical tax increase was equal to 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 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. As we show below, our estimate of the extensive margin participation elasticity from this large tax increase is consistent with the large, quasi-experimental literature on cigarette taxes. 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.

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

[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%. As we show below, even under this large tax increase, the implied participation elasticity from our survey is right in the middle of the ranges of estimates from the literature.

[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.

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

Given the natural ordering of expected expenditure changes, we specify an ordered logit estimator of the five response possibilities as a function of the tax treatment and baseline characteristics, X:

$$y_i^* = \gamma_0 + \gamma_1 1[Treatment_i] + X_i\delta + \nu_i. \tag{2}$$

In Equation 2, latent variable y_i^* captures individual $i's$ expectations about future behavior and expenditures. We observe the ordinal response y_i, which corresponds to the individual's response to a given survey question. Thus, we estimate parameters γ and cut points that dictate for the relevant thresholds. Because our parameter of interest, γ_1 represents the log-odds of a given response, we report the average marginal effects of the treatment indicator on the probability that a person responds "spend somewhat less" or "spend much less" to each question.

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 cause 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,

[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.

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 1 provides 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. These 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 adaptive behaviors may attenuate the measured effects on consumption.

To investigate how the hypothetical tax affects other types of expenditures, Figure 2 presents the marginal effects on the likelihood that a respondent indicated either "spend somewhat less" or "spend much less" in the response to questions about expected expenditures on a variety of categories. The figure also reports the control group mean for each category. Those exposed to the hypothetical tax are 4.3pp (22.6%) more likely to spend less on cigarettes, and they are 5.5pp (37.8%), 5pp (17.0%), 7.7pp (50.7%), 4.1pp (30.8%), 5.2pp (57.8%), and 3pp (14.5%) more likely to spend less on groceries, clothing, transportation, healthcare, housing, and education, respectively. These results highlight an important nuance of responsiveness: smokers do not anticipate offsetting tax increases with higher total spending. Results in Figure 2 demonstrate that smokers consider significant and broad reallocation away from other goods when cigarette taxes increase.

To investigate heterogeneity in these effects, Table 2 presents estimates of the same marginal effects for several relevant groups, including baseline heavy smokers and those with low income ($< \$30k$/year), less education (high school degree or less), and children, and those who primarily purchase cigarettes at gas

[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.

stations. Column 1 of Table 2 presents marginal effects for the full sample, where the standard errors are in parentheses and the control means are in brackets. These results are also present in Figure 2. Column 2 presents results just for the 414 heavy smokers in our sample, which we define as smoking 20 or more cigarettes per day at baseline. In contrast to the full sample results, the hypothetical tax does not change the likelihood of spending less on cigarettes relative to the control group. 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 should expect to spend significantly less on other consumption. Relative to the full sample, the heavy smoker marginal effects are larger in magnitude, which suggests that more heavy smokers indicate that they are likely to spend less on the relevant categories when cigarette taxes increase. In the case of expenditures on dining out, there was no significant effect of the tax overall, but for heavy smokers, the tax caused a 5.7pp (13.2%) increase in the likelihood that respondents expect to spend less. Importantly, heavy smokers expect to spend more on alternative tobacco products: there is a 10.9pp (24.6%) reduction in the likelihood of claiming to spend less on these products.

Columns 3 and 4 present results for those with low income and less education, respectively. These results are also larger in magnitude relative to the full sample results. For example, whereas the effect of the tax was to increase the likelihood of spending less on transportation by 7.7pp (50.7%), these effects were 9.7pp (53.9%) and 11pp (80.3%) for lower income and less educated respondents, respectively. Heterogeneity in columns 2-4 provide evidence that those who are more addicted to nicotine or are of lower SES expect to broadly reallocate their budgets more when cigarette taxes increase. The final two columns, for those with children and those who purchase cigarettes at gas stations, are largely similar to the full sample results.

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 alternatives, particularly e-cigarettes. 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. Fourth, 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. Finally, lower SES smokers face larger reallocation when taxes increase.

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, 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. Appendix Section 2 documents our sample construction methods provides further details on the 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

[10] Neither the CE-I nor CE-D include 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.

cigarette spending is framed weekly.

Importantly for this study, cigarette expenditures are directly recorded as a standalone category and 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**: To complement our direct analysis of tobacco spending, this measure focuses on other incidental purchases made during routine gas station or convenience store visits. The goal is to capture potential substitution in non-tobacco items. It 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 from convenience retail industry reports,[13] and explicitly excludes tobacco products.

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 analyze the effect of state-quarter tax variation, summing state cigarette taxes with population-weighted local rates, on household-quarter spending outcomes. From Q1 1996 to Q4 2022, these tax rates span from \$0.17 to \$5.01, with within-state cumulative increases up to \$4.36.

Our primary estimation strategy relies on the Dynamic Difference-in-Differences Estimator developed by De Chaisemartin and d'Haultfoeuille (2024) (DCDH) 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

[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.

adoption and continuous variation in treatment levels. This is particularly well suited for our setting because it accommodates non-binary treatments, such as varying cigarette tax increases, while allowing time-varying policy controls. 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.

To use the DCDH estimator, we group the cumulative tax into \$1-wide bins: $[0, 1)$, $[1, 2)$, $[2, 3)$, $[3, 4)$, and $[4, \infty)$. A "treatment" occurs the first time a state changes bins; observations remaining in the same bin serve as controls. To study the effects of subsequent tax threshold changes, we re-define the sample to state–time observations after the state has reached the tax threshold level immediately prior to the threshold change we wish to evaluate. In doing so, the estimator treats the next threshold change as the treatment, allowing us to estimate its effects separately from the previous.[15]

Two data issues deserve clarification: the presence of cigarette taxes before our sample begins and the use of unbalanced samples for later threshold changes. Cigarette taxes have been in place in all states since 1970 (Orzechowski and Walker, 2022), so the fact that taxes existed at low levels before our sample period is not unique to this study but common across the cigarette tax literature. What distinguishes our approach is how we handle subsequent threshold changes: the analysis relies on an unbalanced sample, with each state's sample beginning at the date of its last threshold change. This design means some states enter the sample later than others. However, the close similarity of estimates for the first threshold change (balanced data) and the second threshold change (unbalanced data) suggests that this imbalance has little effect on our findings.

Of the tax changes that meet our inclusion criteria, 71.4 percent are large enough to trigger a bin change and are therefore classified as treatments. While the DCDH estimator has significant strengths in terms of its ability to handle dynamic treatment effects in the case of a continuous treatment variable like ours, limitations include being able to estimate the effect of only one threshold change per unit at a time, and the requirement of binning introduces inefficiencies as well. For these reasons, we complement this estimation strategy with more traditional two-way fixed effects (TWFE) and household fixed effects (HHFE) strategies. In general, we find the magnitudes of our results to be relatively consistent across approaches, though standard errors tend to be large when using DCDH, possibly on account of the inefficiencies mentioned above.

In all approaches, we control for 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

[15] We developed our current approach after first consulting with the authors to confirm that the command only estimates effects of the first threshold change present in the data (see `https://github.com/chaisemartinPackages/did_multiplegt_dyn/issues/132`).

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

DCDH and TWFE models additionally control for demographic variables. 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 (top panel) 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,[16] the tax-induced reduction is slightly larger in absolute terms—2.3 percentage points—which corresponds to a 2.7% decline. Appendix Table 5 meanwhile shows our model's robustness to which tax threshold change is evaluated, showing cigarette spending responses are consistent across multiple threshold changes.[17]

Figure 3 and Appendix Figure 1 (top panel) complement these findings by showing that the likelihood of reporting cigarette expenditures remains relatively stable in the pre-policy period. For the first threshold change (Figure 3), there is evidence of anticipation in the two quarters before adoption, but this anticipation is not present for the second and third threshold changes (Appendix Figure 1), leading us to conclude that this is a statistical anomaly. For all three threshold changes, post-period effects are particularly pronounced for the baseline smoking households, particularly two or more periods after the threshold change.[18]

[16] Baseline smoking households are defined as those that report any cigarette purchases in the first wave of their CE-I participation.

[17] Regardless of outcome, the average tax change for the first threshold is $0.692; which occurred for 35 states. The average tax change for the second threshold is $0.881; observed for 31 states. The average tax change for the third threshold is $0.833; which on average occurred in 12 states.

[18] A heterogeneity analysis of the extensive margin suggests differential responses to tax increases across demographic groups, potentially due to some groups having tighter budget constraints or differing consumption patterns. As shown in Figure 4,

Next, we evaluate the effects of taxes on cigarette spending (bottom panel of Table 4). As shown 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. When the analysis is restricted to households that continue purchasing cigarettes (with average spending of $380.23 per quarter), the initial $1 tax increase is associated with an increase of $33.00 per quarter representing an increase of 8.6%.[19] Appendix Table 5 (bottom) shows our model's robustness to which tax threshold change is evaluated. Heterogeneity in the level of cigarette spending suggests differential effects in the magnitude of spending increases across groups.[20] Figure 5 and Appendix Figure 1 meanwhile provide visual evidence from event study estimates corresponding to these results. We find limited evidence of pre-trends in cigarette spending; across these three figures, only 2 of 48 pre-period coefficients are statistically different from zero at the 95% level, which is within what we would expect from random chance. Similar to what we observed on the extensive margin, post-period effects are particularly pronounced for baseline cigarette households.

We now shift from examining cigarette spending in the interview data to instead examining tobacco spending, which includes spending on cigarettes, cigars, chewing tobacco, and related goods, in the diary data. Diary data may be more accurate on account of purchases being more regularly reported, but the single-week look-back period makes identification of cigarette purchasing households less certain. Moreover, tobacco spending is a broader category than cigarette spending alone, making it more difficult to identify baseline cigarette purchasers. However, we present similarity in our results, which then motivates our exploration of non-tobacco outcomes in the diary data, which are more granular than the categories of spending available in the interview data.

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, which is not statistically significant. Among households actively purchasing tobacco, the effect is substantially larger and statistically significant—rising to $68.35 per quarter—reflecting concentrated price sensitivity among tobacco users.

We now use the diary data to examine 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

point estimates indicate that 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, though these effects are not statistically different from their counterparts.

[19] Corresponding event studies are presented in Appendix Figure 2. Appendix Figure 3 visualizes our model's robustness to evaluation timing that can be specified using DCDH. This figure shows that the choice of pre- and post-periods matters little to estimated effects for total cigarette spending.

[20] As illustrated in Figure 6, among households that continue purchasing cigarettes, point estimates suggest that 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.

supplies, educational items, and goods for children. These categories reflect essential investments in health and long-term well-being. Table 5 reports the estimated effects of cigarette taxes on this composite category. For all households, the initial $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. However, these estimates fall short of conventional significance thresholds.[21] Figure 7 and Appendix Figure 4 provide corresponding event study evidence, showing limited evidence of pre-trends in human capital spending, which supports the identification strategy.

Heterogeneity analyses, however, reveal important variation. Illustrated in Figure 8, below median income families experience a substantially larger ($47.93) and statistically significant reduction in human capital expenditures, indicating that budget constraints drive larger spending reallocation 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 baseline tobacco-purchasing households, for whom gas station spending declines by $10.95 per quarter. These results are robust to alternative threshold changes (Appendix Table 6) and leave-one-out robustness tests across component categories (Appendix Table 8).[22] These findings underscore the extent to which tobacco taxes affect broader retail spending patterns, particularly in locations where multiple types of consumption co-occur.

Given the central role of auto fuel in these gas station purchases, we also isolate this subcategory and

[21] As discussed later, TWFE and HHFE results are statistically significant. Results using other thresholds in Appendix Table 6 show two possible. As reported in Appendix Table 7, results are generally similar when using leave-one-out tests across component categories of human capital spending, with two possible exceptions: coefficients fall sizably when food consumed at home and housekeeping supplies are removed. This suggests that these components may have disproportionate effect on our derived estimates.

[22] Event studies meanwhile, in Figure 9 and Appendix Figure 4 show limited evidence of parallel trends violations. Some pre-period coefficients across all three threshold changes are statistically significant negative for all households, but none are for baseline tobacco-using households.

find that, as presented in Table 5, 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, from an environmental perspective, is lower gasoline usage.[23]

We conduct placebo testing to validate the robustness of our main findings. In each iteration, we simulate tax changes by randomly reassigning each observed first threshold tax increase to a different state, while holding the original enactment dates and tax magnitudes fixed. This ensures that the placebo events preserve the structure of the first threshold changes in the data. We repeat this procedure 100 times for each sample, allowing the set of pseudo-treatment states to vary across iterations. Figure 10 shows that our estimated effects on cigarette spending are more extreme than would be expected under random assignment. This pattern also holds for our estimates of changes in human-capital-forming expenditures among smoking households.

Additionally, we re-estimate our main specifications using conventional TWFE and HHFE models. These results appear in Tables 6 and 7. While this approach does not account for treatment effect heterogeneity in the same manner as the DCDH estimator, one benefit is that all tax changes can be evaluated simultaneously. In general, these results support the DCDH estimates. TWFE point estimates are similar, and HHFE point estimates are often attenuated. This pattern is consistent with differences in the scope for tax avoidance across the two survey instruments. Because the diary data cover only a two-week period, households can avoid higher taxes following a price increase by purchasing cigarettes in advance and drawing down that inventory during the diary window. By contrast, the interview data span a full year, making it far more difficult to rely on stockpiling to avoid taxes over the entire observation period. Additionally, standard errors are generally sizably smaller, possibly on account of being able to use all available tax variation rather than one threshold change as allowed by DCDH. Many previously reported statistically insignificant findings are now statistically significant. In general, this alignment reinforces the robustness of our findings to alternative estimation strategies and provides reassurance that the documented patterns are not artifacts of the specific estimator employed.

Collectively, our results suggest that households 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 of cigarette spending on both the extensive and intensive margins, the CE-D data provide crucial complementary evidence on 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

[23]See Figure 9 for event study estimates, and Appendix Table 6 for alternative threshold changes, which support the main findings.

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.[24]

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.[25] 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 unintended negative consequences if limits to transportation make 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 reductions in human capital spending of $47.93 (1.2%) among low-income smoking households. This aligns with the basket of responses in terms of stated preferences, where respondents indicated heightened willingness to reduce spending on transportation,

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

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

groceries, 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, below-median-income households show the largest reductions in human capital spending. Taken together, these patterns suggest potentially adverse welfare implications for lower-income smokers.

Our survey data is limited in the sense that it must necessarily be prospective on the part of the respondent, 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 in the period 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 the ratio of our estimates from Tables 4 and 5, between 52.3% to 74.9% of the increase in cigarette spending following a tax hike is offset by reductions in human capital expenditures. 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 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 programs. 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 at 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: Marginal Effects of Tax Treatment on Spending Less

	Full Sample	Heavy Smoker	Low Income	Less Edu.	Has Children	Gas Stations
n=	2,005	414	600	448	984	1,639
Cigarettes	0.043	-0.019	0.035	0.062	0.077	0.049
	(0.018)	(0.043)	(0.032)	(0.039)	(0.024)	(0.020)
	[0.597]	[0.512]	[0.630]	[0.607]	[0.592]	[0.608]
Other Tobacco	-0.009	-0.109	-0.030	0.059	0.014	-0.016
	(0.020)	(0.041)	(0.037)	(0.043)	(0.028)	(0.022)
	[0.486]	[0.443]	[0.507]	[0.466]	[0.498]	[0.474]
Entertainment	0.033	0.085	0.088	0.040	0.003	0.034
	(0.018)	(0.040)	(0.034)	(0.040)	(0.025)	(0.020)
	[0.327]	[0.300]	[0.353]	[0.352]	[0.329]	[0.327]
Groceries	0.055	0.097	0.067	0.061	0.047	0.057
	(0.011)	(0.027)	(0.022)	(0.026)	(0.013)	(0.012)
	[0.145]	[0.148]	[0.177]	[0.174]	[0.110]	[0.147]
Dining Out	-0.017	0.057	0.042	0.064	-0.073	-0.023
	(0.019)	(0.043)	(0.036)	(0.042)	(0.027)	(0.022)
	[0.467]	[0.433]	[0.490]	[0.466]	[0.486]	[0.484]
Clothing	0.050	0.139	0.075	0.092	0.000	0.043
	(0.017)	(0.038)	(0.034)	(0.039)	(0.023)	(0.019)
	[0.294]	[0.261]	[0.353]	[0.297]	[0.299]	[0.299]
Transportation	0.077	0.102	0.097	0.110	0.069	0.082
	(0.012)	(0.027)	(0.025)	(0.028)	(0.017)	(0.014)
	[0.152]	[0.143]	[0.180]	[0.137]	[0.145]	[0.141]
Healthcare	0.041	0.065	0.042	0.100	0.035	0.052
	(0.011)	(0.025)	(0.025)	(0.030)	(0.015)	(0.013)
	[0.133]	[0.118]	[0.190]	[0.142]	[0.122]	[0.133]
Housing	0.052	0.072	0.064	0.073	0.037	0.057
	(0.008)	(0.019)	(0.017)	(0.022)	(0.023)	(0.009)
	[0.090]	[0.059]	[0.090]	[0.091]	[0.098]	[0.072]
Education	0.030	0.060	0.076	0.039	0.038	0.023
	(0.014)	(0.034)	(0.028)	(0.038)	(0.016)	(0.016)
	[0.207]	[0.232]	[0.240]	[0.260]	[0.161]	[0.215]

Table 2 presents marginal effects of the tax treatment on the combined likelihood of "spending somewhat less" and "spending much less" for each spending category. Each cell represents an estimate from a separate regression, corresponding to the respective expenditure category and subsample. For each expenditure category and subsample, marginal effects follow from an ordered logit model for expecting to spend much less through much more. Estimates are conditional on the baseline characteristics in Table 1. 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. Standard errors are in parentheses, and brackets indicate the baseline mean of spending less. For each category, the baseline proportion who claim they will spend less is in parentheses. n=2,005

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

	Full Sample		Conditional On Cigarette Purchase	
	(1)	(2)	(3)	(4)
Any Cigarette Purchase				
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
Cigarette Spending				
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 3 and Figure 5. 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 7 and Figure 9. 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.

Table 6: Robustness — Average Treatment Effects of Additional $1 of Tax, Two-Way Fixed Effect and Household Fixed Effect Estimators

	Full Sample			Conditional On Cigarette Purchase		
	(1)	(2)	(3)	(4)	(5)	(6)
Any Cigarette Purchase						
Cigarette Tax ($1)	-0.080	-0.011	-0.093	-0.034	-0.036	-0.035
	(0.003)	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)
Dep. Var. Mean	0.180	0.180	0.180	0.842	0.842	0.842
Observations	572,026	572,026	572,026	129,868	129,868	129,868
Cigarette Spending						
Cigarette Tax ($1)	10.983	11.191	5.394	38.300	39.527	24.771
	(3.172)	(3.433)	(2.934)	(5.559)	(5.432)	(4.932)
Dep. Var. Mean	67.802	67.802	67.802	380.234	380.234	380.234
Observations	572,026	572,026	572,026	104,367	104,367	104,367
Policy Controls	No	Yes	Yes	No	Yes	Yes
Demographic Controls	No	Yes	Yes	No	Yes	Yes
Household Fixed Effects	No	No	Yes	No	No	Yes
Weighted	Yes	Yes	Yes	Yes	Yes	Yes

CE-I microdata, 1996–2022. Entries are coefficients from regressions of outcomes on a $1 increase in cigarette taxes. In (3)/(6) with household fixed effects identification comes from within-household changes over time. Standard errors are clustered by state. All regressions use BLS CE sampling weight `FINLWT21`. "Conditional On" columns restrict to households with positive cigarette purchases in the reference quarter. The extensive-margin outcome is an indicator for any cigarette purchase in the quarter; the intensive-margin outcome is quarterly cigarette spending.

Table 7: Robustness — Average Treatment Effects of Additional $1 of Tax, Two-Way Fixed Effect and Household Fixed Effect Estimators

	Full Sample			Conditional On Cigarette Purchase		
	(1)	(2)	(3)	(4)	(5)	(6)
Tobacco Spending (CE-D)						
Cigarette Tax ($1)	12.342	13.001	7.848	37.023	38.959	28.455
	(4.832)	(4.932)	(3.991)	(6.229)	(6.384)	(4.112)
Dep. Var. Mean	51.586	51.586	51.586	355.169	355.169	355.169
Human Capital Spending (CE-D)						
Cigarette Tax ($1)	-7.883	-8.076	-6.810	-12.847	-13.434	-11.551
	(2.993)	(2.782)	(2.430)	(3.788)	(3.355)	(3.001)
Dep. Var. Mean	1,933.529	1,933.529	1,933.529	2,265.601	2,265.601	2,265.601
Gas Station Spending (CE-D)						
Cigarette Tax ($1)	-8.283	-9.001	-8.812	-10.940	-11.222	-11.433
	(1.998)	(1.993)	(1.867)	(2.405)	(2.344)	(2.293)
Dep. Var. Mean	559.275	559.275	559.275	746.870	746.870	746.870
Automotive Fuel Spending (CE-D)						
Cigarette Tax ($1)	-2.994	-3.554	-3.209	-5.732	-5.048	-4.990
	(2.588)	(2.327)	(2.404)	(3.103)	(2.877)	(3.020)
Dep. Var. Mean	413.374	413.374	413.374	525.772	525.772	525.772
Observations	293,366	293,366	293,366	43,272	43,272	43,272
Policy Controls	No	Yes	Yes	No	Yes	Yes
Demographic Controls	No	Yes	Yes	No	Yes	Yes
Household Fixed Effects	No	No	Yes	No	No	Yes
Weighted	Yes	Yes	Yes	Yes	Yes	Yes

CE-D microdata, 1996–2022. Entries are coefficients from regressions of outcomes on a $1 increase in cigarette taxes. In (3)/(6) with household fixed effects identification comes from within-household changes over time. Standard errors are clustered by state. Human Capital–Forming Expenditures follows Kraay (2018) and aggregates spending on: food at home, housekeeping supplies and services, drugs and medical supplies, personal care products and services, baby food, boys clothing, girls clothing, and infants clothing, household fuels and utilities, school supplies, reading materials, and health-related supplies. Gas Station Expenditures aggregates spending on: snack foods, carbonated soft drinks, cookies, crackers and baked goods, alcoholic beverages consumed away from home, and automotive fuel.

Figure 1: Treatment Effects on Tobacco Behaviors.

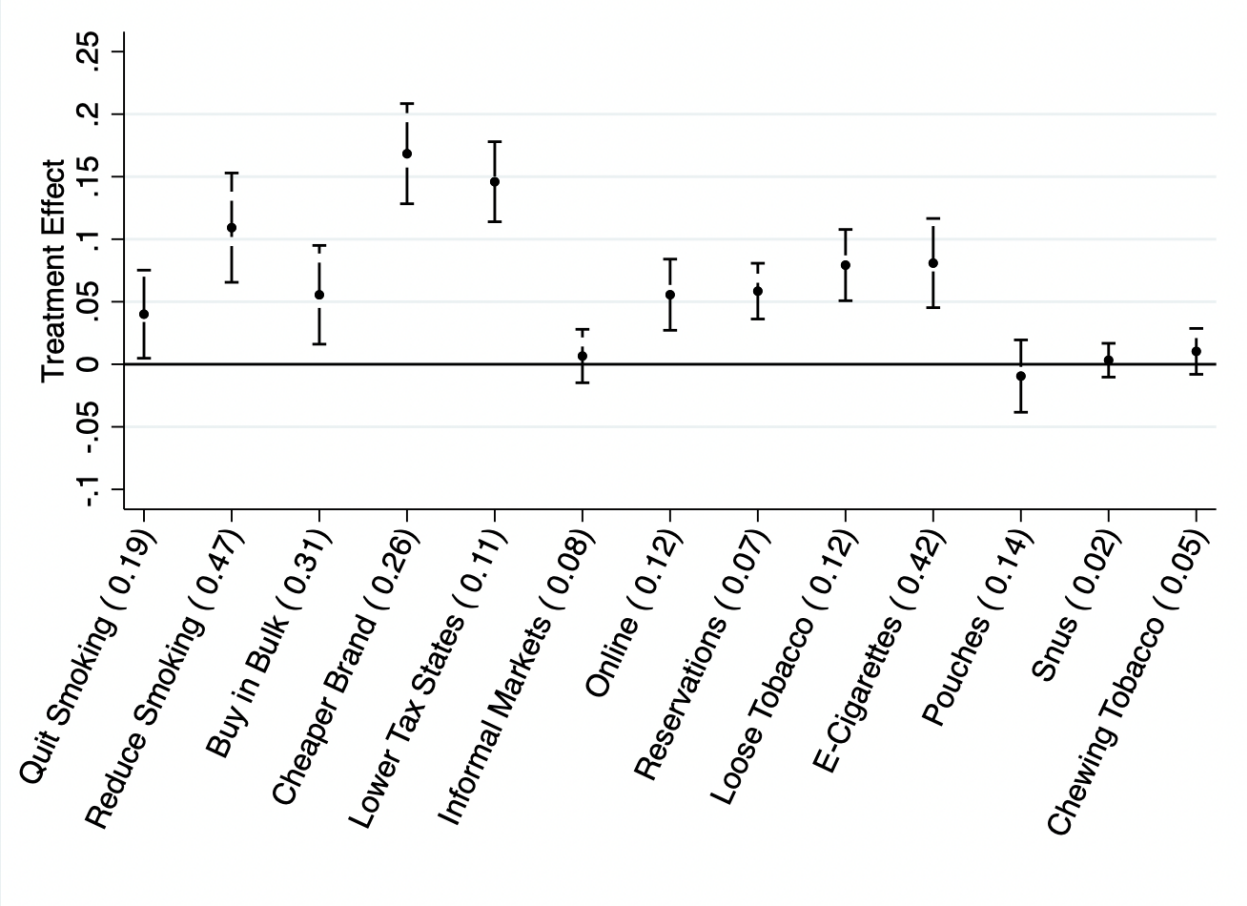


Figure 1 presents treatment effect estimates conditional on the baseline characteristics in Table 1. Each estimate comes from a separate linear probability model of the corresponding behavior. Each behavior is listed with its control group mean. Brackets indicate the 95 % confidence interval. n=2,005

Figure 2: Marginal Effects of Tax Treatment on Spending Less.

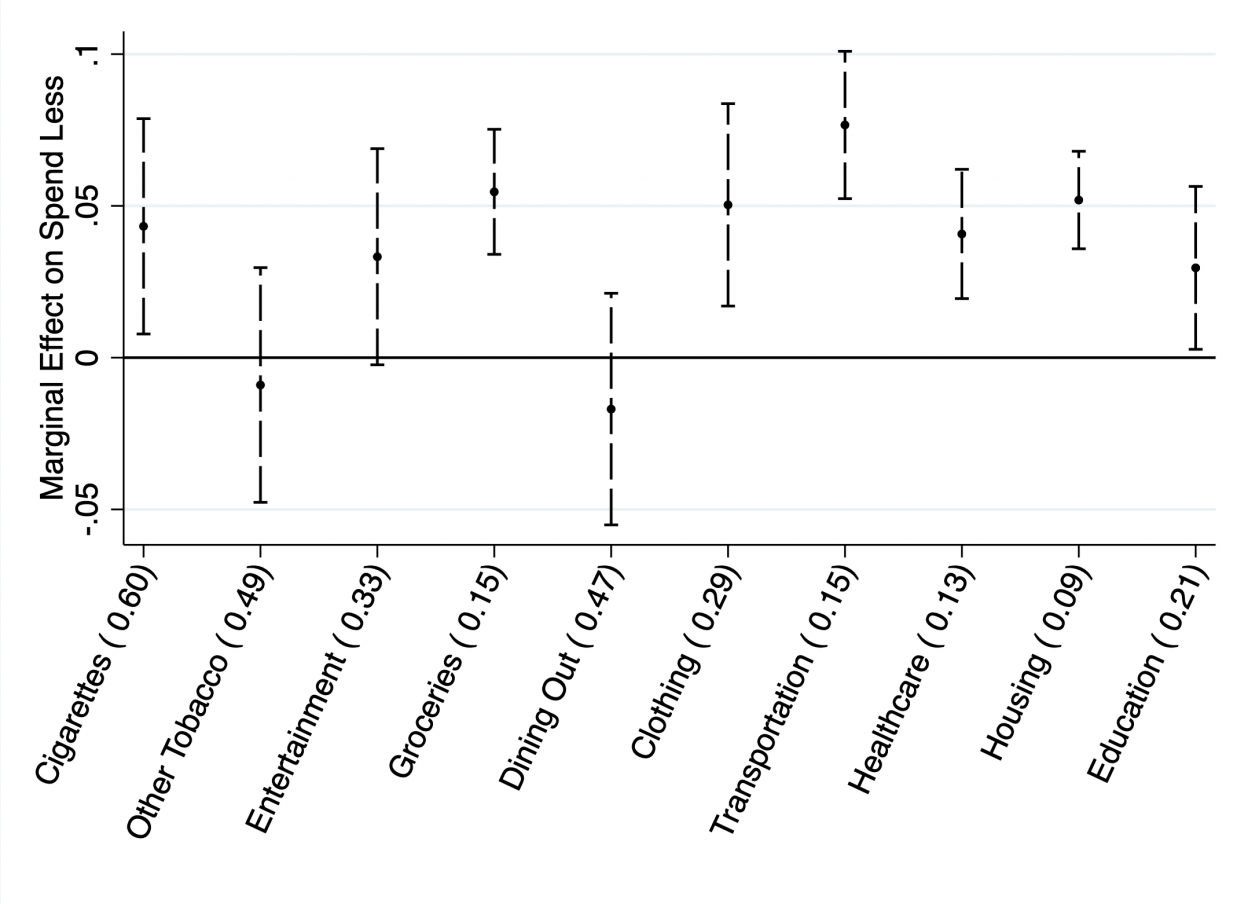
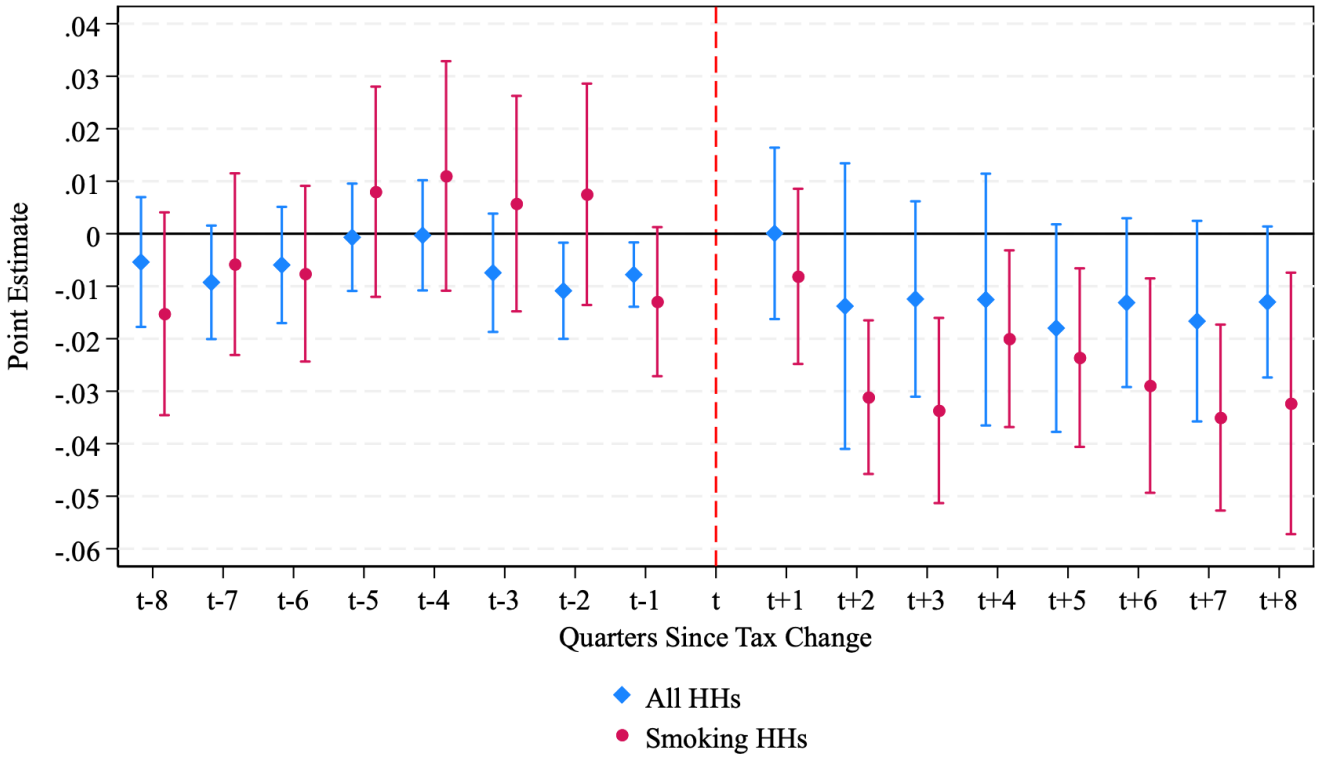


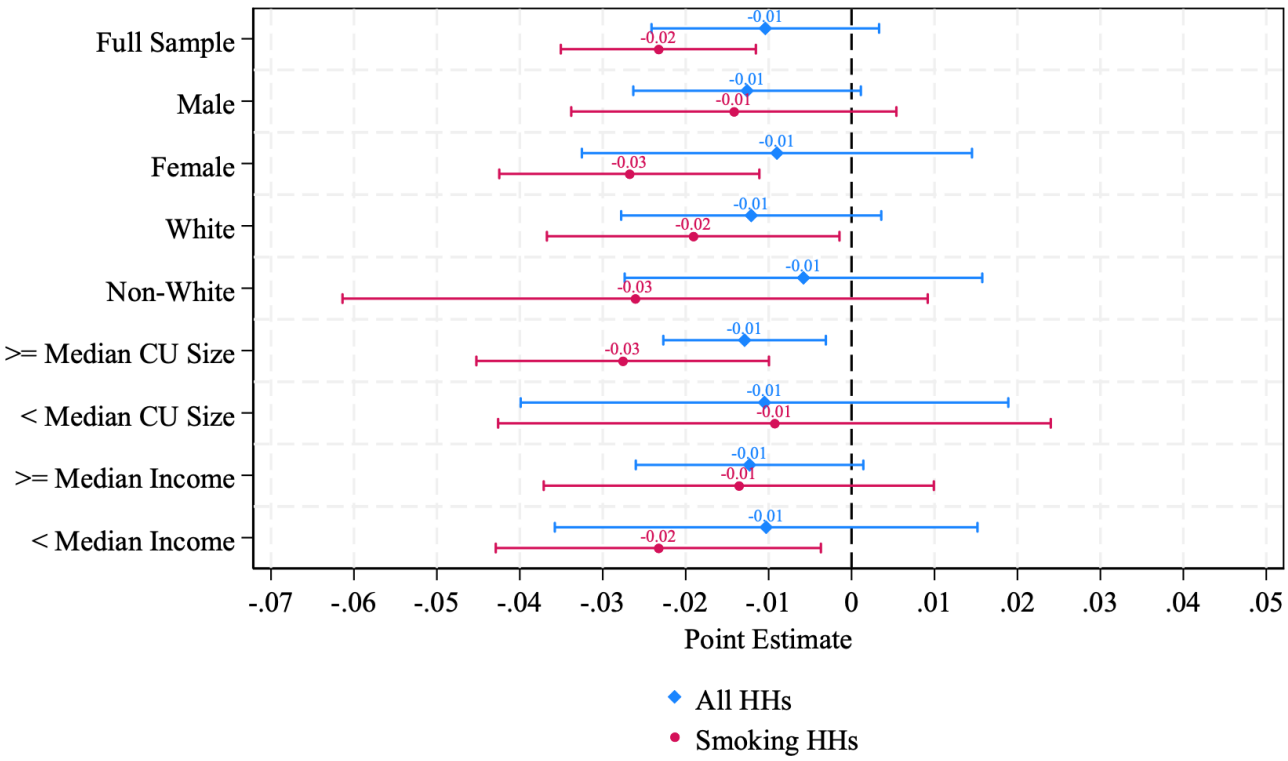
Figure 2 presents marginal effects of the tax treatment on the combined likelihood of "spending somewhat less" and "spending much less" for each spending category. Estimates are conditional on the baseline characteristics in Table 1. For each expenditure category, marginal effects follow from an ordered logit model for expecting to spend much less through much more. Brackets indicate the 95 % confidence interval. For each category, the baseline proportion who claim they will spend less is in parentheses. n=2,005

Figure 3: Event Studies For Average Treatment Effects of an Additional $1 of Cigarette Taxes on Any Quarterly Cigarette Spending



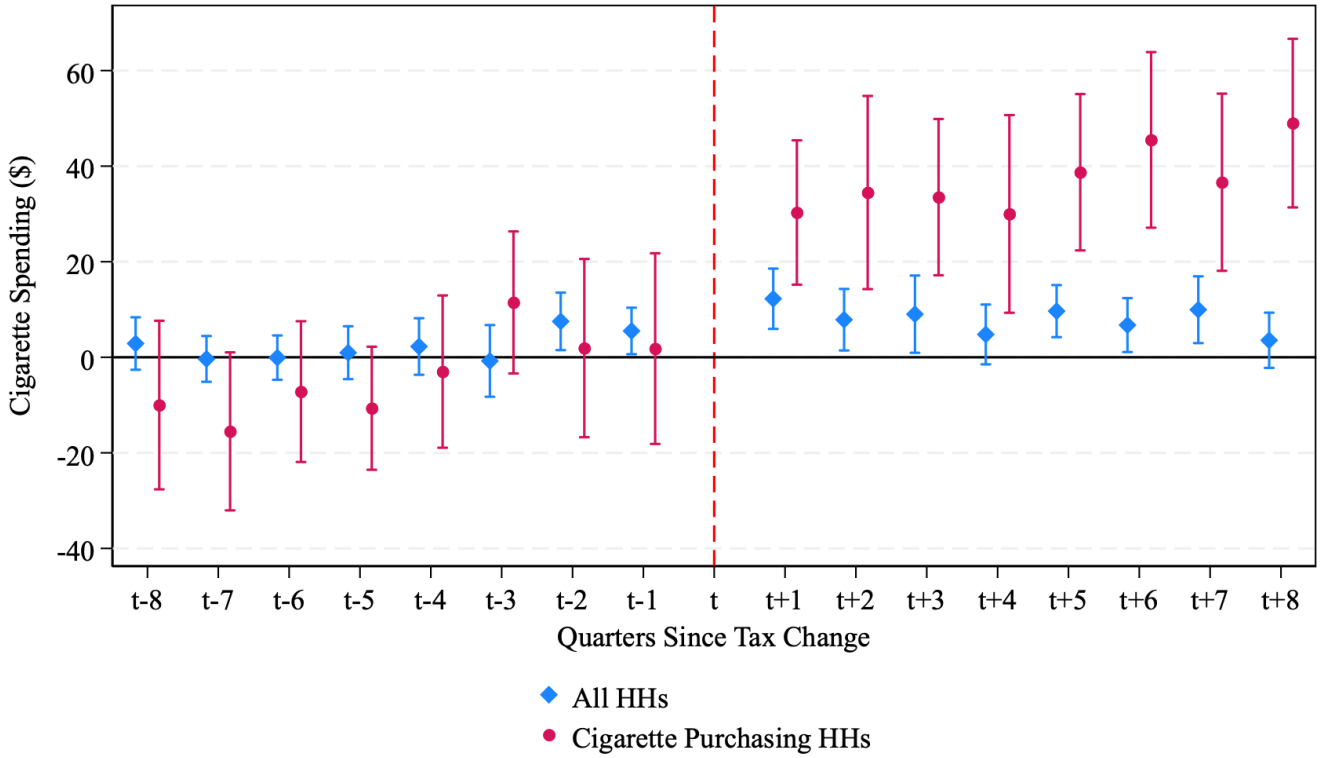
Corresponding ATEs are shown in Table 4. CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Cigarette purchasing households are identified by whether they purchase in the quarter of reference. Regressions are estimated using the De Chaisemartin 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 4: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Any Quarterly Cigarette Spending, Heterogeneity.



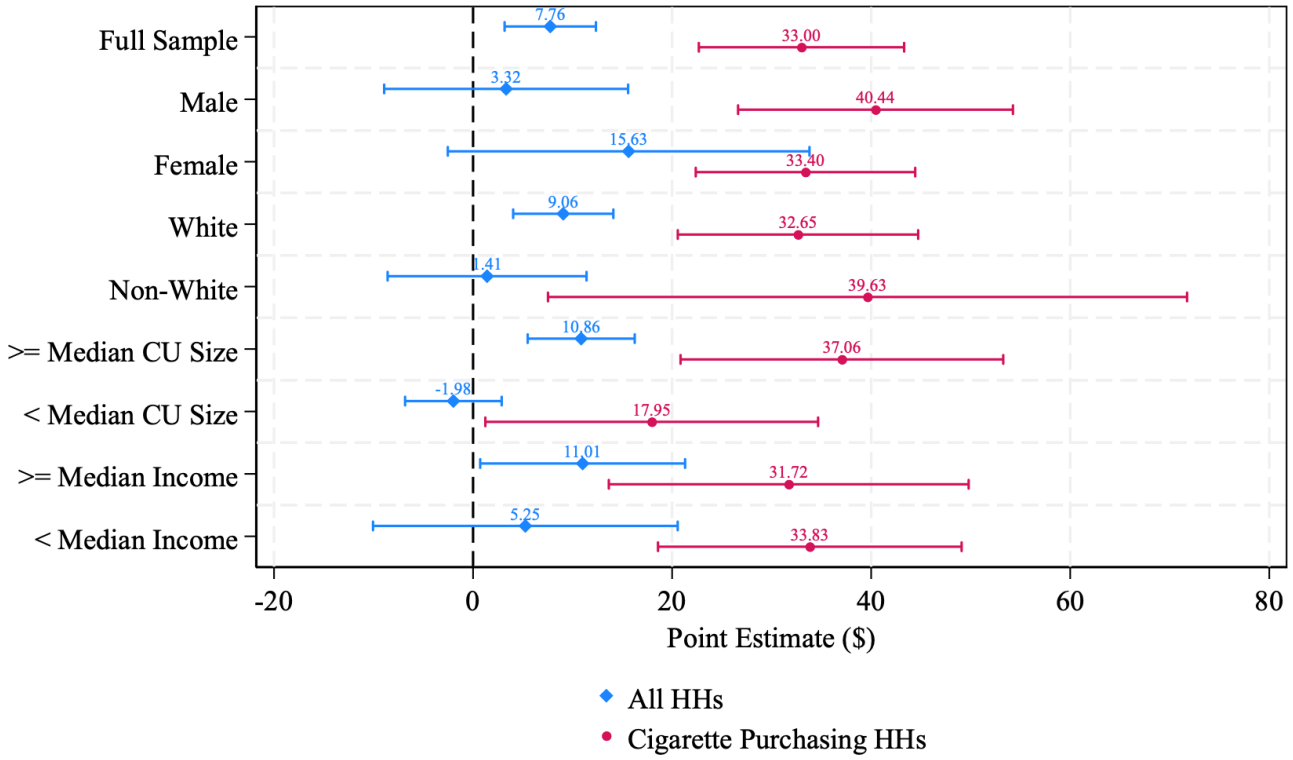
CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Smoking households are identified by whether they purchase any cigarettes in the first wave of their CE interview. ">= Median CU Size" indicates that the CU had greater than or equal to the 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 5: Event Studies For Average Treatment Effects of an Additional $1 of Cigarette Taxes on Quarterly Cigarette Spending



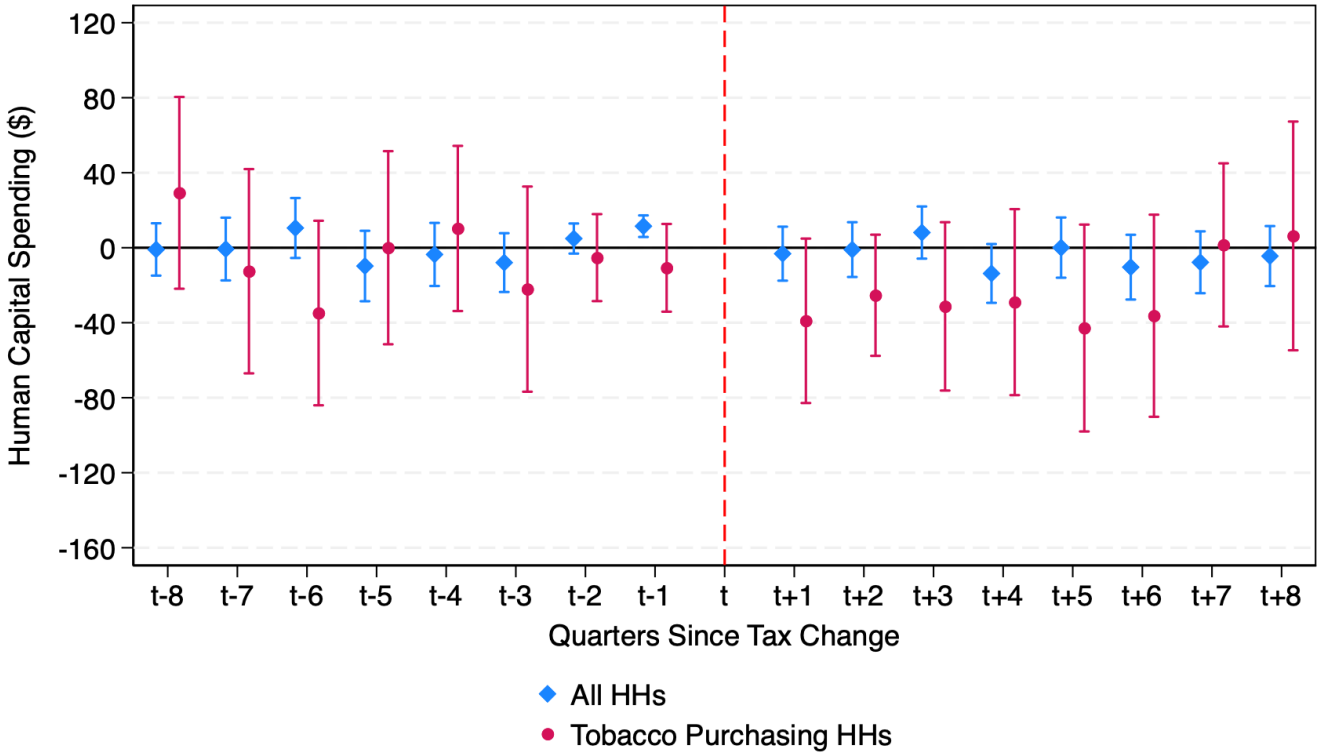
Corresponding ATEs are shown in Table 4. CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Cigarette purchasing households are identified by whether they purchase in the quarter of reference. Regressions are estimated using the De Chaisemartin 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 6: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Quarterly Cigarette Spending, Heterogeneity.



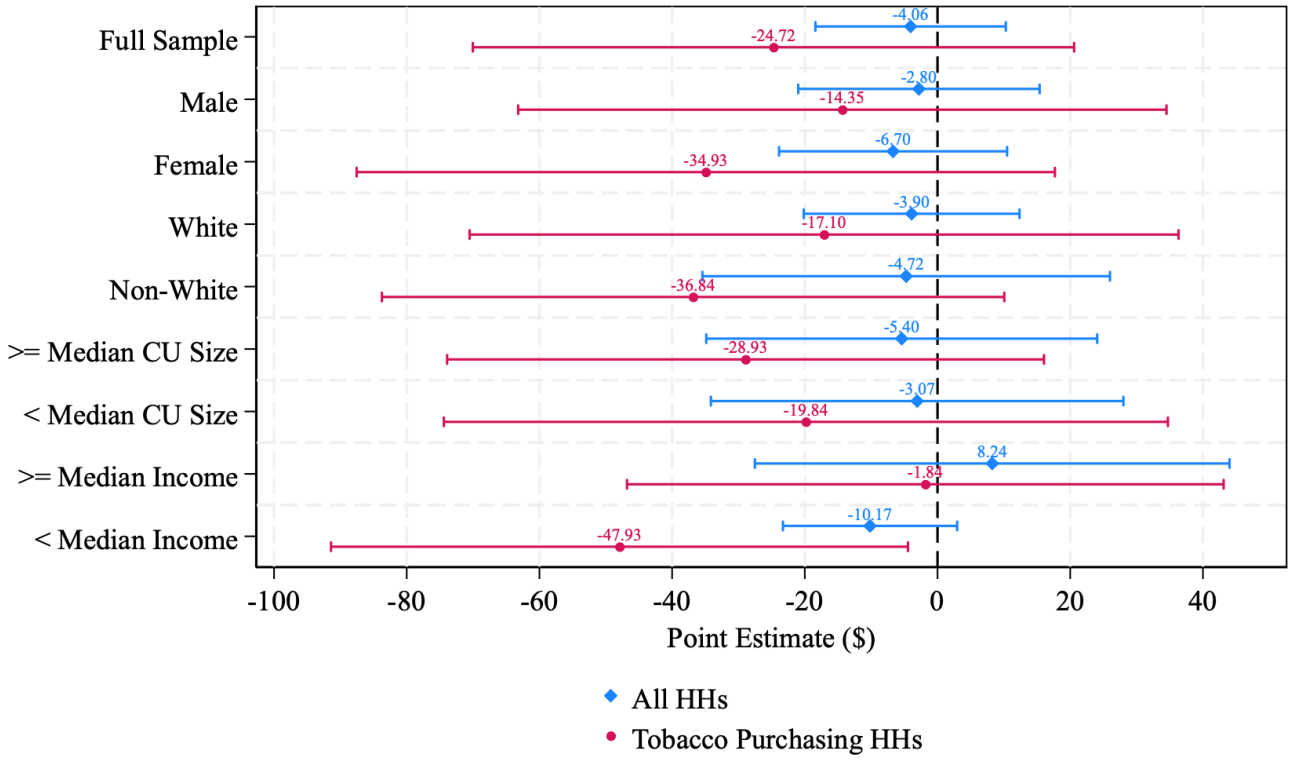
CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Cigarette purchasing households are identified by whether they purchase in the quarter of reference. ">= Median CU Size" indicates that the CU had greater than or equal to the median number 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 7: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Human Capital Expenditures, Diary Data.



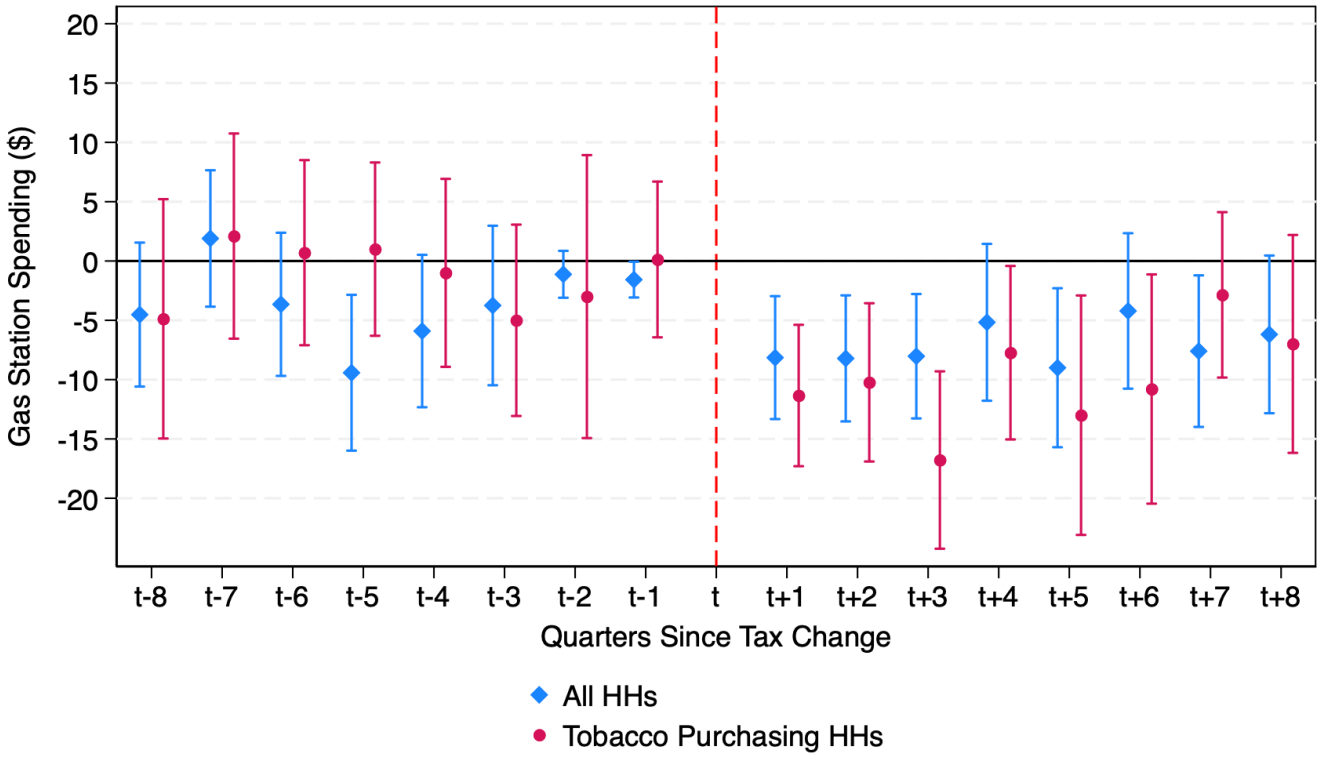
CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. "Human Capital Forming Expenditures" is an aggregate spending category based on the work in Kraay (2018). Following this work, we examine four components of human capital development: shelter, clothing, education, and health. We map these to the CE-D spending variables for human capital–forming expenditures following Kraay (2018). This Category aggregates spending on: food at home, housekeeping supplies and services, drugs and medical supplies, personal care products and services, baby food, boys clothing, girls clothing, and infants clothing, household fuels and utilities, school supplies, reading materials, and health-related supplies. 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 8: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Human Capital Expenditures, Diary Data, Heterogeneity.



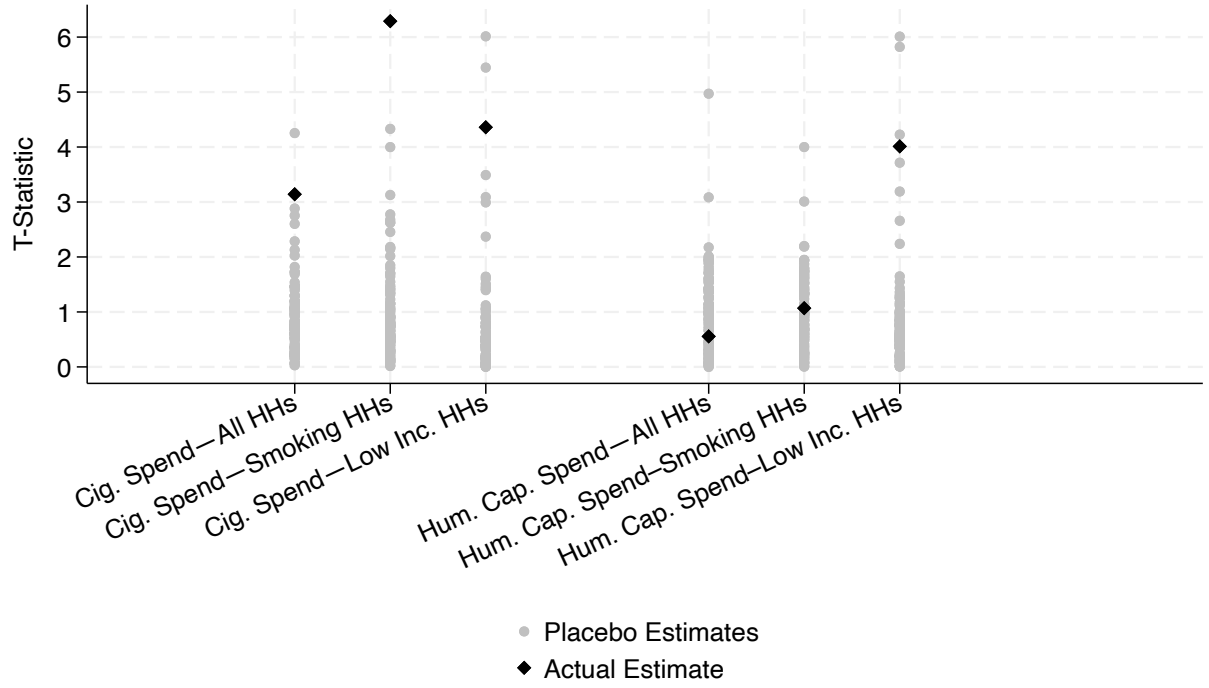
CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Tobacco purchasing households are identified by whether they purchase in the quarter of reference. ">= Median CU Size" indicates that the CU had greater than or equal to the median number 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 9: 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. "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. "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 10: Placebo Test – Effect of an Additional $1 of Cigarette Taxes on Spending, Randomization



CE-I and CE-D data from 1996 to 2022. Main results found in Table 4, Table 5, and Figure 9; t-statistics from these results are denoted with black diamonds. 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.

Cigarette Taxes and the Household Budget: APPENDIX

Michael E. Darden[1,4], Reginald B. Hebert[2], Michael F. Pesko[3], and Samuel Sturm[1]

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1 Survey–Additional Details and Results

We constructed our survey in Qualtrics, and we received IRB approval to field the survey from the Johns Hopkins Homewood IRB.[1]. The survey was posted to the survey research platform Prolific on January 29th, 2025. Prolific allows researchers to screen potential survey respondents on a wide variety of socioeconomic, demographic, health, and behavioral characteristics. As our interest is in cigarette smokers, we restricted our pool of potential respondents to those in either of the following categories:

- I am a current smoker (smoke at least 5 cigarettes a day and have smoked this amount for at least one year)
- I am a recent smoker (smoke at least 5 cigarettes a day and have smoked this amount for less than one year).

We also excluded a small number of Prolific respondents who had previously taken pilot versions of our survey. The Prolific platform identified 5,323 potential respondents who met the above criteria and had been active on the platform within the previous 90 days. We specified, and pre-paid, for a sample of 2,200 participants to receive $12/hour for a survey that we advertised as taking seven minutes. Data collection was completed on January 31st, and the median time taken was five minutes and 15 seconds.

The main survey data was collected directly in Qualtrics. Prolific also provides a core set of demographic information on each respondent, including age and race, as well as statistics on each respondent usage of Prolific. We merged these data to our survey data on a unique respondent identifier generated by Prolific for a final sample of 2,202 respondents. Of these observations, we handled missing values in two ways. First, for those with missing values of baseline characteristics asked in our survey (including education, children in the household, zip code, income, and smoking behavior), we created a binary variable that indicated a missing value and kept the observation. Table 1 presents balance statistics as in the main paper on these missing value binary variables. No variable was missing in more than 1.3% of cases. We include these variables in our main regression models. Second, for those with missing values in any of the experimental questions, we dropped the observation entirely. In total, we eliminated 159 observations for missing responses to key smoking questions. We also dropped 18 additional respondents who claimed in our baseline assessment of smoking behavior to have never smoked cigarettes (in contradiction of the Prolific screens above). We also dropped 20 observations whose demographic information from Prolific was missing. The resulting sample included 2,005 respondents corresponding to the sample in the main paper.

Table 1: Balance Table: Missing Values

	Overall	Tax Treatment		p-value
		No (n=1,019)	Yes (n=986)	
Education	0.001	0.002	0.000	0.164
Income	0.004	0.004	0.005	0.701
Children	0.012	0.017	0.008	0.084
Cigarettes/Day	0.013	0.014	0.012	0.756
Purchasing Behavior	0.000	0.000	0.001	0.309
Noncombustibles	0.001	0.002	0.000	0.164

Table 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. 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.

Appendix Tables 2-4 provide the full regression model results that correspond to Figure 1 of the main paper. Here, we estimate Equation 1 of the main paper for 14 outcomes variables on the treatment indicator and a series of baseline controls.

[1] https://homewoodirb.jhu.edu/.

Table 2: Treatment Effect Regression Estimates

	Quit Smoking	Reduce Smoking	Buy in Bulk	Cheaper Brands	Lower Tax States
Tax Treatment	0.042	0.108	0.053	0.166	0.149
	(0.018)	(0.022)	(0.020)	(0.020)	(0.017)
Age	0.003	-0.000	-0.002	-0.000	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Education					
High School	0.054	0.008	-0.002	-0.105	0.075
	(0.070)	(0.086)	(0.079)	(0.079)	(0.066)
Some College	0.079	-0.071	0.002	-0.107	0.062
	(0.069)	(0.085)	(0.077)	(0.078)	(0.065)
College Grad.	0.034	0.021	0.022	-0.130	0.121
	(0.070)	(0.087)	(0.079)	(0.080)	(0.066)
Graduate Degree	-0.049	-0.019	0.107	-0.067	0.158
	(0.074)	(0.091)	(0.083)	(0.084)	(0.069)
Race/Ethnicity					
Black	-0.002	0.038	0.090	-0.062	0.031
	(0.027)	(0.033)	(0.030)	(0.031)	(0.025)
Asian	0.056	-0.018	-0.068	0.018	-0.018
	(0.060)	(0.074)	(0.068)	(0.068)	(0.056)
Mixed	0.064	0.016	-0.015	-0.091	0.026
	(0.038)	(0.047)	(0.043)	(0.043)	(0.036)
Other	-0.020	-0.045	0.003	0.020	0.024
	(0.051)	(0.063)	(0.057)	(0.058)	(0.048)
Children in Home	0.027	0.010	0.044	0.033	0.012
	(0.019)	(0.024)	(0.022)	(0.022)	(0.018)
Income ($10,000)	-0.003	0.001	0.003	-0.015	0.003
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Tobacco Behaviors					
Daily Smoking	0.004	-0.008	0.049	-0.041	-0.029
	(0.026)	(0.032)	(0.029)	(0.029)	(0.024)
Cigarettes/Day	-0.003	-0.002	-0.001	0.005	0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
Cigarette Spending/Week $	-0.000	0.000	0.002	0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Never uses Non-Combustibles	0.041	-0.048	-0.068	-0.117	-0.043
	(0.021)	(0.027)	(0.024)	(0.024)	(0.020)
Daily Non-Combustible Use	0.052	-0.067	-0.031	-0.051	-0.028
	(0.027)	(0.033)	(0.030)	(0.030)	(0.025)
Purchasing Behavior					
Carton	-0.064	0.021	0.214	-0.001	0.034
	(0.025)	(0.031)	(0.028)	(0.028)	(0.023)
Loose	-0.109	0.007	-0.103	-0.254	-0.038
	(0.039)	(0.048)	(0.044)	(0.044)	(0.037)
Single	0.113	-0.069	-0.097	-0.140	0.065
	(0.050)	(0.062)	(0.057)	(0.057)	(0.047)
Missing					
Education	0.161	-0.384	0.274	0.061	0.015
	(0.301)	(0.372)	(0.339)	(0.343)	(0.283)
Income	0.302	-0.154	-0.208	0.002	-0.174
	(0.139)	(0.171)	(0.156)	(0.158)	(0.130)
Children	0.124	-0.102	0.031	-0.015	-0.046
	(0.081)	(0.101)	(0.092)	(0.093)	(0.076)
Cigarettes/Day	-0.028	0.032	-0.035	0.229	-0.016
	(0.081)	(0.101)	(0.092)	(0.093)	(0.076)
Purchasing Behavior	-0.254	0.403	-0.304	-0.164	-0.246
	(0.402)	(0.498)	(0.454)	(0.458)	(0.378)
Noncombustibles	0.377	-0.561	-0.330	-0.389	-0.162
	(0.286)	(0.354)	(0.323)	(0.326)	(0.269)
Constant	0.026	0.527	0.226	0.532	0.062
	(0.080)	(0.099)	(0.090)	(0.091)	(0.075)

The table presents the full regression results that correspond to the treatment effects in Figure 1 of the main paper. Each column reports a different binary outcome. The reduce smoking column indicates the respondent claims that they will reduce but not quit smoking cigarettes.

Table 3: Treatment Effect Regression Estimates

	Informal Markets	Online Shopping	N. American Reservations	Loose Tobacco	E-Cigarettes
Tax Treatment	0.003	0.051	0.061	0.079	0.080
	(0.012)	(0.015)	(0.013)	(0.015)	(0.018)
Age	-0.003	-0.000	-0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Education					
High School	0.015	0.115	0.039	-0.023	0.046
	(0.045)	(0.059)	(0.051)	(0.057)	(0.071)
Some College	0.005	0.095	0.044	-0.047	0.077
	(0.045)	(0.058)	(0.050)	(0.056)	(0.070)
c College Grad.	0.004	0.168	0.063	-0.064	0.113
	(0.046)	(0.060)	(0.051)	(0.057)	(0.071)
Graduate Degree	0.096	0.214	0.043	-0.039	0.166
	(0.048)	(0.063)	(0.054)	(0.060)	(0.075)
Race/Ethnicity					
Black	0.056	0.081	-0.038	-0.042	0.026
	(0.017)	(0.023)	(0.020)	(0.022)	(0.027)
Asian	-0.014	0.044	0.018	-0.012	0.111
	(0.039)	(0.051)	(0.044)	(0.049)	(0.061)
Mixed	0.038	0.021	0.044	0.001	-0.003
	(0.025)	(0.032)	(0.028)	(0.031)	(0.038)
Other	-0.056	-0.021	-0.018	-0.029	-0.045
	(0.033)	(0.043)	(0.037)	(0.041)	(0.051)
Children in Home	-0.021	-0.017	-0.027	-0.001	0.022
	(0.013)	(0.016)	(0.014)	(0.016)	(0.020)
Income ($10,000)	-0.000	0.001	0.001	-0.003	0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Tobacco Behaviors					
Daily Smoking	-0.058	-0.034	0.012	0.037	-0.069
	(0.017)	(0.022)	(0.019)	(0.021)	(0.026)
Cigarettes/Day	-0.000	-0.001	0.001	0.003	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Cigarette Spending/Week $	0.001	0.001	0.001	0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Never uses Non-Combustibles	-0.036	-0.068	0.010	-0.031	-0.470
	(0.014)	(0.018)	(0.016)	(0.018)	(0.022)
Daily Non-Combustible Use	-0.002	-0.026	0.019	0.006	0.133
	(0.017)	(0.023)	(0.019)	(0.022)	(0.027)
Purchasing Behavior					
Carton	-0.013	-0.001	0.075	-0.034	0.027
	(0.016)	(0.021)	(0.018)	(0.020)	(0.025)
Loose	0.025	0.103	0.039	0.638	-0.039
	(0.025)	(0.033)	(0.028)	(0.032)	(0.039)
Single	0.069	-0.035	-0.035	0.000	-0.136
	(0.033)	(0.043)	(0.037)	(0.041)	(0.051)
Missing					
Education	-0.080	0.004	-0.032	-0.160	0.186
	(0.196)	(0.256)	(0.220)	(0.246)	(0.305)
Income	0.030	0.017	-0.067	-0.032	-0.044
	(0.090)	(0.118)	(0.101)	(0.113)	(0.140)
Children	0.028	-0.022	-0.012	-0.058	-0.112
	(0.053)	(0.069)	(0.060)	(0.067)	(0.082)
Cigarettes/Day	0.024	0.032	0.078	0.080	0.014
	(0.053)	(0.069)	(0.060)	(0.067)	(0.082)
Purchasing Behavior	-0.017	-0.202	-0.152	0.904	-0.795
	(0.262)	(0.342)	(0.295)	(0.329)	(0.407)
Noncombustibles	-0.017	-0.202	-0.152	0.904	-0.795
	(0.262)	(0.342)	(0.295)	(0.329)	(0.407)
Constant	0.209	0.001	-0.005	0.155	0.570
	(0.052)	(0.068)	(0.058)	(0.065)	(0.081)

The table presents the full regression results that correspond to the treatment effects in Figure 1 of the main paper. Each column reports a different binary outcome. The reduce smoking column indicates the respondent claims that they will reduce but not quit smoking cigarettes.

Table 4: Treatment Effect Regression Estimates

	Nicotine Pouches	Snus	Chewing Tobacco
Tax Treatment	-0.010	0.003	0.007
	(0.015)	(0.007)	(0.010)
Age	-0.002	-0.000	-0.000
	(0.001)	(0.000)	(0.000)
Education			
High School	0.020	0.030	0.033
	(0.058)	(0.027)	(0.038)
Some College	0.001	0.016	0.020
	(0.057)	(0.027)	(0.037)
College Grad.	0.071	0.031	0.055
	(0.058)	(0.027)	(0.038)
Graduate Degree	0.094	0.041	0.111
	(0.061)	(0.029)	(0.040)
Race/Ethnicity			
Black	0.005	0.001	0.034
	(0.022)	(0.010)	(0.015)
Asian	0.074	-0.027	-0.006
	(0.050)	(0.023)	(0.032)
Mixed	-0.047	-0.007	-0.028
	(0.031)	(0.015)	(0.020)
Other	-0.029	0.003	-0.006
	(0.042)	(0.020)	(0.027)
Children in Home	-0.000	0.004	0.025
	(0.016)	(0.008)	(0.010)
Income ($10,000)	0.002	0.000	0.002
	(0.002)	(0.001)	(0.001)
Tobacco Behaviors			
Daily Smoking	-0.056	-0.013	-0.030
	(0.021)	(0.010)	(0.014)
Cigarettes/Day	0.004	0.001	-0.001
	(0.001)	(0.001)	(0.001)
Cigarette Spending/Week $	0.000	0.000	0.001
	(0.000)	(0.000)	(0.000)
Never uses Non-Combustibles	-0.162	-0.032	-0.056
	(0.018)	(0.008)	(0.012)
Daily Non-Combustible Use	-0.020	-0.007	-0.044
	(0.022)	(0.010)	(0.014)
Purchasing Behavior			
Carton	0.010	0.019	0.022
	(0.020)	(0.010)	(0.013)
Loose	0.050	0.008	0.038
	(0.032)	(0.015)	(0.021)
Single	-0.021	0.004	-0.017
	(0.041)	(0.020)	(0.027)
Missing			
Education	-0.129	-0.000	0.022
	(0.249)	(0.117)	(0.163)
Income	0.153	-0.008	-0.016
	(0.115)	(0.054)	(0.075)
Children	-0.055	-0.023	0.037
	(0.067)	(0.032)	(0.044)
Cigarettes/Day	0.199	0.018	0.015
	(0.067)	(0.032)	(0.044)
Purchasing Behavior	0.805	-0.026	0.957
	(0.333)	(0.157)	(0.218)
Noncombustibles	-0.350	-0.050	-0.141
	(0.237)	(0.111)	(0.155)
Constant	0.230	-0.010	0.018
	(0.066)	(0.031)	(0.043)

The table presents the full regression results that correspond to the treatment effects in Figure 1 of the main paper. Each column reports a different binary outcome. The reduce smoking column indicates the respondent claims that they will reduce but not quit smoking cigarettes.

2 Bureau of Labor Statistics (BLS) Consumer Expenditure (CE) Survey–Additional Details

Consumer Unit

The BLS refers to a "consumer unit" (CU) as the surveyed unit (i.e. household). The BLS provides the following definition:

> "A consumer unit comprises either:
>
> 1. all members of a particular household who are related by blood, marriage, adoption, or other legal arrangements;
> 2. a person living alone or sharing a household with others or living as a roomer in a private home or lodging house or in permanent living quarters in a hotel or motel, but who is financially independent;
> 3. two or more persons living together who use their income to make joint expenditure decisions.
>
> Financial independence is determined by the three major expense categories: Housing, food, and other living expenses. To be considered financially independent, at least two of the three major expense categories have to be provided entirely, or in part, by the respondent."

Respondent

The individual in the CU who responds to the BLS interviewer or fills out the diary data. The respondent can also be the reference person, but this is not necessarily true. The data does not identify the respondent in the CU, but does include some variables indicating how the respondent answered some questions (e.g. with difficulty, used a bill/statement as a reference). The BLS contacts CUs in advance to make sure a qualified adult respondent is on hand and prepared for the interview.

Reference Person

The individual in the CU named by the survey respondent as "the person or one of the persons who owns or rents the home." Only one person in the CU is listed as the reference person. Relationships coded by the BLS in the CU such as "spouse" or "child" are indicated with respect to this named reference person.

Diary Survey

This is a survey performed over two weeks, focusing on purchases which would be less likely to be remembered in a quarterly survey. The respondent received a scheduled visit from the interviewer, who documents demographic and other CU details. The respondent will complete the two weekly expenditure diaries, which are then returned and processed by BLS.

Interview Survey

This survey is performed over four quarterly interviews (3 months apart), focusing on longer-term and larger purchases than the diary. As with the diary, the respondent receives a scheduled interview. Questions are asked in multiple forms and compared. From the CE documentation:

> Expenditure data are collected in each interview via multiple question patterns depending on the types of expenditures collected. One question pattern asks the respondent for the month of purchase of each reported expenditure. A second question pattern asks for quarterly amounts of expenditures. A third question pattern asks for the payment frequency and the amount based on said frequency.

Data on income and wages are collected in the first and fourth surveys, and are asked in multiple patterns as above. Expenditures collected in each quarter are in reference to the prior three months, so if a CU is interviewed in April, expenditures will cover January-March. The questions are in reference to this 3-month period, rather than individual monthly estimates.

Data Collection Procedures

The addresses selected for contact are drawn by the CE survey from Census Bureau data within each geographical region. These addresses are sent a letter indicating selection and the purpose of the survey. Both diary and interview surveys are conducted primarily by a scheduled in person interviewer visit and some telephone contact. If a CU moves during the interview period, they are dropped from the survey, so a subsequent household is not interviewed just because they are present at the same address as the former one.

Sample Design

The BLS attempts to gather nationally representative samples of the population using clusters or "primary sampling units" (PSU) of Census Bureau-defined core- based statistical areas (CBSA). Addresses are drawn from within a set of the largest PSUs along with a representative sample of smaller PSUs, using residential addresses from Census Bureau's master address file.[2].

Response Rates (as of 2020)

For the interview survey, approximately 13,000 addresses are contacted each quarter, with usable interviews performed at approximately 5,000 of these addresses each quarter. For the diary survey, approximately 18,000 addresses are contacted annually with 6,700 usable two-week surveys collected. After dropping non-responsive addresses (e.g. no response, vacant, destroyed home, nonresidential, refusal) both surveys had a 53% interview rate in 2019.2[3]

Urban and Rural CUs

The BLS offers definitions for urban and rural which accord with the Census Bureau. The sample is over 98% urban by the following definitions:

> Urban Consumer Units are all persons living in a Metropolitan Statistical Area (MSA) (defined by the Office of Management and Budget) and in Urban Places of 2,500 or more persons (defined by the Census Bureau) outside of MSAs.
>
> [Rural Consumer units are] all persons living outside a Metropolitan Statistical Area (MSA) and within an area with a population of less than 2,500 persons.

Spouse-tagged variables:

The BLS defines several categories of relation to the reference person, including unmarried partner, spouse, child or adopted child, grandchild, etc. Each of these is tagged in the MEMI or MEMD file using a CU_CODE. The CU_CODE for unmarried partner is zero and the CU_CODE for spouse is two. The spouse variables in the FMLI and FMLD files are coded explicitly for the condition CU_CODE = 2. Thus, these only apply to individuals reported as spouses by the respondent, rather than unmarried partners.

[2]For more details, see https://www.bls.gov/opub/hom/cex/design.htm
[3]See the response table, ibid.

3 Bureau of Labor Statistics (BLS) Consumer Expenditure Survey–Additional Results

Table 5: Robustness — Average Treatment Effects of Each Additional Tax Threshold Change

	Full Sample		Conditional On Tobacco Purchase		Average Tax Change
	(1)	(2)	(3)	(4)	(5)
Any Cigarette Purchase					
1st Threshold	-0.009	-0.010	-0.021	-0.023	0.692
	(0.007)	(0.007)	(0.006)	(0.006)	
2nd Threshold	-0.009	-0.010	-0.021	-0.024	0.881
	(0.007)	(0.006)	(0.005)	(0.006)	
3rd Threshold	-0.010	-0.011	-0.023	-0.025	0.833
	(0.006)	(0.006)	(0.005)	(0.005)	
Dep. Var. Mean	0.180	0.180	0.842	0.842	
Observations	572,026	572,026	129,868	129,868	
Cigarette Spending					
1st Threshold	7.092	7.757	31.032	32.998	0.692
	(2.572)	(2.334)	(6.324)	(5.262)	
2nd Threshold	6.392	7.648	29.650	31.726	0.881
	(2.673)	(2.533)	(6.481)	(5.627)	
3rd Threshold	5.933	7.334	28.868	28.624	0.833
	(2.808)	(2.754)	(7.011)	(5.866)	
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	

CE-I data from 1996 to 2022. Cigarette purchasing households are identified by whether they purchase in the week of reference. Regressions are estimated using de Chaisemartin et al.'s (2022) estimator (-did_multiplgt_dyn-). State is specified as the unit and year-by-quarter is specified as the period. 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 6: Robustness — Average Treatment Effects of Each Additional Tax Threshold Change

	Full Sample		Conditional On Tobacco Purchase		Average Tax Change
	(1)	(2)	(3)	(4)	(5)
Tobacco Spending					
1st Threshold	13.329	13.361	67.936	68.345	0.692
	(20.900)	(19.979)	(29.932)	(27.694)	
2nd Threshold	12.728	12.816	63.521	62.327	0.881
	(20.950)	(20.187)	(31.486)	(28.611)	
3rd Threshold	12.306	12.646	62.790	61.368	0.833
	(22.895)	(20.790)	(33.845)	(29.890)	
Dep. Var. Mean	66.809	66.809	357.0125	357.0125	
Human Capital Spending					
1st Threshold	-3.932	-4.060	-21.994	-24.720	0.692
	(8.144)	(7.320)	(26.093)	(23.120)	
2nd Threshold	-3.986	-4.072	-23.640	-24.769	0.881
	(7.693)	(7.303)	(24.859)	(20.962)	
3rd Threshold	-4.275	-4.347	-24.243	-25.416	0.833
	(7.423)	(6.943)	(24.745)	(18.902)	
Dep. Var. Mean	1,933.529	1,933.529	2,265.601	2,265.601	
Gas Station Spending					
1st Threshold	-6.199	-7.390	-9.737	-10.953	0.692
	(3.233)	(2.594)	(3.723)	(3.452)	
2nd Threshold	-5.795	-7.127	-9.500	-9.917	0.881
	(3.505)	(2.674)	(3.757)	(3.610)	
3rd Threshold	-5.394	-6.708	-8.631	-9.161	0.833
	(3.618)	(2.755)	(3.910)	(3.827)	
Dep. Var. Mean	559.275	559.275	746.870	746.870	
Automotive Fuel Spending					
1st Threshold	-4.824	-5.369	-7.022	-7.782	0.692
	(3.434)	(2.859)	(3.425)	(2.662)	
2nd Threshold	-4.574	-4.950	-6.670	-7.120	0.881
	(3.591)	(3.144)	(3.598)	(2.735)	
3rd Threshold	-4.286	-4.794	-6.578	-6.938	0.833
	(3.661)	(3.328)	(3.673)	(2.944)	
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	

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 et al. (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. "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 de Chaisemartin et al.'s (2022) estimator (-did_multiplgt_dyn-). State is specified as the unit and year-by-quarter is specified as the period. 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 7: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Quarterly Human Capital Spending, Leave-One-Out

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
All Households													
Cigarette Tax ($1)	2.754	-3.123	-6.551	-4.263	-3.969	-3.838	-3.693	-4.040	-4.145	-5.043	-3.795	-5.128	-3.846
	(8.048)	(7.935)	(4.972)	(6.550)	(8.823)	(7.359)	(7.378)	(6.235)	(6.906)	(2.885)	(7.693)	(6.820)	(7.785)
Observations	293,366	293,366	293,366	293,366	293,366	293,366	293,366	293,366	293,366	293,366	293,366	293,366	293,366
Conditional On Cigarette Purchase													
Cigarette Tax ($1)	-5.961	-18.061	-22.231	-26.701	-25.870	-25.574	-25.403	-25.615	-27.012	-21.012	-24.901	-26.483	-22.751
	(12.848)	(21.617)	(25.312)	(23.425)	(24.112)	(23.334)	(23.703)	(23.511)	(24.624)	(6.761)	(23.500)	(23.468)	(23.168)
Observations	43,272	43,272	43,272	43,272	43,272	43,272	43,272	43,272	43,272	43,272	43,272	43,272	43,272

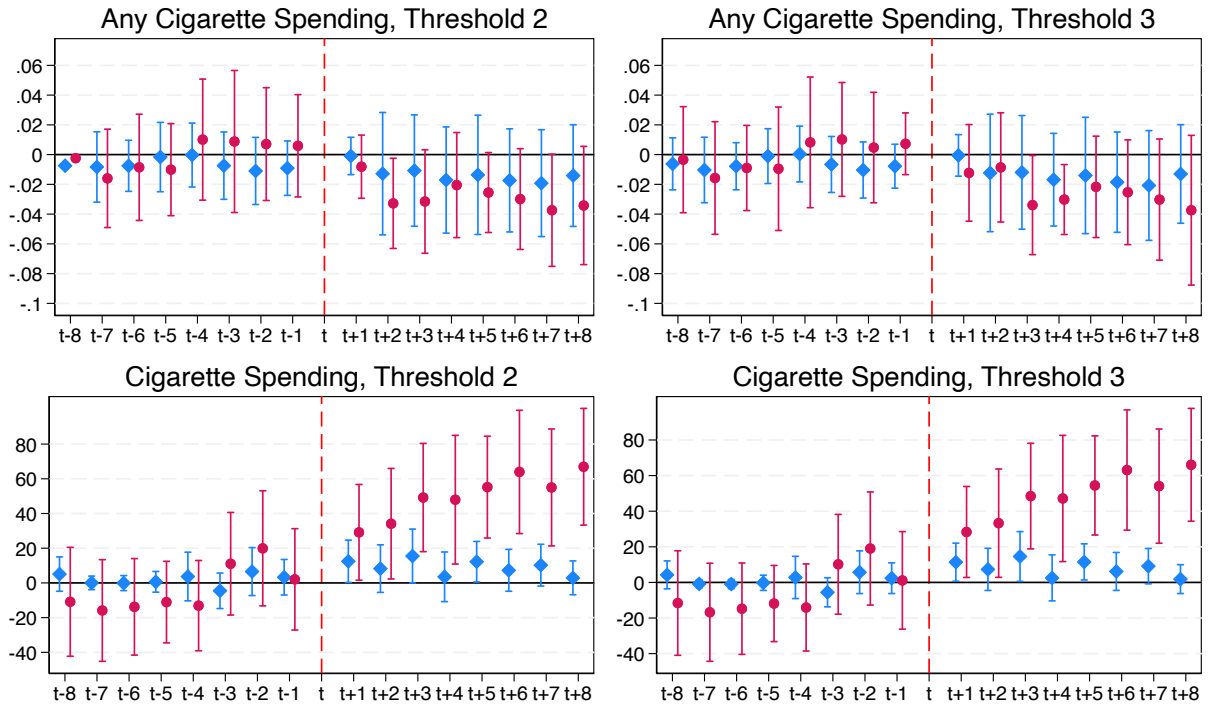
CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Outcomes are the total spending from the aggregate "Human Capital" spending category less (1) Food Consumed at Home, (2) House Keeping, (3) Drugs Supplies, (4) Personal Care Products, (5) Personal Care Services, (6) Baby Food, (7) Boy Child Spending, (8) Girl Child Spending, (9) Infant Spending , (10) Utilities and Heating Fuel, (11) School Supplies, (12) Reading Supplies, (13) Health Supplies. Regressions are estimated using de Chaisemartin et al.'s (2022) 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. State-clustered standard errors in parentheses.

Table 8: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Quarterly Gas Station Spending, Leave-One-Out

	(1)	(2)	(3)	(4)	(5)	(6)
All Households						
Cigarette Tax ($1)	-6.639	-7.334	-6.866	-7.172	-1.156	-7.782
	(2.581)	(2.775)	(2.359)	(2.572)	(0.570)	(3.113)
Observations	293,366	293,366	293,366	293,366	293,366	293,366
Conditional On Cigarette Purchase						
Cigarette Tax ($1)	-9.193	-10.983	-9.337	-11.404	-3.778	-10.069
	(3.105)	(3.184)	(3.309)	(3.574)	(1.328)	(3.436)
Observations	43,272	43,272	43,272	43,272	43,272	43,272

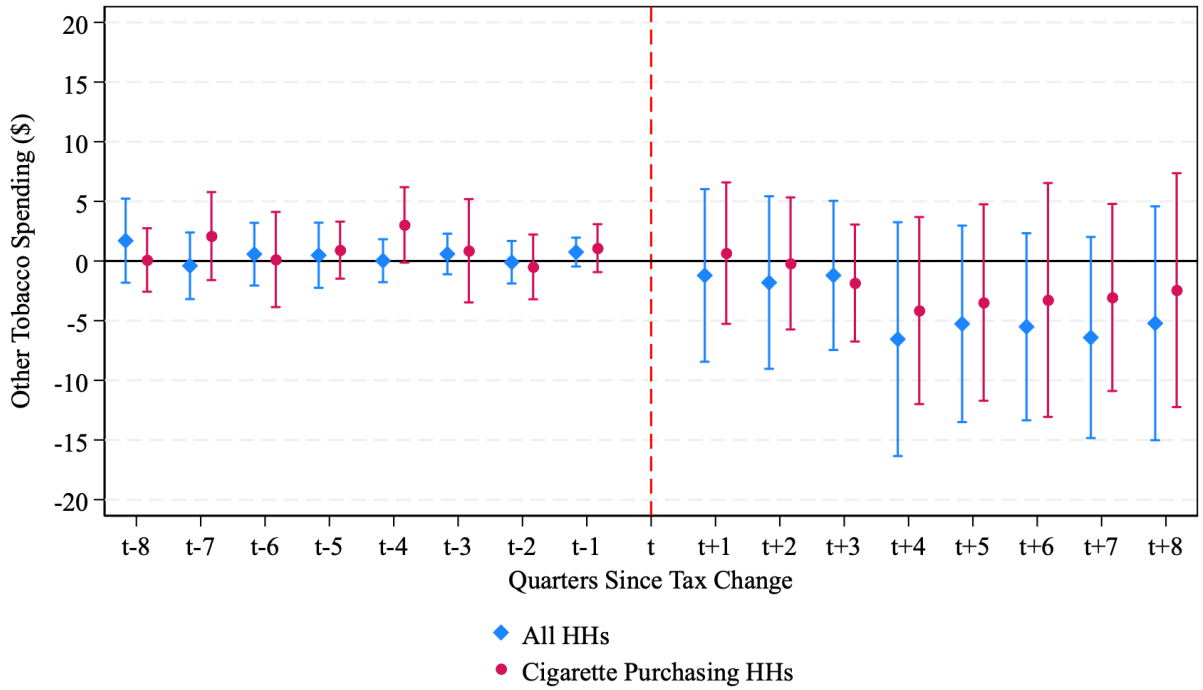
CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Outcomes are the total spending from the aggregate "Gas Station" spending category less (1) Snacks/Chips, (2) Cola/Soda, (3) Cookies/Crackers, (4) Beer, (5) Auto Fuel, (6) Lottery Tickets. Regressions are estimated using de Chaisemartin et al.'s (2022) 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. State-clustered standard errors in parentheses.

Figure 1: Average Treatment Effects of an Additional $1 of Cigarette Taxes by Tax Threshold on Cigarette Spending



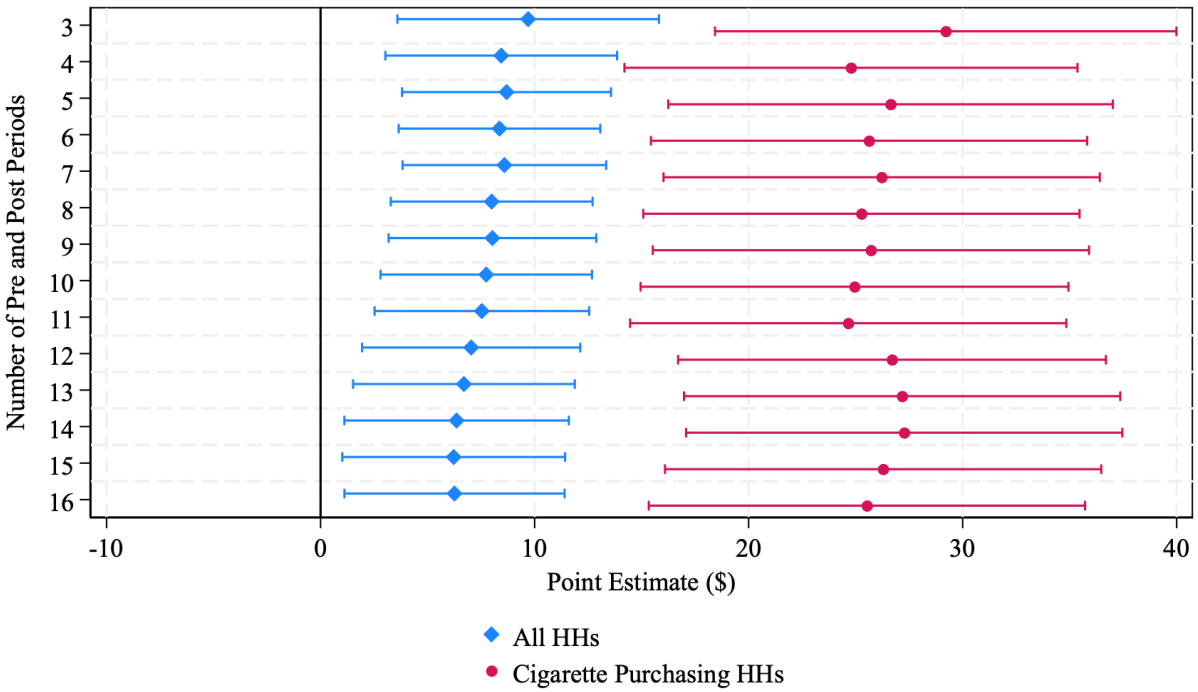
CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Y-axis is measured in U.S. dollars. Blue circles plot estimates for all households; red diamonds plot estimates for smoking households (positive cigarette purchases in the reference period). Regressions are estimated using de Chaisemartin et al.'s (2022) estimator (did multiplgt dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: periods chosen for pre and post-period estimation specified, 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

Figure 2: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Other Tobacco Spending



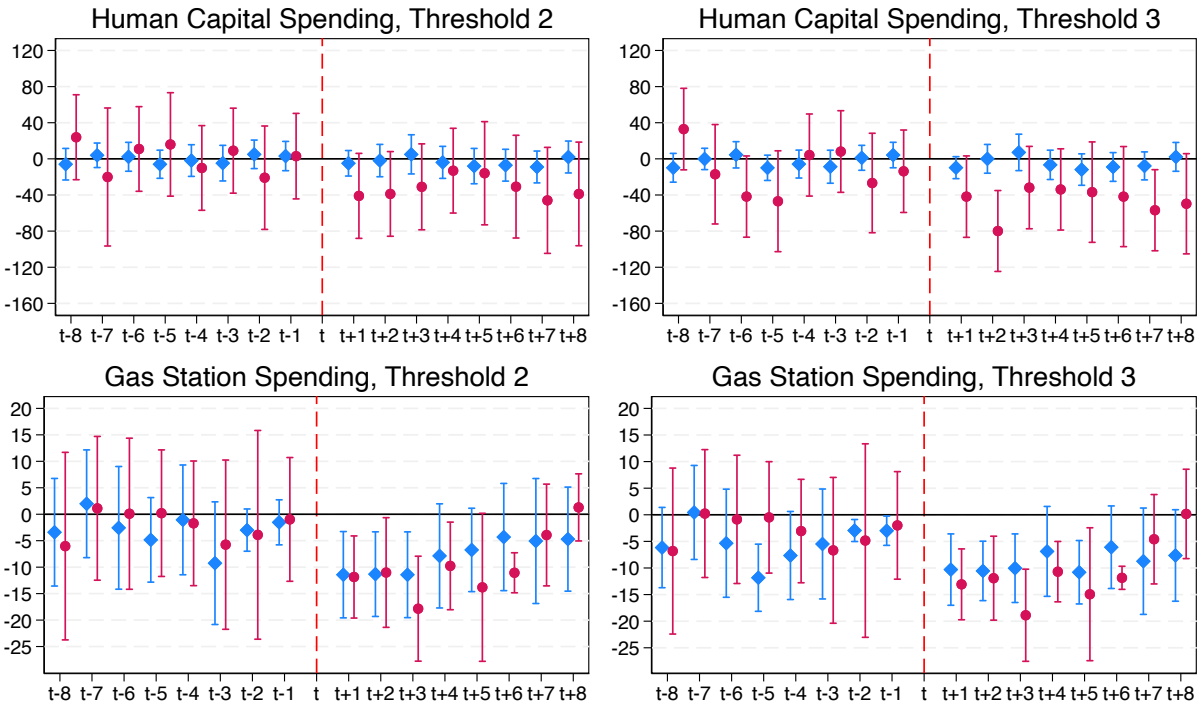
CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Regressions are estimated using de Chaisemartin et al.'s (2022) estimator (did multiplgt dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: periods chosen for pre and post-period estimation specified, 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

Figure 3: Average Treatment Effects of an Additional $1 of Cigarette Taxes on Quarterly Cigarette Spending by Number of Pre and Post-Periods Observed



CE-I data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Regressions are estimated using de Chaisemartin et al.'s (2022) estimator (did multiplgt dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: periods chosen for pre and post-period estimation specified, 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

Figure 4: Average Treatment Effects of an Additional $1 of Cigarette by Tax Threshold on Human Capital and Gas Station Spending



CE-D data from 1996 to 2022. Coefficients show the average treatment effect of a $1 increase in cigarette taxes. Y-axis is measured in U.S. dollars. Blue circles plot estimates for all households; red diamonds plot estimates for smoking households (positive cigarette purchases in the reference period). Regressions are estimated using de Chaisemartin et al.'s (2022) estimator (did multiplgt dyn). State is specified as the unit and year-by-quarter is specified as the period. The following options are specified: periods chosen for pre and post-period estimation specified, 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