Raising the Stakes: Experimental Evidence on the Endogeneity of Taxpayer Mistakes

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Abstract

Recent evidence suggests consumers fail to account for taxes that are excluded from a good's displayed price. Bounded rationality models predict such "salience effects" should decline with tax size but empirical evidence is lacking. We conducted a laboratory shopping experiment with real stakes to study the effect of tax size on salience. Our results rule out all but a modest effect: at most, taxpayers were 86 percent more unresponsive to an 8 percent tax compared to a 22 percent tax. Salience effects may persist at tax rates substantially greater than those currently employed in the United States.

Introduction

A growing body of evidence suggests that the design of a tax affects taxpayer decision-making. With respect to commodity taxes, several studies suggest that consumers fail to fully account for taxes that are not included in the displayed price of the taxed good, and, as a result, choose to purchase more of the taxed good than when the tax is included in the displayed price. These so-called "salience" effects have been documented for commodity taxes both experimentally and in observational studies.[1]

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[1]For experimental evidence, see Chetty, Looney and Kroft (2009); Feldman and Ruffle (2015). Observational studies have studied commodity tax salience in the context of purchases of beer (Chetty, Looney and Kroft, 2009),

In this paper, we move beyond the question of whether individuals behave differently when faced with high versus low salience taxes and instead investigate a potential determinant for whether taxpayers account for low salience taxes when making purchasing decisions: the magnitude of the tax being levied.[2] The larger the magnitude of the tax rate imposed on a particular good, the greater the utility costs will be of failing to account for that tax when making purchasing decisions involving the taxed good. Thus, even if individuals fail to account for low salience taxes in low-stakes situations, it could be the case that such salience effects fade away as the utility cost of ignoring the tax increases.

Understanding the relationship between salience and the size of the tax is important for two reasons, one positive and one normative. First, it can help differentiate two plausible positive models of decision-making. On the one hand, biases and heuristics may be hard-wired into human behavior and thus insensitive to features of the decision-making environment (including the size of the tax). In contrast, decision-makers may be boundedly rational, in that they trade-off the utility costs and benefits of alternate decision-making strategies. When the utility benefits of accounting for low-salience taxes outweigh the utility costs of doing so (e.g., cognitive costs, opportunity costs of diverted attention), a bounded-rationality model of tax salience would suggest that consumers become more attentive when facing higher taxes.[3] A finding that salience effects are strongest at small taxes – where the utility cost of ignoring the tax is the lowest – would constitute evidence in favor of the bounded rationality model of behavior and against the hard-wired mistake model. Despite the popularity of bounded rationality models of mistake-making (e.g. rational inattention), to our knowledge we are among the first to directly investigate thi core assumption.

The second reason to care about the relationship between tax size and salience is a normative one.

hybrid cars (Gallagher and Muehlegger, 2011), soft drinks (Colantuoni and Rojas, 2015), cigarettes (Goldin and Homonoff, 2013), and airline tickets (Bradley and Feldman, 2015). Outside of the commodity tax context, salience effects have been documented for highway tolls (Finkelstein, 2009), property tax payments (Cabral and Hoxby, 2012; Hayashi, Nakamura and Gamage, 2013; Bradley, 2015), and shipping and handling fees (Brown, Hossain and Morgan, 2010).

[2] A "high salience" tax is a tax for which the after-tax price is prominent, such as a VAT or excise tax, where the good's displayed price includes the tax. In contrast, a "low salience" tax is one for which the full after-tax price is not prominent, such as a sales tax, where the tax is added to the displayed price at the conclusion of the purchase.

[3] Models of bounded rationality are commonly employed in behavioral economics but the underlying assumption – our focus here – is rarely tested directly. Suggestive evidence is provided by a number of papers that document behavioral biases in settings with high stakes, such as retirement savings defaults (Madrian and Shea, 2001), car purchases (Lacetera, Pope and Sydnor, 2012), and investment decisions (De Bondt and Thaler, 1990). Such findings are not incompatible with a bounded rationality model of behavior, but to reconcile them one must assume that the utility cost of optimizing is quite high.

Several recent papers find that low-salience tax instruments can offer efficiency and distributional benefits in certain circumstances (Goldin, 2014; Goldin and Homonoff, 2013). However, the potential benefits of such taxes are limited by the extent to which consumers become more attentive to them as governments levy them at higher rates. In fact, if salience increases with the size of the tax rate, Reck (2015) shows that there must exist some rate for the low salience tax above which further increases generate more deadweight loss than a high salience tax . Thus, understanding the range of realistic commodity tax rates at which decision-makers become more attentive to low-salience taxes is crucial for assessing the benefits of employing such taxes to further policy goals.[4]

A small but growing literature that is closely related to ours examines whether behavioral biases fade as the stakes involved in the decision grow larger. A classic study in this area is Camerer and Hogarth (1999), which provides evidence that increasing the financial stakes in experiments does not induce subjects to avoid cognitive biases or other errors. More recently, Haggag and Paci (2014) study how changing the default tip suggestions in taxicabs affects tipping behavior;they find that larger defaults result in higher tips among those who choose to tip but also a higher number of customers opting out. Similarly, Brown et al. (2013) documents similar behavior with respect to decisions about setting the temperature; when nudged towards slightly cooler temperatures, people do not adjust the thermostat but when nudged towards much cooler temperatures then people change the setting.[5] To our knowledge, we are the first to investigate such phenomena in the domain of taxation.

To investigate the relationship between salience and the size of a tax, we designed a lab experiment with real stakes to simulate an online shopping environment. Subjects were provided with $18 and an opportunity to purchase various products. Depending on the treatment group to which a subject was assigned, the subject made purchasing decisions facing tax-inclusive (TI) or tax-exclusive (TE) prices, at high (22%) or low (8%) tax rates. To measure salience effects at a given tax rate, we compared the gap between subjects' expenditures at TE versus TI prices. By

[4]In extreme cases, it seems almost certain that individuals would account for a low salience tax when making purchasing decisions. For example, if a $1,000 unit tax were imposed on one brand of toothpaste, it seems likely that virtually all consumers would take the tax into account when deciding which brand of toothpaste to buy. The interesting question is whether taxpayers become attentive to the tax at high, yet policy-relevant rates.

[5]A closely related literature looks to see if people's preferences become less altruistic as the amount at stake is increased. For example, Camerer (2003) finds that people's behavior in ultimatum games is largely unaffected by changing the stakes. In that article, Camerer nicely sums up the intuitions of many economists on this question: "If I had a dollar for every time an economist claimed that raising the stakes would drive ultimatum behavior toward self-interest, I'd have a private jet on standby all day."

observing this quantity at the low tax rate and the high tax rate, we are able to measure changes in the salience of the tax across treatments. In order to isolate variation in behavior caused by changes in the cost of accounting for the tax, we adjusted the pre-tax price of the available products so that after-tax prices were equalized between the high- and low-tax treatment groups. We interpret the results of the experiment using a simple model to identify whether observed differences in consumption between treatment groups are due to changes in tax size or salience.

Our results provide no support to the hypothesis that tax salience increases in tax size within the range of tax rates that we considered. At the high (22%) tax rate, the expenditure gap between TE and TI prices was positive (approximately \$1) and statistically significant, suggesting that salience effects do not entirely fade out at that high rate. In fact, our point estimates suggest that the salience of the high tax is somewhat lower than the salience of the low tax – the opposite of what a bounded rationality model would predict – although the difference in salience is not statistically significant. Although we cannot rule out any increase in salience in moving from the low to the high tax rate, a 95% confidence interval provides an informative upper bound: salience effects were no more than 1.86 times as large at the low tax than at the high tax; that is, although the high tax was 2.75 times larger than the low tax, at most consumers were 1.86 times as unresponsive at the low tax compared to the high tax. Given the large difference in tax rates tested in the experiment, our estimated upper bound is quite modest: if a commodity tax in the neighborhood of 8 percent was associated with a level of salience of 0.35 (as reported in Chetty, Looney and Kroft (2009)), our results suggest that increasing the tax rate by 175 percent – up to 22 percent – would result in a new level of salience of less than 0.65 This result suggests that taxpayers would continue to exhibit salience effects even for commodity tax rates substantially greater than those currently imposed in the United States.

The remainder of the paper proceeds as follows. Section I develops the theoretical framework. Section II describes the experimental design. Section III presents descriptive statistics concerning the experiment subjects. Section IV presents the empirical results. Section V concludes.

I. Theoretical Framework

This section develops a simple model to motivate the experimental design and to facilitate the interpretation of the experimental results. A representative consumer chooses between two goods, x and y. Good x has displayed price p and is subject to an additional specific tax t. Good y is untaxed and we normalize its price to 1. The budget constraint is given by

$$(p+t)\,x + y = Z \tag{1}$$

where Z represents the consumer's (fixed) endowment. The consumer's demand for x and y is a function of the displayed price and the tax: $x = x(p,t)$ and $y = y(p,t)$.

As in Chetty, Looney and Kroft (2009), the salience of a tax θ_t is defined in terms of how the tax affects consumer demand for the taxed good:

$$\theta_t = \frac{\frac{\partial x}{\partial t}}{\frac{\partial x}{\partial p}} \tag{2}$$

A fully-salient tax ($\theta_t = 1$) is one to which consumers respond as if the change had been to the pre-tax price; when $\theta_t = 0$, a change in the tax does not cause any change in demand for the taxed good. We index θ by t to make explicit the possibility that the size of a tax affects the salience associated with it. In particular, if consumers are more inattentive to lower taxes, we would have $t > t' \implies \theta_t > \theta_{t'}$.[6]

To investigate differences in salience across tax sizes, we experimentally generated consumption data across variation in tax sizes (high versus low) and tax designs (tax-inclusive versus tax-exclusive prices). To isolate differences in consumption arising from differences in the utility cost of accounting for the tax, we held the after-tax price constant across treatments. Let s denote the low tax and $s+\delta$ denote the high tax ($\delta > 0$). For the after-tax price to be held fixed, the displayed price must vary as well; let q denote the displayed price in the high tax treatment and $q+\delta$ the displayed price in the low tax treatment. Thus under both treatments, the after-tax price is given by $q+s+\delta$. At tax-exclusive (TE) prices, consumption of x is given by $x(q, s+\delta)$ under the high tax treatment and by $x(q+\delta, s)$ under the low tax treatment. In contrast, when prices are tax-inclusive (TI), the

[6] To understand why θ might vary with t, note that the utility costs from ignoring a low salience tax are greater when the tax is large than when it is small. See Chetty, Looney and Kroft (2007) for a formal model.

displayed price includes the tax and consumption is given by $x(q+\delta+s,0)$.

Let $\Delta_x(t)$ denote the *expenditure gap,* the difference between consumption of x at TE versus TI prices:

$$\Delta_x(t) \equiv x(p,t) - x(p+t,0) \tag{3}$$

Adding and subtracting $x(p,0)$ and taking Taylor approximations allows us to rewrite (3) as $\Delta_x(t) = x(p,t) - x(p,0) + x(p,0) - x(p+t,0) \approx t\,\frac{\partial x}{\partial t} - t\,\frac{\partial x}{\partial p}$.[7] Substituting (2) into this equation allows us to express the expenditure gap as a function of salience:

$$\Delta_x(t) \approx -(1-\theta_t)\,t\,\frac{\partial x}{\partial p} \tag{4}$$

At the low tax, the expenditure gap is given by $\Delta_x(s) \approx -(1-\theta_s)\,s\,\frac{\partial x}{\partial p}$. Similarly, at the high tax we have $\Delta_x(s+\delta) \approx -(1-\theta_{s+\delta})\,(s+\delta)\,\frac{\partial x}{\partial p}$. Consequently, by scaling $\Delta_x(s)$ and $\Delta_x(s+\delta)$ (which we can observe) by the difference in taxes, we can back out changes in salience between the two taxes:

$$UR \equiv \frac{1-\theta_s}{1-\theta_{s+\delta}} \approx \frac{\Delta_x(s)}{\Delta_x(s+\delta)}\,\frac{s+\delta}{s} \tag{5}$$

This quantity, $\frac{1-\theta_s}{1-\theta_{s+\delta}}$, can be interpreted as the Unresponsiveness Ratio (UR) between the two taxes. When both taxes have the same salience, i.e., when consumers are equally unresponsive to both taxes, we have $UR = 1$. In contrast, when the salience of a tax is increasing in the tax's size, this implies $UR > 1$, i.e., taxpayers are more unresponsive to the smaller tax. The larger the value of UR, the more that salience increases when moving from the low to the high tax.[8]

Finally, to make the experiment more realistic, we presented taxes as ad valorem rates rather than specific taxes. Let τ_l indicate the low tax rate and τ_h indicate the high tax rate. Let p_l denote the pre-tax price associated with the low tax and p_h denote the pre-tax price associated with the high tax. Because the after-tax prices are equal, we know

$$(1+\tau_l)p_l = (1+\tau_h)p_h \tag{6}$$

[7] Note that $\frac{\partial x}{\partial p}$ and $\frac{\partial x}{\partial t}$ are evaluated at $(p,0)$, so that comparing Δ_x at different taxes (with different displayed prices) requires assuming local linearity in demand for x.

[8] Note that $UR \to \infty$ if a low salience tax is increased up to the point at which all taxpayers become perfectly responsive.

Note that δ translated into this notation is equal to $p_l - p_h$ and s is given by $\tau_l p_l$. From (6), we have $p_h = \frac{1+\tau_l}{1+\tau_h} p_l$, so that $\delta = p_l - \frac{1+\tau_l}{1+\tau_h} p_l = p_l \frac{\tau_h - \tau_l}{1+\tau_h}$. Substituting these identities into (5) yields

$$UR = \frac{\Delta_x(\tau_l)}{\Delta_x(\tau_h)} \frac{\tau_h(1+\tau_l)}{\tau_l(1+\tau_h)} \tag{7}$$

II. Experimental Design

A. Set Up

The experiment consisted of 16 total rounds of purchasing decisions by experimental subjects (undergraduate students at Princeton University). In each round, subjects were endowed with $18 and presented with consumption goods that they could purchase. Subjects were informed that they could purchase as many of each good as they wished (up to their $18 endowment) and that they would get to keep however much of their endowment was left over. At the end of the experiment, one round was randomly chosen for payment. The actual goods and any leftover money from the $18 were distributed in the lab. These aspects of the design were similar to Feldman and Ruffle (2015).

As with all laboratory experiments, an important concern is with external validity. To make the experiment as realistic as possible, we attempted to simulate a typical online shopping experience. We purchased an internet domain and directed subjects to proceed to the website, which is where they made their purchasing decisions. The sequence of events was as follows: first, subjects were presented with an instruction screen (Figure 1). The instruction screen stated that all goods were subject to a tax and stated the tax rate (either 8 percent or 22 percent). Additionally, subjects were informed either that the displayed prices already included the sales tax (the TI treatment) or did not include the sales tax (the TE treatment).[9] In the latter case, the instructions stated that the sales tax would be added upon the final purchase of the good.

After viewing the instructions, subjects proceeded to the first round. The main portion of the experiment consisted of two parts, with four rounds per part. Each round began with a shopping stage, in which subjects were presented with a set of products and prices available for purchase

[9] We choose 8 percent as the low tax rate because this is a typical sales tax rate imposed on the East Coast of the U.S., where the experiment was conducted. The 22% high tax is more typical of the VAT rates imposed in Europe.

(Figure 2).[10] The menu of available consumption goods varied by round. Of the ten total products available for purchase, a menu of five were available for purchase in a single round. There were four such menus that were available.[11]

After selecting how many of each good to purchase, subjects proceeded to the checkout screen, which summarized the selected purchase (Figure 3). The checkout screen always contained the after-tax prices, regardless of whether the prices on the previous screen were TE or TI prices. At checkout, subjects had the option of either finalizing their purchase or going back to modify it. Additionally, for subjects who attempted to spend more than their $18 endowment, the checkout screen informed them that they were required to go back and select fewer goods. Once the purchase was finalized, subjects proceeded to the shopping stage for the next round. At the conclusion of the experiment, one of the rounds was randomly selected and subjects received their purchased products and leftover endowment from that round.[12]

B. Experimental Groups

Before the experiment began, subjects were assigned to one of four experimental treatment groups: (1) high-tax treatment, (2) high-tax control, (3) low-tax treatment, and (4) low-tax control. Those in the high-tax group faced a tax rate of 22 percent throughout the experiment whereas the tax rate was 8 percent for those in the low-tax group.[13]

The difference between the treatment and control groups concerned the rounds in which the displayed prices were tax inclusive or tax exclusive. When prices were tax-inclusive, the displayed prices included the tax at both the shopping and checkout stages. When prices were tax-exclusive, the displayed prices were pre-tax; the taxes did not get added until they proceeded to the checkout. The experiment was divided into two parts, with half of the rounds in each part.[14] For the treatment

[10] To minimize the corner solution in which subjects opted to keep their entire endowments, we offered the products at pre-tax prices that were roughly 50 percent of their normal retail pre-tax prices.

[11] The menus were: Menu A: {Nutella, Coconut Water, Clif Bar, Pens, Laundry Detergent} Menu B: {Starbuck's Via Instant Coffee, Trail Mix, Ferrero Rocher Chocolates, Coconut Water, Laundry Detergent} Menu C: {Starbuck's Via Instant Coffee, Clif Bar, 5-hour, Pringles, Nutella} and Menu D: {Trail Mix, Ferrero Rocher Chocolates, 5-hour, Pringles, Pens}.

[12] To avoid imposing transaction costs associated with acquiring the goods (e.g., traveling to a store, exchanging a voucher for the goods), we purchased all of the goods ahead of time and paid subjects according to their choices at the conclusion of each experimental session.

[13] We chose not to vary the tax rate within subject over the course of the experiment to avoid calling attention to the tax rate and artificially increasing its salience.

[14] To minimize the repetitiveness of the experiment, halfway through the experiment all subjects were shown two short videos containing advertisements for laptop computers and asked which advertisement they preferred.

groups, the displayed prices were TE in Part I and TI in Part II. For the control groups, the displayed prices were TI in both Parts I and II.[15] Each subject received each product menu twice during the main portion of the experiment: once in Part I and once in Part II (the order in which subjects received each menu in each set of four rounds was randomized to reduce any order effect). Between Parts I and II, subjects in both treatment groups were informed that they would be shopping in a new store for the remainder of the experiment, and that prices in the new store included the sales tax (Figure 4).

C. Product Prices

Table 1 presents the pre- and post-tax prices for the experimental goods. The prices were chosen to facilitate comparison between the various treatment groups. In particular, during the main portion of the experiment, each treatment group faced the same after-tax price of each good. For example, the after-tax price for Nutella was \$3.49. Treatment subjects assigned to the 8 percent tax group had the pre-tax price of \$3.23 displayed to them while shopping in Part I and the after-tax price of \$3.49 displayed to them while shopping in Part II. Similarly, treatment subjects assigned to the 22 percent tax saw the pre-tax price of \$2.86 while shopping in Part I and the after-tax price of \$3.49 while shopping in Part II. In contrast, subjects assigned to the control groups saw the after-tax price of \$3.49 while shopping throughout the entire experiment. Because the after-tax price was the same for each treatment group, all subjects saw the same \$3.49 price during checkout for all rounds.

Finally, we included an additional four rounds in each Part at an increased price level for the purpose of testing whether demand was downward sloping in after-tax price changes (see Section IV.B.2). The prices in the high price rounds are displayed in Table 2.

[15] No treatment group went in the reverse order because we expected that altering the salience of the tax between rounds would increase its salience; going from TE to TI avoids that problem as long as the displayed prices in the final rounds are already fully salient.

III. Experimental Data

A. Experimental Subject Characteristics

The experimental subjects were 227 Princeton University undergraduate students. Table 3 presents demographic information for the sample. On average, subjects were 20 years old and 70 percent of them were in either their junior or senior year of college. Thirty percent of subjects were male, half were minorities, and half described their financial status as upper class or wealthy. These figures are roughly similar to the school average, though our sample contained a higher proportion of upperclassmen and females than the general student body.

B. Randomization Checks

Subjects were randomly assigned into one of four treatment groups. To maximize statistical power, one-third of subjects were assigned to each of the two treatment groups and one-sixth of subjects were assigned to each of the two control groups. Although the realized distribution of subjects across treatment groups did not precisely coincide with these assignment probabilities,[16] Table 3 shows that subjects' demographic characteristics did not appear to systematically vary with their treatment status, consistent with the randomization working properly.[17]

C. Purchasing Behavior

Table 4 summarizes the purchasing decisions of the experimental subjects. On average, subjects spent \$6.16 per round, just over one-third of their \$18 budget. Only 13 percent of subjects chose not to purchase any products throughout the experiment. On average, subjects purchased at least one item in 67 percent of their rounds. Forty-three percent of subjects purchased at least one item in every round. The distribution of expenditures per round is displayed in Figure 5.

One concern is that if subjects' budget constraints were binding, that would mechanically limit salience effects as they would be unable to increase the amount of their purchases even if they failed to account for the after-tax price. However, subjects spent over \$15 in only 17 percent of rounds

[16] This likely occurred because the treatment group was assigned with replacement and the assignment probabilities were held constant over the course of the experiment.

[17] In particular, an F-test fails to reject equality across the four experimental groups for each of these demographic variables.

on average, and only 7 percent of subjects did so in every one of their rounds. This suggests that most subjects were not constrained by their allotted budgets.

IV. Empirical Results

A. Tax Salience Effects

This section turns to our primary question: how do subjects respond differently to tax-inclusive (TI) versus tax-exclusive (TE) prices, and does the effect vary by tax rate? To investigate this, we first estimate the expenditure gap (the difference in expenditures between TE and TI prices) for subjects in each tax group. Next, we use the theoretical results from Section I to translate the estimated expenditure gap into the Unresponsiveness Ratio, which measures changes in salience between the high and low tax rates.

1. Expenditure Gap

Because subjects in the treatment group were presented with TE prices during Part I and TI prices during Part II, we can use a within-subject design to estimate the effect of tax salience on purchasing behavior. As subjects may have changed their purchasing behavior as the experiment progressed for reasons unrelated to differences in salience between the early and late rounds, we include round fixed effects to capture differences in purchasing behavior associated with the timing of the purchasing decision within the experiment. The round fixed effects are identified by the control groups, who do not experience variation in salience over the course of the experiment.

Figure 6 presents the unadjusted mean expenditure per round for Part I and Part II by tax rate and treatment group while Table 5 presents the results of the regression analysis. As a baseline, Column 1 pools the high- and low-tax groups to check for the presence of a salience effect. The econometric model in the pooled analysis is given by:

$$y_{imr} = \beta TE_{ir} + \alpha_{im} + \gamma_r + \varepsilon_{imr}$$

In this model, y_{imr} denotes total expenditures for subject i facing menu of goods m in round r, TE_{ir} equals one in rounds where taxes are excluded from the displayed price and zero in rounds

where taxes are included in the price, α_{im} are subject-by-menu fixed effects, and γ_r are round fixed effects. In this model, the β coefficient is a within-subject measure of the difference in expenditures between rounds that include versus exclude taxes from the displayed price (the "expenditure gap").

The results of this analysis are consistent with past findings from the salience literature. In particular, subjects spend approximately \$0.57 more when taxes are excluded from the displayed price, suggesting a salience effect.

Turning to the un-pooled analysis, the second column in Table 5 repeats this analysis but allows the salience effects to differ by tax rate. The econometric model for the un-pooled analysis is given by:

$$y_{imr} = \beta_{low} TE_{ir} * LowTax_i + \beta_{high} TE_{ir} * HighTax_i + \alpha_{im} + \gamma_r + \varepsilon_{imr}$$

where $LowTax_i$ is an indicator variable for being assigned to the 8 percent tax rate and $HighTax_i$ is an indicator variable for being assigned to the 22 percent tax rate.

Column 2 of Table 5 documents a striking disparity in sensitivity to tax exclusive pricing by tax rate. Both treatment groups spent less when presented with TI prices, but the magnitude of the difference was much greater for those in the high tax group. The estimated coefficient for β_{high} suggests that the high tax group spent approximately \$1.07 more per round when prices were TE than when prices were TI (the effect was statistically significant at the 1 percent level). In contrast, we estimate that the low tax group spent only \$0.17 more per round under TE prices, and the effect was not statistically distinguishable from zero. These results suggest that the expenditure gap increases with the tax rate. However, as discussed in Section I, the expenditure gap alone does not measure subjects' attentiveness to tax rates; the key parameter needed for answering that question is the Unresponsiveness Ratio.

2. Estimated Unresponsiveness Ratio

To determine whether salience varied by tax rate, we can substitute the estimated expenditure gap into the Unresponsiveness Ratio formula derived in Section I (equation (7)):

$$UR = \frac{\Delta_x(\tau_l)}{\Delta_x(\tau_h)} \frac{\tau_h(1+\tau_l)}{\tau_l(1+\tau_h)}$$

From the regression, we know the ratio of expenditure gaps: $\frac{\Delta_x(\tau_l)}{\Delta_x(\tau_h)} = \frac{0.17}{1.07} = 0.16$. Substituting the other parameters into the formula yields

$$\hat{UR} = (0.16)\frac{(0.22)\,(1.08)}{(0.08)\,(1.22)} = 0.40$$

with a standard error of 0.75.

This point estimate is surprising, as it suggests that salience is *decreasing* in tax size, the opposite of what the bounded rationality theory would suggest. However, the estimate is not sufficiently precise to allow us to reject a value of the Unresponsiveness Ratio that is greater than 1. However, our results do allow us to estimate an upper bound on what the Unresponsiveness Ratio might be. That is, although we cannot rule out an increase in salience when moving from the lower to the higher tax rate, we can bound the size of that increase. In particular, the 95 percent upper bound on the Unresponsiveness Ratio is equal to 1.86, meaning that taxpayers were no more than 86 percent more unresponsive at the low tax rate than they were at the high tax rate

To give some context for interpreting this result, if a commodity tax in the neighborhood of 8 percent was associated with a level of salience of 0.35 (as reported in Chetty, Looney and Kroft (2009)), our results suggest that increasing the tax rate by 175 percent – up to 22 percent – would result in a new level of salience of less than 0.65. That is, even at the 22 percent tax rate, consumers would respond to the tax only about two-thirds as much as to a comparable change in the displayed price of the taxed good.

Finally, suppose that over some feasible range of rates, the salience of a tax is directly proportional to the rate, $\theta = \beta\,\tau$. Applying this formula to the definition of the Unresponsiveness Ratio allows us to write $\frac{1-\beta\tau_l}{1-\beta\tau_h} = UR$. Substituting the values of the tax rates and our estimated upper bound for $\hat{UR}$ yields $\beta = 2.61$, suggesting that a 1 percentage point increase in a tax's rate is associated with a 2.61 percentage point increase in the tax's salience. For context, this value of β suggests it would take a 38 percentage point increase to make a fully non-salient tax ($\theta = 0$) fully salient ($\theta = 1$). Although there is no theoretical reason to predict that this relationship would in fact be linear (and with two tax rates we are unable to test it), this exercise underscores that our estimated upper bound for the change in salience is extremely modest given the magnitude of the tax rate differential present in the experiment.

B. Robustness Checks

1. Understanding the Experiment

The findings in Section IV.A are robust to several sample selection checks. One concern with all lab experiments is that subjects did not understand the instructions of the experiment. We included two checks on subjects' knowledge of the directions in a post-experiment questionnaire. The first check asked subjects about their perceptions of their own understanding of the experiment. Column 1 of Table 6 repeats the analysis from Table 5 excluding the 27 subjects who reported less than perfect understanding of the directions. The second check asks subjects to recall the tax rate that they faced in the experiment. Column 2 of Table 6 excludes the three subjects who incorrectly reported their assigned tax rate. The results are almost completely unchanged.

2. Price Responses

In addition to the eight experimental rounds that were used to estimate the expenditure gap and the Unresponsiveness Ratio, we also ran another eight rounds where we increased the prices of each of the goods by a fixed percentage in order to assess the effect of pre-tax price increases on subjects' purchasing decisions.[18] Relative prices among the goods remained unchanged. Following the law of demand, we would expect that as long as subjects were paying attention to their purchasing decisions with which they were faced overall demand would fall. Such a finding would provide further confidence that the subjects were otherwise acting rationally and thoughtfully.

Like the low-price rounds described earlier, each subject was exposed to a high after-tax price level for each product menu. This allows us to compare per-round expenditures within subject between the high and the low prices. The econometric model is as follows:

$$y_{imr} = \beta * HighPrice_{ir} + \alpha_{im} + \gamma_r + \varepsilon_{imr}$$

In this model, y_{imr} denotes total expenditures for subject i facing menu of goods m in round r, $HighPrice_{ir}$ equals one in rounds where subjects face high prices, α_{im} are subject-by-menu, and

[18] Whereas the after-tax prices were held constant across treatment groups at the low price level, subjects in the high- and low-tax treatment groups faced different after-tax prices in the high-price level rounds (and therefore were not used in the expenditure gap calculations of Section IV.A). The high price was equal to 1.22 times the low price for the high tax group and 1.08 times the low price for the low tax group – these prices were chosen to make the price variation comparable to the observed tax variation.

γ_r are round fixed effects. Thus the β coefficient is a within-subject measure of the difference in expenditures between rounds with high and low price levels.

Table 7 presents the results from this analysis. The results are broken up by treatment group because the difference between the high and low price levels was greater for the high tax groups (for whom the price difference was 22%) than for the low tax groups (for whom the price difference was 8%).

The results confirm that subjects' demand was downward-sloping: both groups purchased more in the low-price rounds than in the high-price rounds. As expected, the difference was greater for the high-tax groups, who faced a larger difference in prices.

3. Censoring

As mentioned earlier, our results will be mechanically biased toward finding no evidence of tax salience if our subjects are censored from above or below. For example, subjects who are not interested in purchasing any of the products we offer will have the same expenditure under both TE and TI pricing. Similarly, subjects who prefer to buy as many products as their budget allows will also have the same expenditure under the two tax-framing treatments. Table 8 determines whether this censoring biased our salience estimates by excluding these two groups of subjects. Column 1 excludes the 25 subjects who never purchased any items in any round and column 2 drops the 12 subjects who spent over $15 in every round. As expected, the salience estimates increase, though our results are largely unaltered under each of these tests.[19]

Additionally, we analyze whether the presentation and size of the tax affected purchasing decisions separately by the extensive and intensive margins, that is, whether people purchased anything at all and how much those making purchases spent. The results are presented in Table 9. On both margins, the results are qualitatively similar as in the main analysis. On the extensive margin, subjects were more likely to make a purchase when facing TE prices, and this effect was approximately twice as large at the high tax rate than at the low tax rate. On the intensive margin, those subjects making purchases while facing the high tax rate spent approximately 16 cents more per round when facing TE prices, whereas the effect was close to zero and statistically insignificant for subjects at

[19] Additional (unreported) results from censored least absolute deviation estimation also confirm our main results and estimate a slightly higher salience effect for the high tax rate.

the low tax rate.

4. Tax Aversion

When interpreting our results, one concern is that subjects have different preferences for paying taxes than they do for other types of prices. One example of this would be if subjects were tax-averse, i.e., they are more likely to avoid taxes than other costs of the same amount. If this were the case, treatments that highlighted the size of the tax rather than the overall after-tax price may lead subjects to spend less on their purchases, independent of any salience effect. To test for the presence of tax aversion, we use a between-subjects design to compare the expenditure of subjects in the high- and low-tax groups. We include only tax-inclusive rounds; hence both the after-tax and displayed prices were constant. All that varies in this analysis is how much of the after-tax price is tax as opposed to pre-tax price. If subjects are tax-averse, it suggests they would spend more when the tax rate is 8 percent compared to 22 percent. Our results are presented in Table 10. Column 1 includes only control group subjects who were presented with TI prices throughout the entire experiment while column 2 extends the analysis to include the TI rounds of treatment subjects. The results show that the differences in expenditure are not significantly different when the tax rates are higher; in fact, the point estimates suggest that subjects if anything spend more when taxes are a higher fraction of the after-tax price.

5. Learning Over the Course of the Experiment

It is possible that in this lab setting, subjects do not fully understand the tax system they face at the start of the experiment. This could cause us to find a salience effect simply because it took the subjects in the TE group longer to understand that the taxes would be added on to the price at checkout despite the fact that they received clear feedback at the end of each round. To test for this possibility, we allow the subjects time to learn about the experimental design in the first rounds of the experiment. Column 1 of Table 11 drops the first half of the rounds of Part I while column 2 drops the first quarter of the rounds of each Part. Our results are very similar to those presented in the main analysis; if anything, our results show a slightly larger salience effect.

C. Other Analyses

1. Time Spent per Round

The results from the previous section suggest that subjects are less attentive to low-salience taxes. One reason subjects may respond more to taxes that are included in the displayed prices is that it is time-consuming or mentally costly to calculate the tax-inclusive price when taxes are excluded from the displayed price. Table 12 examines whether subjects spend more time deciding on what to purchase in rounds where taxes are not included in the displayed price. Column 1 pools subjects across both tax rates to determine the overall effect of tax-exclusive pricing on the duration of each purchasing decision. The average subject spends 12 seconds per round which does not differ statistically when taxes are included versus excluded from the price. In fact, the point estimate on β_{TE} is negative suggesting that subjects spend slightly less time on their purchasing decisions when they are required to compute the after-tax price themselves. Column 2 tests for the effect of tax-exclusive pricing by tax rate. These results show that subjects spend the same amount of time on tax-inclusive and tax-exclusive rounds at both high and low tax rates.

These results may provide support for a heuristic model of decision-making – all subjects ignore taxes that are not included in the displayed price regardless of the size of the tax and, therefore, spend the same amount of time making their purchasing decisions when prices are tax-inclusive versus tax-exclusive. Therefore, if the duration of the purchasing decision is an accurate proxy for the cognitive cost required to pay attention to low-salience taxes, these null results provide evidence against a bounded rationality model.

2. Number of Attempts

As mentioned in Section II, subjects were presented with a list of products on the first screen of the experiment then sent to a checkout screen that displayed the total after-tax price of their basket once they had selected which products they would like to purchase. However, after viewing their bill, they were able to return to the previous screen and adjust their purchases; in fact, customers whose basket exceeded the allotted budget of $18 were forced to do so. If subjects facing tax-exclusive prices ignore the taxes when selecting their basket but realize they have over-spent once reaching the checkout page, we would expect to see an impact of tax-framing on the number of attempts a

subject takes to decide on his final basket. Similarly, we might expect to see a larger salience effect if we limit our sample to subjects who did not alter their purchasing decision after reaching the checkout page.

Table 13 presents the results of this analysis. Column 1 presents results of the effect of the treatment on the number of attempts a subject makes per round. While some subjects took up to 10 attempts in a given round, the average subject took 1.3 attempts to make his final purchase. We find that facing tax-exclusive pricing had no effect on the number of attempts. Column 2 repeats our main analysis, but limits the sample only to rounds in which the subject did not adjust his purchases after reaching the checkout. Column 3 also repeats the main analysis, but uses a subject's selected purchases from the first attempt, i.e., before making any adjustments after seeing the checkout screen. We find that the salience effect at both tax rates increases in size in Columns 2 and 3, suggesting that allowing for adjustment after displaying the after-tax total amount may dampen the true salience effect.

3. Self-Reported Attention

Our salience parameter throughout this paper is measured by differences in expenditure when confronted with tax-exclusive versus tax-inclusive prices. However, this tax-framing may have an effect on whether an individual thinks about taxes when making a purchasing decision. In our post-experiment questionnaire, we ask subjects directly about whether they considered the taxes they faced when making their purchasing decisions. Table 14 compares this self-reported measure of attention for subjects in the treatment versus control groups. We find that subjects facing tax-exclusive prices in Part I (the treatment group) were significantly more likely to report considering taxes when making purchasing decision during those rounds. So while subjects in this group are more likely to overspend on their purchases, they are also more likely to be aware of taxes when making this decision.

V. Conclusion

A growing body of evidence has established that consumers are less likely to incorporate a tax into a final price if taxes are presented separately from posted prices, that is, the "salience effect."

What has not hitherto been tested is how the magnitude of the tax impacts this result. That is, conditional upon the same final tax inclusive price for a given good, are consumers more or less likely to incorporate the tax into their decision making as the tax increases? Using a series of laboratory experiments that differ only in their treatment of the tax (whether it is included in the posted price or excluded in the posted price and only added on later upon purchase) and in the magnitude of the tax (8 or 22%), we find no support to the hypothesis that tax salience increases in tax size within the range of tax rates that we considered. At the high (22%) tax rate, the expenditure gap between TE and TI prices was positive (approximately $1) and statistically significant, suggesting that salience effects do not entirely fade out at that high rate. In fact, our point estimates suggest that the salience of the high tax is somewhat lower than the salience of the low tax – the opposite of what a bounded rationality model would predict – although the difference in salience is not statistically significant. Although we cannot rule out any increase in salience in moving from the low to the high tax rate, a 95% confidence interval provides an informative upper bound: salience effects were no more than 1.86 times as large at the low tax than at the high tax; that is, although the high tax was 2.75 times larger than the low tax, at most consumers were 1.86 times as unresponsive at the low tax compared to the high tax. Given the large difference in tax rates tested in the experiment, our estimated upper bound is extremely modest: linearly extrapolating the estimate implies that each percentage point increase in the tax rate is associated with (at most) a 2.6 percentage point increase in salience.

We also show that our data is internally consistent, that is, subjects followed the law of demand and our results are robust to dropping subjects that did not fully understand the experiment. In addition, we find that even when taking into account censoring of the dependent variable (due to the budget constraint) or leaning over the course of the experiment, our empirical findings remain quite stable. Tax aversion also does not appear to play a role in our findings. Thus, our results suggest that taxpayers would continue to exhibit salience effects even for commodity tax rates substantially greater than those currently imposed in the United States.

Disclosures

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Table 1: Displayed Product Prices by Tax Rate Group

	8 Percent Tax Rate		22 Percent Tax Rate	
	Tax-Exclusive	Tax-Inclusive	Tax-Exclusive	Tax-Inclusive
Nutella	3.23	3.49	2.86	3.49
Starbucks Via Instant Coffee (Box of 12)	3.95	4.27	3.50	4.27
Coconut Water	0.94	1.02	0.84	1.02
Trader Joe's Trail Mix	2.92	3.15	2.58	3.15
Clif Bars (Box of 12)	4.87	5.26	4.31	5.26
Ferrero Rocher Chocolate Truffles	5.22	5.64	4.62	5.64
Pilot G-2 Gel Pens (3-Pack)	2.03	2.20	1.80	2.20
5-Hour Energy Drink (3-Pack)	3.40	3.67	3.01	3.67
Laundry Detergent (50 oz)	3.59	3.88	3.18	3.88
Pringles	1.06	1.14	0.93	1.14

Displayed prices are the prices subjects see prior to checkout.
Treatment group participants are shown tax-exclusive prices in Part I of the experiment and tax-inclusive prices in Part II.
Control group participants are shown tax-inclusive prices in both Part I and Part II.

Table 2: Pre-Tax Product Prices by Tax Rate Group

	8 Percent Tax Rate		22 Percent Tax Rate	
	Low Price	High Price	Low Price	High Price
Nutella	3.23	3.49	3.23	4.26
Starbucks Via Instant Coffee (Box of 12)	3.95	4.27	3.95	5.21
Coconut Water	0.94	1.02	0.94	1.24
Trader Joe's Trail Mix	2.92	3.15	2.92	3.84
Clif Bars (Box of 12)	4.87	5.26	4.87	6.42
Ferrero Rocher Chocolate Truffles	5.22	5.64	5.22	6.88
Pilot G-2 Gel Pens (3-Pack)	2.03	2.20	2.03	2.68
5-Hour Energy Drink (3-Pack)	3.40	3.67	3.40	4.48
Laundry Detergent (50 oz)	3.59	3.88	3.59	4.73
Pringles	1.06	1.14	1.06	1.39

Low prices, which are consistent across tax rates, are used for the majority of analyses.
High prices are used in Section IV.B.2.

Table 3: Randomization Checks

	Age	Male (%)	White (%)	High-Income (%)	Upperclassmen (%)	# Subjects
	(1)	(2)	(3)	(4)	(5)	(6)
Control - 8%	20.2	27.9	39.5	34.9	72.1	43
	(1.2)	(45.4)	(49.5)	(48.2)	(45.4)	
Control - 22%	20.5	32.4	40.5	45.9	75.7	37
	(1.5)	(47.5)	(49.8)	(50.5)	(43.5)	
Treatment - 8%	20.1	31.7	51.2	56.1	68.3	82
	(1.2)	(46.8)	(50.3)	(49.9)	(46.8)	
Treatment - 22%	20.6	29.2	52.3	53.8	67.7	65
	(3.4)	(45.8)	(50.3)	(50.2)	(47.1)	
Overall	20.3	30.4	47.6	49.8	70.0	227
	(2.1)	(46.1)	(50.1)	(50.1)	(45.9)	
$P-Value$	0.55	0.96	0.42	0.12	0.82	

P-value is associated with the F-test to reject equality between treatment groups.
"High-income" indicates students who self-identified as upper-class or wealthy.
"Upperclassmen" indicates juniors or seniors in college.

Table 4: Descriptive Statistics

	Overall
Expenditures Per Round ($)	
Mean	6.16
Standard Deviation	6.15
Median	5.26
Fraction of Rounds (%)	
with Expenditures > $0	67.1
with Expenditures> $15	16.6
Fraction of Individuals (%)	
with Expenditures> $0 in Any Rounds	87.2
with Expenditures > $0 in All Rounds	43.1
with Expenditures > $15 in All Rounds	6.6
N	227

Table 5: The Effect of Tax-Inclusive and Tax-Exclusive Prices on Demand

	(1) Pooled	(2) By Tax Rate
Tax-Exclusive	0.5694*	
	(0.3158)	
Tax-Exclusive (8 Percent)		0.1741
		(0.3487)
Tax-Exclusive (22 Percent)		1.0726***
		(0.4094)
Round Fixed Effects	Yes	Yes
Individual-by-Menu Fixed Effects	Yes	Yes
N	1,814	1,814

Standard errors clustered by individual reported in parentheses.
Outcome=total expenditures per round.
Analysis is a within-subject comparison of expenditure in Part I versus Part II.
$*p < .10, **p < .05, ***p < .01$

Table 6: Robustness: Understanding

	(1) Understood	(2) Recall Correct Tax
Tax-Exclusive (8 Percent)	0.1711	0.1780
	(0.3562)	(0.3581)
Tax-Exclusive (22 Percent)	1.0946***	1.1202***
	(0.3887)	(0.4175)
Round Fixed Effects	Yes	Yes
Individual-by-Menu Fixed Effects	Yes	Yes
N	1,598	1,766

Standard errors clustered by individual reported in parentheses.
Outcome=total expenditures per round.
Analysis is a within-subject comparison of expenditure in Part I versus Part II.
Column 1: drops subjects without perfect self-reported understanding.
Column 2: drops subjects recalling incorrect tax rate in a post-experiment survey.
$*p < .10, **p < .05, ***p < .01$

Table 7: Robustness: Downward-sloping Demand

	(1) Low Tax Group	(2) High Tax Group
8 Percent Price Increase	-0.5856***	
	(0.1669)	
22 Percent Price Increase		-0.9195***
		(0.2196)
Round Fixed Effects	Yes	Yes
Individual-by-Menu Fixed Effects	Yes	Yes
N	1,343	1,111

Standard errors clustered by individual reported in parentheses.

Outcome=total expenditures per round.

Analysis is a within-subject comparison of expenditure in rounds with high versus low prices.

$*p < .10, **p < .05, ***p < .01$

Table 8: Robustness: Censoring

	(1) Drop All Zero	(2) Drop All Max
Tax-Exclusive (8 Percent)	0.1927	0.1917
	(0.3900)	(0.3651)
Tax-Exclusive (22 Percent)	1.2266***	1.1439***
	(0.4538)	(0.4297)
Round Fixed Effects	Yes	Yes
Individual-by-Menu Fixed Effects	Yes	Yes
N	1,614	1,718

Standard errors clustered by individual reported in parentheses.

Outcome=total expenditures per round.

Analysis is a within-subject comparison of expenditure in Part I versus Part II.

Column 1: drops subjects with zero purchases in every round.

Column 2: drops subjects spending over 15 dollars in every round.

$*p < .10, **p < .05, ***p < .01$

Table 9: The Effect of Tax-Inclusive and Tax-Exclusive Prices on Demand, Extensive and Intensive Margin

	(1) Extensive	(2) Intensive
Tax-Exclusive (8 Percent)	0.0474	-0.0056
	(0.0388)	(0.0624)
Tax-Exclusive (22 Percent)	0.0811*	0.1650**
	(0.0418)	(0.0788)
Round Fixed Effects	Yes	Yes
Individual-by-Menu Fixed Effects	Yes	Yes
N	1,814	1,218

Standard errors clustered by individual reported in parentheses.
Column 1: outcome=indicator for positive expenditures per round.
Column 2: outcome=log expenditures per round for rounds with positive expenditures.
Analysis is a within-subject comparison of expenditure in Part I versus Part II.
$*p < .10, **p < .05, ***p < .01$

Table 10: Robustness: Tax Aversion

	(1) Controls Only	(2) TI Rounds
22 Percent Tax Rate	1.4312	0.6269
	(1.1398)	(0.7065)
Round Fixed Effects	Yes	Yes
Menu Fixed Effects	Yes	Yes
N	639	1,227

Standard errors clustered by individual reported in parentheses.
Outcome=total expenditure per round.
Analysis is a between-subject comparison of expenditure by subjects.
randomly assigned to the high versus low tax rate groups.
Column 1: includes control group subjects only.
Column 2: includes rounds with tax-inclusive prices only.
Includes individual-level covariates: age, race, sex, income, and class year.
$*p < .10, **p < .05, ***p < .01$

Table 11: Robustness: Learning

	(1)	(2)
Tax-Exclusive (8 Percent)	0.3866	0.5397
	(0.4587)	(0.5123)
Tax-Exclusive (22 Percent)	1.0737*	1.0938*
	(0.5950)	(0.5950)
Round Fixed Effects	Yes	Yes
Individual-by-Menu Fixed Effects	Yes	Yes
N	1,355	1,359

Standard errors clustered by individual reported in parentheses.
Outcome is total expentitures per round.
Analysis is a within-subject comparison of expenditure in Part I versus Part II.
Columns 1: drops the first 50% of rounds in Part I.
Columns 2: drops the first 25% of rounds in Part I and II.
$*p < .10, **p < .05, ***p < .01$

Table 12: Duration

	(1) Pooled	(2) By Tax Rate
Tax-Exclusive	-1.1276	
	(1.1206)	
Tax-Exclusive (8 Percent)		-0.8486
		(1.3234)
Tax-Exclusive (22 Percent)		-1.4828
		(1.4220)
Round Fixed Effects	Yes	Yes
Individual-by-Menu Fixed Effects	Yes	Yes
N	1,814	1,814

Standard errors clustered by individual reported in parentheses.
Outcome=time spent on purchasing decision per round in seconds.
Analysis is a within-subject comparison of duration in Part I versus Part II.
$*p < .10, **p < .05, ***p < .01$

Table 13: Attempts

	(1) Number of Attempts	(2) Expenditure (One Attempt Only)	(3) Expenditure (First Attempt)
Tax-Exclusive (8 Percent)	0.0030	0.3584	0.3952
	(0.0976)	(0.3826)	(0.3998)
Tax-Exclusive (22 Percent)	0.0844	1.4169***	1.9003***
	(0.1051)	(0.4801)	(0.5202)
Round Fixed Effects	Yes	Yes	Yes
Individual-by-Menu Fixed Effects	Yes	Yes	Yes
N	1,814	1,550	1,814

Standard errors clustered by individual reported in parentheses.
Columns 1: outcome=number of attempts per round.
Columns 2: outcome=total expenditure per round for rounds with only one attempt.
Columns 3: outcome=total expenditure per round in first attempt.
Analysis is a within-subject comparison of the number of attempts or expenditure in Part I versus Part II.
* p $<$ .10 , ** p $<$ .05, *** p $<$ .01

Table 14: Self-Reported Attention to Taxes

	Self-Reported Attention
Treatment (8 Percent)	0.3168***
	(0.0829)
Treatment (22 Percent)	0.4315***
	(0.0909)
High Tax Rate (22 Percent)	0.0040
	(0.0974)
N	227

Standard errors clustered by individual reported in parentheses.
Outcome=indicator for self-reporting having considered taxes in Part I.
Analysis is a between-subject comparison of expenditure by subjects randomly assigned to each experimental group.
Includes individual-level covariates: age, race, sex, income, and class year.
$\{*p < .10, **p < .05, ***p < .01\}$

Figure 1: Instructions

(a) Treatment

There are 16 rounds in the experiment. In each round you will receive $18, which you can spend on various products sold by the online store. The products are very cheap – the store is selling them at heavily-reduced prices. The prices at the store do not include tax - when you proceed to the checkout, an 8% tax will be added to your bill.

You can buy as many of each product as you like, but you cannot spend more than $18 in one round. You do not have to spend your entire budget. Remember, you get $18 to spend in each round.

At the end of the experiment, one of the rounds will be randomly selected. You will receive the products that you purchased in that round plus any money from that round that you did not spend.

Please make all decisions on your own without talking to anyone else. Based on your decisions, you can leave here with $18 of cash and no products, only products and no cash, or some combination of cash and products.

(b) Control

There are 16 rounds in the experiment. In each round you will receive $18, which you can spend on various products sold by the online store. The products are very cheap – the store is selling them at heavily-reduced prices. The prices at the store include an 8% tax – there is nothing extra added to your bill when you checkout.

You can buy as many of each product as you like, but you cannot spend more than $18 in one round. You do not have to spend your entire budget. Remember, you get $18 to spend in each round.

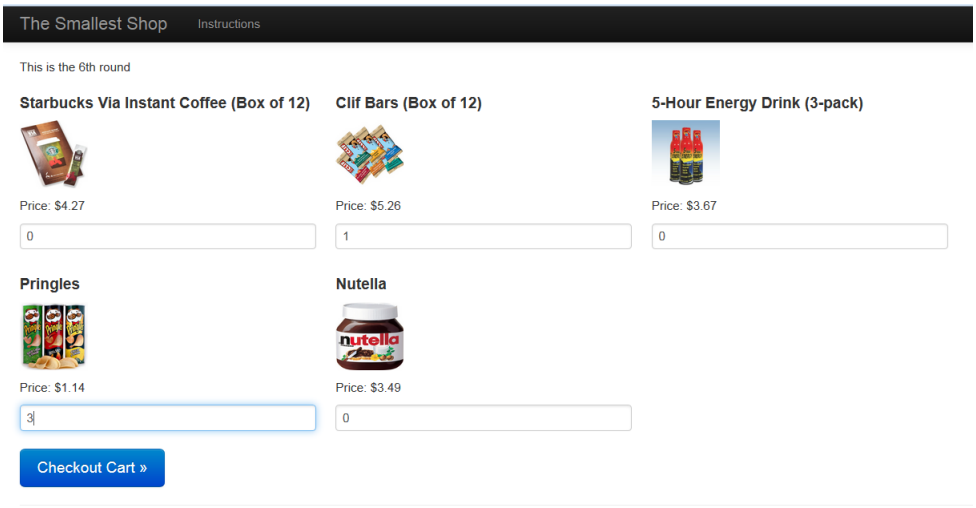
At the end of the experiment, one of the rounds will be randomly selected. You will receive the products that you purchased in that round plus any money from that round that you did not spend.

Please make all decisions on your own without talking to anyone else. Based on your decisions, you can leave here with $18 of cash and no products, only products and no cash, or some combination of cash and products.

Note: Instructions are for subjects assigned to the low-tax groups.

For the high-tax groups, "8%" is replaced by "22%".

Figure 2: Shopping Screen



Note: The products on this shopping screen are of one of the four possible product menus.

Sample prices above are tax-inclusive.

Figure 3: Checkout Screen

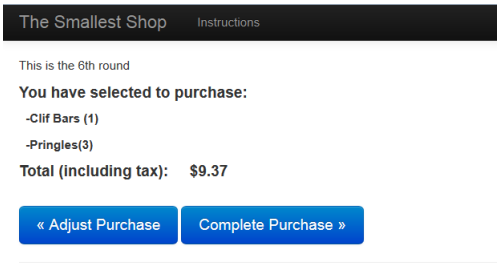
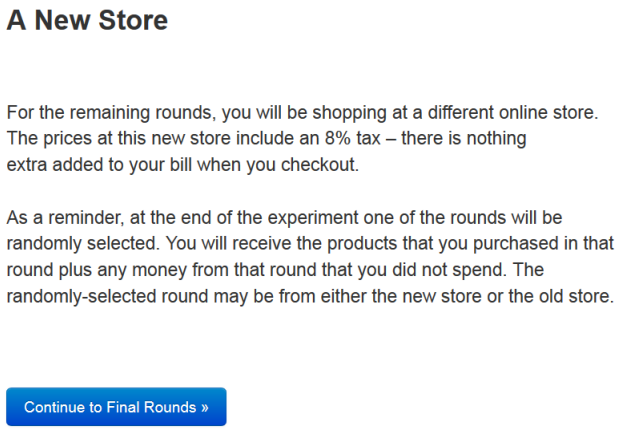


Figure 4: New Store Screen



Note: Instructions are for subjects assigned to the low-tax groups.

For the high-tax groups, "8%" is replaced by "22%".

Figure 5: Distribution of Expenditures Per Round

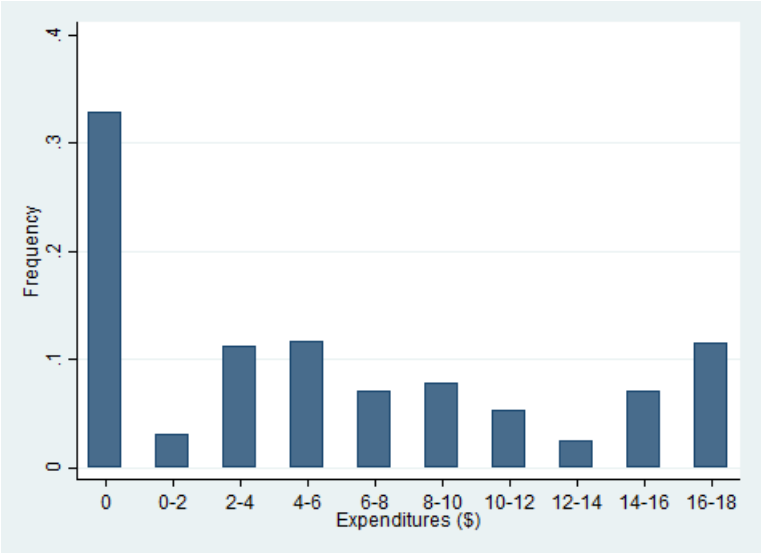
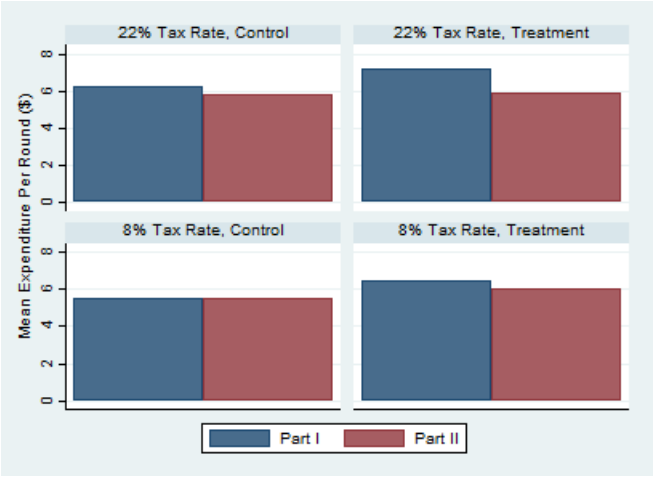



Figure 6: Mean Expenditure by Tax Rate, Treatment Group, and Experimental Section