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Abstract

My dissertation uses insights from the field of behavioral economics to suggest how to design more effective public policies. Chapter 1 examines a simple element of incentive design – whether an incentive takes the form of a fee for bad behavior or a reward for good behavior – to assess how the framing of an incentive impacts the policy's effectiveness. I address this question through the evaluation of two policies aimed at reducing consumption of disposable grocery bags: a five-cent tax on disposable bag use and a five-cent bonus for reusable bag use. I find that the tax decreased disposable bag use by a substantial amount while the bonus generated virtually no effect on behavior, evidence consistent with a model of loss aversion. Chapter 2, coauthored with Jacob Goldin, evaluates another component of incentive design – whether a tax is included in the posted price or taken at the register – to assess how the form of a tax affects the distribution of a tax's burden. Previous research suggests that consumers under-react to register taxes versus posted taxes, implying that a tax's salience does affect behavior. We expand on this analysis by allowing different income groups to differ in their attentiveness to the register tax. We find that while low-income consumers respond to both types of taxes, high-income consumers ignore register taxes. This implies that levying a greater proportion of a commodity tax at the register shifts the tax's burden away from low-income consumers, making the tax less regressive. Chapter 3, also coauthored with Jacob Goldin, examines the effect of payday loan bans on borrowing behavior. While payday lenders offer access to credit to liquidity-constrained consumers, these loans have very high interest rates and evidence suggests that customers often borrow more than they can afford, possibly due to behavioral biases. Concerns about chronic indebtedness have caused several states to regulate the use of payday loans. We find that, while these regulations are effective at reducing the use of payday loans, this reduction is almost completely offset by the use of other high-interest credit products. However, customers who continue to use high-interest credit after a ban are more likely to use these loans to smooth consumption over temporary shocks and not to cover long-term expenses.

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Chapter 1

Can Small Incentives Have Large Effects? The Impact of Taxes versus Bonuses on Disposable Bag Use

Abstract

Financial incentives are an important policy tool for encouraging prosocial behavior. However, evidence on the effect of very small financial incentives is mixed. Drawing on an original data set, I investigate the effect of a five-cent shopping bag tax imposed in the Washington Metropolitan Area. Despite the small size of the incentive, I find that the tax decreased the fraction of customers using a disposable bag by a substantial amount. In contrast, a similar policy that offered customers a five-cent bonus for reusable bag use generated virtually no effect on behavior. This pattern is consistent with a model of loss aversion and underscores the importance of the form a financial incentive takes – a tax versus a bonus – when designing policies aimed at shaping consumer behavior.

Can small incentives have large effects on prosocial behavior? Standard economic theory suggests that financial incentives will be effective if the costs an individual associates with changing his behavior are smaller than the incentive provided for doing so. For example, small fees on residential trash collection have been shown to increase recycling (Fullerton and Kinnaman, 1996), while small "sin" taxes on soft drinks had only a negligible effect on consumption (Sturm et al., 2010). While, in practice, financial incentives can either take the form of a fee for bad behavior or a bonus for good behavior, this theory suggests that individuals should respond similarly to the two types of incentives provided that they are the same amount. In contrast, evidence from the field of behavioral economics (Kahneman and Tversky, 1979) suggests that individuals perceive losses more strongly than gains, implying that a fee would be more effective than a bonus of the same size. I address whether small incentives and their design matter through the evaluation of two policies in the Washington Metropolitan Area aimed at reducing the use of disposable grocery bags: a five-cent tax on disposable bag use and a five-cent bonus for reusable bag use. Using variation in incentive policies across time and location, I am able to determine if the framing of the incentive influences the policy's effectiveness.

Growing concern over the environmental impact of plastic bags has prompted several governments across the world to regulate the use of disposable bags; many countries in Europe, Asia, and Africa require grocery stores to charge a fee for each bag the store provides. In 2010, Washington, D.C. became the first city in the United States to pass legislation calling for grocery stores to tax customers for the use of disposable bags. Two years later, Montgomery County, an area of Maryland bordering D.C., passed its own bag tax. Similar legislation has been passed in several counties and cities in California, Colorado, and Washington.

Despite the growing popularity of such laws, rigorous empirical work that assesses their effectiveness has been lacking. This is the first study to use design-based research to estimate the effectiveness of such a policy in the U.S. context. Scanner data from a retail chain of grocery stores provides a description of disposable bag use after the tax and suggests a large decline in bag use in the first few weeks of implementation. I also collected data on individual-level consumption of disposable and reusable bags by observing customers as they exited the grocery store. The data set contains information on over 16,000 customers in Montgomery County and in surrounding areas outside of the county in the months before and after the tax's implementation. This data allows me to analyze the effect of the tax on demand using a difference-in-differences research design. While 82 percent of customers in Montgomery County used at least one disposable bag per shopping trip prior to the tax, this estimate declined by 42 percentage points after the tax was implemented. Additionally, customers who continued to use disposable bags after the tax used fewer bags per trip, leading to an overall reduction in demand of just over one disposable bag per shopping trip. These effects imply a reduction of over 18 million disposable bags per year if each household in Montgomery County shopped once per week.

It is possible that the tax reduced disposable bag use through purely economic channels – if five cents is larger than the cost customers attach to the inconvenience of bringing a reusable bag or carrying one's groceries without a bag, they will use fewer disposable bags. If this is the case, neoclassical economics suggests that a five-cent bonus should have the same impact on behavior as a five-cent tax. However, if customers are loss-averse, in that they adjust their behavior more in response to losses than in response to gains, the bonus is likely to be less effective than a tax of the same magnitude.

Prior to the implementation of the tax, several stores offered their own incentive to reduce the use of disposable bags: a five-cent bonus for reusable bag use. I use the cross-sectional variation in policies across stores to compare the effect of the bonus to the effect of the tax. In stores that offer no incentive, 84 percent of customers use at least one disposable bag. While 82 percent of customers in bonus stores used disposable bags, only 39 percent of customers in stores that charge a tax used disposable bags. These results suggest that, while the tax has a substantial impact on disposable bag use, a bonus of the same amount has almost no effect on behavior, evidence consistent with a model of loss aversion. A survey of consumer attitudes on the effectiveness of bag taxes and bonuses also supports a role for loss aversion in explaining the observed pattern of consumer behavior.

I present a simple model of reference-dependent preferences and estimate this model using my observational data. I estimate a coefficient of loss aversion that is larger than those previously found in the literature. I explore the possibility that customers receive an added benefit from acquiring a product, i.e., a disposable bag, for free (Shampanier, Mazar and Ariely, 2007). This would generate a discontinuous jump in the utility function at a zero-price reference point, leading to an estimate of the coefficient of loss aversion.

The paper concludes by exploring mechanisms other than loss aversion that may have caused customers in stores charging a tax to use fewer disposable bags than customers in stores offering a bonus. First, I show that differences in demographic composition of customers at the two types of stores do not affect my results. Second, while survey data suggests that customers are less aware of the bonus than the tax, the differences in awareness cannot fully account for the difference in effectiveness of the two policies. Next, I investigate whether the results are driven by customers responding to a shift in social norms associated with the tax. I surveyed customers on their attitudes about the use of disposable bags and pollution regulation before and after the implementation of the Montgomery County tax and found no change in social norms between the two periods. Lastly, recent evidence suggests that customers are more likely to avoid any charge that is framed as a tax (as opposed to a fee). To explore the possibility that such “tax aversion” explains the discrepancy in consumer behavior when faced with a bonus versus a tax, I conducted an experiment in which participants were asked how they would respond to a hypothetical five-cent penalty for using a disposable bag, randomizing whether the penalty was framed as a government tax or as a fee instituted by the store. I find no difference between the two scenarios.

This paper is organized as follows. Section I reviews the history of disposable bag regulations, both internationally and domestically. Section II presents two models of the customer’s choice to bring a reusable bag. Section III describes the various data sources used in the empirical analysis. Section IV presents estimates of the impact of the disposable bag taxes in the Washington Metropolitan Area. Section V contains an analysis of the mechanisms which may have contributed to the effectiveness of the tax policy. Section VI concludes.

I. Background on Disposable Bag Regulations

A. International Policies

Plastic bags were first introduced to grocery store customers in the 1970s and are now used in almost every store in the United States. Clapp and Swanston (2009) report that Americans consume 100 billion plastic bags each year, with worldwide estimates reaching as high as 1.5 trillion. While these plastic bags are often recyclable, the Environmental Protection Agency (EPA) estimated that only 5.2 percent of plastic bags in the United States in 2005 were actually recycled (USEPA, 2006). The uncontrolled disposal of plastic bags has caused environmental problems across the globe.

In an effort to reduce pollution caused by the consumption of disposable bags, several domestic and international governments have passed various policies to curb plastic bag consumption. Starting in the early 2000s, several countries, mostly in Africa, banned the use of plastic bags. As an alternative to an outright ban, Ireland became one of the first countries to levy a tax on consumers for plastic bag use; the €0.15 tax per bag led to a dramatic 94 percent decrease in consumption in the first year (Convery, McDonnell and Ferreira, 2007). South Africa combined the two types of policies, banning the use of all plastic bags under a certain thickness as well as prohibiting stores from offering free plastic bags.[1] Dikgang, Leiman and Visser (2012) and Hasson, Leiman and Visser (2007) conclude that these policies led to an immediate reduction in plastic bag use. A similar policy in China led to a 49 percent reduction in plastic bag consumption (He, 2010).

B. Washington Metropolitan Area Disposable Bag Regulations

The Anacostia River, located in Washington, D.C. and Maryland, suffers from excessive litter and pollution. The buildup of disposable bags degrades water quality, harms aquatic life and causes flooding by clogging storm drains.[2] In December 2008, the District Department of the Environment conducted a study to assess the types and sources of trash that were polluting the river. The study showed that plastic bags comprised 47 percent of all trash in tributary streams and estimated that it would cost $32.4 million to clean up the river (DDOE, 2008).

In response to the report, D.C. enacted the Anacostia River Cleanup and Protection Act in 2009. This law requires all retailers in the district that sell food[3] to charge five cents per single-use plastic or paper bag starting on January 1, 2010, making D.C. the first city in the United States to charge a fee for the use of disposable bags. The law also requires that the fee be charged at the point of purchase and not be included in the cost of other items. One to two cents of the tax goes to the retailer to cover costs associated with the tax's implementation while the remainder goes to a fund dedicated to cleaning up the Anacostia River.[4]

[1] While the price per bag was originally fixed, after three months retailers were able to set the price of a bag without restriction. This led to a substantial decrease in the charge per bag (Dikgang, Leiman and Visser (2012)).

[2] In addition, the river is in danger of violating the EPA's Total Maximum Daily Load (TMDL) of allowable trash, which could cost D.C. millions of dollars in fines.

[3] This includes all retailers holding a Retail Food Establishment or Class A & B liquor license holders, i.e., grocery stores, food vendors, convenience stores, drug stores, restaurants, and liquor stores.

[4] Retailers who offer customers a discount for bringing a reusable bag retain two cents for every five collected; all other retailers retain one cent.

Inspired by D.C.'s policy, Montgomery County, an affluent county in Maryland that borders D.C. to the northwest, passed a similar initiative. As of January 1, 2012, all retail establishments[5] in Montgomery County were required to charge a five-cent tax for each disposable bag that a customer used. Proceeds from the tax enter the county's Water Quality Protection Charge. Similar bills have been suggested in other jurisdictions in the Washington Metropolitan Area, but none have passed as of this date.

Additionally, prior to the implementation of either tax, several retail chains offered their own incentive for bringing a reusable bag. Customers shopping at these stores receive a five-cent bonus for each reusable bag they use instead of taking a new disposable bag. Of the four stores with the largest market share in the Washington Metropolitan Area, two provided such a bonus.

C. Other Domestic Regulation of Disposable Bags

The state of California has been a hotbed for disposable bag regulation over the past few years. San Francisco became the first U.S. city to regulate the use of disposable bags with a ban on plastic bags in 2007. On July 1, 2011, Los Angeles County not only banned plastic bags but began charging a minimum of ten cents for paper bags. Over the next year, the cities of Santa Monica, San Jose, and Sunnyvale, as well as the counties of Marin, Santa Clara, and Santa Cruz passed similar laws.

As of this date, disposable bag taxes have been proposed in states as diverse as Arizona and Pennsylvania. While I will focus mostly on the impact of the policies in D.C. and Montgomery County, I will provide some descriptive evidence on the effectiveness of regulations in other locations as well.

II. Modeling Responses to Financial Incentives

Consider a customer who is choosing whether or not to use a disposable bag or bring a reusable bag. A customer has utility $U_i(w_i, b_i)$, where w_i represents customer i's wealth and b_i is a binary

[5] Unlike in D.C., the Montgomery County tax applies to all retailers, not just those selling food or alcohol. Additionally, retailers do not receive any financial incentive for offering a reusable bag bonus program.

choice variable which equals one if the customer chooses to bring a reusable bag and zero otherwise. Customers also have idiosyncratic preferences for reusable bag use and incur a utility cost for bringing a reusable bag, c_i, which can be a positive cost (for example, a psychological cost for remembering to bring a bag) or a negative cost (for example, a warm glow from helping the environment). For simplicity, customers must use one of the two types of bags and require only one bag. Assume that c_i enters the utility function linearly and that the customer's utility is additively separable between c_i and w_i so that utility when there is no external incentive can be defined as $U_{N,i}(w_i, b_i) = u(w_i) - b_i c_i$. Now suppose that customers are subject to a tax of x for using a disposable bag. The individual's utility function then becomes $U_{T,i}(w_i, b_i) = u(w_i - (1 - b_i)x) - b_i c_i$. Similarly, if we consider a policy where customers receive a bonus of x for using a reusable bag, the utility function becomes $U_{B,i}(w_i, b_i) = u(w_i + b_i x) - b_i c_i$.

When will customers choose to bring a reusable bag rather than use a disposable bag? The table below outlines the conditions under which a customer would choose to bring a reusable bag under different policies. If no financial incentives are provided, customers will bring a bag if $0 > c_i$, i.e., if they derive a personal benefit from bringing a reusable bag. If customers are charged a tax for disposable bag use, they will bring a reusable bag if the decrease in utility they suffer from having to pay the tax is larger than the cost of bringing a reusable bag. Similarly, if customers are awarded a bonus for reusable bag use, they will bring a reusable bag if the utility gain from receiving the bonus is larger than the cost of bringing a reusable bag.

	Utility Function	Condition to Bring a Bag
No Incentive	$U_{N,i}(w_i,b_i)=\begin{cases} u(w_i)-c_i & if\, b_i=1 \\ u(w_i) & if\, b_i=0 \end{cases}$	$0 > c_i$
Tax Policy	$U_{T,i}(w_i,b_i)=\begin{cases} u(w_i)-c_i & if\, b_i=1 \\ u(w_i-x) & if\, b_i=0 \end{cases}$	$u(w_i)-u(w_i-x) > c_i$
Bonus Policy	$U_{B,i}(w_i,b_i)=\begin{cases} u(w_i+x)-c_i & if\, b_i=1 \\ u(w_i) & if\, b_i=0 \end{cases}$	$u(w_i+x)-u(w_i) > c_i$

Should we expect that customers will have the same response to a bonus and a tax of the same size? The following section presents two models with different predictions for the relative effectiveness of the tax and bonus policies.

A. Neoclassical Model

In this paper, I consider the effect of tax and bonus policies with a very small x, i.e., five cents. Standard economic theory predicts that if c_i is also very small, these incentives could still have large effects on consumer behavior. In other words, a small financial incentive will be effective as long as demand for disposable bags is elastic.

Suppose customers maximize utility over wealth and that utility is strictly increasing and weakly concave ($u'(w_i) > 0$ and $u''(w_i) \leq 0$), i.e., marginal utility is diminishing in wealth. Then customers will derive less utility from a gain in wealth than from a loss of the same magnitude due to the curvature of the utility function and the proportion of customers bringing a reusable bag will be larger under the tax policy than under the bonus policy. However, Rabin (2000) demonstrates that individuals must be approximately risk neutral over small stakes in order for expected-utility models to imply reasonable levels of risk aversion over large stakes. His calibrations suggest that the consumption value of a dollar should not change significantly over changes in wealth up to $1000. Given that the incentives considered in this study are only five cents per bag, it is reasonable to assume that utility is linear, i.e., $u(w_i) = \gamma w_i$, over the change in wealth caused by these policies. With this assumption, neoclassical economics predicts that the conditions under which customers would bring a reusable bag under the tax policy and under the bonus policy are the same (see table below).

	Utility Function	Condition to Bring a Bag
No Incentive	$U_{N,i}(w_i,b_i)=\begin{cases}\gamma w_i - c_i & if\, b_i=1\\ \gamma w_i & if\, b_i=0\end{cases}$	$0>c_i$
Tax Policy	$U_{T,i}(w_i,b_i)=\begin{cases}\gamma w_i - c_i & if\, b_i=1\\ \gamma(w_i-x) & if\, b_i=0\end{cases}$	$\gamma x>c_i$
Bonus Policy	$U_{B,i}(w_i,b_i)=\begin{cases}\gamma(w_i+x) - c_i & if\, b_i=1\\ \gamma w_i & if\, b_i=0\end{cases}$	$\gamma x>c_i$

B. Reference-Dependent Model

Prospect theory, developed by Kahneman and Tversky (1979), proposes that, while utility is defined in terms of net wealth, value is defined in terms deviation from a reference point (i.e., gains and losses). They suggest that individuals perceive losses more strongly than gains of the same size, a phenomenon referred to as loss aversion. Consider a simple reference-dependent utility function where utility is linear in wealth but with a kink at a reference point, w^*:

$$u(w_i)=\begin{cases}\gamma(w_i-w^*) & if\, w_i>w^*\\ \alpha\gamma(w_i-w^*) & if\, w_i\leq w^*\end{cases},\text{where } \alpha>1.$$

If an individual's reference point is his wealth level in the absence of any incentive policy, then the conditions for using a reusable bag simplify to the equations in the following table.

	Utility Function	Condition to Bring a Bag
No Incentive	$U_{N,i}(w^*,b_i)=\begin{cases}-c_i & if\, b_i=1\\ 0 & if\, b_i=0\end{cases}$	$0>c_i$
Tax Policy	$U_{T,i}(w^*,b_i)=\begin{cases}-c_i & if\, b_i=1\\ -\gamma\alpha x & if\, b_i=0\end{cases}$	$\gamma\alpha x>c_i$
Bonus Policy	$U_{B,i}(w^*,b_i)=\begin{cases}\gamma x - c_i & if\, b_i=1\\ 0 & if\, b_i=0\end{cases}$	$\gamma x>c_i$

Since $\alpha > 1$, this model predicts that customers are more likely to bring a reusable bag when the financial incentive takes the form of a tax rather than a bonus. The following sections empirically test whether customers respond similarly to the two policies, as predicted by neoclassical theory, or if customers exhibit loss aversion.

III. Data

The first part of this paper assesses the effectiveness of the tax in reducing the use of disposable bags using two data sets. First, I use transaction-level scanner data from a large retail chain of grocery stores in several areas that have implemented a disposable bag regulation. The data set includes a ten percent sample of all transactions in multiple stores in D.C., Montgomery County, Santa Monica, San Jose, and Santa Cruz County in the months following the implementation of the disposable bag tax in each area.[6] Each observation corresponds to a purchased product and includes information on date, store location, and a transaction identifier used to link all purchases in a given transaction. In addition, the data includes a line item for whether or not the customer was charged for the use of a store-provided paper or plastic bag. This data allows me to calculate the percent of customers using at least one disposable bag in the days following the implementation of the tax; however, I am not able to compare demand before and after the tax, nor am I able to compare demand in cities with a tax to demand in cities without a tax.

To address this limitation, the main analysis utilizes a data set I collected containing information on demand for disposable[7] and reusable[8] bags before and after the implementation of the Montgomery County tax. I collected data at sixteen stores in the Washington Metropolitan Area

[6]The data set includes eleven stores in D.C., sixteen in Montgomery County, three in Santa Monica, ten in San Jose, and three in Santa Cruz County. The sample includes an average between 2000 and 2500 transactions per day for D.C., Montgomery County, and San Jose, and between 500 and 1000 transactions in for Santa Monica and Santa Cruz County.

[7]A disposable bag refers to either paper or plastic single-use bags. I do not consider the two types of bags separately because almost all customers chose to use plastic bags when they were offered. Additionally, four of the stores in the sample are an organic retail chain that only provides paper bags.

[8]A reusable bag refers to any multiple-use bag. While most customers used typical reusable bags sold by the store, this category also includes shopping carts, backpacks, tote bags, or disposable bags brought from home.

– eight stores in Montgomery County, four stores in Virginia, and four stores in D.C. – approximately two months before and two months after the implementation of the tax.[9] These stores include three different grocery store chains and one organic market chain. To obtain measures of demand, I stood by the register at each store for an average of ten thirty-minute intervals per store, randomizing the order in which I visited each store, and recorded the number and type of bags each customer used, as well as the customer's gender and race. The final sample contains information on 16,251 customers. This data set enables me to compare the change in demand in Montgomery County before and after the policy to that in control stores in D.C. and Virginia. The stores in Maryland and Virginia that I selected for this study are located in the cities of Bethesda, Silver Spring, and Arlington, which border D.C. and are popular communities for those employed in the district. While the city of D.C. is poorer and more diverse than these suburban commuter towns, the D.C. stores selected for this study are located in the wealthier areas of the city in order to maintain comparability to the samples from Maryland and Virginia.[10]

The second part of this paper addresses the question of whether a five-cent bonus for using a reusable bag can have the same effect on behavior as a tax of the same amount. The observational data mentioned above was collected at four different grocery store chains – two of which offered a bonus program, two of which did not. The primary analysis uses this data to determine whether customers shopping at stores that charge a five-cent tax for disposable bag use exhibit similar behavior to those shopping at stores offering a five-cent bonus for reusable bag use.

I use two additional data sets to investigate whether the differences in behavior I observe in bonus versus tax stores suggests that customers are loss-averse with respect to incentives for reusable bag use or if there are other mechanisms at work that could cause this discrepancy. First, I conducted in-person surveys of customers as they exited the store after their shopping trip before

[9]Data in the pre-period was collected from late September to early November of 2011 while data in the post-period was collected from late February to early March of 2012. All data was collected from Monday through Friday between the hours of eleven in the morning and eight at night.

[10]The cities of Bethesda and Silver Spring have a median household income of $133,480 and $67,918, respectively, with a non-Hispanic white population of 78 and 36 percent. Arlington's population is 64 percent non-Hispanic white with a median household income of $94,880. The percent non-Hispanic white ranges from 32 to 81 percent in the four D.C. zip codes considered and median household income ranges from $64,134 to $153,174.

and after the policy change in Montgomery County. These surveys were conducted at twelve different locations at two grocery store chains in Maryland, Virginia, and D.C. I collected data for the pre-period from September to October of 2011 and returned to the same stores[11] to conduct the post-period interviews in March of 2012.[12] The survey yielded a response rate of 56 percent for a total of 1,624 respondents. Customers were asked how many disposable and reusable bags they used that day, whether they knew if the store provided a bonus for bringing a reusable bag or charged for taking a disposable bag, personal demographic characteristics, subjective measures of how much both of these policies did or would encourage them to use a reusable bag, and attitudes toward plastic bag use, environmentalism, and government regulation of pollution. Second, in order to test customers' response to other hypothetical disposable bag regulations, I use data from an online survey administered through Amazon's Mechanical Turk (Mturk), a crowdsourcing web service.[13]

IV. The Effect of the Washington Metropolitan Area Bag Tax

Can small financial incentives deter undesirable behavior? This section reviews previous studies on this question and provides evidence of the effectiveness of the tax policies in the Washington Metropolitan Area at reducing consumption of disposable bags.

A. Literature Review on Small Financial Incentives

For decades, taxes on commodities that impose negative externalities on society have been popular among federal and local governments. Several of these "sin taxes" have not only provided a substantial source of government revenue, but they have also been effective in curbing behavior

[11] Although I attempted to include the same stores in the pre- and post-period, two of the twelve stores include data from only one period. Exclusion of these two stores does not change the results shown in Section IV.

[12] I approached customers as they exited the store between the hours of noon and six and asked if they would be willing to participate in a short survey for a research project on shopping behavior. If a customer chose not to participate in the survey, I recorded him as a non-respondent and moved on to the next customer who exited the store.

[13] While Mturk participants tend to be younger and more educated than the general population, Paolacci, Chandler and Ipeirotis (2010) show that the sample population is generally representative of the U.S. population and they are able to replicate the findings of several well-known behavioral economics experiments using this subject pool.

that is unhealthy for the individual or harmful to the public. For example, cigarette taxes have been shown to decrease smoking rates, leading to better health outcomes for smokers and their families (Chaloupka and Warner, 2000*a*). However, these taxes constitute a substantial portion of the after-tax price of the commodity.[14] In contrast, taxes on soft drinks, which are much smaller – around four percent – showed only a very small impact on consumption (Sturm et al., 2010).

Similarly, evidence on the effectiveness of other types of monetary incentives is mixed. Fullerton and Kinnaman (1996) show that charging individuals for residential waste disposal reduced waste and increased recycling. Lacetera, Macis and Slonim (2012) present evidence that financial incentives positively affect blood donations and that the affect increases with the size of the incentive. However, Titmuss (1970) suggests that financial incentives may not, in fact, increase public goods contributions and in some cases could deter such prosocial behavior. Several theories have been proposed for why incentives aimed at promoting prosocial behavior may have these unintended consequences. Gneezy and Rustichini (2000*b*) show that students asked to collect money door-to-door for charity exhibit less effort when offered a small financial incentive and suggest that this extrinsic motivation (i.e., the financial incentive) crowds out an individual's intrinsic motivation (e.g., altruism). Another theory suggests that the introduction of a financial incentive shifts the decision to contribute the the public good from a social frame to a monetary frame (Gneezy and Rustichini, 2000*a*; Heyman and Ariely, 2004).

B. Demand for Disposable Bags After Tax Implementation

As a first step, I use grocery store scanner data to investigate consumers' disposable bag use in the weeks following a tax's implementation. This data allows me to determine if a customer was charged for using a disposable bag during a given transaction.[15] Because the measure of disposable bag use is derived from bag tax collections, I only have information on disposable bag

[14] In 2011, state and federal cigarette excise taxes ranged from 25 to 54 percent of the total price of a pack of cigarettes (*Orzechowski and Walker (2011)*)

[15] The data allows me to compute aggregate daily averages of the percent of customers using disposable bags, but are not informative as to the number of bags used by a particular consumer.

consumption in areas that charge a tax and only after a store has implemented the tax. Therefore, I cannot compare consumption before and after the policy, nor can I compare stores that charge a tax to those that do not. However, this data provides a description of how disposable bag use changed in the first few weeks after implementation, as well as in the long-term.

Figure 1.1a plots the percent of customers using a disposable bag in stores located in D.C. for the first year of the tax policy, starting on January 1, 2010.[16] The figure shows that 58.1 percent of customers used at least one disposable bag on the first day the tax was implemented and 52.7 percent used a disposable bag in the first week of implementation. This estimate decreased to 41.5 percent by the last week in January and remains at or below 40 percent for most of the year. I replicate this analysis for stores located in Montgomery County in Figure 1.1b. This figure shows that on January 1, 2012, the first day of the Montgomery County tax, 39.8 percent of customers used at least one disposable bag. By the last week in January, only 26.3 percent of customers were charged a tax.

One concern with interpreting changes in behavior as a response to the tax is that there may be seasonal fluctuations in disposable bag use that could conflate the effect of the tax. While I do not have data on bag use in either area before the tax was implemented, Figure 1.1c compares behavior in D.C. in the first year of implementation to behavior in the following two years. While the figure shows a substantial drop in disposable bag use during the first month of 2010 (when the tax was implemented), it does not show a similar change in behavior during the first month of 2011 or 2012. This suggests that differences in bag use across different dates in January are unlikely to be driving these results.

As mentioned in Section I.C, several cities and counties in California banned the use of plastic bags and imposed a ten-cent charge for disposable paper bags. Figure 1.1d plots the percent of customers charged for a paper bag in stores in the cities of Santa Monica and San Jose as well as unincorporated areas of the Santa Cruz County. Santa Monica implemented its policy on September 1, 2011, San Jose on January 1, 2012, and Santa Cruz County on March 20, 2011. While the

[16] I drop two days in February 2010 where I observe an unusually low number of transactions, likely due to a blizzard in the area.

data for the California locations is much noisier than the data from D.C. and Montgomery County due to the smaller number of stores in the sample, there is still a slight decrease in paper bag use during the first week of the policy. It is also interesting to note that the percent of customers using a paper bag on all dates is notably lower in the California stores than in the stores in the Washington Metropolitan Area. This may have to do with differences in policies – the California policy involves a plastic bag ban and charges a higher fee for paper bag use – or may be due to differences in behavior across locations prior to policy implementation.

C. The Effect of the Montgomery County Bag Tax: A Difference-in-Differences Analysis

While the scanner data allows a precise descriptive analysis of bag use behavior in the months following the implementation of the tax, the lack of pre-tax scanner data prevents me from using the data to draw any causal interpretations about the tax's effect. Evaluations of the South African plastic bag levy (Hasson, Leiman and Visser, 2007; Dikgang, Leiman and Visser, 2012) suffer from the same criticism – both studies use firm-level data from a small number of retailers to examine plastic bag consumption over time, but neither includes data prior to the policy implementation. While the evaluations of the Irish bag tax (Convery, McDonnell and Ferreira, 2007) and the Chinese bag fee (He, 2010) utilize plastic bag consumption data before and after the policy, neither study collects data on a set of control stores or locations. This can be a problem if there are factors that affect bag consumption other than the plastic bag regulation that were changing at the same time as the policy implementation, i.e., shifts in social norms around environmental behavior, seasonal patterns in disposable bag use, or changing economic conditions that affected either production of disposable bags or grocery shopping behavior.

To deal with these issues, I expand on the descriptive analysis using data on grocery bag demand collected before and after the implementation of the Montgomery County bag tax. As mentioned in Section III, I collected data on disposable and reusable bag use at sixteen stores in the Washington Metropolitan Area including stores in Montgomery County (where there is a policy change), D.C. (where a tax had already been imposed two years prior to data collection), and Virginia (which has no bag tax) before and after the implementation of the Montgomery County tax. This allows me to perform a difference-in-differences analysis to assess the impact of the tax on various measures of bag consumption.

Table 1.1 contains the mean values of the demographic characteristics of customers in the sample by state and time period. While the three locations vary slightly in their racial composition, all three areas are predominantly white with a similar gender ratio. In addition, the demographics within a location do not change significantly between the two time periods.

The analysis begins with a simple comparison of means of various measures of demand across locations and time periods. While reusable bags are the most common substitute for disposable bags, customers may opt to not use any bags at all; therefore, the majority of the analyses presented in this paper will include measures of demand for both disposable and reusable bags to create a complete picture of the changes in behavior as a result of the bag regulations. Figures 1.2a and 1.2b show the percent of customers using any disposable and any reusable bags, respectively, in the three locations before and after the implementation of the Montgomery County tax. Recall that in D.C., stores are required to charge a five-cent tax in both periods, while there is no bag regulation in Virginia in either period. In the pre-period, customers in the Virginia sample used at least one disposable bag 82 percent of the time while customers in D.C. used a disposable bag only 45 percent of the time. Similarly, Virginia customers rarely brought a reusable bag when shopping, only 16 percent of the time, compared to 46 percent in D.C. These numbers changed only slightly between the two periods. In contrast, demand in Montgomery County shifted dramatically after the implementation of the tax. Behavior in the pre-period resembled that observed in Virginia – 82 percent of customers used at least one disposable bag while only 16 percent brought a reusable bag. However, behavior in Montgomery County after the tax mirrored the behavior observed in D.C. – 40 percent of customers used a disposable bag while 49 percent brought a reusable bag. Table 1.2 contains the statistics corresponding to those displayed in the figures as well as means for additional measures of bag demand. I consider demand for the two types of bags on the extensive margin (the percent of customers using each type of bag), the intensive margin (how many bags each customer uses given that they use that particular type of bag), and overall demand (the unconditional number of bags of each type the customer uses). While the effect of the tax seems to have the largest impact on demand on the extensive margin, Montgomery County customers who continue to use disposable bags after the tax use fewer bags per trip. The data also shows an increase in the proportion of customers choosing not to use any bags at all.

I then use a regression framework to evaluate the effect of the Montgomery County tax on these measures of demand controlling for various individual- and store-level covariates. The empirical model follows a difference-in-differences strategy and takes the following form:

$$Y = \theta_0 + \theta_1 MD * Post + \theta_2 Post + \theta_3 MD + \lambda X + \varepsilon,$$

where Y is a measure of demand on the extensive and intensive margin, respectively, *Post* is an indicator for observations after the implementation of the Montgomery County tax, *MD* is an indicator for customers shopping in Montgomery County, and X is a set of controls.[17] The coefficient of interest is θ_1, the coefficient on the interaction of *Post* and *MD*, which measures the effect of the tax on demand in Montgomery County relative to changes in demand in the control stores.

Table 1.3 presents results for the effect of the tax on one measure of consumption, demand for disposable bags on the extensive margin, using different control variables in each specification. The model in column 1 controls for time period, state, and the interaction of shopping in Montgomery County and shopping in the post-period only. The results show that the tax caused a decrease in the proportion of customers using at least one disposable bag by 41.7 percentage points. Column 2 adds controls for the available individual-level demographic characteristics, race and gender. If certain demographic groups are more likely to use reusable bags instead of disposable bags, differences in demographics across locations and time periods could bias my results. While minorities and males are more likely to use a disposable bag in general, the estimate of the effect of the tax is unchanged by the inclusion of these controls. Third, while I randomized the

[17] I estimate demand on the extensive margin with a linear probability model. A Probit model yields similar results.

order in which I visited each store, differences in the time of data collection across locations could affect the results. I control for time of day in column 3 and find only the slightest change in the estimates.[18] Finally, the study includes several different chains of grocery stores in various locations throughout the cities considered. While I attempted to choose comparable stores, differences in the location or size of the store, additional store policies about reusable bag use, or neighborhood demographics could affect whether customers choose to use a disposable bag. To account for this possibility, my preferred specification in column 4 includes store fixed effects. As with the other controls, the addition of store-level fixed effects has little impact on the estimated effect of the tax.

Using this preferred specification, Table 1.4 repeats the analysis for the other measures of demand. The table includes measures of demand for both disposable and reusable bags on the extensive and intensive margins, respectively, as well as a binary measure for using no bags of either type. On the extensive margin, the imposition of the tax led to a decrease in disposable bag use of 42.0 percentage points and an increase in reusable bag use of 32.7 percentage points. In addition, the percent of customers who used no bags at all increased by 11.1 percentage points.[19] On the intensive margin, I observe smaller, but still statistically significant, effects on bag consumption – the number of bags used by disposable bag users decreased by 0.22 bags and the number of bags used by reusable bag users increased by 0.15 bags, a change of approximately eight and nine percent, respectively.

In order to provide a measure of the overall effect of the tax on demand, I can combine the extensive and intensive margin estimates following McDonald and Moffitt (1980*b*). In particular, I can decompose the conditional expectation of demand into its extensive and intensive components:

$$E[y|x] = E[y|x, y > 0] * P(y > 0|x),$$

where y represents demand and x represents the covariates. Using the product rule, the total effect

[18]Time of day is broken into three categories: eleven to one thirty ("Morning"), two to four thirty ("Afternoon"), and five to eight ("Evening").

[19]A small fraction of customers used both reusable and disposable bags, which is why the increase in reusable bag use and customers choosing not to use any bags is not completely offset by the decrease in plastic bag use on the extensive margin.

of a change in one of the covariates on demand is given by:

$$\frac{\partial E[y|x]}{\partial x} = \frac{\partial E[y|x, y > 0]}{\partial x} * P(y > 0|x) + \frac{\partial P(y > 0|x)}{\partial x} * E[y|x, y > 0].$$

By utilizing sample estimates of $P(y > 0|x)$ and $E[y|x, y > 0]$, evaluated at the sample mean of each covariate, I can combine the estimated coefficients from the extensive and intensive margin regressions into a rough estimate of the overall effect of the taxes on demand.[20] Table 1.5 presents these results. The estimates suggest that the tax decreased the number of disposable bags used by 1.26 bags and an increased the number of reusable bags used by 0.62 bags per customer per shopping trip.[21]

V. Loss Aversion and Incentive Design

Can a bonus for reusable bag use have the same impact on consumer behavior as a tax on disposable bag use? The previous section documented that Montgomery County's five-cent tax was associated with a substantial reduction in consumers' use of disposable bags. Prior to the implementation of both the D.C. and Montgomery County taxes, several grocery store chains in the Washington Metropolitan Area offered customers a five-cent bonus for each reusable bag they used instead of taking a disposable bag. In this section I use this natural experiment to compare the effect of these two policies to assess the importance of framing when designing financial incentives.

A. Literature Review

For customers shopping in stores that offer a bonus program, the economic incentive to use a reusable versus disposable bag is five cents, the same as under the tax. Consequently, neoclassical models of behavior suggest that these two policies should have the same effect on behavior; the

[20] When calculating standard errors for the aggregate effect, I ignore uncertainty in the sample averages of $P(y > 0|x)$ and $E[y|x, y > 0]$.

[21] The estimates are larger but qualitatively similar when using a Tobit model as opposed to the combined demand model used above.

form that the incentive takes – a bonus versus a tax – should not affect demand. However, work in behavioral economics suggests that this equivalence may not hold in practice. Evidence from both lab and field experiments (Kahneman, Knetsch and Thaler, 1991; DellaVigna, 2009) indicates that individuals perceive losses more strongly than gains of the same size. If grocery store customers are loss-averse, then a policy that charges customers for disposable bag use may be more effective than a policy that rewards customers for using reusable bags, even if the incentives are financially equivalent.

Several recent studies conduct experiments which test the effectiveness of economic incentives with these behavioral insights in mind. New York University's (NYU) School of Law conducted an experiment in which the university randomized the framing of an income-contingent loan repayment program that encourages graduates to enter the public sector. Students who received the tuition subsidy upfront but were told that they would need to repay the amount if they did not enter the public sector upon graduating (the "loss" group) were more likely to take a job in public interest law and more likely to enroll at NYU than students whose loans would be repaid only after entering the public sector (the "gain" group) (Field, 2009). Using a similar experimental design, Hossain and List (2009) altered whether employees in a Chinese manufacturing facility received performance bonuses before production which were then reduced if certain productivity quotas were not met or if they were awarded a bonus only after they reached the quota. They found that employees who received bonuses framed as a loss were more productive than those who received bonuses framed as a gain. Most recently, Fryer et al. (2012) tested the effectiveness of a pay-for-performance program for teachers in Chicago public schools and found that students of teachers in the "loss" treatment showed significant gains in reading and math, while students of teachers in the "gain" treatment did not perform any better than those whose teachers did not receive any financial incentive.

This paper contributes to this growing literature by investigating the impact of incentive-framing and provides new insights on a variety of dimensions. To my knowledge, this is the first paper to determine the existence of these behavioral mechanisms in the context of prosocial environmental behavior. This is also the first study to use taxes as a policy tool to exploit the influence of framing. Lastly, the majority of papers that test for loss aversion in the field provide individuals with rather large incentives. For example, the highest performing teachers in the Chicago schools experiment received an $8,000 bonus, which is roughly equivalent to 16 percent of the average teacher salary in the area. To put that value in context of the incentives examined in this paper, a customer would need to use 438 bags per day for a full year in order for these incentives to be equivalent. This study provides evidence as to whether these behavioral findings hold with low-stakes incentives.

B. The Effect of Taxes versus Bonuses

1. Evidence from Observational Data in the Washington Metropolitan Area

Unlike with the tax policy, I do not have data before and after the implementation of the bonus program; therefore, I cannot perform a difference-in-differences analysis on the effectiveness of the bonus policy as I have with the tax. However, I am able to provide a cross-sectional comparison of the behavior of customers at stores with different policies. Of the twelve stores considered in this analysis, six of them offer a five-cent bonus per reusable bag.[22] Each store falls into one of four policy types. Type I stores provide no incentives for using a reusable bag or reducing use of disposable bags. These are grocery store chains that do not offer a bonus and were not required to charge a tax. Type II stores offer a bonus for reusable bag use, but do not charge a tax for disposable bag use. Type III stores do not offer a bonus, but do charge a tax. Finally, Type IV stores offer both a bonus for reusable bag use and charge a tax for disposable bag use since all of the stores in the sample that provided a bonus prior to the tax continued to provide a bonus after the tax was implemented. Figures 1.3a and 1.3b show the percent of customers using at least one disposable

[22] I exclude four stores from a large organic market chain from this analysis. Since this analysis, unlike that in the previous section, compares store policies across chains, it relies on the comparability of the chains in all aspects other than the store's bag regulation. Reusable bag use in all locations and time periods is slightly higher in these stores than in the non-organic chains, possibly due to the environmentally-conscious reputation of the company. Based on this, I believe that these stores are different enough from the other stores considered to warrant excluding them from the analysis. However, inclusion of these stores leaves the results in this section qualitatively unchanged.

(reusable) bag by policy type with each bar representing a policy-location-period. For example, bonus stores in Montgomery County represent a bar in the Type II category in the pre-period and a bar in the Type IV category in the post-period.

In Figure 1.3a, an average of 84.3 percent of customers use at least one disposable bag in Type I stores, i.e., stores with no incentive policy. This estimate is much higher than that in stores with both a tax and a bonus – only 40.4 percent of customers used a disposable bag in Type IV stores. What is most striking, however, is the comparison of stores that offer only a five-cent incentive but that differ in whether the incentive takes the form of a tax or a bonus. Customers in stores with only a tax used a disposable bag 40.8 percent of the time, similar to customers in stores offering both a tax and a bonus. However, customers in stores that offered only a bonus used a disposable bag 81.9 percent of the time. This estimate is much closer to the percent of customers using a disposable bag in stores that provided no incentive than it is to stores offering an incentive of the *same amount*, but in the form of a tax instead of a bonus.

Figure 1.3b tells a similar story for the proportion of customers using a reusable bag. Customers shopping in stores with both a bonus and a tax used a reusable bag 47.8 percent of the time, which is similar to, though statistically significantly larger than, the 44.2 percent of customers who used a reusable bag in stores that charge a tax but do not provide a bonus. However, only 15.4 percent of customers bring a reusable bag in stores that offer a bonus only. This estimate is much smaller than that in stores that charge a tax, though only slightly larger than the 13.1 percent of customers who shop at stores with no incentive policies.[23]

I then consider a similar analysis using a regression framework with the following econometric model that allows me to control for factors that might confound the simple comparison of means:

$$Y = \theta_0 + \theta_1 Tax + \theta_2 Bonus + \lambda X + \varepsilon.$$

Y is a measure of bag demand, Tax is an indicator for whether a store charges a five-cent tax, $Bonus$ is an indicator for whether the store offers a five-cent bonus for reusable bag use, and X is a set

[23] See Table 1.8 for corresponding standard errors.

of controls including individual-level demographics, time of day, and store location. If I assume that, conditional on these controls, there are no unobservable differences between the customers of bonus and non-bonus stores that would affect their response to the two types of incentives or to their demand in the absence of a bag regulation, I can interpret estimates of θ_1 as the effect of the tax policy and θ_2 as the effect of the bonus policy.

Table 1.6 presents the results for disposable and reusable bag use on the extensive margin. Columns 2 and 4 control for individual demographic characteristics and time of day while columns 1 and 3 do not. As with the evaluation of the tax policy in Table 1.4, men and minority racial groups are more likely to use disposable bags, but the inclusion of these controls does not change the estimates of the effect of the tax or bonus policies. Customers are significantly less likely to use a disposable bag in stores that charge a tax – 44.5 percentage points lower – whereas customers shopping at stores that offer a bonus program do not differ significantly from those shopping at stores without the program. While customers are significantly less likely to use a reusable bag in both tax and bonus stores than in stores that offered no incentive, the magnitude of difference is much larger in tax stores than in bonus stores – 32.7 versus 2.9 percentage points.[24]

As mentioned before, I do not have a natural experiment around the implementation of the bonus so the comparisons in this section should not be interpreted as a causal relationship. If customers choose to shop at the store closest to where they live and stores that offer a bonus program are located in areas where customers are less likely to use a reusable bag regardless of the incentive policy, then the tax and bonus policies could be equally effective and the results in this paper could still be observed. However, given that many of the stores in the sample that offer a bonus program are within a ten minute walk from those that do not, it is unlikely that differences in local demographics are driving the results. Similarly, one might expect that customers who already bring reusable bags might choose to shop at stores that reward them for doing so. However, this pattern would likely bias my approach *against* finding evidence of loss aversion.

[24]In order to test for possible non-linearities in the effect of the incentives, I include a term for the interaction of the two policies. This term is positive and significant, though small in magnitude, for reusable bag use and insignificant for disposable bag use. This suggests that increasing the total economic incentive to ten cents has little effect on behavior, at least when the additional incentive is framed as a bonus.

2. Survey Measure of Policy Effectiveness

To investigate loss aversion without assuming comparability between customers at bonus and non-bonus stores, I surveyed grocery store customers about how they would respond to a hypothetical tax or bonus policy. I asked respondents if a five-cent incentive influenced their decision to bring a reusable bag when shopping at that store, randomizing whether the incentive was framed as a tax or a bonus.[25] Participants were instructed to give one of the following five responses: definitely, quite a bit, somewhat, very little, or not at all. Table 1.7 presents results of the following linear probability model:

$$Y = \theta_0 + \theta_1 Tax + \lambda X + \varepsilon,$$

where Y is the probability that the survey participant responded that the incentive would definitely influence his decision to bring a reusable bag or influence his decision quite a bit (the top two categories), Tax is an indicator variable which takes the value of one if the participant was asked about a tax policy and zero for a bonus policy, and X is a vector of individual demographic characteristics including gender, race, age, education, and income. Customers who were asked about the influence of the bonus program responded 28.1 percent of the time that the policy would definitely influence their decision or influence their decision quite a bit. This average is significantly lower – 31.4 percentage points lower – than the proportion of customers who responded similarly when the incentive was framed as a tax.[26]

C. Estimating the Coefficient of Loss Aversion

Section II described two models with different predictions for the relative effectiveness of the tax and the bonus policies. The neoclassical model predicts that, while the response to either incentive

[25] This question was phrased as a hypothetical for customers in stores that did not already have the policy or for customers who were previously unaware of the existence of the policy.

[26] The results are qualitatively similar when the dependent variable is the probability that the survey participant responded that the incentive would definitely influence his decision to bring a reusable bag only or when using an ordered probit.

depends on the change in utility due to the incentive relative to the cost of bringing a reusable bag, the response to the two types of incentives should be the same as long as the incentives are small. In contrast, the reference-dependent preferences model predicts that a tax should have a larger effect than a bonus of the same size. The empirical analysis shown in the previous section suggests that customers were much more likely to use a reusable bag when the incentive was framed as a tax rather than a bonus, evidence that is consistent with a model of loss aversion rather than the neoclassical model.

In the reference-dependent utility function used in Section II.B, α is the slope of the utility function for wealth levels above the reference point (w^*) relative to the slope below the reference point, i.e., the sharpness of the kink in the utility function at w^*. This parameter is often referred to as the "coefficient of loss aversion" (Wakker and Tversky, 1993). Several papers have estimated the coefficient of loss aversion using lab experiments and find $\alpha \approx 2$. In this section, I provide an estimate of α using my observational data.

The table below repeats the conditions required for a customer to choose to bring a reusable bag under the three policies assuming reference-dependent preferences from Section II.B. If F is the distribution of c_i, the proportion of customers bringing a reusable bag when there is no incentive, when there is a tax, and when there is a bonus are $F(0)$, $F(\gamma\alpha x)$, and $F(\gamma x)$, respectively. Recall that we observe these proportions in the data in the previous section. Therefore, if I make an assumption about the distribution of c_i, I can estimate the coefficient of loss aversion.

	Utility Function	Condition to Bring a Bag	% Bringing a Bag	% Bringing a Bag (from Data)
No Incentive	$U_{N,i}(w^*,b_i)=\begin{cases}-c_i & if\, b_i=1\\ 0 & if\, b_i=0\end{cases}$	$0>c_i$	$F(0)$	13.1
Tax Policy	$U_{T,i}(w^*,b_i)=\begin{cases}-c_i & if\, b_i=1\\ -\gamma\alpha x & if\, b_i=0\end{cases}$	$\gamma\alpha x>c_i$	$F(\gamma\alpha x)$	44.2
Bonus Policy	$U_{B,i}(w^*,b_i)=\begin{cases}\gamma x-c_i & if\, b_i=1\\ 0 & if\, b_i=0\end{cases}$	$\gamma x>c_i$	$F(\gamma x)$	15.4

If I take a first-order Taylor approximation of $F(\gamma\alpha x)$, the proportion of customers bringing a reusable bag under the tax policy around zero yields the equation $F(\gamma\alpha x) \approx F(0) + \gamma\alpha x f(0)$. Similarly, I can approximate the proportion of customers bringing a reusable bag under the bonus policy as $F(\gamma x) \approx F(0) + \gamma x f(0)$. From these two equations, α is equivalent to the ratio of the increase in reusable bag usage under the tax policy to the increase in reusable bag usage under the bonus policy: $\alpha \approx \frac{F(\gamma\alpha x) - F(0)}{F(\gamma x) - F(0)}$. Therefore, if I assume that the first-order approximation is exact (for example, if c_i is locally uniformly distributed) then $\alpha = 13.9$. However, if it is the case that f' is large on the interval between zero and x, a first-order approximation will not be good estimate. As a robustness check, I assume that c_i is normally distributed with a mean of $-\Phi^{-1}(.131)$ and a variance of one and find an estimate of $\alpha = 9.5$.

These estimates of α are considerably larger than previous estimates from the literature. Why might this data imply large values of α? The majority of the literature that estimates the coefficient of loss aversion does so using outcomes that are much larger than five cents. Kahneman and Tversky (1979) propose that the value function is generally concave for gains and convex for losses (i.e., S-shaped) and is steeper for losses than for gains. Loewenstein and Prelec (1992) extend this model to account for various discounted utility anomalies. They suggest that the value function is more elastic for outcomes that are larger in absolute magnitude, meaning that, for small outcomes, the value function is steep, but, for large outcomes, it straightens out. Therefore, if previous studies calculate α on the flatter portion of the value function and this study calculates α directly around the reference point, it may not be surprising that this data estimates an α larger than two. Experimental literature supports the idea that the gain-loss asymmetry is larger for small outcomes than for large outcomes (Thaler, 1981; Benzion, Rapoport and Yagil, 1989). Figure 1.4a presents a value function that satisfies these properties.

An alternative model of reference-dependent preferences assumes that, for certain reference points, there is a discontinuous jump in utility at the reference point rather than a kink. Shampanier, Mazar and Ariely (2007) present a model of this kind suggesting that the benefits derived from receiving a free product are larger than the simple reduction in price. For example, individuals may receive higher intrinsic benefit from receiving free goods or, alternatively, may experience lower costs from not having to pay for a non-free good. This theory implies that, if a disposable bag is a typical consumption good, a customer's utility should decrease discretely when the store policy shifts from offering no incentive to charging a tax (i.e., when the good is no longer free) by some amount δ:

$$u(w_i) = \begin{cases} \gamma w_i & if\, w_i \geq w^* \\ \gamma w_i - \delta & if\, w_i < w^* \end{cases}, \text{where } \delta > 0.$$

Figure 1.4b presents a value function with this form. My data does not allow me to distinguish between these two possible models of reference-dependent preferences; however, previous evidence that suggests that zero is a special price may shed some light on why I estimate such a large coefficient of loss aversion. Suppose that prior to the implementation of any incentive policy, grocery stores charged customers ten cents per disposable bag. Since neither a five-cent tax nor a five-cent bonus would cause disposable bags to be free, perhaps we would not observe such a dramatic difference in response to the two types of incentives.

D. Alternative Mechanisms

This paper provides evidence of the relative effectiveness of the two policies that is consistent with a model in which individuals are loss-averse, causing them to respond to the tax but not to the bonus policy. However, loss aversion is not the only possible explanation for the observed difference in behavior across stores with different policies. This section investigates other potential theories or mechanisms that might explain the results described above.

1. Marketing and Awareness

One reason the tax may have been more effective at changing customer behavior is that consumers were more aware of the tax than the bonus. The tax was highly visible in several dimensions. First, both D.C. and Montgomery County conducted a campaign that informed residents of the impending tax. Second, stores in the sample posted announcements by the register detailing the rules involved with the new law. Third, the tax was covered widely in the press in the weeks leading up to its implementation. While stores that offered a bonus advertised the policy through announcements posted at the register and on the racks where reusable bags were sold, the additional marketing involved with the implementation of the tax may have generated a difference in awareness of the two policies.

To investigate possible discrepancies in awareness, I surveyed customers at the sample stores about their knowledge of the store's tax and bonus policies. While almost all customers (98 percent) were aware of the tax, only 52 percent of customers in stores that offered a bonus were aware of that program.

To determine whether these differences in awareness could generate the observed difference in demand across stores with different policies, I develop the following model. The previous analysis tested the null hypothesis that demand in stores that charge a tax was equal to demand in stores that offered a bonus of the same amount:

$$H_0 : P(Y|NB,T) = P(Y|B,NT)$$

where Y is a measure of bag demand, B and NB indicate the presence and absence of a bonus program, respectively, and T and NT indicate the respective presence and absence of a tax.

Using language borrowed from the literature on local average treatment effects, I define three types of consumers. "Always Takers" are customers who would use a reusable bag (or not take a disposable bag) regardless of whether the store offers an incentive. "Never Takers" are customers who do not use a reusable bag even if the store provides an incentive. Lastly, "Compliers" are customers who bring a reusable bag only if the store offers an incentive to do so.

Using these terms, I can reinterpret the components of the null hypothesis. In stores with a tax policy, both the always takers and the tax policy compliers will bring a reusable bag so $P(Y|NB,T) = P(Always_T) + P(Complier_T)$. Similarly, $P(Y|B,NT) = P(Always_B) + P(Complier_B)$.

Since always takers bring a reusable bag regardless of the store policy and I am assuming that customers in the two types of stores are equivalent, $P(Always_T) = P(Always_B) = P(Always)$. In terms of measures defined in the data, $P(Always)$ is equivalent to $P(Y|NB,NT)$. Using these definitions, I can redefine the null hypothesis as:

$$H_0 : P(Complier_T) = P(Complier_B).$$

That is, the null hypothesis states that the fraction of customers who are compliers with respect to a tax is equal to the fraction of customers who are compliers with respect to a bonus.

Now suppose that not all customers are aware of a store's policy. As seen with the survey data, this is the case for the bonus policy, but not the tax policy. Since always takers will bring a reusable bag regardless of the store policy, it does not matter whether these customers are aware of the bonus. In contrast, only compliers who are *aware* of the policy will bring their own bags in stores that offer a tax or bonus. In particular, $P(Y|B,NT) = P(Always) + P(Complier_B) * P(Aware_B|Complier_B)$, where $P(Aware_B|Complier_B)$ is the probability that a customer is aware of the bonus program given that he is a bonus complier. So unlike with the tax policy, the effect of the bonus policy may be muted due to under-awareness.

Adjusting for awareness of the bonus policy, a little bit of algebra yields the following null hypothesis:

$$H_0 : P(Y|NB,T) - P(Y|NB,NT) = \frac{P(Y|B,NT) - P(Y|NB,NT)}{P(Aware_B|Complier_B)}$$

While I observe the majority of the components in the equation above in the data, I do not have a measure of awareness of the bonus among compliers since I cannot identify who is a bonus complier in the survey data. Customers who use a reusable bag in bonus stores are either bonus compliers who were aware of the bonus or always takers. Similarly, customers who do not use a reusable bag in bonus stores are either bonus compliers who were unaware of the bonus or never takers. However, I can provide plausible bounds on the awareness of bonus policy among bonus compliers using estimates from the survey data. This allows me to determine if my results may simply be driven by the fact that more customers are aware of the tax than the bonus.

Table 1.8 presents these results. Estimates in each column assume that 100 percent of customers are aware of the tax policy.[27] In contrast, each column assumes a different value of the awareness of the bonus program among bonus compliers. Case I assumes complete awareness of the bonus policy, a lower bound on the effectiveness of the bonus policy. Case II assumes that the percent of compliers who are aware of the bonus program is equivalent to that of all survey participants shopping in stores with a bonus program, regardless of whether they used a reusable or a disposable bag – 52.0 percent. Lastly, Case III assumes that compliers have an equivalent awareness to that of survey participants who did not use a reusable bag on the day of the survey – 38.0 percent. As mentioned above, this group contains a combination of bonus compliers who were unaware of the bonus and never takers. If I assume that awareness among the never takers is no larger than the awareness of bonus compliers, this estimate is an upper bound for the effectiveness of the bonus.

Recall that 84.3 percent of customers used a disposable bag in stores with no incentive policy, 81.9 percent in stores with only a bonus program, and 40.8 percent in stores with only a tax policy (see Panel A of Table 1.8). Panel B of Table 1.8 presents estimates of the effect of the two policies after adjusting for awareness. In all cases, the estimate of the effect of the tax policy ($P(Complier_T)$) is equivalent to the difference in behavior between customers at stores with a tax policy and stores that offer no incentive to bring a reusable bag ($P(Y|NB,T) - P(Y|NB,NT)$) – a decrease of 43.5 percentage points. Similarly, Case I assumes that compliers are completely aware of the bonus policy so the estimate of the effect of the bonus policy ($P(Complier_B)$) is equivalent to the difference in behavior between customers at stores with a bonus policy and stores that offer no incentive to bring a reusable bag ($P(Y|B,NT) - P(Y|NB,NT)$) which is 2.4 percentage points. In contrast, Case II and III incorporate the possibility for less-than-perfect awareness of the bonus policy and the estimate of the effect of the bonus policy becomes $\frac{P(Y|B,NT) - P(Y|NB,NT)}{P(Aware_B|Complier_B)}$. In Case II, 4.6 percent of customers did not use a disposable bag as a result of the bonus program. In Case

[27]While the survey data shows that awareness of the tax policy is slightly less than perfect, I assume 100 percent awareness of the tax in order to provide the most conservative estimates.

III, the upper bound of the effect of the bonus, this estimate increases only slightly to 6.3 percent which is seven times smaller than the estimated effect of the tax. In fact, in order for the effect of the bonus to be as large as the effect of the tax, it would require that only 5.5 percent of bonus compliers were aware of the bonus, which is unlikely given the survey estimates of awareness. Results for the percent of customers using a reusable bag are presented in Columns 4 through 6 and tell a qualitatively similar story. So while differences in awareness may affect the observed impact of the two different policies, it is unlikely that increasing awareness of the bonus policy could account for all of the differential response to the tax and the bonus.

I can also use this adjustment procedure to reevaluate the estimates of the coefficient of loss aversion in Section V.C. Using the lower bound awareness estimates from Case III, I estimate an α of 3.9 if I assume that c_i is normally distributed with a variance of one and an α of 5.1 if c_i is uniformly distributed. These estimates are still large, but are much closer to the estimates previously found in the literature.

2. Changing Social Norms

Many legal theorists have investigated the "expressive function of law," the idea that a law has an effect on behavior independent of the sanction. For example, the law may shift individual preferences by making a statement about what behavior warrants punishment. Funk (2007) shows that voter turnout in Switzerland decreased significantly after a mandatory voting law with negligible penalties (less than one dollar) was repealed. Galbiati and Vertova (2008) conduct an experiment in which participants play a public goods game that requires players to contribute a minimum amount or pay a small fine for refusing and finds that this "obligation" increases contributions even when the optimal strategy is to free-ride.

This theory would suggest that a small tax on disposable bags may have a larger effect than a bonus of a similar size because the passage of the policy changes social norms about bag consumption. It is difficult to rule out the hypothesis that the tax caused a shift in preferences; however, this section provides some evidence that the law may not have had a large impact on social norms.

Recall that the main analysis of the effectiveness of the tax focuses on the implementation of the Montgomery County tax. However, this was not the first tax of its kind in the Washington Metropolitan Area – D.C. passed a similar tax two years prior. Given that the sample draws from stores in areas that are close to D.C., it is likely that many of the customers in the sample had been exposed to the D.C. bag tax prior to the implementation of the Montgomery County tax. Results from the in-store survey show that 73.7 percent of respondents in Virginia and 83.7 percent of Montgomery County respondents were aware of the D.C. tax.[28] In addition, 50.3 percent of respondents in Montgomery County were aware that the Montgomery County law had been approved in the pre-period survey and that they would soon be charged five cents for each disposable bag. This suggests that if individuals adjust their behavior simply due to the moral statement made by the announcement of the law, these customers should have already changed their behavior before the beginning of the sample period; however, I still observe a large change in behavior after the implementation of the Montgomery County tax.

Additionally, I collected survey data at seven grocery stores before and after the implementation of the Montgomery County bag tax that included questions aimed at measuring social norms about the use of disposable bags. I use the same difference-in-differences strategy as described in Section IV.C, controlling for gender, race, age, education, and income. Customers were asked if they felt guilty when they used a disposable bag ("Guilt"), felt social pressure to use fewer disposable bags ("Pressure"), got upset when they saw other customers use too many disposable bags ("Upset"), thought the number of disposable bags they used was wasteful ("Wasteful"), and whether they would support a law that required stores to tax customers five cents for each disposable bag ("Support"). If the implementation of the tax were to cause a shift in social norms, the results should show positive and significant estimates of the coefficient on $Post * MD$, the difference-in-difference estimator, for each of these measures. Table 1.9 presents the results of this analysis. I do not find that any of these measures of social norms significantly change as a result of the implementation of

[28]This question was only asked in the post-period. While this should not affect the validity of the responses from Virginia, the Montgomery County results may be biased upward since they may have learned about the D.C. tax only after the implementation of the Montgomery County tax.

the tax. While the standard errors are rather large, the sign of the various measures are not all in the same direction – for example, the percent of customers reporting that they felt guilty when using a plastic bag increased after the implementation of the tax, while the percent reporting that they felt social pressure to use fewer plastic bags decreased. These results are by no means conclusive, but they do not provide any evidence that the law changed customers' social norms regarding the use of disposable bags.

3. Tax Aversion

Another potential explanation for why more customers use reusable bags in stores with a five-cent penalty than in stores with a five-cent bonus may be that the penalty takes the form of a tax rather than a fee. Recent work by Sussman and Olivola (2011) present evidence that consumers are "tax averse," in that they are more likely to avoid taxes than other costs of the same amount. This model would suggest that customers respond more strongly to the tax simply because it is a tax, and not because it is framed as a loss rather than a gain.

I am not aware of any existing policies that charge a fee for disposable bag use that is not framed as a tax, so I am not able to exploit policy variation of this kind in the field. Instead, I use an online survey to run a randomized experiment to test for tax aversion in this context. The survey questions mirror the questions asked in the in-store survey described in Section V.B, but instead of asking customers about their perceived response to a bonus versus a tax, they are asked how they believe they would respond to a store-imposed fee versus a government-imposed tax. I use the same specifications and controls as in Section V.B and present results in Table 1.10. I observe no difference in the likelihood of reporting that the tax would influence whether a customer brought a reusable bag compared to a fee of the same amount.

VI. Conclusion

This paper investigates the impact of a new “eco-sin” tax, a five-cent tax on disposable bags. I find that the tax policy reduced the overall demand for disposable bags by over half and prompted consumers to substitute to reusable alternatives; this is particularly notable given the relatively small size of the tax itself. The large effect of the tax is also striking in light of the similarity between reusable bag use at stores offering a five-cent bonus and stores that offered no financial incentive in the period before the tax was imposed, a result that is consistent with a model in which customers are loss-averse. I show that differences in awareness of the two policies and changes in social norms cannot fully account for my results.

These findings suggests the importance of accounting for behavioral insights when designing a wide variety of environmental incentives. For example, Starbucks Coffee rewards customers who bring their own coffee mugs with a ten-cent discount. My results suggest that this policy might be more effective if Starbucks instead reduced the price of coffee by ten cents, but charged for using a paper cup. Similarly, the federal government awards a tax credit to customers who purchase environmentally-friendly Energy Star products. This policy might increase consumption of these products if they were taxed for purchasing energy-inefficient products.

It is interesting to note that the effect of this tax is not only large in absolute terms, but also in comparison to previous estimates of the impact of other types of sin taxes. There are several possible explanations for this discrepancy. First, the elasticity of demand for disposable bags may be substantially greater than the elasticity of demand for other goods. Second, the visibility of the bag tax, which is prominently displayed at grocery store registers, may help explain why it has had a larger effect than other taxes, which tend to be less salient (Goldin, 2012*a*). Third, the large change in demand for disposable bags following the tax may stem from levying a price on a good that had previously been free (Shampanier, Mazar and Ariely, 2007). Finally, even a small initial impact of the tax can generate large effects if the reputational costs of using disposable bags increases by way of a social multiplier (Benabou and Tirole, 2011).

Table 1.1: Demographics

	D.C.		Maryland		Virginia	
	Pre	Post	Pre	Post	Pre	Post
	(1)	(2)	(3)	(4)	(5)	(6)
Female	58.5	59.7	59.8	61.2	53.1	56.9
	(49.3)	(49.1)	(49.0)	(48.7)	(49.9)	(49.5)
White	63.8	63.3	59.3	59.7	77.8	76.6
	(48.1)	(48.2)	(49.1)	(49.1)	(41.6)	(42.4)
Black	23.3	22.0	27.9	26.3	10.1	9.7
	(42.3)	(41.4)	(44.9)	(44.0)	(30.2)	(29.6)
N	1,207	1,649	3,799	4,515	2,006	3,075

Standard deviations in parentheses.
Table reports mean values of each variable.

Table 1.2: Demand Before and After the Montgomery County Bag Tax

	D.C.		Maryland		Virginia	
	Pre	Post	Pre	Post	Pre	Post
	(1)	(2)	(3)	(4)	(5)	(6)
Extensive Margin						
Disposable	44.5	45.7	81.7	39.6	82.2	80.8
	(49.7)	(49.8)	(38.6)	(48.9)	(38.3)	(39.4)
Reusable	46.0	46.6	15.9	49.2	16.3	17.2
	(49.9)	(49.9)	(36.5)	(50.0)	(36.9)	(37.7)
No Bags	14.9	11.3	5.7	15.4	4.7	4.8
	(35.6)	(31.7)	(23.2)	(36.1)	(21.1)	(21.5)
Intensive Margin						
Disposable	2.23	1.76	2.32	1.76	2.37	2.14
	(2.17)	(1.43)	(2.05)	(1.43)	(2.02)	(1.82)
Reusable	1.63	1.52	1.67	1.66	1.79	1.65
	(1.07)	(0.95)	(1.14)	(1.09)	(1.27)	(1.15)
Overall Demand						
Disposable	1.00	0.81	1.90	0.70	1.95	1.73
	(1.82)	(1.31)	(2.06)	(1.25)	(2.04)	(1.84)
Reusable	0.75	0.71	0.26	0.82	0.29	0.28
	(1.09)	(1.00)	(0.76)	(1.13)	(0.84)	(0.78)
N	1,207	1,649	3,799	4,515	2,006	3,075

Standard deviations in parentheses.
Table reports the probability of using a bag (extensive), demand among users (intensive), and unconditional demand (overall) for each type of bag.

Table 1.3: Effect of Tax Policy on Disposable Bags - Extensive Margin

	(1)	(2)	(3)	(4)
Post*MD	-0.417***	-0.417***	-0.419***	-0.420***
	(0.014)	(0.014)	(0.014)	(0.014)
Post	-0.005	-0.003	-0.002	-0.002
	(0.010)	(0.010)	(0.010)	(0.010)
MD	0.001	-0.013	-0.009	
	(0.010)	(0.010)	(0.010)	
DC	-0.362***	-0.372***	-0.372***	
	(0.011)	(0.011)	(0.011)	
Black		0.100***	0.100***	0.099***
		(0.009)	(0.009)	(0.009)
Other Race		0.025**	0.025**	0.025**
		(0.010)	(0.010)	(0.010)
Female		-0.068***	-0.067***	-0.066***
		(0.007)	(0.007)	(0.007)
Afternoon			0.005	0.003
			(0.008)	(0.008)
Evening			0.027***	0.026***
			(0.008)	(0.009)
Store FE	No	No	No	Yes
N	16,251	16,251	16,251	16,251

Robust standard errors in parentheses.

Outcome variable: probability of using at least one disposable bag.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Effect of Tax Policy on Demand - Extensive and Intensive Margins

	Extensive Margin			Intensive Margin	
	(1)	(2)	(3)	(4)	(5)
	Disposable	Reusable	No Bags	Disposable	Reusable
Post*MD	-0.420***	0.327***	0.111***	-0.215***	0.150**
	(0.014)	(0.013)	(0.009)	(0.070)	(0.069)
Post	-0.002	-0.002	-0.006	-0.227***	-0.116**
	(0.010)	(0.010)	(0.006)	(0.051)	(0.047)
Black	0.099***	-0.102***	-0.001	-0.153***	-0.185***
	(0.009)	(0.009)	(0.006)	(0.046)	(0.039)
Other Race	0.025**	-0.057***	0.022***	-0.133***	-0.217***
	(0.010)	(0.010)	(0.007)	(0.051)	(0.043)
Female	-0.066***	0.153***	-0.061***	0.381***	0.203***
	(0.007)	(0.007)	(0.005)	(0.035)	(0.031)
Afternoon	0.003	0.031***	-0.024***	0.265***	0.026
	(0.008)	(0.008)	(0.006)	(0.043)	(0.038)
Evening	0.026***	0.009	-0.032***	0.270***	-0.062*
	(0.009)	(0.008)	(0.006)	(0.043)	(0.037)
Store FE	Yes	Yes	Yes	Yes	Yes
N	16,251	16,251	16,251	10,314	5,003

Robust standard errors in parentheses.

Outcome variables: probability of using at least one bag or no bags (extensive) and demand among users (intensive) for disposable and reusable bag demand, respectively.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.5: Effect of Tax Policy on Demand - Overall Effect

	(1) Disposable	(2) Reusable
Post*MD	-1.260***	0.622***
	0.057	0.033
Post	-0.143***	-0.037*
	0.039	0.022
Black	0.077**	-0.253***
	0.035	0.020
Other Race	-0.081**	-0.191***
	0.039	0.021
Female	-0.025	0.307***
	0.029	0.016
Afternoon	0.059*	0.032*
	0.035	0.019
Night	0.129***	-0.032*
	0.034	0.019
Store FE	Yes	Yes
N	16,251	16,251

Robust standard errors in parentheses.

Outcome variables: bag demand in levels.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.6: Effect of Tax vs. Bonus Policy on Demand - Extensive Margin

	Disposable		Reusable	
	(1)	(2)	(3)	(4)
Tax	-0.445***	-0.445***	0.329***	0.327***
	(0.011)	(0.011)	(0.011)	(0.011)
Bonus	-0.009	-0.013	0.026***	0.029***
	(0.008)	(0.008)	(0.008)	(0.008)
MD	-0.003	-0.015	0.001	0.014
	(0.009)	(0.010)	(0.009)	(0.009)
DC	0.057***	0.041**	-0.027	-0.008
	(0.017)	(0.017)	(0.017)	(0.017)
Black		0.102***		-0.102***
		(0.010)		(0.010)
Other Race		0.027**		-0.064***
		(0.011)		(0.011)
Female		-0.055***		0.150***
		(0.008)		(0.008)
Afternoon		0.013		0.033***
		(0.010)		(0.009)
Evening		0.032***		0.011
		(0.010)		(0.009)
F-stat	949.19	946.44	465.23	471.99
prob>F	0.00	0.00	0.00	0.00
N	11,678	11,678	11,678	11,678

Robust standard errors in parentheses.

Outcome variable: probability of using at least one disposable (reusable) bag.

Tax is a binary variable with a value of one if the store charges a five-cent tax per disposable bag. *Bonus* is a binary variable with a value of one if the store offers a five-cent bonus per reusable bag.

The F-stat is associated with the test of equality between the tax and bonus coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7: Effect of Tax vs. Bonus Policy on Demand - Survey Measure of Influence

	(1)
Tax (vs. Bonus)	0.293***
	(0.025)
White	-0.104***
	(0.028)
Female	0.053**
	(0.026)
Age	-0.004
	(0.005)
Age Squared	0.000
	(0.000)
>=High School	0.042
	(0.034)
Income<$50k	0.025
	(0.032)
N	1,279

Robust standard errors in parentheses.
Outcome variable: probability respondent answered "definitely" or "quite a bit" when asked if the five-cent incentive influenced his decision to bring a reusable bag. *Tax* is a binary variable equal to one if the incentive was framed as a tax and zero if it was framed as a bonus.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8: Effect of Tax vs. Bonus Policy on Extensive Margin Demand - Awareness Adjustment

Panel A	Disposable	Reusable
	(1)	(2)
Demand Under Different Policies		
No Incentive ($P(Y\|NB,NT)$)	0.843	0.131
	(0.007)	(0.007)
Tax Policy ($P(Y\|NB,T)$)	0.408	0.442
	(0.010)	(0.010)
Bonus Policy ($P(Y\|B,NT)$)	0.819	0.154
	(0.006)	(0.006)

Panel B	Disposable			Reusable		
	Case I	Case II	Case III	Case I	Case II	Case III
	(1)	(2)	(3)	(4)	(5)	(6)
Awareness Among Compliers ($P(Aware\|Complier)$)						
Tax Policy	1.000	1.000	1.000	1.000	1.000	1.000
Bonus Policy	1.000	0.520	0.380	1.000	0.520	0.380
Effect of Policy Adjusted for Awareness ($P(Complier)$)						
Tax Policy	-0.435	-0.435	-0.435	0.311	0.311	0.311
Bonus Policy	-0.024	-0.046	-0.063	0.023	0.044	0.061

Robust standard errors in parentheses in Panel A.

Outcome variable in Panel A: probability of using at least one disposable (reusable) bag in percent.

The effect of policy i, $P(Complier_i)$, is equivalent to $[P(Y|i,Nj) - P(Y|Ni,Nj)]/P(Aware_i|Complier_i)$ for i in $\{Tax, Bonus\}$ and j in $\{Bonus, Tax\}$.

Table 1.9: Change in Social Norms after Implementation of Tax Policy

	(1) Guilt	(2) Pressure	(3) Upset	(4) Wasteful	(5) Support
Post*MD	0.072	-0.059	0.027	-0.103	0.040
	(0.073)	(0.073)	(0.063)	(0.074)	(0.068)
Post	-0.036	0.044	0.052	-0.133**	-0.042
	(0.056)	(0.056)	(0.048)	(0.057)	(0.050)
MD	-0.074	0.087	0.006	0.003	0.019
	(0.069)	(0.069)	(0.055)	(0.071)	(0.067)
DC	0.087	0.113**	0.023	-0.088	0.120**
	(0.055)	(0.056)	(0.048)	(0.056)	(0.052)
Female	0.220***	0.102***	0.108***	0.032	0.169***
	(0.035)	(0.035)	(0.031)	(0.036)	(0.035)
White	0.068*	0.064	-0.033	-0.008	0.063
	(0.040)	(0.040)	(0.036)	(0.041)	(0.041)
Age	0.008	0.007	-0.007	0.002	-0.006
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Age Squared	-0.000**	-0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
>=High School	-0.086*	0.004	0.007	-0.046	-0.066
	(0.044)	(0.044)	(0.042)	(0.046)	(0.048)
Income<$50k	-0.040	-0.008	0.023	-0.090**	-0.058
	(0.043)	(0.042)	(0.039)	(0.043)	(0.045)
N	743	742	742	742	685

Robust standard errors in parentheses.

Outcome variable: probability of responding affirmatively to the social norms survey question.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

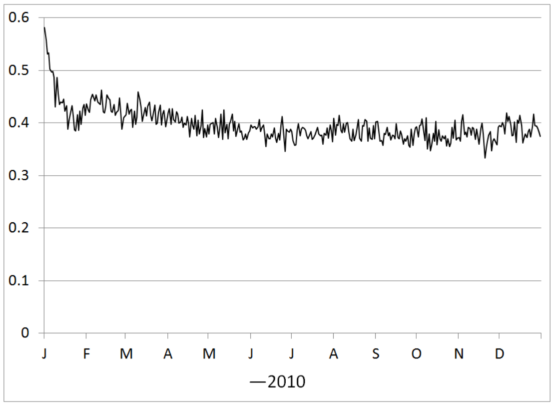
Table 1.10: Effect of Tax vs. Fee on Demand - Survey Measure of Influence

	(1)
Tax (vs. Fee)	0.025
	(0.082)
White	0.080
	(0.097)
Female	0.183**
	(0.083)
Age	-0.029
	(0.027)
Age Squared	0.000
	(0.000)
>=High School	0.032
	(0.090)
Income<$50k	0.034
	(0.083)
N	147

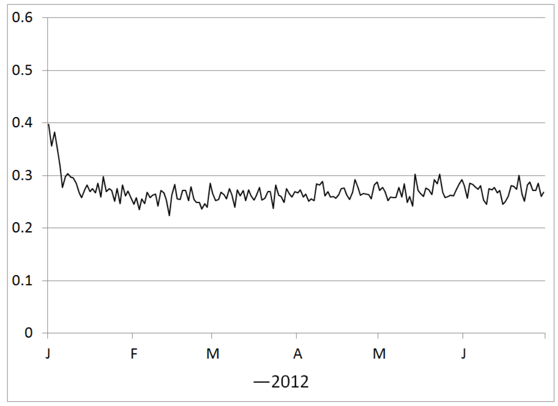
Robust standard errors in parentheses.
Outcome variable: probability respondent answered "definitely" or "quite a bit" when asked if the five-cent incentive influenced his decision to bring a reusable bag. *Tax* is a binary variable equal to one if the incentive was framed as a tax and zero if it was framed as a fee.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1.1: Proportion of Customers Using a Disposable Bag

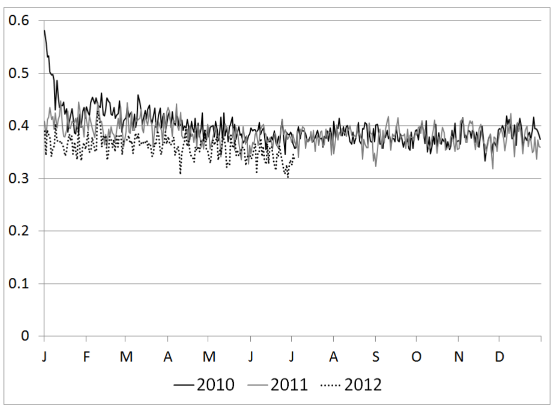
(a) Washington D.C., 2010



(b) Montgomery County, 2012



(c) Washington D.C., 2010-2012



(d) California, September 2011 - June 2012

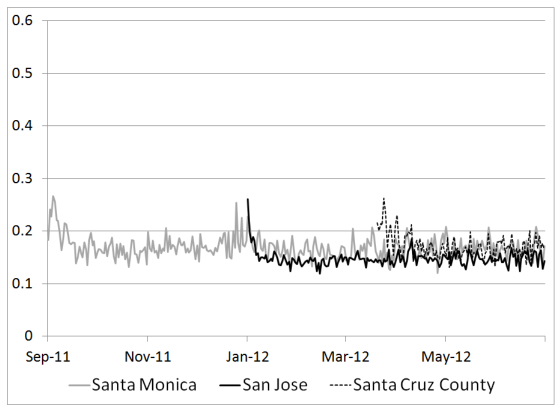
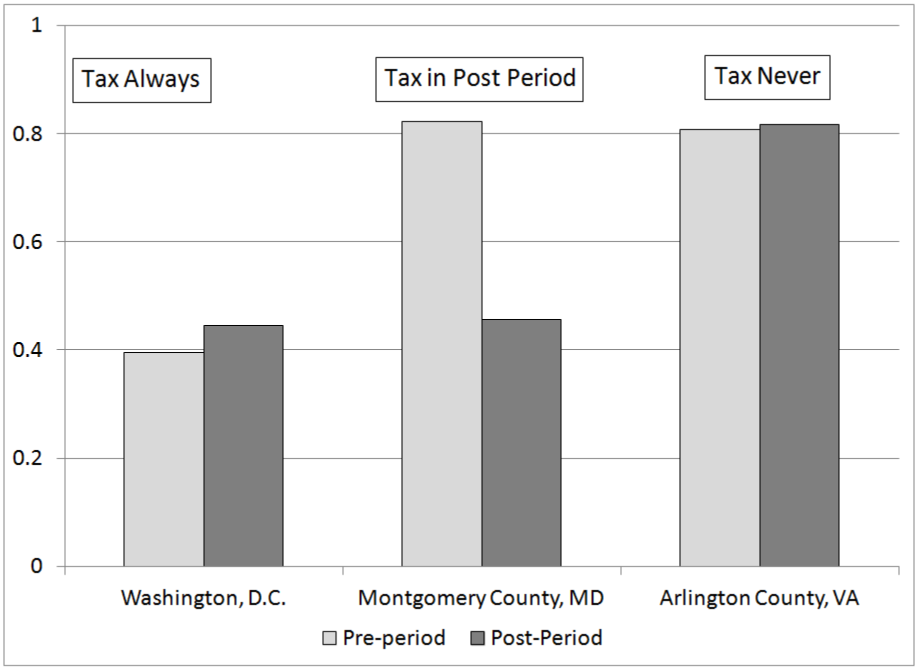


Figure 1.2: Demand by Location and Time Period

(a) Proportion of Customers Using a Disposable Bag



(b) Proportion of Customers Using a Reusable Bag

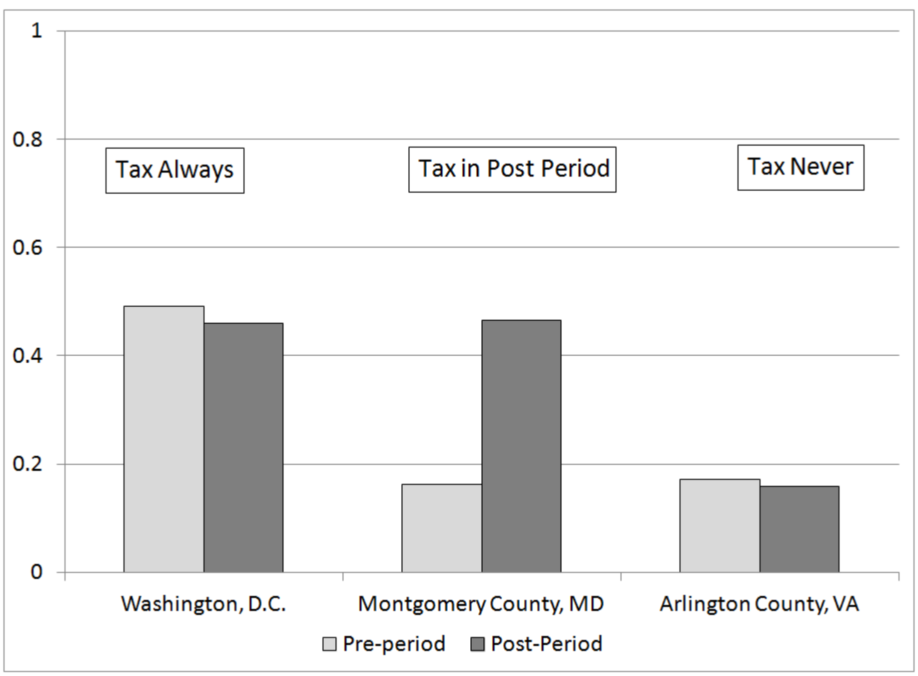
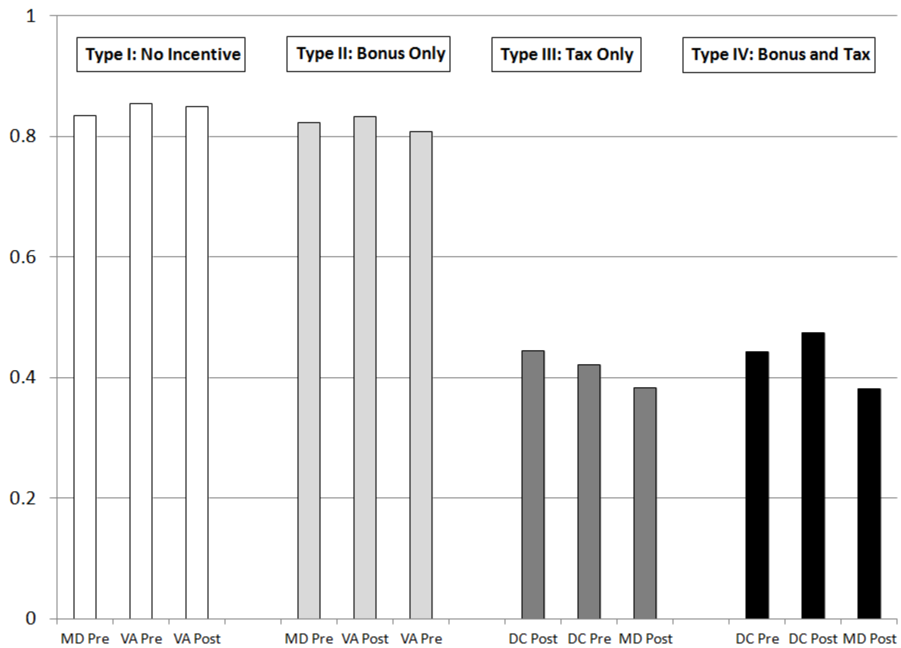


Figure 1.3: Demand by Store Policy

(a) Proportion of Customers Using a Disposable Bag



(b) Proportion of Customers Using a Reusable Bag

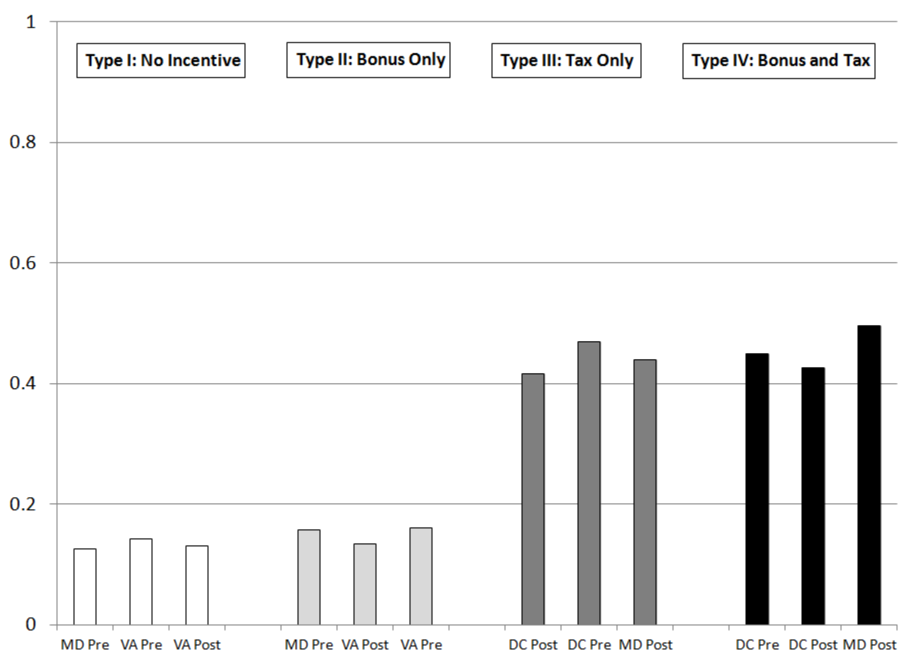
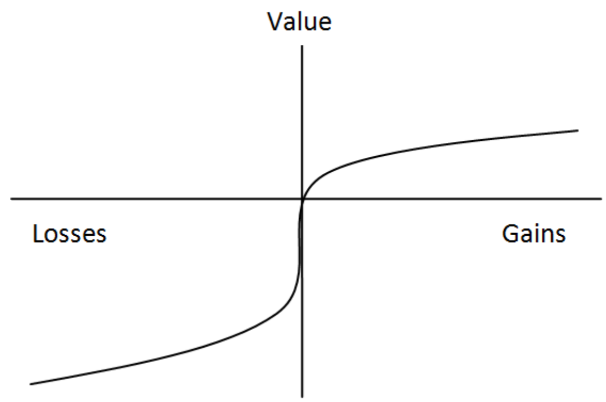
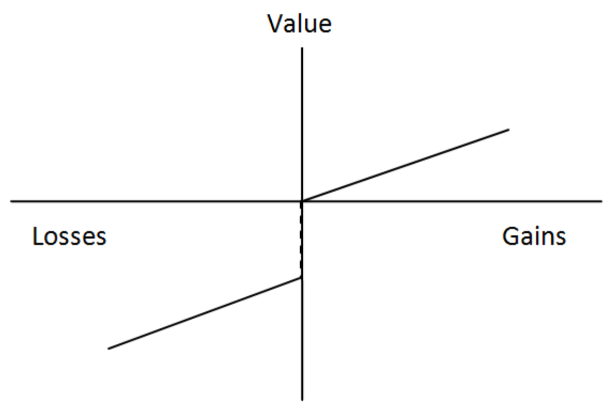


Figure 1.4: Value Functions

(a) S-Shaped



(b) Discontinuous Jump



Chapter 2

Smoke Gets in Your Eyes: Cigarette Tax Salience and Regressivity

(with Jacob Goldin)

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Abstract

Recent evidence suggests consumers pay less attention to commodity taxes levied at the register than to taxes included in a good's posted price. If this attention gap is larger for high-income consumers than for low-income consumers, policymakers can manipulate a tax's regressivity by altering the fraction of the tax imposed at the register. We investigate income differences in attentiveness to cigarette taxes, exploiting state and time variation in cigarette excise and sales tax rates. Whereas all consumers respond to taxes that appear in cigarettes' posted price, our results suggest that only low-income consumers respond to taxes levied at the register.

Should governments levy commodity taxes at the register or include them in a good's posted price? Traditional approaches to the economics of taxation offer little guidance to policymakers choosing between the two tax types. Indeed, neoclassical theory suggests that this aspect of tax design – the choice between "posted" and "register" taxes – does not affect consumer welfare because consumers correctly compute and account for all taxes that will be assessed on a given transaction. However, a series of recent findings call that invariance prediction into doubt. For example, Chetty, Looney and Kroft (2009) (CLK) present compelling evidence that consumers pay more attention to goods' posted prices than to register taxes because the former are more salient – consumers see the posted tax-inclusive price when making their purchasing decisions. Related empirical findings by Finkelstein (2009) and Cabral and Hoxby (2010) are also consistent with the hypothesis that the salience of a tax shapes the extent to which consumers perceive it. This line of research suggests that the policy choice between posted and register taxes may not be as irrelevant as neoclassical theory predicts.

This paper investigates the distributional effects of the government's choice between posted and register taxes. Part I considers the case in which consumers differ in their attentiveness to register taxes – that is, when only some consumers take register taxes into account when making purchasing decisions. Drawing on a stylized model of consumer behavior, we show how a revenue-neutral shift from posted to register taxes reduces the tax burden on attentive consumers, unambiguously improving the welfare of that group.

We then turn to a practical implication of this insight. A concern with many commodity taxes is that they are regressive – they constitute a proportionately greater burden for low-income taxpayers. However, if low-income consumers pay more attention to register taxes than high-income consumers do, policymakers can reduce a tax's regressivity by adding it at the register instead of including it in the commodity's posted price. Conversely, when low-income consumers are relatively less attentive to register taxes, reducing a tax's salience will exacerbate its regressivity. Hence, knowing how consumers' attentiveness to register taxes varies by income is essential for understanding the distribution of a tax's burden.

Part II investigates that question empirically in the context of cigarette taxes. Cigarette purchases are typically subject to two types of taxes in the United States: an excise tax, which is included in the cigarette's posted price, and a sales tax, which is added at the register. Drawing on individual survey data about cigarette consumption, we exploit state and time variation in cigarette sales and excise tax rates to estimate the relation between the two tax types and cigarette demand. We find that both high- and low-income consumers respond to changes in the cigarette excise tax, but that only low-income consumers respond to changes in the sales tax rate on cigarettes. Although the empirical results are not conclusive, they are consistent with the hypothesis that attentiveness to cigarette register taxes declines by income. In conjunction with the theoretical insights from Part I, our empirical findings support the notion that a revenue-neutral shift from posted to register taxes could reduce the burden of the cigarette tax on low-income consumers.

Because the choice between register and posted taxes is a practical question that policymakers must confront, the lack of economic literature on the topic is surprising. Although the recent paper by Chetty, Looney, and Kroft (discussed above) provides important insights into the relative efficiency of posted and register taxes, our analysis builds on theirs by investigating how the choice between the two tax designs affects the distribution of the tax's burden between consumers. In particular, the aggregate nature of their data preclude CLK from investigating heterogeneity in consumer attentiveness – our focus here. Moreover, the welfare analytic tools developed in CLK are geared toward assessing the efficiency of a tax in the context of a representative-agent, rather than a tax faced by heterogeneous consumers. To our knowledge, our paper is the first in the literature to investigate the link between the salience of a tax and the distribution of its burden across consumers.

Our paper also fits into a nascent behavioral literature investigating heterogeneity in the extent to which individuals depart from neoclassical models of decision-making. For example, Hall (2010) documents income differences in the mental accounting heuristics that individuals employ when making financial decisions and Bar-Gill and Warren (2008) present survey evidence suggesting that low-income consumers are more likely to make financial mistakes. Similarly, Mullainathan and Shafir (2009) argue that a number of behavioral phenomena affect the poor in distinctive ways because that group lacks many of the resources used by higher-income consumers to improve decision-making quality (such as access to financial advising). In a different context, Shue and Luttmer (2009) present evidence that low-income voters are particularly prone to accidentally selecting the wrong candidate when voting ballots are designed in confusing ways. [29]

Our paper contributes to this growing literature by exploring a particular context in which cognitive limitations faced by all decision-makers (e.g. bounded attention and computational abilities) affect high- and low-income consumers in distinctive ways. Most notably, whereas other studies have found deviations from optimal decision-making to be greatest for low-income decision-makers, we find the opposite. At least in the context of cigarette taxation, it appears that lower-

[29] One theory that has been advanced to explain these findings is the notion of "cognitive depletion," the idea that making complicated or high-stakes decisions can deplete individuals' cognitive resources, worsening the quality of subsequent decisions they make. If low-income decision-makers must make more of these decisions throughout the day, they may exhibit a greater number of behavioral biases than do higher-income decision-makers. See Spears (2011) or Mullainathan and Shafir (2010).

income consumers do a better job of accounting for register taxes when making purchasing decisions. Apart from our empirical results, the theoretical framework we employ can be readily applied to other contexts in which agents differ in the extent to which they respond optimally to policy changes.

The paper is organized as follows. Part I constructs a stylized model of consumer behavior and uses it to analyze the welfare effects of a policy shift from posted to register taxes. The model takes as its starting point the assumption that consumers differ in their attentiveness to register taxes. Part II investigates that assumption empirically, in the context of cigarette taxation. In particular, we investigate whether high- and low-income consumers respond differently to cigarette register taxes, using those groups' responsiveness to posted taxes on cigarettes as a baseline. Part III concludes.

I. Tax Salience and Distribution

Part I demonstrates that when consumers differ in their attentiveness to register taxes, the government's choice between posted and register taxes affects the distribution of a tax's burden. In particular, replacing a posted tax with a register tax increases total tax revenue because only attentive agents consider the full after-tax price when determining their demand for the taxed good. That extra revenue accommodates a reduction in the total tax rate, generating a positive welfare effect for attentive consumers. Inattentive consumers also benefit from the reduction in the total tax rate, but their welfare gains are offset by optimization error induced by the register tax.

A. Setup

Our modeling approach is similar to that employed in Chetty, Looney and Kroft (2007), except that we allow for heterogeneity in agents' attentiveness to register taxes. Suppose that society is composed of two agents (*A* and *B*) who make consumption decisions between some good *x,* and a composite of all other goods, *y*. Good *x* is subject to both a register tax and a posted tax, whereas good *y* is left untaxed. Both agents pay attention to posted taxes when making their consumption decisions, but only A takes register taxes into account. B ignores the register tax when choosing how much x to consume, treating it as if it was zero. The agents share a utility function $U(x,y)$, and both have budget constraints of the form

$$BC_i : (p+t_p+t_r)x_i + y_i \leq M_i \tag{2.1}$$

where the agent's type is denoted by $i \in \{A,B\}$, p is the pre-tax price of x, t_p is the posted tax , t_r is the register tax , M is income, and the pre-tax price of y is normalized to one.

Consumption is determined in two steps. First, agents choose their intended consumption bundle according to their perceived budget constraint $\left(\widehat{BC_i}\right)$. A is attentive to the register tax, so her perceived budget constraint matches her true budget constraint, $BC_A = \widehat{BC_A}$. In contrast, B misperceives the register tax to be zero: $\widehat{BC_B} : (p+t_p)x_i + y_i \leq M_i$. The (x,y) pair that maximizes utility subject to the agent's perceived budget constraint is the *intended consumption bundle* $(\widehat{x_i},\widehat{y_i})$.[30] Note that B's intended consumption bundle will be infeasible when it fails to satisfy her true budget constraint.

Because the bundle that agents consume must ultimately be feasible, closing the model requires specifying the final consumption bundle for agents whose intended consumption bundle is infeasible. Because A chooses a feasible bundle to begin with, her final bundle always equals her intended bundle, $(x_A,y_A) \equiv (\widehat{x}_A,\widehat{y}_A)$. To pin down consumption for B, we assume that agents who over-spend on x reduce their expenditures on y by the amount that they overspent on x. In our notation: $x_B = \widehat{x}_B$ and $y_B = M_B - (p+t_p+t_r)\widehat{x}_B$.[31] This assumption is natural for the case in which y represents all goods other than x and agents make at least some of their consumption decisions after purchasing x; consumers who accidentally overspend on x will have less income available to spend on their remaining purchases (which are all part of y).[32]

[30]That is, $(\widehat{x_i},\widehat{y_i})$ satisfies argmax $U(x_i,y_i)$ s.t. $\widehat{BC_i}$ holds.

[31]Note that we are implicitly assuming that x is a small enough portion of total consumption that an agent's intended consumption of x is never infeasible, even after taking the register tax into account.

[32]In principle, one could choose a different rule for mapping consumers' sub-optimal decision-making into feasible consumption bundles. Chetty, Looney and Kroft (2007) identify three intuitive "budget adjustment rules": the one that we employ, as well as two others. Appendix B demonstrates that the qualitative results in this section are robust to

We are now in a position to link consumer demand to the two tax types. Assume for now that production of x is governed by constant returns to scale technology and that the market for x is perfectly competitive, so that p is fixed at the (constant) marginal cost of x. Holding the pre-tax price and agents' income fixed, we can express demand as a function of the taxes, $x_i = x_i(t_p, t_r)$ and $y_i = y_i(t_p, t_r)$. For A, final consumption always equals intended consumption, so demand corresponds to the solution of the standard utility maximization problem: $(x_A, y_A) = \arg\max_{x,y} U(x,y)$ s.t. BC_A. Because the tax rates do not enter the utility function directly and because they appear symmetrically in the budget constraint, A's demand will depend only on the total tax rate – the portion of taxes included in the posted price does not matter. Hence we can write $x_A(t_p, t_r) = x_A(t_p + t_r, 0)$, or $x_A(t_p + t_r)$ for short. And similarly for y: $y_A(t_p, t_r) = y_A(t_p + t_r, 0)$, or $y_A(t_p + t_r)$ for short. Note that in accordance with the neoclassical model's invariance prediction, we have $\frac{\partial x_A}{\partial t_r} = \frac{\partial x_A}{\partial t_p} = \frac{\partial x_A}{\partial p}$ and $\frac{\partial y_A}{\partial t_r} = \frac{\partial y_A}{\partial t_p} = \frac{\partial y_A}{\partial p}$.

Deriving B's demand is complicated by the fact that her intended consumption departs from her final consumption whenever she faces a positive register tax. By assumption, all of the income B overspends on x comes out of intended expenditures on y; hence B's final consumption of x equals B's intended consumption of x: $x_B(t_p, t_r) = \widehat{x_B}(t_p, t_r)$ for all values of t_p and t_r. Moreover, because B's *intended* consumption of x is insensitive to register taxes, $\widehat{x_B}(t_p, t_r) = \widehat{x_B}(t_p, t_r')$ for all t_r and t_r', it must also be the case that B's *final* consumption of x is insensitive to register taxes, $x_B(t_p, t_r) = x_B(t_p, t_r')$ for all t_r and t_r'. Consequently, we can write B's final consumption of x as a function of the posted tax alone: $x_B(t_p, t_r) = x_B(t_p)$. Finally, because B's perceived budget constraint matches her true budget constraint in the special case that $t_r = 0$, we can conclude that B's demand for x under any non-zero register tax corresponds to B's optimal demand for x when the register tax is zero:

$$x_B(t_p, t_r) = x_B(t_p) = x_B^*(t_p, 0) \tag{2.2}$$

all three of those rules. More generally, the Appendix demonstrates that our main result holds as long as individuals who misperceive the price of x to be lower than it really is end up allocating more of their income to x and less of their income to y relative to the case in which they take the true after-tax price of x into account.

where x_B^* represents B's optimal consumption of x, i.e. the amount of x that B would choose if her perceived budget constraint were equal to her true budget constraint. [33]

By substituting (2.2) into B's true budget constraint, we can solve for B's final consumption of y:

$$y_B(t_p, t_r) = M_B - (p + t_p + t_r)\, x_B(t_p)\,. \tag{2.3}$$

Note that in contrast to the neoclassical model, B responds differently to the two types of taxes: $\frac{\partial x_B}{\partial t_p} = \frac{\partial x_B}{\partial p} < \frac{\partial x_B}{\partial t_r} = 0$ and $\frac{\partial y_B}{\partial t_p} = \frac{\partial y_B}{\partial p} > \frac{\partial y_B}{\partial t_r}$.

B. The Role of Tax Policy

To incorporate tax policy into the model, consider a government that must raise a fixed amount of revenue, $\overline{R}$, from register and posted taxes on x. How does the government's choice between register and posted taxes affect the well-being of the agents? In particular, the policy we consider is a revenue-neutral increase in the register tax – that is, an increase in the register tax coupled with a reduction in the posted tax by an amount that leaves total revenue unchanged (at $\overline{R}$). Let R denote total revenue collected by taxes on x, so that $R(t_p, t_r) = (t_p + t_r)(x_A + x_B)\,.$

If both agents were fully attentive to both types of tax, a one-dollar increase in the register tax could accommodate a one-dollar reduction in the posted tax; changing the balance between register and posted taxes would not affect the combined tax rate necessary to raise a given amount of revenue. When some agents are inattentive, however, the demand reduction that typically accompanies a tax increase will be muted. As a result, an incremental increase in the posted tax will, all else equal, raise less revenue than an incremental increase in the register tax:

$$\begin{aligned} \frac{\partial R}{\partial t_p} &= (x_A + x_B) + (t_p + t_r)\left(\frac{\partial x_A}{\partial p} + \frac{\partial x_B}{\partial p}\right) \\ &< (x_A + x_B) + (t_p + t_r)\left(\frac{\partial x_A}{\partial p} + 0\right) = \frac{\partial R}{\partial t_r} \end{aligned}$$

[33] That is, x_B^* is the value of x that solves $argmax_{(x,y)} U(x,y)$ s.t. BC_B holds.

The reduction in the posted tax associated with a *revenue-neutral* increase in the register tax can be found by totally differentiating the government's budget constraint:

$$\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} = -\frac{x_A + x_B + (t_p + t_r)\frac{\partial x_A}{\partial p}}{x_A + x_B + (t_p + t_r)\frac{\partial x_A}{\partial p} + (t_p + t_r)\frac{\partial x_B}{\partial p}}. \tag{2.4}$$

How does a revenue-neutral increase in the register tax affect the combined tax rate, $t_p + t_r$? The effect of the shift is given by $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}} = \left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} + 1$. Assuming that x is a normal good, (2.4) implies that $\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} < -1$.[34] Consequently, a revenue-neutral increase in the register tax is associated with a net reduction in the combined tax rate, $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}} < 0$. Put differently, the government can maintain revenue-neutrality while reducing posted taxes more than one-for-one with each register tax increase.

C. Welfare Effects for Attentive Consumers

What are the welfare effects of such a revenue-neutral shift towards register taxes? First, consider the effect of the shift on A's welfare. Indirect utility for A is given by $V_A(t_p, t_r) = U(x_A(t_p, t_r), y_A(t_p, t_r))$. The welfare effect of the shift for A is thus:

$$\left.\frac{dV_A}{dt_r}\right|_{\overline{R}} = \frac{\partial V_A}{\partial t_r} + \frac{\partial V_A}{\partial t_p}\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} = -U_y(x_A, y_A)\, x_A \left(\left.\frac{d(t_p + t_r)}{dt_r}\right|_{\overline{R}}\right). \tag{2.5}$$

where the second inequality follows by application of the envelope theorem.[35]

Equation (2.5) states that the welfare effect of a revenue-neutral shift towards register taxes for attentive consumers stems entirely from the effect of the shift on the after-tax price of x. Increasing the register tax by one-dollar accommodates a reduction in the combined tax rate on x of $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}}$. For each dollar that the after-tax price of x is reduced, A has x_A dollars more of income to spend on other goods (y). The greater the marginal utility of y, the greater A's welfare gain will be.

[34] Note that the denominator in (2.4) equals $\frac{\partial R}{\partial t_p}$. Hence it is positive as long as demand for x is not so sensitive that raising the posted tax would actually decrease revenue, an assumption we maintain throughout.

[35] Note that the envelope theorem applies here because A's final consumption bundle matches her optimal consumption bundle.

Because we know that a revenue-neutral shift towards register taxes reduces the combined tax rate, $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}} < 0$, we can conclude that the shift unambiguously increases the welfare of the attentive agent.

Equation (2.5) is the main result of the section, and the intuition is straightforward. Replacing a posted tax with a register tax raises total revenue because only attentive agents reduce their demand for x in response to the higher after-tax price. The extra revenue accommodates a reduction in the combined tax rate on x, generating a positive welfare effect for attentive consumers.[36]

In addition to allowing us to sign the welfare effect of the policy for attentive consumers, (2.5) also highlights that the magnitude of the welfare effect depends upon the extent to which increases in the register tax accommodate revenue-neutral reductions in the posted tax, $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}}$. With some algebra, we can rewrite (2.4) to obtain:

$$\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}} = -\frac{\tau \varepsilon_B \phi_B}{1-\tau(\varepsilon_A \phi_A + \varepsilon_B \phi_B)} \tag{2.6}$$

where $\tau \equiv \frac{t_p+t_r}{p+t_p+t_r}$ is tax as a fraction of the tax-inclusive price of x, $\varepsilon_i \equiv -\frac{\partial x_i}{\partial p}\frac{p+t_p+t_r}{x_i}$ is the own-price elasticity of demand for x_i, and $\phi_i \equiv \frac{x_i}{x_i+x_{-i}}$ is the fraction of x consumed by type i.

The magnitude of the reduction in the combined tax rate permitted by a revenue-neutral shift thus depends upon three factors: the fraction of x consumed by inattentive consumers, the sensitivity of demand for x, and taxes as a share of price. To understand the role of these factors, recall that inattentive consumers are the only ones who behave differently under the two taxes. The greater the fraction of x consumed by that group, the more important their inattentiveness will be in determining revenue from the taxes. Similarly, when B's demand for x is highly elastic, the revenue advantage of a register tax over a posted tax is especially large – the posted tax causes a large amount of substitution away from the taxed good that the register tax avoids. Finally, the larger

[36]This result can be weakened by endogenizing agents' decisions about whether to pay attention to register taxes. If a small increase in the register tax causes a large number of formerly inattentive agents to start taking register taxes into account, the shift might necessitate an *increase* in the combined tax rate. In such cases, the shift to register taxes would actually generate a negative income effect, reducing the welfare of all agents. Consequently, the results presented here are most applicable to situations in which small changes in the tax rate do not induce dramatic shifts in which agents are attentive.

are taxes as a share of x's price, the more that changes in those taxes affect consumer behavior for a given price-elasticity. Thus the welfare effect of a revenue-neutral shift towards register taxes is positive for A, and increasing in τ, ε, and ϕ_B.

D. Welfare Effects for Inattentive Consumers

What about the inattentive agent? The change in B's indirect utility following a revenue-neutral shift towards register taxes is given by

$$\left.\frac{dV_B}{dt_r}\right|_{\overline{R}} = U_x(x_B, y_B)\frac{\partial x_B}{\partial t_p}\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} + U_y(x_B, y_B)\left(\frac{\partial y_B}{\partial t_p}\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} + \frac{\partial y_B}{\partial t_r}\right)$$

Differentiating B's budget constraint with respect to t_p and t_r yields:

$$\frac{\partial y_B}{\partial p} = -x - (p + t_p + t_r)\frac{\partial x_B}{\partial p} \quad \text{and}$$

$$\frac{\partial y_B}{\partial t_r} = -x.$$

Substituting those conditions into the above expression gives the effect of the shift on B's welfare:

$$\left.\frac{dV_B}{dt_r}\right|_{\overline{R}} = -U_y(x_B, y_B)\, x_B\left(\left.\frac{d(t_p + t_r)}{dt_r}\right|_{\overline{R}}\right) + \left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}}\frac{\partial x_B}{\partial p}\mu \tag{2.7}$$

where $\mu \equiv U_x(x_B, y_B) - (p + t_r + t_p)\, U_y(x_B, y_B)$.

From (2.7), we can see that the net welfare effect for inattentive consumers is ambiguous. The first term is strictly positive: like the attentive consumer, B benefits from the fact that the shift accommodates a reduction in the combined tax rate. In particular, a revenue-neutral shift that increases the register tax by \$1 reduces the combined tax rate by $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}}$, which frees up $-\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}} x_B$ dollars of income. On the other hand, the second term in (2.7) is negative and reflects the fact that by raising the register tax, the shift pushes B further from her privately optimal consumption bundle. To understand the pieces of the term, note that a revenue-neutral shift that increases the register tax by one-dollar is associated with a posted tax reduction of $\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}}$, which prompts B to increase her consumption of x by $\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} \frac{\partial x_B}{\partial p}$ and reduce her consumption of y by $-\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} \frac{\partial x_B}{\partial p}(p+t_p+t_r)$. If B's consumption bundle were optimal, this substitution would not have any utility cost because the marginal utilities of expenditures on x and y would be equal.[37] However, because B consumes too much x and too little y relative to the amounts that would be privately optimal given her true budget constraint, declining marginal utility in x and y implies that $\mu = U_x(x_B, y_B) - (p+t_r+t_p)U_y(x_B, y_B) \leq 0$. Thus a revenue-neutral increase in the register tax generates optimization error that reduces B's utility by $\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} \frac{\partial x_B}{\partial p}\mu$. In general, either the positive or the negative welfare effect in (2.7) may dominate.

That even inattentive consumers can be made better off by a shift towards register taxes is somewhat surprising. The explanation lies in the fact that when the register tax is small, the utility cost of optimization error stemming from the register tax is small as well, but the positive welfare effect stemming from the lower combined tax rate can still be sizable. In particular, when the register tax is small, (x_B, y_B) will be close to (x_B^*, y_B^*) – the optimal bundle in B's true budget set. Consequently, the marginal utilities of expenditures on x and y will be close in size, implying a value of μ near zero.[38] In contrast, the magnitude of the positive welfare effect in (2.7) depends on the *level* of the marginal utility of y, not the *difference* in the marginal utilities of x and y; hence it stays positive even when $t_r \approx 0$. Thus when the register tax is small, revenue-neutral increases in t_r tend to benefit both types of consumers. In the special case that $t_r = 0$, the optimization error associated with a small increase in t_r is exactly zero, implying that a revenue-neutral shift towards register taxes always benefits inattentive consumers.[39]

To better understand the other factors that determine whether a shift will benefit inattentive

[37] That is, the standard first-order condition $U_x(x_B, y_B) = (p+t_r+t_p)U_y(x_B, y_B)$ implies $\mu = 0$.

[38] Formalizing this intuition is straightforward. Assume that utility is additively separable in x and y so that $U(x,y) = u(x) + v(y)$. Then Taylor approximations of μ around (x_B^*, y_B^*) and of x_B around x_B^* yield $\mu \approx -t_r \frac{\partial x_B}{\partial p}\left(U_{xx}(x_B^*) + (p+t_r+t_p)^2 U_{yy}(y_B^*)\right)$.

[39] Because the shift also benefits attentive consumers, this result implies that the optimal register tax is always positive.

consumers, we can substitute (2.4) into (2.7) and rearrange terms to obtain:

$$\left.\frac{dV_B}{dt_r}\right|_{\overline{R}} > 0 \iff \phi_B \left(U_y\left(x_B, y_B\right)\left(t_p + t_r\right) + \mu\right) + \phi_A \, \mu \, \left(1 - \tau \varepsilon_A\right) > 0 \tag{2.8}$$

When x is primarily consumed by attentive consumers, i.e. $\phi_A \approx 1$, (2.8) shows that revenue-neutral shifts towards register taxes tend to harm inattentive consumers.[40] Intuitively, a revenue-neutral shift towards register taxes accommodates only a small reduction in the combined tax rate when most consumers are attentive because the revenue differences between the two tax types will be small. However, inattentive consumers still bear the full utility costs of optimization errors that stem from the higher register tax following the shift. In contrast, when $\phi_B \approx 1$, inattentive consumers benefit from a shift whenever $U_y\left(x_B, y_B\right)\left(t_p + t_r\right) + \mu > 0$. Whether this condition holds depends on the relative welfare effects of the reduction in the combined tax rate and the optimization error induced by $t_r > 0$.[41]

E. Summary and Extensions

In summary, while a revenue-neutral shift towards register taxes always benefits attentive consumers, the net welfare effect for inattentive consumers is ambiguous. Like A, B benefits from the lower combined tax on x associated with the shift. However, unlike A, B is driven by the shift to misallocate income between x and y (relative to the allocation that maximizes B's private utility). When register taxes are small, the utility cost of that misallocation is small as well, and the positive welfare effect dominates. But when register taxes are large, the utility cost of the misallocation may be large as well. Additionally, when x is primarily consumed by attentive consumers, the positive welfare effects of the shift are muted for attentive and inattentive consumers alike.

For simplicity, we have assumed that the pre-tax price of x is fixed at p. In reality, firms may adjust the price they charge for x in response to changes in the type of tax imposed. If a shift from

[40] Note that $\tau\varepsilon < 1$ follows from our maintained assumption that $\frac{\partial R}{\partial t_p} > 0$, i.e. that demand for the taxed good is not so sensitive that increasing the posted tax reduces revenue.

[41] Because our focus in the rest of the paper is on heterogeneity between agents, we do not further develop the case in which all agents are inattentive. See Goldin (2012*b*) for further results.

posted to register taxes induced firms to raise p by a sufficient quantity, the policy could end up *increasing* the after-tax price of x, generating a negative welfare effect for all consumers.

Appendix A expands the model to the case of endogenous producer prices. We show that a revenue-neutral shift towards register taxes makes attentive consumers better off when supply of the taxed good is relatively elastic, in particular when $\varepsilon^S \tau > 1$, where $\varepsilon^S \equiv \frac{\partial s(p)}{p} \frac{(p+t_p+t_r)}{x_A+x_B}$ is the supply elasticity of x with respect to its after-tax price. In contrast, when $\varepsilon^S \tau < 1$, the reduction in t_p caused by the shift is more than offset by an increase in p, resulting in a net increase in the after-tax price of x. Intuitively, when the supply of x is inelastic, the incidence of a posted tax falls on producers; reducing the posted tax and replacing it with a register tax – to which some consumers are less sensitive – allows producers to shift the incidence of the tax back on to consumers. Thus once one accounts for the endogeneity of producer prices, the welfare results presented in Part I apply only to goods for which demand is relatively inelastic and supply is relatively elastic – that is, goods for which posted taxes are most likely to be passed on to consumers.[42]

II. The Relation Between Cigarette Tax Attentiveness and Income

In Part I, we showed that policymakers can manipulate the salience of a tax to redistribute the tax's burden between attentive and inattentive agents. In practice, policymakers are often concerned with how the burden of a tax is distributed by income. In particular, a concern with many commodity taxes is that they are regressive – that is, they constitute a disproportionately greater burden for low-income consumers. An implication of the results in Part I is that if the poor tend to pay more attention to register taxes than the rich, a shift towards register taxes will make a commodity tax more progressive. On the other hand, if low-income consumers are less attentive to register taxes,

[42] An important implication of this result concerns the case in which $\varepsilon^S \tau < 1$. For such goods, a revenue-neutral shift from posted to register taxes – the *opposite* of the policy considered above – will benefit both attentive and inattentive consumers. Attentive consumers benefit because the reduction in the pre-tax price more than offsets the increase in the total tax on x, resulting in a net decrease in x's after-tax price. Inattentive consumers also benefit from the after-tax price reduction, and because the shift is from register to posted taxes, it reduces the magnitude of their optimization error. Of course, the final incidence of either type of shift depends on the relative degree to which each type of consumer gains from producer surplus.

such a shift would exacerbate the tax's regressivity. As such, it is important to determine whether attention to register taxes varies by income, and if so, whether high- or low-income consumers are the more attentive.

In Part II, we undertake that task in the context of cigarette taxes. There are good reasons to expect that low-income consumers will be more attentive to register taxes on cigarettes. In particular, the utility cost of optimization errors will tend to be greater for those with less income to spend on other goods. As a result, low-income consumers should be particularly motivated to spend the effort required to take register taxes into account. On the other hand, other factors could push high consumers to pay more attention to register taxes. For example, because the rich tend to consume more of each good, the magnitude of their optimization errors tends be greater as well. Appendix C utilizes a cognitive cost model to explore these tensions more formally. For the case of cigarettes, the analysis suggests that attentiveness to cigarette register taxes is likely to decline by income.[43] However, because it is difficult to predict which group will be more attentive on the basis of theory alone, the remainder of Part II is primarily empirical.

Our goal is to investigate whether low-income cigarette consumers are more attentive to register taxes than high-income consumers are. Cigarette purchases are subjected to two types of tax in the United States: an excise tax, which consumers see reflected in the posted price, and a sales tax, which is typically added at the register. We use state and time variation in these tax rates to estimate how consumers respond to each type of tax. We assume that consumers fully account for posted taxes, so that inattention to register taxes can be measured by the gap between consumers' responsiveness to register taxes and their responsiveness to posted taxes.

Part II is structured as follows. We begin by investigating whether the general population appears to pay more attention to register taxes than to posted taxes on cigarettes. The analysis applies the basic empirical strategy of CLK to a new product (cigarettes instead of beer) and at a different unit of observation (individual instead of aggregate consumption data). We then turn

[43]The framework we develop does not make a uniform prediction for all goods, but rather highlights the factors that determine which income group will be more attentive to register taxes on a particular good. In general, high-income consumers tend to be less attentive to register taxes on goods, like cigarettes, for which demand is relatively insensitive to income.

to our central question, whether attentiveness to cigarette register taxes differs by income, which we assess empirically by interacting the excise and sales tax variables with respondents' income. Finally, we undertake a number of robustness tests to investigate whether our results actually reflect heterogeneous attentiveness to register taxes as opposed to various alternative explanations.

A. Data

We obtain cross-sectional micro data on cigarette consumption from the Behavioral Risk Factor Surveillance System (BRFSS), supported by the National Center for Chronic Disease Prevention and Health Promotion and the Centers for Disease Control and Prevention. The BRFSS is a state-based telephone survey system that tracks health conditions and risk behaviors of individuals 18 years and older. The number of states participating in the survey has grown over time, from 15 in 1984 to 50 in 1994 (as well as the District of Columbia).[44] We follow CLK by dropping two states from the analysis: Hawaii, because sales taxes in that state are included in the posted price, and West Virginia, because of frequent changes to that state's sales tax base over the sample period. After dropping observations that are missing demographic variables, our final data set contains approximately 1.3 million observations. Because the survey disproportionately samples certain groups, we use weighted regressions to obtain representative estimates.

The BRFSS data contains two measures of smoking demand: whether the respondent is a smoker (smokes at least one cigarette every day) and how many cigarettes the respondent typically consumes each day. Although the BRFSS questionnaire asked respondents about smoking participation in each year of the survey, data on the number of cigarettes consumed are only available through 2000. Consequently, our analysis restricts the sample to those interviewed between 1984 and 2000.[45] The BRFSS also collects information on a number of demographic variables, including income.[46]

[44]To investigate whether the changing composition of states was biasing our results, we restricted our analysis to the 33 states that have been in the sample since 1987. The qualitative results were unchanged by that restriction.

[45]Extending our empirical analysis through 2009 and using only the outcome variables available in the later years yields results similar to those obtained from our sample.

[46]We make use of information concerning the respondent's age, race, sex, educational attainment, marital status,

Data on state-level cigarette excise tax rates, sales tax rates, and average cigarette prices were obtained from the *Tax Burden on Tobacco* 2008 report, published by Orzechowski and Walker (and previously by the Tobacco Institute). We gathered information on the exact date of enactment of sales tax changes from a number of sources including the World Tax Database (University of Michigan), state government websites, and archives of local newspaper accounts. Following convention, our measure of state tax rates includes local taxes to the extent that they are uniform across the state.

Whereas the sales tax is an ad valorem tax (consumers are charged a fixed fraction of a good's price), the excise tax is a unit tax (consumers pay a set dollar amount per pack, regardless of the pre-tax price). In order to make the two types of taxes comparable for the empirical analysis, we convert the excise tax to an ad valorem tax using the method described in CLK.[47]

Both sales and excise taxes increased between 1984 and 2000 (Figures 2.1a and 2.1b). In 1984, 38 states imposed sales taxes on cigarettes, and the median sales tax rate was 4 percent. By 2000, 45 states imposed sales taxes on cigarettes, and the median sales tax rate had climbed to 5 percent. Similarly, median state excise taxes on cigarettes increased from 14 cents in 1984 to 34 cents in 2000. In addition, the federal excise tax on cigarettes increased three times over the same period, climbing from 16 to 34 cents per pack. Table 2.1 presents summary statistics on U.S. cigarette taxation.

Figure 2.2 shows that aggregate cigarette consumption in the United States declined between 1984 and 2000. That decline, however, was not uniform across the population. Figure 2.3 separately plots smoking participation rates over time for the highest and lowest income quartiles.

employment status, and income. Household income is measured in terms of income-categories. Two problems arise when using this variable. First, the income measure is top-coded at a relatively low value ($75,000 for much of our sample period). Second, the income categories are not adjusted for inflation, making it difficult to compare respondents in the same category over time. Rather than attempt to convert the BRFSS income category data into a measure of real income, we measure income in percentile terms, assigning respondents the midpoint of the percentiles of their income category boundaries. For example, if 10 percent of the sample in one year reports an income between zero and $10,000, all individuals in that income category in that year are assigned a value of 0.05. This approach is similar to that employed by Franks et al. (2007).

[47] We divide the excise tax by the average national price of a pack of cigarettes in 2000, adjusted for inflation. The rationale for using the inflation-adjusted national price in 2000 as a proxy for the true price is to avoid endogeneity problems arising from the fact that changes in cigarette prices are likely correlated with unobserved shocks to smoking demand.

Low-income individuals were more likely to smoke than high-income ones in 1984, and that gap widened over time. Smoking demand measures are summarized in Table 2.2.

B. Attentiveness to Cigarette Taxes in the General Population

We begin our empirical analysis by investigating whether consumers in the general population respond differently to register and posted taxes on cigarettes. The neoclassical model predicts that the salience of a tax (e.g., whether it is included in the posted price or added at the register) should not affect how consumers respond to it. To see this formally, suppose that demand for a good x depends on a consumer's income I and the price of x, p_x: $x = x(p_x, I)$.

Purchases of x are subject to both a sales tax and an excise tax, so that the final price of x is given by $p_x = p(1+t)(1+s)$, in which p is the pre-tax price of x, t is the excise tax rate, and s is the sales tax rate.[48]

Because the excise and sales tax affect the price of x symmetrically, we have that

$$\frac{\partial x}{\partial \log(1+t)} = \frac{\partial x}{\partial \log p_x}\frac{\partial \log p_x}{\partial \log(1+t)} = \frac{\partial x}{\partial \log p_x}\frac{\partial \log p_x}{\partial \log(1+s)} = \frac{\partial x}{\partial \log(1+s)}$$

In words, how consumers adjust their demand for x in response to a tax change should not depend on whether the change occurred in the excise tax rate or the sales tax rate.[49]

CLK assess this prediction for the case of beer by linking changes in aggregate beer consumption by state to changes in the state's sales tax rate and excise tax on beer. They find that changes in the beer excise tax are negatively and significantly correlated with changes in beer consumption, whereas sales tax changes appear to have little effect. As a result, CLK conclude that the neoclassical model is mistaken and that the salience of a tax affects how consumers respond. Because

[48] Some states do not include the excise tax in the price used to calculate the sales tax, so that final prices are given by $p_x = p(1+t+s)$. Because the excise and sales tax still affect the price of x symmetrically in such states, the neoclassical model predicts that demand should respond identically to sales and excise tax changes of the same proportion.

[49] Two assumptions are important for this result: first, that tax rates only enter consumer utility through their effect on product prices, and second, that p_x is the only price that affects demand for x. We maintain the first assumption throughout but consider the implications of relaxing the second in Section II.E.

they lack disaggregated consumption data, CLK are unable to assess whether the salience of a tax affects different parts of the population differently, our goal in Section II.C.

Our analysis in this section differs from CLK by focusing on cigarettes instead of beer and by using individual survey data rather than aggregate state consumption data. Our baseline empirical model takes the form:

$$y_{ismt} = \alpha + \beta_1 \tau^e_{smt} + \beta_2 \tau^s_{smt} + \gamma x_{smt} + \delta z_{ismt} + \mu_s + \lambda_t + \pi_m + \varepsilon_{ismt} \tag{2.9}$$

where the unit of observation is an individual i in state s, calendar month m, and year t. The dependent variable (y) represents cigarette demand, τ^e is the log excise tax rate, τ^s is the log sales tax rate, x are covariates that do not vary between individuals interviewed in the same state, month, and year, and z are individual-level covariates. We include state fixed effects μ_s to capture unobserved factors that are correlated with both state tax rates and the level of smoking demand. Year fixed effects λ_t capture time trends in smoking demand as well as yearly shocks to national cigarette consumption, such as a national anti-smoking campaign. Finally, π_m is a calendar month effect, which accounts for seasonal or monthly patterns in cigarette demand.

As is standard in the cigarette demand literature,[50] we model the decision of whether an individual smokes (the extensive margin) separately from the decision of how much to smoke, conditional on being a smoker (the intensive margin). Consequently, in some specifications y is a binary choice variable indicating whether the individual reports being a smoker, and in other specifications y is the non-zero count of the number of cigarettes consumed in the last month, where the sample is restricted to self-reported smokers. This "double-hurdle" model is common in the cigarette demand literature because the decision of whether to smoke may be fundamentally different than the decision of how much to smoke, and is informative as to whether taxes affect consumption by turning smokers into non-smokers or by inducing current smokers to reduce the number of cigarettes they smoke.[51]

[50] See Chaloupka and Warner (2000*b*) for a helpful review of the extensive literature on estimating cigarette demand.

[51] A drawback of the two-part approach is that estimation results for the intensive margin may be biased by changes to the composition of the smoking population. We investigate the robustness of this specification in Section II.E.2.

Table 2.3 presents the results of this analysis.[52] The specifications in Columns 1 and 4 regress smoking demand on the two tax rates, individual demographic variables, and state, year, and calendar month fixed-effects. Since state taxes are often increased to meet budgetary shortfalls in bad economic times, it is likely that tax rate changes are correlated with state-level economic variables that are not captured by state fixed effects. If cigarette consumption is also correlated with the business cycle, this omitted variable could bias our results. To account for this possibility, Columns 2 and 5 include state-level measures of real income and unemployment rate.[53]

Columns 3 and 6 add an interaction between income and a linear time trend. To motivate this addition, recall that smoking participation rates fell more steeply over the sample period for higher income consumers (Figure 2.3). Although this decline might stem from rising tax rates over the sample period, it could also reflect a secular trend in smoking consumption at the top of the income spectrum, such as a shift in cultural attitudes about smoking among high SES individuals. Because tax rates trend upwards over the sample period, a secular trend in smoking demand among high-income consumers could be conflated with the two tax-income interaction terms in the regression. The inclusion of the time trend in Columns 3 and 6 accounts for this possibility.

The regressions in Table 2.3 show the effect of taxes on the intensive and extensive margins separately. In order to provide a better picture of the overall effect of a tax change on cigarette demand, Table 2.4 follows the procedure laid out in McDonald and Moffitt (1980*a*) to combine the intensive and extensive margin estimates. In particular, one can decompose the conditional expectation of cigarette demand into its intensive and extensive components:

$$E[y|x] = E[y|x, y > 0] * P(y > 0|x),$$

where y represents cigarette demand and x represents the covariates. Using the product rule, the

[52]We estimate demand on the extensive margin with a linear probability model. A Probit model yields similar results. Because unobserved shocks to smoking demand may be correlated across time for consumers living in the same state, all tables report standard errors that are clustered at the state level.

[53]Real state income data comes from the Bureau of Economic Analysis and the state unemployment rate data comes from the Bureau of Labor Statistics. Both variables are measured quarterly.

total effect of a change in one of the covariates on cigarette demand is given by:

$$\frac{\partial E[y|x]}{\partial x} = \frac{\partial E[y|x,y>0]}{\partial x} * P(y>0|x) + \frac{\partial P(y>0|x)}{\partial x} * E[y|x,y>0].$$

By utilizing sample estimates of $P(y > 0|x)$ and $E[y|x,y > 0]$, evaluated at the sample mean of each covariate, we can combine the estimated coefficients from the intensive and extensive margin regressions into a rough estimate of the overall effect of the taxes on cigarette demand.[54]

The results in Tables 2.3 and 2.4 are consistent with a salience effect on the intensive margin: under our preferred specification, a one-percent increase in the cigarette excise tax is associated with a 0.34 percent reduction in cigarettes per month among smokers, whereas the point estimate on the sales tax term is close to zero and is not statistically significant. However, the coefficient on the sales tax is measured imprecisely, and consequently, we cannot reject the null hypothesis of equality between the two coefficients. On the extensive margin, the point estimate of the sales tax is slightly greater in magnitude than that of the excise tax, although here too the difference is not statistically significant.[55] The coefficients on the excise tax estimates imply price elasticities of -0.52 on the extensive margin, -0.31 on the intensive margin, and -0.87 for combined demand. For the sales tax, the implied price elasticities are -0.32 on the extensive margin, -0.02 on the intensive margin, and -0.32 for overall cigarette demand.[56] Overall, the evidence is inconclusive regarding

[54] When calculating standard errors for the aggregate effect, we ignore uncertainty in the sample averages of $P(y > 0|x)$ and $E[y|x,y > 0]$. This approximation is reasonable because the size of our sample guarantees those quantities are estimated precisely.

[55] One complication also confronted by CLK is that the simple comparison between estimated tax coefficients can be misleading as a test of salience if the two tax types are passed through to consumers at different rates – that is, if $\frac{\partial(p+t_p+t_r)}{\partial t_r} \neq \frac{\partial(p+t_p+t_r)}{\partial t_p}$. Although a finding of differential pass-through is consistent with tax salience – see CLK pp. 1167-69 – it could also arise solely from differences in the two tax bases. As explained in Section E.1 below, we address this issue by comparing the sales tax coefficient with the effect of the pre-sales tax price of x, instrumented with the excise tax. For the general population analysis, an IV approach differs from Table 2.3 in that the estimated excise tax coefficient becomes greater in magnitude than the estimated effect of the sales tax, consistent with a salience effect. However, for both margins, the difference between the estimated coefficients remains statistically insignificant.

[56] To compute the price elasticity implied by a tax coefficient, one must scale the coefficient by the rate at which the tax is passed through to the after-tax price, $\varepsilon_{x,p} = \frac{1}{x}\frac{\partial x}{\partial \log(p_x)} = \frac{1}{x}\frac{\partial \log(x)}{\partial \log(1+t)}\frac{\partial \log(1+t)}{\partial \log(p_x)} = \varepsilon_{x,t}/\varepsilon_{p,t}$. The pass-through rate may be obtained from Table 2.11. The estimated excise tax elasticities we find are on the larger side of those typically reported in the smoking literature. For example, Chaloupka and Warner (2000*b*) report that recent estimates of (overall) cigarette demand range from elasticities of -0.14 to -1.23, but that most fall in the narrower range of -0.3 to -0.5. Gruber and Koszegi (2004) find an implied excise tax elasticity of -0.66 using the Consumer Expenditure Survey. Sales tax elasticities are not typically estimated in the smoking literature.

the presence of a salience effect for the general population.

C. Attentiveness to Cigarette Taxes by Income

The inconclusive results for the general population in Section II.B might mask heterogeneous responsiveness across income groups. We now turn to our primary question of interest, whether low-income consumers are particularly attentive to cigarette register taxes. The baseline empirical model for this section is given by:

$$\begin{aligned} y_{ismt} &= \alpha + \beta_1 \tau^e_{smt} + \beta_2 \tau^s_{smt} + \rho_1 \tau^e_{smt} LI_{ismt} + \rho_2 \tau^s_{smt} LI_{ismt} + \eta LI_{ismt} + \\ &\quad \gamma x_{smt} + \delta z_{ismt} + \mu_s + \lambda_t + \pi_m + \varepsilon_{ismt} \qquad (2.10) \end{aligned}$$

where *LI* is a binary variable indicating whether the respondent is low-income, defined as having income below the 25th percentile. Compared to the econometric model in II.2, this specification adds interaction terms between low-income status and the two tax rate variables.[57] The coefficients on the two tax types, β_1 and β_2, describe how high-income consumers modify their demand in response to changes in the excise and sales taxes, respectively. In turn, the coefficients on the income-interaction terms, ρ_1 and ρ_2, measure whether low-income consumers are more or less sensitive to changes in the two tax types.

Our primary question is whether attentiveness to the sales tax varies by income. In answering this question, one must distinguish between attentiveness – the extent to which consumers account for a tax when making their consumption decisions – and price-sensitivity – which describes how a tax that consumers account for affects their optimal purchase. The sales*low-income interaction term (ρ_2) may reflect differences in attentiveness between high- and low-income consumers, but it may also reflect differences in price-sensitivity by income. That is, a negative coefficient on ρ_2 could stem from high-income smokers being less sensitive to cigarette prices *in any form*, even if high- and low-income smokers were equally attentive to the sales tax.

[57] In addition to the main effect for low-income status, the individual demographics vector z also includes a continuous measure of income.

To deal with this possibility, it is useful to introduce the notion of the "attention gap," the amount by which a consumer's responsiveness to the excise tax exceeds her responsiveness to the sales tax. For high-income consumers, the estimated attention gap is simply $\beta_2 - \beta_1$. In turn, for low-income consumers, the estimated attention gap is given by $(\beta_2 + \rho_2) - (\beta_1 + \rho_1)$. Recall that the neoclassical model described above predicts that consumers should respond identically to excise and sales taxes that they take into account. Consequently, we interpret a non-zero value of the attention gap as evidence that consumers account for one type of tax more than the other.

Although the sign and magnitude of the attention gap for a particular income group are interesting in their own right, more relevant to our analysis are *changes* in the attention gap by income. That is, we are less concerned with whether a particular group of consumers pays more attention to the excise tax relative to the sales tax, and more concerned with whether low-income consumers pay more attention to the sales tax (relative to the excise tax) than high-income consumers do.[58] It is easy to see that the estimated difference in attentiveness between high- and low-income consumers is given by:

$$\begin{aligned} \Delta AttentionGap &= [(\beta_2 + \rho_2) - (\beta_1 + \rho_1)] - [\beta_2 - \beta_1] \\ &= \rho_2 - \rho_1 \end{aligned} \tag{2.11}$$

Intuitively, the sales*low-income coefficient (ρ_2) reflects changing responsiveness to the sales tax by income, and the excise*low-income coefficient (ρ_1) removes the portion of that change due to changes in consumers' price sensitivity. Hence, the gap between the coefficients on the two interaction terms rates, $\rho_2 - \rho_1$, measures the extent to which attentiveness to the register tax changes as income rises. When $\rho_2 - \rho_1 < 0$, high-income consumers pay less attention to the sales tax (relative to the excise tax) than low-income consumers do.

Table 2.5 presents our results. Columns 1 and 4 include the two tax rates, on their own and

[58] After all, it is the *differences* in behavior between high- and low-income consumers that shapes the distribution of a tax's burden.

interacted with income. In addition, the regressions include demographic variables as well as state, year, and month fixed-effects. As before, Columns 2 and 5 add real state income and the state unemployment rate, and Columns 3 and 6 include an interaction between income and a linear time trend to capture the changing relationship between income and smoking behavior over time. The estimated coefficients on the demographic and macroeconomic variables are qualitatively similar to those reported in Table 2.3, and are omitted. Table 2.6 combines the intensive and extensive margin estimates into an overall effect, using the method described in Section II.B.[59]

The results in Tables 2.5 and 2.6 are consistent with the theory that attentiveness to register taxes declines with income. As before, Columns 3 and 6 are our preferred specification.[60] On both the intensive and extensive margins, the estimated tax coefficients suggest that high-income consumers respond less negatively to the sales tax than to the excise tax. The excise tax coefficients are negative and statistically significant, whereas the sales tax coefficients are statistically indistinguishable from zero.[61] An F-test suggests that the difference in magnitude between the high-income tax coefficients is statistically significant on both margins.

For low-income consumers, the results paint a dramatically different picture. The coefficient on the interaction between low-income status and the sales tax and is negative and significant, implying that an increase in the sale tax is associated with a larger reduction in demand for low-income consumers than for high-income consumers. The small coefficient on the excise*low-income interaction term suggests that the result reflects a difference in attentiveness rather than a mere difference in price-sensitivity by income.[62]

[59]The robustness checks that follow use the specification in Columns 3 and 6 as their baseline.

[60]The only qualitative difference between specifications is the coefficient on the excise*low-income interaction, which declines sharply in magnitude once the income time trend is added to the model.

[61]The high-income consumer price elasticities implied by these estimates are -0.61 (excise) and -0.06 (sales) on the extensive margin, and -0.31 (excise) and 0.18 (sales) on the intensive margin.

[62]The low-income consumer price elastcities implied by these estimates are -0.30 (excise) and -1.13 (sales) on the extensive margin, and -0.30 (excise) and -0.59 (sales) on the intensive margin. One interesting result is that on both margins, the point estimate of the sales tax is more negative than point estimate of the excise tax for low-income consumers, although the difference is only significant on the intensive margin. This result could stem from differences in the goods included in the excise and sales tax bases, a possibility explored in Section II.D. Of course, it is also possible that the estimated sales tax coefficient is biased downward due to some omitted variable. However, unless that omitted variable was differentially correlated with smoking demand by high- and low-income consumers, it would not drive the differences in sales tax responsiveness that we observe.

Recall from (2.11) that changes in the attention gap by income are captured by $\rho_2 - \rho_1$. Hence, to investigate whether low-income consumers are particularly attentive to register taxes, we test whether $\rho_1 = \rho_2$. The associated F-tests are reported in Tables 2.5 and 2.6. Under our preferred specifications, the F-statistics for the extensive and intensive margins are 8.60 and 5.14, respectively. Hence, our results are consistent with the hypothesis that low-income consumers pay more attention to cigarette register taxes than do high-income consumers. [63]

So far, we have divided the analysis into low-income consumers on the one hand (those below the 25th percentile in income) and medium- to high-income consumers on the other. Although that aggregation is convenient for exposition, it may mask differences in attentiveness between medium- and high-income consumers. The regressions in Tables 2.7 and 2.8 introduce additional flexibility into the model by allowing consumers in each income quartile to respond to the taxes in different ways.[64] The resulting specification is given by

$$\begin{aligned} y_{ismt} &= \alpha + \beta_1 \tau^e_{smt} + \beta_2 \tau^s_{smt} + \sum_{j \in \{II,III,IV\}} \left\{ \eta^j Q^j_{ismt} + \rho^j_1 \tau^e_{smt} Q^j_{ismt} + \rho^j_2 \tau^s_{smt} Q^j_{ismt} \right\} + \\ &\quad \gamma x_{smt} + \delta z_{ismt} + \mu_s + \lambda_t + \pi_m + \varepsilon_{ismt} \end{aligned} \tag{2.12}$$

where Q^j_{ismt} indicates whether consumer i falls into income quartile j.

As before, we find that income differences in how consumers respond to the excise tax tend to be small and statistically insignificant. In contrast, responsiveness to the sales tax declines monotonically with income. F-tests for the equality of the attention gap between consumers in

[63] Although the results from both margins are consistent with low-income consumers being more attentive than high-income consumers, several features of the analysis make the intensive margin results less convincing than those from the extensive margin. In particular, our finding that responsiveness to the two tax types varies by income on the *extensive* margin suggests the possibility that selection effects may confound our comparison of responsiveness on the *intensive* margin. Additionally, the positive point-estimate of the sales tax coefficient for high-income consumers, although not close to statistically significant, may indicate the presence of a selection effect or some other form of bias. A positive sales tax effect could also arise if the other goods in the sales tax base were strong substitutes for cigarettes; this possibility would bias our results if the substitution patterns between cigarettes and the other covered goods differed for high- and low-income consumers, a possibility explored in Appendix D.

[64] The results are similar when we include income as a linear interaction with the tax rates, or use the 20th or 30th income percentile to define the low-income group.

different income quartiles are reported in Tables 2.7 and 2.8 as well. The results suggest that attentiveness to cigarette register taxes declines monotonically by income.[65]

D. Tax Base Differences Between the Excise and Sales Tax

Our strategy for measuring attentiveness has been to compare consumer responsiveness to excise and sales tax rates. When demand for cigarettes depends only on the price of cigarettes and income, any gap between how consumers respond to the sales tax and how they respond to the excise tax implies a departure from the neoclassical model (as explained in Section II.B). In reality, the price of goods other than cigarettes may enter the cigarette demand function as well; if some of those other goods are also covered by the sales tax, the effect of a sales tax increase on cigarette demand will differ from the effect of an excise tax increase. This observation complicates our analysis because income differences in the attention gap may be due to differences in the excise and sales tax bases, rather than differences in attentiveness.

To clarify the nature of the problem, it will be helpful to discuss this tax-base effect in some detail. Under the neoclassical model, a tax can affect cigarette demand in two ways: by raising the price of cigarettes (a direct effect), and by raising the price of other goods (an indirect effect). Because the excise tax applies only to cigarettes, it generates only a direct effect. In contrast, the sales tax applies to many goods,[66] and consequently, it generates both a direct effect and an indirect effect on cigarette consumption. As a result, income differences in the attention gap could reflect both income differences in attentiveness as well as income differences in the nature of the sales

[65] An implicit assumption in our analysis (and throughout the smoking literature) is that changes in cigarette taxes are uncorrelated with unobserved shocks to individuals' cigarette consumption. However, cigarette taxes are not set randomly; a positive shock to cigarette demand might prompt state legislators to raise excise taxes to capture additional revenue. Although such correlations could provide an alternative explanation for the discrepancy between the excise and sales tax coefficients in Section II.B, it is more difficult to imagine them driving the heterogeneous attentiveness results in Section II.C. That is, although there are many possible reasons for cigarette taxes to be correlated with unobserved shocks to smoking demand, there are fewer plausible reasons why adoption of such laws would be differently correlated with shocks to cigarette demand for high and low-income consumers. Moreover, to the extent that policymakers do consider cigarette demand by high- and low-income consumers differently when setting tax rates, it would be surprising if they took such behavior into account when setting the *sales tax* (for which cigarette sales constitute only a small fraction of total revenue). So although it appears unlikely that the endogenous adoption of tax laws is driving our main results, we cannot rule that possibility out definitively.

[66] Approximately 40 percent of retail sales, according to CLK.

tax's indirect effect. In particular, if the indirect effect of the sales tax on cigarette demand were more negative for low-income consumers than for high-income ones, it could be that a tax base effect rather than changing attentiveness is driving our results. That is, low-income consumers' greater responsiveness to the sales tax could stem from income differences in how consumers adjust cigarette demand in response to price changes on other sales-taxed good.

Might income differences in the indirect effect of the sales tax be driving our results? It is difficult to dismiss this possibility out of hand. The indirect effect of the sales tax can be decomposed into an income effect and a substitution effect. By raising the price of many goods at once, the sales tax diminishes consumers' purchasing power, causing them to reduce their consumption of cigarettes (the income effect). In addition, raising the price of other goods might cause consumers to substitute toward or away from cigarettes, depending on whether the other goods covered by the sales tax are primarily substitutes or complements to cigarettes (the substitution effect). In theory, either of these effects could be more negative for low-income consumers. For example, the other sales-taxed goods could be important substitutes with cigarettes for well-off consumers, but not for low-income consumers. Similarly, the loss in real income associated with a sales tax increase could induce a bigger reduction in cigarette demand for low-income consumers.

Although we are unable to reject the possibility, we present two pieces of evidence that tax base effects are not responsible for all of the observed differences in consumer behavior by income. Our first check is motivated by the fact that some states impose a general sales tax, but exempt cigarettes from it.[67] In states that exempt cigarettes from the sales tax, changes in the sales tax rate would not directly affect the price of cigarettes; the sales tax would not have a direct effect on cigarette consumption. However, sales tax changes would still affect the price of other sales tax-eligible goods. Hence, the indirect effect of the sales tax would still occur. Consequently, analyzing the effect of the sales tax in cigarette-exempting states allows us to measure income differences in the indirect effect of the sales tax.

If indirect effects were responsible for the observed differences in responsiveness to the sales

[67] In our sample, seven states exempt cigarettes from the sales tax base for at least one year.

tax by income, responsive to the sales tax should decline by income as much in states that exempt as in states that do not. Table 2.9 compares the effect of the sales tax in states that exempt cigarettes from the sales tax ("exempt states") with the effect of the sales tax in states that include cigarettes in the sales tax base ("non-exempt states"). To do so, we modify our econometric model to allow heterogeneity in the effect of the sales tax between exempt and non-exempt states:

$$\begin{aligned} y_{ismt} \;=\;& \alpha+\beta_1\tau^e_{smt}+\beta_2\tau^s_{smt}*E_s+\beta_3\tau^s_{smt}*(1-E_{st})+ \\ & \rho_1\tau^e_{smt}LI_{ismt}+\rho_2\tau^s_{smt}LI_{ismt}*E_s+\rho_3\tau^s_{smt}LI_{ismt}*(1-E_{st})+ \\ & \phi E_{st}+\eta LI_{ismt}+\gamma x_{smt}+\delta z_{ismt}+\mu_s+\lambda_t+\pi_m+\varepsilon_{ismt} \end{aligned} \tag{2.13}$$

where E_{st} indicates whether state s exempts cigarettes from the sales tax base in year t.

Table 2.9 presents the results of this analysis. In all specifications, the small number of state-year cells in the exempt category makes inference difficult. Columns 1 - 3 show that the effect of the sales tax on cigarette demand appears to vary substantially more by income in non-exempt states than in exempt states. The estimated sales*low-income coefficient in exempt states, ρ_2, is small in size and is statistically insignificant on both the extensive and intensive margins. In contrast, the sales*low-income coefficient in the non-exempt states, ρ_3, remains large and statistically significant. On the extensive margin, we are able to reject the hypothesis that the sales*low-income coefficient in the exempt states is as large as in the non-exempt states. A concern with these specifications is that high- and low-income consumers may exhibit different smoking behavior in exempt versus non-exempt state-years, independent of the sales tax. To address this possibility, Columns 4 - 6 introduce an interaction for low-income*exempt.[68] On the extensive margin, Column 4 shows that the estimated effect of the sales tax on low-income consumers is only slightly more negative in non-exempt versus exempt states. In contrast, the intensive margin results in Column 5 are similar to those reported in Column 2: the point estimate of the sales*low-income coefficient is

[68]The low-income*exempt interaction is not statistically significant in any of the specifications.

substantially more negative in exempt states, but the large standard errors on the sales*low-income coefficient for exempt states make conclusions of statistical significance impossible.

We also present a second check that tax base effects are not driving our results. Tax base effects are most likely to dampen the impact of the sales tax relative to the excise tax when the excise tax exempts important substitutes for cigarettes.[69] Because other tobacco products constitute likely substitutes for cigarettes, there is less potential for tax base differences to play a role in states where the excise tax also applies to other tobacco products. Consequently, we restrict the analysis in Section II.C to states that apply the excise tax to cigars and smokeless tobacco. Table 2.10 shows that the difference in sales tax responsiveness between high- and low-income consumers persists after restricting the sample to those states.

In summary, there are plausible reasons to believe that differences in the excise and sales tax bases could generate results similar to those presented in Section II.C. However, the evidence in Tables 2.9 and 2.10, while not conclusive, suggests that tax base effects cannot fully supplant attentiveness as an explanation for the large differences in behavior that we find between high- and low-income consumers.

E. Robustness Checks

1. Including Pre-Tax Prices in the Regression

One variable not included in our basic econometric model is the pre-tax price of cigarettes. On the one hand, the pre-tax price depends on both supply and demand; including it as a regressor could bias our results if it were correlated with unobserved shocks to consumer demand (the classic simultaneous systems problem). On the other hand, the pre-tax price enters the consumer's demand function symmetrically with the excise and sales tax rates; excluding it from the regression may create an omitted variable bias if pre-tax price fluctuations were not equally correlated with the

[69] For example, raising the excise tax might reduce cigarette demand substantially by inducing cigarette smokers to switch to cigars. In contrast, raising the sales tax would raise the price of both cigarettes and cigars, dampening the effect on cigarette consumption.

two tax types for high- and low-income consumers.[70]

In this section, we modify our empirical strategy to account for the pre-tax price of cigarettes. Whereas previously we compared sales tax changes to excise tax changes, we now compare sales tax changes to changes in the posted price of cigarettes (the pre-tax price plus the excise tax). As before, this approach isolates income differences in attentiveness rather than changing price-sensitivity. The econometric model takes the following form:

$$y_{ismt} = \alpha + \beta_1 pp_{smt} + \beta_2 \tau^s_{smt} + \rho_1 pp_{smt} LI_{ismt} + \rho_2 \tau^s_{smt} LI_{ismt} + \gamma x_{smt} + \delta z_{ismt} + \mu_s + \lambda_t + \pi_m + \varepsilon_{ismt} \tag{2.14}$$

where *pp* represents the (excise tax inclusive) log posted price of cigarettes. To address the possible correlation between pre-tax prices and unobserved demand shocks, we utilize the excise tax as a supply shifter. In particular, we employ the excise tax (τ^e) and the excise*low-income interaction ($\tau^e * LI$) as instruments for the posted price (pp) and the posted price*low-income interaction ($pp * LI$). This identification strategy is valid under the same assumptions as the main specification, namely that cigarette tax changes are uncorrelated with unobserved shocks to cigarette demand. Tables 2.11 and 2.12 show that the results from the IV specification are similar to the specifications that omit pre-tax prices. Table 2.11 shows that both excise and sales tax changes are passed on slightly differently for high- and low-income consumers (e.g. retailers may decide how much to raise prices based on neighborhood income) but that these differences are quite small in magnitude, particularly for the sales-tax. Table 2.12 confirms that these differential pass-through rates do not drive our finding of increasing attentiveness by income.

2. Additional Robustness Checks

Appendix D investigates the robustness of our analysis to three additional concerns. First, our use of a two-part model for smoking demand may be biased by changes to the composition of the smoking population. To investigate this issue, we estimate smoking demand for the entire

[70] For example, Harding, Leibtag and Lovenheim (Forthcoming) find that excise taxes are passed through differently to high- and low-income consumers.

population with a linear regression and with a Tobit model censored at zero. Second, our results could reflect differences in the amount of time it takes high- and low-income consumers to learn about sales tax changes. Consequently, we include lagged tax rate values to determine whether the attentiveness gap fades over time. Finally, we try including state-specific time trends to account for the possibility that changes in a state's tax rates are correlated with unobserved trends in that state's smoking demand (such as anti-smoking sentiment). As detailed in the Appendix, all three robustness checks are consistent with the results of the main analysis.

III. Conclusion

Policymakers at all levels of government depend on commodity taxes to raise revenue, but such taxes are typically regressive, constituting a greater burden for low-income consumers. This paper has suggested a novel way for policymakers to lessen that regressivity: manipulating the fraction of the tax that is levied at the register as opposed to being included in a good's posted price. In particular, we showed that levying a greater proportion of a commodity tax at the register shifts the tax's burden away from attentive consumers. When low-income consumers pay more attention to register taxes than high-income consumers do, designing a tax in this way can lessen its regressivity. Conversely, when high-income consumers are the more attentive, imposing a commodity tax at the register will exacerbate its regressivity.

With this motivation in mind, we investigated whether high- and low-income consumers respond differently to register taxes on cigarettes. Exploiting state and time variation in tax rates, we found that low-income consumers reduce cigarette demand in response to both excise and sales taxes on cigarettes, whereas higher-income consumers only reduce cigarette demand in response to excise taxes. Although the empirical results do not allow us to definitively rule out alternative explanations, our findings are consistent with the hypothesis that attentiveness to cigarette register taxes declines by income. Hence, policymakers may be able to ease the financial burden of cigarette taxes on the poor by levying such taxes at the register instead of including them in cigarettes' posted price.

How important are these welfare effects quantitatively? To provide a rough idea, recall from Part I that the welfare effect for attentive consumers of a revenue-neutral shift towards register taxes stemmed from the effect of the shift on the after-tax price of *x*, $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}}$.[71] From Equation (2.5), we can express the combined tax change in terms of estimable quantities, $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}} = -\frac{\tau\varepsilon_B\phi_B}{1-\tau(\varepsilon_A\phi_A+\varepsilon_B\phi_B)}$. In our sample, the (weighted) average ratio of taxes to the after-tax price is $\tau = 0.33$. Determining the share of cigarettes consumed by attentive consumers (ϕ_A) is complicated by the fact that our empirical procedure is designed to assess whether income differences in attentiveness exist, rather than identify exactly which consumers are attentive and which are not. In particular, our results suggest that the bottom income quartile of consumers are more attentive to cigarette register taxes than higher income consumers, but the evidence in Tables 2.7 and 2.8 is consistent with consumers in the second income quartile also falling into the attentive group. To be conservative, we compute the welfare effect assuming that only consumers below the 50th income percentile are in the attentive group; the magnitude of the effect increases when the attentive group is defined as consumers with income below the 25th percentile. From Table 2.2, we know that consumers above the 50th income percentile consume approximately 48 percent of cigarettes, so that $\phi_A = 0.52$ and $\phi_B = 0.48$. From Table 2.8, we compute the overall elasticity of cigarette demand with respect to the posted price to be $\varepsilon_A = 0.84$ and $\varepsilon_B = 0.96$. Using (2.5), these values imply $\left.\frac{d(t_p+t_r)}{dt_r}\right|_{\overline{R}} = -0.21$, so that a \$1.00 increase in the cigarette register tax could accommodate a \$1.21 reduction in the cigarette posted tax. For perspective, that revenue-neutral shift would free up approximately \$77 a year for an attentive consumer who smokes a pack of cigarettes per day.[72]

Three qualifications are important when interpreting our results. First, we have treated cigarettes as a standard consumption good, abstracting away from their addictive nature. However, the fact that cigarettes are addictive could alter the welfare implications of our results. For example, mod-

[71] For this approximation, we ignore the effect of the shift on the pre-tax price of *x*. As Appendix B shows, that omission is justified when posted cigarette taxes are fully passed on to consumers, a condition consistent with the results in Table 2.10.

[72] For comparison, defining the inattentive group threshold at the 25th income percentile implies that a revenue-neutral \$1.00 increase in the register tax accommodates a \$1.34 reduction in the posted tax, resulting in yearly savings of \$123 for an attentive pack-a-day smoker.

els along the lines suggested by Gruber and Koszegi (2004) or Gruber and Mullainathan (2002) suggest that cigarette taxes can benefit consumer welfare when voters adopt such taxes as a method of exercising self-control; consequently, shifting a cigarette tax to the register could deprive some consumers of a valuable tool for self-discipline. At the other extreme, a rational-addiction model such as that presented in Becker, Grossman and Murphy (1994) would imply that cigarette consumption decisions are informed by consumers' expectations concerning future prices; if such expectations are important, the demand equations employed here are misspecified.

Second, readers should be cautious about extrapolating our results to goods other than cigarettes. Although we have presented some evidence that attentiveness to cigarette register taxes declines by income, the cognitive cost model presented in Appendix C highlights the fact that this result can vary between goods. In particular, low-income consumers may well be less attentive to register taxes on goods that are relatively sensitive to income and that constitute a larger share of expenditures for high-income consumers. Moreover, Appendix A shows that in certain makets, shifting to a register tax has the potential to induce producers to raise a good's pre-tax price. In particular, for goods characterized by elastic demand and inelastic supply, shifting to a register tax could actually worsen the burden of those taxes on all consumers, including the poor.

Finally, much of our analysis implicitly assumes that consumers' attentiveness to register taxes is fixed. In reality, however, a revenue tax increase may drive some inattentive consumers to *become* attentive by increasing the utility loss from ignoring the tax (discussed in Appendix C). If one endogenizes the boundaries of the attentive and inattentive groups, a sufficiently large shift towards register taxes could necessitate a net increase in the combined tax rate if the register tax's revenue advantage was more than offset by the reduction in revenue caused by some inattentive consumers becoming attentive. Similarly, our empirical specifications may be incomplete if high-income consumers' attentiveness to cigarette register taxes depends on the size of the register tax already in place. Although our data lack the power to confirm that theoretical prediction convincingly, policymakers should be cautious before adopting large shifts towards register taxes on the basis of results like ours.

Although our discussion has focused on taxes designed to raise revenue, the empirical findings presented here also speak to broader questions of tax design. For example, a number of public health advocates have suggested raising taxes on soft drinks as a way to combat population obesity, with some proponents calling for an expanded tax of any form on those products (Engelhard, Garson and Dorn (2009)) and others arguing that including the tax in the posted product price would be most effective (Brownell et al. (2009)). Our results suggest an important consideration is missing from this discussion, namely that taxes imposed at the register may affect the eating habits of high- and low-income consumers in different ways. Such issues deserve further investigation.

Table 2.1: Summary of Cigarette Tax Changes

	Excise Tax		Sales Tax		Pre-Tax Price	
	1984	2000	1984	2000	1984	2000
Minimum	\$0.29	\$0.36	0.0%	0.0%	\$0.91	\$1.84
Maximum	\$0.68	\$1.43	7.5%	7.5%	\$1.28	\$2.47
Mean	\$0.50	\$0.75	3.8%	5.0%	\$1.03	\$2.20
# State Changes	91		45			
# Federal Changes	3		n/a			

Excise taxes and prices are denoted in 2000 dollars.

Table 2.2: Average Cigarette Consumption by Income Quartile

	Smoking Rate (Extensive Margin, %)			Daily Consumption among Smokers (Intensive Margin, cigarettes)			Share of Total Consumption (%)
	1984	2000	All Years	1984	2000	All Years	All Years
Q1	30.1	21.7	25.3	18.3	17.7	17.7	24.6
Q2	29.2	20.1	25.1	19.8	18.2	18.5	27.6
Q3	29.1	17.4	22.5	20.6	18.2	19.1	26.9
Q4	23.7	11.9	16.8	21.1	18.1	19.5	20.8
All	27.8	17.6	22.3	20.0	18.0	18.7	100.0

Table 2.3: Effect of Taxes on Cigarette Demand - Extensive and Intensive Margins

	Extensive Margin			Intensive Margin		
	(1)	(2)	(3)	(4)	(5)	(6)
Excise Tax	-0.127***	-0.116***	-0.117***	-0.358***	-0.336***	-0.341***
	(0.030)	(0.026)	(0.026)	(0.049)	(0.055)	(0.056)
Sales Tax	-0.261*	-0.132	-0.132	-0.136	-0.019	-0.022
	(0.140)	(0.100)	(0.101)	(0.273)	(0.289)	(0.290)
Income	-0.099***	-0.099***	-0.075***	0.007	0.007	0.058**
	(0.005)	(0.005)	(0.008)	(0.019)	(0.019)	(0.027)
Female	-0.039***	-0.039***	-0.039***	-0.187***	-0.187***	-0.187***
	(0.003)	(0.003)	(0.003)	(0.007)	(0.007)	(0.007)
White	0.082***	0.082***	0.082***	0.452***	0.452***	0.452***
	(0.006)	(0.006)	(0.006)	(0.026)	(0.026)	(0.026)
H.S. Grad	-0.062***	-0.062***	-0.062***	-0.058***	-0.058***	-0.058***
	(0.009)	(0.009)	(0.009)	(0.014)	(0.014)	(0.014)
College Grad	-0.123***	-0.123***	-0.123***	-0.144***	-0.144***	-0.144***
	(0.005)	(0.005)	(0.005)	(0.009)	(0.009)	(0.009)
Married	-0.067***	-0.067***	-0.067***	-0.019***	-0.019***	-0.019***
	(0.002)	(0.002)	(0.002)	(0.006)	(0.006)	(0.006)
Unemployed	0.086***	0.087***	0.087***	0.054***	0.055***	0.056***
	(0.004)	(0.004)	(0.005)	(0.007)	(0.007)	(0.007)
Age	0.034***	0.035***	0.034***	0.018	0.018	0.018
	(0.005)	(0.005)	(0.005)	(0.012)	(0.012)	(0.012)
Age2	-0.001***	-0.001***	-0.001***	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log Unemp. Rate		-0.028***	-0.028***		-0.020	-0.021
		(0.008)	(0.008)		(0.013)	(0.013)
Log State Income		-0.014	-0.014		0.063	0.065
		(0.029)	(0.029)		(0.058)	(0.058)
Income Trend			-0.003**			-0.006***
			(0.001)			(0.001)
Economic Conditions		x	x		x	x
Income Trend			x			x
F-stat	0.97	0.02	0.02	0.59	1.05	1.05
prob>F	0.33	0.88	0.89	0.45	0.31	0.31
N	1,288,031	1,288,031	1,288,031	274,137	274,137	274,137

Standard errors clustered at the state level in parentheses.
All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.
Third- and fourth-order age polynomials are included in the regression but not displayed.
Outcome variables: probability of smoking (extensive) and log cigarette demand (intensive).
The F-stat is for the test of equality between the excise tax and the sales tax coefficients.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Effect of Taxes on Cigarette Demand - Combined Effect

	(1)	(2)	(3)
Excise Tax	-3.749***	-3.508***	-3.577***
	(0.481)	(0.479)	0.479
Sales Tax	-5.007**	-2.350	-2.366
	(2.156)	(2.200)	2.199
Economic Conditions		x	x
Income Trend			x
F-stat	0.30	0.25	0.27
prob>F	0.58	0.62	0.60
N	1,288,031	1,288,031	1,288,031

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.

Outcome variable: cigarette demand in levels.

The F-stat is for the test of equality between the excise tax and the sales tax coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Effect of Taxes on Cigarette Demand by Income - Extensive and Intensive Margins

	Extensive Margin			Intensive Margin		
	(1)	(2)	(3)	(4)	(5)	(6)
Excise Tax	-0.152***	-0.141***	-0.132***	-0.391***	-0.370***	-0.346***
	(0.030)	(0.028)	(0.027)	(0.053)	(0.061)	(0.060)
Sales Tax	-0.152	-0.023	-0.025	0.211	0.328	0.318
	(0.135)	(0.114)	(0.115)	(0.311)	(0.340)	(0.340)
Excise*Low-income	0.099*	0.099*	0.058	0.119	0.121	0.020
	(0.054)	(0.054)	(0.056)	(0.086)	(0.086)	(0.112)
Sales*Low-income	-0.502**	-0.502**	-0.501***	-1.389**	-1.390**	-1.379**
	(0.191)	(0.191)	(0.182)	(0.668)	(0.669)	(0.663)
Income	-0.125***	-0.125***	-0.103***	-0.027**	-0.027**	0.027
	(0.005)	(0.005)	(0.010)	(0.013)	(0.013)	(0.024)
Income Trend			-0.002**			-0.007***
			(0.001)			(0.002)
Economic Conditions		x	x		x	x
Income Trend			x			x
F-stat	9.91	9.74	8.60	5.48	5.49	5.14
prob>F	0.00	0.00	0.01	0.02	0.02	0.03
N	1,288,031	1,288,031	1,288,031	274,137	274,137	274,137

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.

Outcome variables: probability of smoking (extensive) and log cigarette demand (intensive).

The F-stat is associated with the test for equality between the excise*low-income and sales*low-income interaction coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Effect of Taxes on Cigarette Demand by Income - Combined Effect

	(1)	(2)	(3)
Excise Tax	-4.420***	-4.184***	-3.846***
	(0.496)	(0.494)	(0.494)
Sales Tax	-2.290	0.365	0.265
	(2.196)	(2.242)	(2.239)
Excise*Low-income	2.543***	2.554***	1.047*
	(0.544)	(0.544)	(0.546)
Sales*Low-income	-11.987***	-11.989***	-11.888***
	(1.915)	(1.916)	(1.917)
Income	-2.375***	-2.375***	-1.565***
	(0.079)	(0.079)	(0.147)
Income Trend			-0.094***
			(0.012)
Economic Conditions		x	x
Income Trend			x
F-stat	45.45	45.52	36.17
prob>F	0.00	0.00	0.00
N	1,288,031	1,288,031	1,288,031

Standard errors clustered at the state level in parentheses.
All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.
Outcome variable: cigarette demand in levels.
The F-stat is associated with the test for equality between the excise*low-income and sales*low-income interaction coefficients.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Effect of Taxes on Cigarette Demand by Income Quartile - Extensive and Intensive Margins

	Extensive Margin			Intensive Margin		
	Excise	Sales	F-stat	Excise	Sales	F-stat
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline (Q1)	-0.081	-0.521***		-0.329***	-1.072*	
	(0.056)	(0.166)		(0.101)	(0.566)	
Excise*Q2	-0.104**	0.284*	6.09**	0.015	0.883	3.04*
	(0.044)	(0.153)	(0.02)	(0.137)	(0.536)	(0.09)
Excise*Q3	-0.050	0.574**	6.88***	-0.019	1.551*	4.19**
	(0.067)	(0.220)	(0.01)	(0.103)	(0.802)	(0.05)
Excise*Q4	0.002	0.628***	9.92***	-0.069	1.875**	5.56**
	(0.072)	(0.190)	(0.00)	(0.130)	(0.839)	(0.02)
N	1,288,031	1,288,031	1,288,031	274,137	274,137	274,137

Standard errors clustered at the state level in parentheses in columns 1, 2, 4, and 5.

All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.

Outcome variables: probability of smoking (extensive) and log cigarette demand (intensive).

Tax*income group interactions represent the difference between that income group's sensitivity to the tax rate and the baseline group's sensitivity to the tax rate.

The F-stats are associated with testing $\rho_{2,j} - \rho_{1,j} = 0$ for j in $\{2,3,4\}$. Prob>F in parentheses in columns 3 and 6.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Effect of Taxes on Cigarette Demand by Income Quartile - Combined Effect

	Excise	Sales	F-stat
	(1)	(2)	(3)
Baseline (Q1)	-2.917***	-11.575***	
	(0.641)	(2.663)	
Excise*Q2	-1.628**	7.055***	10.85***
	(0.665)	(2.347)	(0.00)
Excise*Q3	-0.999	13.686***	33.50***
	(0.643)	(2.260)	(0.00)
Excise*Q4	-0.279	14.810***	33.65***
	(0.660)	(2.323)	(0.00)
N	1,288,031	1,288,031	1,288,031

Standard errors clustered at the state level in parentheses in columns 1 and 2.
All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.
Outcome variable: cigarette demand in levels.
Tax*income group interactions represent the difference between that income group's sensitivity to the tax rate and the baseline group's sensitivity to the tax rate.
The F-stats are associated with testing $\rho_{2,j} - \rho_{1,j} = 0$ for j in $\{2,3,4\}$.
Prob>F in parentheses in column 3.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Sales Tax Exemptions for Cigarettes

	Extensive (1)	Intensive (2)	Combined (3)	Extensive (4)	Intensive (5)	Combined (6)
Excise Tax	-0.124***	-0.338***	-3.701***	-0.124***	-0.333***	-3.693***
	(0.029)	(0.061)	(0.688)	(0.029)	(0.061)	(0.688)
Excise*Low-income	0.045	-0.016	0.794	0.047	-0.035	0.772
	(0.060)	(0.114)	(1.395)	(0.060)	(0.115)	(1.401)
Sales Tax*Non-exempt	-0.451*	0.334	-7.146	-0.453*	0.358	-7.151
	(0.262)	(0.572)	(6.656)	(0.262)	(0.573)	(6.656)
Sales*Low-income*Non-exempt	-0.501**	-1.450*	-11.872**	-0.489*	-1.587**	-11.884**
	(0.249)	(0.755)	(5.812)	(0.253)	(0.762)	(5.911)
Sales Tax*Exempt	-0.392	-1.398	-13.158	-0.281	-2.037	-10.516
	(0.364)	(1.355)	(9.193)	(0.338)	(1.752)	(9.538)
Sales*Low-income*Exempt	0.001	-0.276	0.224	-0.420	1.553	-9.849
	(0.346)	(0.962)	(7.000)	(0.473)	(2.347)	(13.450)
Low-income	-0.016	0.023	-0.201	-0.016	0.030	-0.202
	(0.018)	(0.048)	(0.448)	(0.018)	(0.048)	(0.456)
Exempt	2.982**	2.813	75.639***	3.092**	-5.541	49.926*
	(1.360)	(3.048)	(28.828)	(1.226)	(3.932)	(27.631)
Exempt*Low-income				0.018	-0.002	0.718
				(0.033)	(0.122)	(0.815)
F-stat	9.92	1.94	23.00	0.02	1.60	0.02
prob>F	0.00	0.17	0.00	0.90	0.21	0.89
N	1,288,031	274,137	1,288,031	1,288,031	274,137	1,288,031

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics, state, year, and calendar month fixed effects, and linear time trends interacted with exempt and low-income. Columns 4-6 include linear time trends in exempt*low-income.

Outcome variables: probability of smoking (extensive), log cigarette demand (intensive), and cigarette demand in levels (combined).

The F-stat is for the test of equality between the sales*low-income interaction between states that exempt cigarettes from the sales tax and states that do not.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: States that Apply Excise Tax to Other Tobacco Products

	Extensive Margin (1)	Intensive Margin (2)	Combined Effect (3)
Excise Tax	-0.081***	-0.309***	-2.980***
	(0.026)	(0.101)	(0.625)
Sales Tax	0.319*	1.060	6.686**
	(0.163)	(0.805)	(2.737)
Excise*Low-income	0.066	0.101	1.419
	(0.064)	(0.110)	(1.471)
Sales*Low-income	-0.757***	-1.512**	-16.982***
	(0.178)	(0.690)	(4.503)
F-stat	17.52	5.47	15.22
prob>F	0.00	0.02	0.00
N	904,206	185,740	904,206

Standard errors clustered at the state level in parentheses.
All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.
Outcome variables: probability of smoking (extensive), log cigarette demand (intensive), and cigarette demand in levels (combined).
The F-stat is associated with the test for equality between the excise-poor and sales-poor interaction coefficients.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11: Instrumenting for Price with Excise Tax - First Stage

	Extensive Margin		Intensive Margin	
	(1)	(2)	(3)	(4)
Excise Tax	1.018***	-0.296***	1.126***	-0.332***
	(0.182)	(0.058)	(0.161)	(0.068)
Sales Tax	0.822*	0.185	0.793*	0.183
	(0.466)	(0.137)	(0.432)	(0.149)
Excise*Low-income	-0.041***	2.284***	-0.050***	2.298***
	(0.011)	(0.096)	(0.018)	(0.105)
Sales*Low-income	0.009	0.138	0.008	0.127
	(0.020)	(0.309)	(0.028)	(0.314)
R-square	0.95	0.98	0.96	0.98
N	1,288,031	1,288,031	274,137	274,137

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.

(1) and (3): Dependent variable = excise tax-inclusive price

(2) and (4): Dependent variable = excise tax-inclusive price*low-income

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.12: Instrumenting for Cigarette Prices with Excise Tax

	Extensive Margin		Intensive Margin		Combined Effect	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Posted Price	-0.051***	-0.124***	-0.093***	-0.306***	-1.301***	-3.533***
	(0.013)	(0.030)	(0.029)	(0.067)	(0.250)	(0.781)
Sales Tax	-0.040	0.070	0.227	0.557	-0.477	2.992
	(0.116)	(0.117)	(0.348)	(0.384)	(2.739)	(2.984)
Posted Price*Low-income	0.019	0.026	0.016	0.003	0.372	0.444
	(0.016)	(0.025)	(0.040)	(0.050)	(0.399)	(0.606)
Sales*Low-income	-0.492***	-0.495***	-1.411**	-1.368**	-11.547***	-11.723***
	(0.179)	(0.182)	(0.679)	(0.654)	(4.431)	(4.362)
F-test	8.44	8.04	4.68	4.73	7.80	8.14
prob>F	0.01	0.00	0.04	0.03	0.01	0.00
N	1,288,031	1,288,031	274,137	274,137	1,288,031	1,288,031

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.

Outcome variables: probability of smoking (extensive), log cigarette demand (intensive), and cigarette demand in levels (combined).

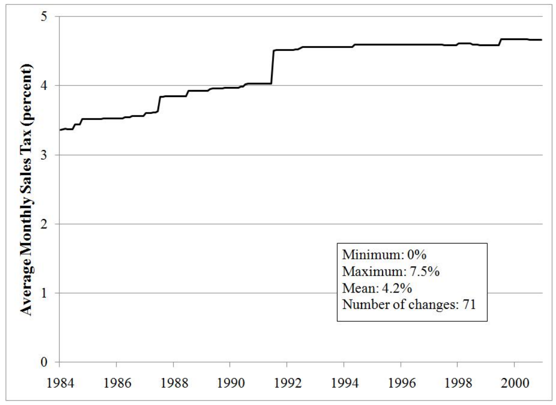
Price includes the excise tax.

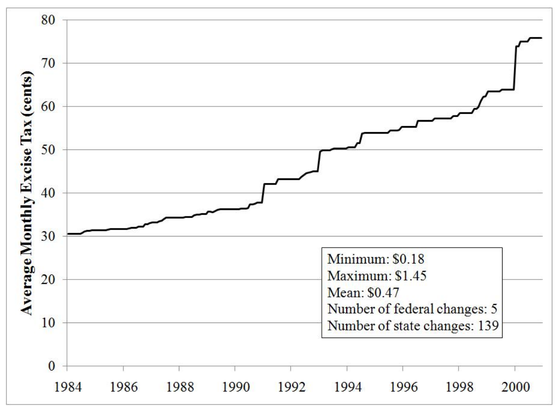
The F-stat is for the test of equality between the price*low-income and sales*low-income interaction coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.1: Average Monthly Taxes, 1984-2000

(a) Sales Tax





(b) Excise Tax

Figure 2.2: Aggregate Cigarette Consumption

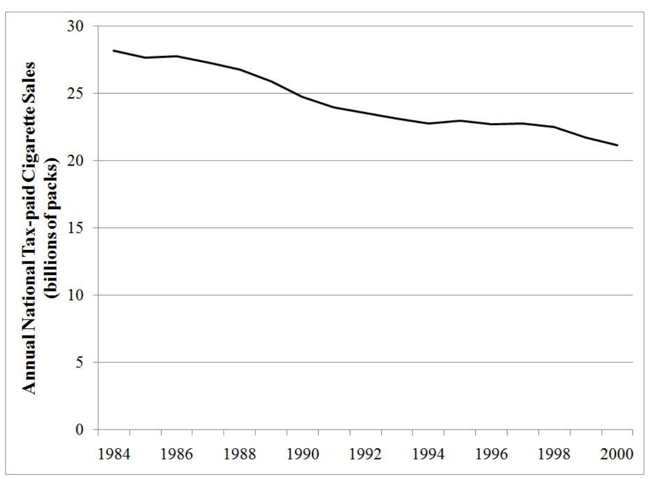
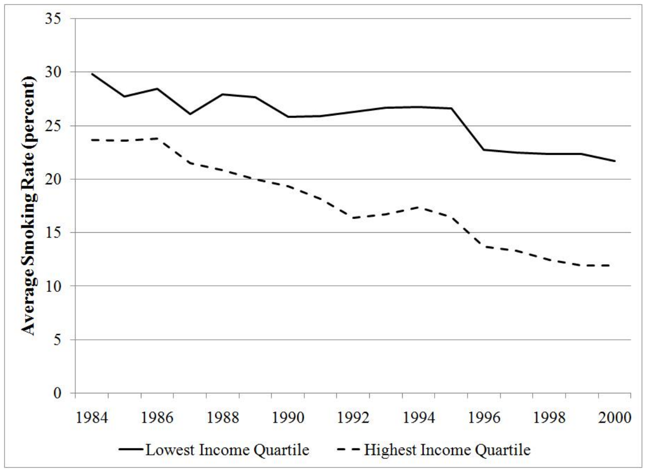


Figure 2.3: Smoking Rates by Income



IV. Appendices

A. Welfare Analysis under Endogenous Producer Prices

This Appendix expands the model developed in Part I to the setting in which firms adjust their prices in response to changes in the type of tax imposed. As before, the policy we consider is an increase in the register tax coupled with a reduction in the posted tax calibrated to keep government revenue unchanged. Like CLK, we assume that taxes on x are fully-salient for producers.

Let p_x denote the after-tax price of p, $p_x \equiv p + t_p + t_r$. The net effect of the shift on the after-tax price of x is given by

$$\left.\frac{dp_x}{dt_r}\right|_{\overline{R}} = \left.\frac{\partial p}{\partial t_r}\right|_{\overline{R}} + \left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} + 1 \tag{2.15}$$

Applying the same approach as in Part I, it is straightforward to show that the welfare effects of the shift for the two types of agents are given by

$$\left.\frac{dV_A}{dt_r}\right|_{R} = -U_y(x_A, y_A), x_A \left(1 + \left.\frac{\partial p}{\partial t_r}\right|_{R} + \left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}}\right) \tag{2.16}$$

and

$$\left.\frac{dV_B}{dt_r}\right|_{\overline{R}} = -U_y\left(x_B, y_B\right)x_B\left(1 + \left.\frac{\partial p}{\partial t_r}\right|_R + \left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}}\right) + \left(\left.\frac{\partial p}{\partial t_r}\right|_R + \left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}}\right)\frac{\partial x_B}{\partial p}\mu \tag{2.17}$$

Let $s(p)$ denote the supply of x as a function of x's pre-tax price (p), so that the price-elasticity of supply is given by $\varepsilon^S \equiv \frac{\partial s(p)}{\partial p}\frac{p_x}{s}$. Moreover, supply and demand of x must be equal in equilibrium:

$$s(p) \equiv x_A(p + t_p + t_r) + x_B(p + t_p) \tag{2.18}$$

Totally differentiating (2.18) along with the government's revenue constraint yields:

$$\left.\frac{\partial p}{\partial t_r}\right|_R = \frac{\varepsilon_B \phi_B}{\overline{\varepsilon} + \varepsilon^S(1 - \tau\overline{\varepsilon})} \tag{2.19}$$

and

$$\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} = -\frac{1 - \gamma\tau\varepsilon_A\phi_A}{1 - \gamma\tau\overline{\varepsilon}} \tag{2.20}$$

where $\overline{\varepsilon} \equiv \varepsilon_A\phi_A + \varepsilon_B\phi_B$ and $\gamma \equiv \frac{\varepsilon^S}{\varepsilon^S + \overline{\varepsilon}}$.[73]

Equation (2.19) shows that for $\gamma < 1$, a revenue-neutral shift towards register taxes results in a higher pre-tax price for all consumers. Because some consumers are more sensitive to posted taxes than to register taxes, replacing the former with the latter allows producers to shift some of the tax's incidence back on to consumers. In turn, the higher pre-tax price reduces demand for x, necessitating a larger t_p than otherwise in order for the government to meets its revenue constraint. Consequently, the reduction in the combined tax rate accommodated by the shift is smaller than when producer prices are fixed.

To illustrate, suppose that the supply of x is completely inelastic, $\varepsilon^S = 0$. What are the effects

[73]Recall that $\tau \equiv \frac{t_p + t_r}{p + t_p + t_r}$, $\varepsilon_i \equiv -\frac{\partial x_i}{\partial p}\frac{p + t_p + t_r}{x_i}$ and $\phi_i \equiv \frac{x_i}{x_i + x_{-i}}$. Note that $\tau\overline{\varepsilon} < 1$ follows from our maintained assumption that $\frac{\partial R}{\partial t_p} > 0$.

of a revenue-neutral increase in register taxes in this setting? As always, the increase in the register tax accommodates a reduction in the posted tax. Because $\varepsilon^S = 0$, producers had absorbed the entire incidence of the posted tax; as t_p is reduced, the pre-tax price rises one for one. If all consumers were inattentive, the story would end here; for a \$1 increase in the register tax, the posted tax would fall by $\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}}$ and the pre-tax price would rise by $\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}}$. When some consumers are attentive, the pre-tax price of x will fall somewhat in response to the new register tax; but as long as *some* consumers ignore the tax, producers will not have to reduce the pre-tax price in the full amount of the register tax increase. Hence the net effect of the shift on the after-tax price will be positive.

From (2.16), it is clear that a shift towards register taxes benefits attentive consumers if and only if the net effect of the shift on x's after-tax price is negative. By substituting (2.19) and (2.20) into (2.15), it follows that:

$$\left.\frac{dV_A}{dt_r}\right|_{R} > 0 \iff \left.\frac{dp_x}{dt_r}\right|_{\overline{R}} > 0 \iff \tau\varepsilon^S > 1 \tag{2.21}$$

Thus when ε^S is sufficiently small, shifting towards a register tax makes even the attentive consumers worse off.[74]

Similarly, $\tau\varepsilon^S > 1$ is a necessary condition for inattentive consumers to benefit from a shift towards register taxes. When $\tau\varepsilon^S \leq 1$, (2.21) implies that $\left.\frac{dp_x}{dt_r}\right|_{\overline{R}} \geq 0$, which in turn implies that the first term in (2.17) is non-positive. Also, from (2.19) and (2.20), one can show that $\left.\frac{\partial p}{\partial t_r}\right|_R + \left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} \leq 0$, implying that the second term in (2.17) is non-positive as well. Thus when $\tau\varepsilon^S < 1$, shifting from register to posted taxes makes all consumers worse off.

Finally, even when the supply of a taxed good is too inelastic for the government to raise welfare by shifting towards register taxes, the government's choice between posted and register taxes still has important effects on consumer welfare. In particular, when $\tau\varepsilon^S < 1$, the government

[74] Another way to understand this dynamic is to observe that B's inattentiveness to register taxes impose two distinct externalities on A. First, B's inattentiveness benefits A because it reduces the tax rate (which is levied on both A and B) needed for the government to obtain a given amount of revenue. Second, B's inattentiveness harms A vis-a-vis producers because it reduces the overall market sensitivity to higher prices for x. When some consumers are inattentive, demand for x does not fall as much in response to a given price increase, and consequently, producers do not have to reduce the pre-tax price of x by as much in order to maintain demand. As ε^S shrinks, the second externality grows in importance, and for small enough ε^S, the second externality will dominate the first.

can raise the welfare of all consumers through a revenue-neutral shift towards *posted* taxes – the opposite of the policy considered in Part I. Mechanically, this result follows directly from (2.16), (2.17), and (2.21). In words, when supply of the taxed good is sufficiently inelastic, producers will have to absorb the majority of the incidence of the new posted tax. Although the combined tax rate on x will increase, that increase will be more than offset by the reduction in the pre-tax price. Thus by increasing the salience of the tax for inattentive consumers, the government can precipitate a reduction in the market clearing price faced by attentive consumers. Attentive consumers are better off because of the net reduction in the after-tax price and inattentive consumers benefit both from the lower pre-tax price and because the associated reduction in register taxes reduces the magnitude of their optimization error.[75]

B. Welfare Analysis Under Alternate Budget Adjustment Rules

Part I assumed that inattentive consumers who misperceive the price of x satisfy their budget constraints by reducing expenditures on y. This Appendix considers the robustness of our results to alternate rules for mapping infeasible intended consumption bundles into feasible final consumption bundles.

In addition to the rule that we employ, Chetty, Looney, and Kroft (2007) identify two other "intuitive" budget adjustment rules. First, consumers who misperceive the price of x may satisfy their budget constraints by reducing expenditures on x rather than y. This rule represents the other end of the spectrum from the one that we employ, and would be appropriate if consumers purchased x after completing their purchases of all other goods. Under this rule, it is easy to show that:

$$\frac{\partial x_B}{\partial t_r} = \frac{-x_B}{p+t_r+t_p} \tag{2.22}$$

$$\frac{\partial x_B}{\partial t_p} = \frac{-\left(\frac{\partial y_B}{\partial p}+x_B\right)}{p+t_r+t_p} \tag{2.23}$$

[75] Of course, whether or not such a welfare transfer is socially desirable depends upon how one values the trade off between consumer welfare and producer surplus.

The second alternate budget adjustment considered by Chetty, Looney, and Kroft (2007) is for inattentive agents to reduce consumption of both x and y to make up the income lost to the register tax. Inattentive consumers ignore the register tax when making their consumption decisions, but recognize that their net-of-tax income is lower because of the tax. For example, consumers who purchase x and y repeatedly will eventually realize that they consistently have less money in their bank account than they had anticipated. Inattentive consumers whose behavior is described by this rule will fully account for the tax's income effect but fail to account for the tax's substitution effect. As a result, we have:

$$\frac{\partial x_B}{\partial t_r} = -x_B \frac{\partial x_B}{\partial I} \tag{2.24}$$

$$\frac{\partial x_B}{\partial t_p} = \frac{\partial x_B}{\partial t_r} + \frac{\partial \tilde{x}_B}{\partial p} \tag{2.25}$$

where $\frac{\partial \tilde{x}_B}{\partial p}$ represents Hicksian (compensated) demand.

As before, we consider the welfare effects of a revenue-neutral shift from posted to register taxes. Because the attentive agent optimizes correctly, the welfare effect for that agent is the same as before:

$$\left.\frac{dV_A}{dt_r}\right|_{\overline{R}} = -U_y\left(x_A, y_A\right) x_A \left(1 + \left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}}\right)$$

Totally differentiating the government's budget constraint yields an expression for the posted tax reduction associated with a revenue-neutral increase in the register tax:

$$\left.\frac{\partial t_p}{\partial t_r}\right|_{\overline{R}} = -\frac{x_A + x_B + (t_p + t_r)\left(\frac{\partial x_A}{\partial t_r} + \frac{\partial x_B}{\partial t_r}\right)}{x_A + x_B + (t_p + t_r)\left(\frac{\partial x_A}{\partial t_p} + \frac{\partial x_B}{\partial t_p}\right)}$$

A little algebra reveals that the welfare effect of the shift is positive for attentive consumers if and only if $\frac{\partial x_B}{\partial t_r} > \frac{\partial x_B}{\partial t_p}$, that is, when inattentive consumers reduce their demand for the taxed good by a larger amount in response to a posted tax increase than in response to a register tax increase. Intuitively, this condition ensures that the new register tax will be more effective at raising revenue than the old posted tax was. Consequently, the shift accommodates a reduction in the combined tax rate, thus generating a positive income effect. Using (2.22) - (2.25), it is easy to see that this condition is satisfied under the two alternate budget adjustment rules.[76]

The welfare analysis for inattentive consumers proceeds as in Part I. Under the first alternate rule,

$$\frac{dV_B}{dt_r}\bigg|_{\overline{R}} = -\left(1+\frac{\partial t_p}{\partial t_r}\bigg|_{\overline{R}}\right)U_x(x_B,y_B)\left(\frac{x_{\mathbf{B}}}{p+t_r+t_p}\right)+\frac{\left(\frac{\partial y_B}{\partial p}\right)\frac{\partial t_p}{\partial t_r}\Big|_{\overline{R}}}{(p+t_r+t_p)}\left(U_y(x_A,y_A)(p+t_r+t_p)-U_x(x_A,y_A)\right)$$

Like the result in Part I, the welfare effect for inattentive consumers is ambiguous under this rule. Shifting to a register tax accommodates a reduction in the combined tax rate, generating a positive welfare effect (captured by the first term). Unlike before, however, the magnitude of this effect depends on the marginal utility of x rather than y because providing the consumer with additional income reduces the amount that the consumer must reduce her consumption of x to satisfy the budget constraint. The second term represents the cost of optimization error. Like before, this cost is zero when there are no register taxes and grows in size as register taxes push inattentive consumers further from their optimal bundle.

Under the second alternate rule, the welfare effect of the shift for inattentive consumers is also similar to that found in Part I. Here the welfare effect is given by

$$\frac{dV_B}{dt_r}\bigg|_{\overline{R}} = -\left(1+\frac{\partial t_p}{\partial t_r}\bigg|_{\overline{R}}\right)U_y(x_B,y_B)x_B+\left(\frac{\partial x_B}{\partial p}\,\frac{\partial t_p}{\partial t_r}\bigg|_{\overline{R}}-x_B\frac{\partial x_B}{\partial I}\right)\left(U_x(x_B,y_B)-(p+t_r+t_p)U_y(x_B,y_B)\right)$$

Again, the first term represents a positive income effect and the second term represents a negative welfare effect stemming from optimization error, which grows in size as register taxes increase.

C. A Cognitive Cost Model of Heterogeneous Attentiveness

How does attentiveness to register taxes vary by income? The model we develop in this Appendix does not make a uniform prediction for all goods, but rather highlights the factors that determine which income group will be more attentive for a particular good. We then consider those factors in the context of cigarettes to predict whether high- or low-income consumers are likely to be more attentive to cigarette register taxes.

Suppose all agents have the option of paying attention to register taxes, but that doing so carries

[76] Because y represents all goods other than x, it is reasonable to assume that $\frac{\partial y_B}{\partial p} > 0$.

with it some positive utility cost.[77] This "cognitive cost" could stem from the mental effort needed to remember and calculate a good's tax-inclusive price or might simply represent the opportunity cost of time spent on that task.

Assume that agents' final utility is additively separable between the cognitive cost and consumption so that we can write $W_i = U(x_i, y_i) - b_i c_i$ in which b_i is a binary choice variable indicating whether agent i pays the cognitive cost and c_i is the magnitude of the cost for agent i. We assume that the cognitive cost is fixed for a given individual in that it does not depend on the register tax rate (it requires just as much effort to take a 6 cent register tax into account as a 7 cent one).

The timing of the model with cognitive costs proceeds as in Part I, except here we add an initial step in which agents choose whether or not they will take register taxes into account when deciding on their consumption of x. As before, all agents choose an intended consumption bundle $(\hat{x}, \hat{y})$ subject to their perceived budget constraint, which we can now express as $\widehat{BC}: x_i(p + b_i t_r + t_p) + y_i \leq M_i$.

A few final pieces of notation will be helpful. Let (x_i^*, y_i^*) denote the (optimal) bundle that i would consume if she were to pay attention to the register tax and let $(\tilde{x}, \tilde{y})$ denote the (sub-optimal) bundle she would consume were she to ignore the register tax. Agents who fail to pay the cognitive cost misperceive the after-tax price of x as being lower than it actually is; as a result, they over-spend on x and under-spend on y. The net change in i's utility from taking the tax into account is therefore given by

$$W(x_i^*, y_i^*, 1) - W(\tilde{x}_i, \tilde{y}_i, 0) = G_i - c_i$$

where $G_i \equiv U(x_i^*, y_i^*) - U(\tilde{x}, \tilde{y})$ represents the agent's utility gain from consuming the optimal feasible bundle.

We assume that agents opt to pay the cognitive cost when doing so affords them greater utility: $b_i = 1\{G_i - c_i \geq 0\}$. Although a full-fledged comparison between the utility that would be achieved in the two scenarios would likely require more cognitive effort than simply taking the tax

[77] The cognitive cost model we use as our starting point follows the basic approach laid out in Chetty, Looney, Kroft (2007).

into account in the first place, it seems reasonable that the agents who decide to pay the cognitive cost tend to be the ones for whom doing so has the most benefit.[78]

Under the assumption that utility is additively separable in x and y, Chetty, Looney, and Kroft (2007) show that one can express G_i (the gain in consumption utility from taking the tax into account) as

$$G_i = \frac{1}{2} t^2 \varepsilon_{x,p} x_i^* v'(y_i^*) \left(\frac{1}{p+t} + \mu_i \gamma_i \right)$$

where $U(x,y) = u(x) + v(y)$, $\varepsilon_{x,p}$ is the elasticity (defined to be positive) of x_i^* with respect to its price, $\mu_i \equiv \frac{x_i^*}{y_i^*}$represents the optimal ratio of x to y, and γ_i measures the curvature of $v()$ at y_i: $\gamma_i \equiv \frac{-v''(y_i^*)}{v'(y_i^*)} y_i^*$.

CLK allow differences in the extent to which individuals take taxes into account by assuming heterogeneity in the cognitive costs that agents face (c_i), although they do not model the sources of that heterogeneity. Because our goal is to link differences in attentiveness to agents' income, we allow G_i to vary over individuals while abstracting from individual heterogeneity in cognitive costs: $c_i = \overline{c}$.[79] In particular, we focus on individual heterogeneity that arises from differences in agents' income. For a fixed tax rate and price, we can write G_i as a function of the agent's income(M_i)

$$G(M_i) = \frac{1}{2} t^2 \varepsilon_{x,p} (M_i) \left\{ \frac{x_i^*(M_i)}{p+t} + \mu_i (M_i) \gamma_i (M_i) \right\} v' (y_i^* (M_i))$$

The question we are interested in is whether low- or high-income individuals are more likely to take register taxes into account. Because agents are alike apart from their incomes, the question at hand is whether $G(.)$ is increasing or decreasing in M_i. Differentiating the above expression with

[78] Another justification for this approach is that agents might make a one-time comparison between G_i and c_i to decide whether to pay the cognitive cost in future circumstance. A third possibility is that agents decide attentiveness tax by tax, rather than good by good (as assumed here). If so, low-income consumers may be particularly attentive to sales taxes because such taxes constitute a relatively high share of their expenditures.

[79] In reality, cognitive costs may also be correlated with income. The correlation may be positive, if high earners are better at cognitive tasks of this sort, or negative, if high earners have a greater opportunity cost of time. The extension to either of these cases is straightforward.

respect to income yields:

$$\frac{\partial G_i}{\partial M_i} = \frac{1}{2}t^2\left\{\frac{\partial \varepsilon_{x,p}}{\partial M_i}x_i^* A\, v'\left(y_i^*\right) + \frac{\partial A}{\partial M_i}\varepsilon_{x,p}x_i^*\, v'\left(y_i^*\right) + \frac{\partial v'\left(y_i^*\right)}{\partial M_i}\varepsilon_{x,p}x_i^* A\right\} + \frac{\partial x_i^*}{\partial M_i}\varepsilon_{x,p} A\, v'\left(y_i^*\right)$$

where $A = \frac{1}{p+t} + \mu_i\left(M_i\right)\gamma_i\left(M_i\right)$. Since A, x_i^*, $\varepsilon_{x,p}$ and $v'\left(y_i^*\right)$ are all positive, the key terms to sign are $\frac{\partial \varepsilon_{x,p}}{\partial M_i}$, $\frac{\partial A}{\partial M_i}$, $\frac{\partial x_i^*}{\partial M_i}$, and $\frac{\partial v'\left(y_i^*\right)}{\partial M_i}$.

First, consider $\frac{\partial v'\left(y_i^*\right)}{\partial M_i}$. We know that $\frac{\partial v'\left(y_i^*\right)}{\partial M_i} = v''\left(y_i^*\right)\frac{\partial y_i^*}{\partial M_i} < 0$ assuming concave utility and that y is a normal good. Intuitively, when the marginal utility of income declines rapidly with wealth, consumers who have little income to begin with are made much worse off by accidentally over-spending on x.

Second, consider $\frac{\partial x_i^*}{\partial M_i}$. This term will be positive as long as x is a normal good, but will be smaller in magnitude for goods for which consumption does not much change as income rises. In words, consumers who consume more will gain more from optimizing correctly simply because the consumption difference caused by the optimization error will be larger in magnitude. When demand for x is relatively insensitive to income, contribution of this term will be small.

Next consider $\frac{\partial \varepsilon_{x,p}}{\partial M_i}$. Are high- or low-income consumers more price sensitive in their demand for x? In general, theory is ambiguous as to whether elasticities rise or fall with income (the sign depends upon the magnitude of the third derivative of the utility function with respect to x).

Finally, consider $\frac{\partial A}{\partial M_i} = \frac{\partial \mu_i}{\partial M_i}\gamma_i + \frac{\partial \gamma_i}{\partial M_i}\mu_i$. Let's take the two pieces in turn. $\frac{\partial x_i^*}{\partial M_i}$ is clearly positive as long as x is a normal good. $\frac{\partial \mu_i}{\partial M_i}$ refers to how the optimal ratio of x to y changes with income. This term is zero when preferences are homothetic and negative for consumption goods that constitute a larger share of expenditures for poor consumers than for rich consumers. The second term, $\frac{\partial \gamma_i}{\partial M_i}$, captures change in the curvature of utility from wealth as income rises; it will be weakly negative when consumers exhibit constant or decreasing relative risk aversion.

We have highlighted the factors that determine whether attentiveness to a register tax is increasing or decreasing by income. What does the analysis imply for the case of cigarettes? Regardless of the good in question, low-income consumers suffer more from lost consumption of other goods when they accidentally overspend on the taxed good. The key determinants that vary between goods are $\frac{\partial x}{\partial M_i}$, $\frac{\partial \varepsilon_{x,p}}{\partial M_i}$, and $\frac{\partial \mu}{\partial M_i}$.

For the case of cigarettes, all three of these factors suggest that attentiveness to register taxes should decrease by income. The income elasticity of cigarettes is generally found to be quite small (or even negative), implying a low value for $\frac{\partial x}{\partial M_i}$. Similarly, on average, poor households spend a substantially larger fraction of their income on cigarettes compared to rich households (Chaloupka and Warner 2000), which implies that $\frac{\partial \mu}{\partial M_i} < 0$. Finally, the sign of $\frac{\partial \varepsilon_{x,p}}{\partial M_i}$ hinges on whether low- or high-income consumers are more sensitive to cigarette prices. The empirical literature on this question is mixed, with most studies concluding that low-income smokers are slightly more price sensitive and other studies finding the opposite. In our data, we find the differences in price-sensitivity between rich and poor smokers to be small, implying that $\frac{\partial \varepsilon_{x,p}}{\partial M_i}$ is small in magnitude.

As a whole, our model suggests that attentiveness to cigarette register taxes should decline by income. Low-income consumers suffer more when they over-spend on y because their marginal utility of wealth is greater than that of high-income consumers. Although the magnitude of the optimization error will in general be larger for high-income consumers (the difference between their intended and realized bundles is bigger), this factor is mitigated in the case of cigarettes by the fact that smoking demand is relatively insensitive to income and by the fact that low-income consumers spend a substantially higher fraction of their income on cigarettes compared to high-income consumers.

D. Additional Robustness Checks

This Appendix investigates the sensitivity of our analysis to additional robustness checks.

1. Alternative Specifications

So far, we have followed the approach taken by much of the smoking literature by separately modeling the extensive and intensive margins of cigarette consumption. This approach has the advantage of providing information about the mechanism by which tax changes reduce cigarette demand, in particular whether higher prices reduce demand by motivating smokers to quit or cut back. However, a drawback of this approach is that the intensive margin results may be biased by changes to the composition of the smoking population.[80]

As a robustness check, we estimate smoking demand using a linear regression and a Tobit model censored at zero. The dependent variable in these regressions is the number of cigarettes smoked per day, with the variable assigned a value of zero when the individual in question is not a smoker. Because the entire population of respondents is used, these approaches avoid the problem that tax rate changes affect selection into the smoking population. The flip side of the coin is that these models do not allow variables to differ in how they affect smoking demand on the intensive and extensive margins. Moreover, the Tobit specification relies on the normality of the unobservables and the linear functional form is probably unrealistic for an application in which so many of the observations have a dependent variable equal to zero. The results of the linear and Tobit specifications are presented in Appendix Table 1 and are consistent with the results from the two-part model used in the rest of the paper.

2. Delayed Responses to Tax Changes

So far we have assumed that smoking demand depends only upon current cigarette taxes, but it could be that tax changes affect consumer behavior with a lag. For example, higher prices might motivate smokers to quit, but the quitting process itself could take several months. Alternatively, it could be that consumers take some time to learn about sales tax changes, only gradually incorporating them into their behavior. If these lags were different for high- and low-income consumers, it could provide an alternative explanation for our results.[81] To investigate this issue, we examine the sensitivity of our results to using various lags of the tax rates instead of the current rate.

[80] For example, suppose that smokers' demand for cigarettes were completely insensitive to price changes, but that light smokers quit when the price became too high. In such a world, a tax increase would appear to raise the intensity of smoking demand on the intensive margin merely by raising the fraction of heavy smokers in the smoking population.

[81] For example, high-income consumers may be better able to afford top of the line smoking-cessation products.

$$y_{ismt} = \alpha + \beta_1 \tau^e_{sm,t-k} + \beta_2 \tau^s_{sm,t-k} + \rho_1 \tau^e_{sm,t-k} LI_{ismt} + \rho_2 \tau^s_{sm,t-k} LI_{ismt} + \eta LI_{ismt} + \gamma x_{smt} + \delta z_{ismt} + \mu_s + \lambda_t + \pi_m + \varepsilon_{ismt} \quad (2.26)$$

where k, is three, six, or twelve months. The results are reported in Appendix Table 2 and suggest that our results are not being driven by differences in the time it takes high- and low-income consumers to respond to cigarette tax changes.

3. State Specific Trends

Although including state fixed-effects accounts for unobserved factors that affect the levels of smoking demand by state, it could be that changes in a state's tax rates are correlated with trends in that state's cigarette demand, such as anti-smoking sentiment. To reduce the influence of any such omitted third factors, we add state-specific year trends to the econometric model.[82] Appendix Table 3 shows that the estimated coefficients are largely unchanged by this addition.

$$y_{ismt} = \alpha + \beta_1 \tau^e_{smt} + \beta_2 \tau^s_{smt} + \rho_1 \tau^e_{smt} LI_{ismt} + \rho_2 \tau^s_{smt} LI_{ismt} + \eta LI_{ismt} + \gamma x_{smt} + \delta z_{ismt} + \mu_s + \lambda_t + \xi_s * t + \pi_m + \varepsilon_{ismt} \quad (2.27)$$

[82] Although our tax rate data is probably largely free of measurement error, including state trends could still cause substantial attenuation bias in the current context. Suppose that smoking demand depends upon a function of current and past tax rates, $x_t = x(a(L)x_t)$, where $a(L)$ is some lag polynomial. The situation here is analogous to the standard measurement error problem: although the original tax variable x_t may be highly correlated with the "true" tax variable $a(L)x_t$, the new tax measure after including state trends may only be weakly correlated with the "true" tax rate, causing an attenuation bias.

Table 2.13: Alternative Demand Models (Appendix Table 1)

	(1) Linear	(2) Tobit
Excise Tax	-2.831***	-17.409***
	(0.612)	(3.102)
Sales Tax	-0.696	-0.327
	(2.542)	(12.036)
Excise*Low-income	0.628	4.772
	(1.396)	(5.405)
Sales*Low-income	-10.338**	-39.285***
	(4.464)	(15.199)
F-stat	5.55	7.39
prob>F	0.02	0.01
N	1,281,525	1,281,525

Standard errors clustered at the state level in parentheses.
274,137 observations have non-zero demand.
All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.
The F-stat is associated with the test for equality between the excise*low-income and sales*low-income interaction coefficients.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.14: Timing (Appendix Table 2)

	Extensive Margin			Intensive Margin			Combined Effect		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	3 Month	6 Month	12 Month	3 Month	6 Month	12 Month	3 Month	6 Month	12 Month
Excise Tax	-0.116***	-0.104***	-0.112***	-0.349***	-0.332***	-0.259***	-3.633***	-3.353***	-3.187***
	(0.027)	(0.026)	(0.027)	(0.067)	(0.061)	(0.075)	(0.635)	(0.629)	(0.639)
Sales Tax	-0.057	-0.156	-0.133	0.363	0.417	0.574	0.079	-1.998	-0.976
	(0.109)	(0.105)	(0.085)	(0.352)	(0.308)	(0.386)	(2.836)	(2.252)	(2.050)
Excise*LI	0.046	0.034	0.038	0.048	0.042	0.072	0.932	0.681	0.866
	(0.059)	(0.065)	(0.069)	(0.113)	(0.113)	(0.126)	(1.417)	(1.547)	(1.665)
Sales*LI	-0.460**	-0.470***	-0.471***	-1.420**	-1.427*	-1.360*	-11.099***	-11.200***	-11.268***
	(0.179)	(0.167)	(0.154)	(0.672)	(0.721)	(0.717)	(4.215)	(4.078)	(3.871)
F-stat	7.63	8.51	10.86	5.51	4.55	3.94	9.09	9.23	10.66
prob>F	0.01	0.01	0.00	0.02	0.04	0.05	0.00	0.00	0.00
N	1,285,448	1,282,765	1,277,073	273,406	272,636	271,088	1,285,448	1,282,765	1,277,073

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.

Outcome variables: probability of smoking (extensive), log cigarette demand (intensive), and cigarette demand in levels (combined).

The F-stat is associated with the test for equality between the excise*low-income and sales*low-income interaction coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.15: State Trends (Appendix Table 3)

	Extensive Margin	Intensive Margin	Combined Effect
	(1)	(2)	(3)
Excise Tax	-0.017	-0.348***	-1.357**
	(0.026)	(0.095)	(0.685)
Sales Tax	0.122	0.342	3.436
	(0.131)	(0.425)	(3.336)
Excise*Low-income	0.060	0.019	1.078
	(0.057)	(0.112)	(1.379)
Sales*Low-income	-0.501***	-1.378**	-11.814***
	(0.182)	(0.665)	(4.380)
F-stat	8.62	5.09	8.98
prob>F	0.01	0.03	0.00
N	1,288,031	274,137	1,288,031

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics and state, year, and calendar month fixed effects.

Outcome variables: probability of smoking (extensive), log cigarette demand (intensive), and cigarette demand in levels (combined).

The F-stat is associated with the test for equality between the excise*low-income and sales*low-income interaction coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3

The Effect of Payday Loan Regulations on Borrowing Behavior

(with Jacob Goldin)

Abstract

Payday loans are a controversial form of credit due to their extremely high interest rates. However, payday lenders offer access to credit to high-risk, liquidity-constrained consumers. In spite of their increased popularity, several states have limited access to payday loans through bans and interest rate caps. This paper investigates the effect of recent regulations on borrowing behavior. We find that, while these regulations are effective at reducing the use of payday loans, this reduction is almost completely offset by an increase in the use of pawnshop loans, another high-cost alternative financial service, primarily by customers who could not borrow from traditional banks. We also observe a decrease in the use of high-interest credit to cover basic living expenses, but no effect on the use of these loans to cover unexpected changes to income or personal expenses. This suggests that customers who continue to use alternative financial services after a ban are more likely to be credit-constrained individuals using these loans to smooth consumption over temporary shocks.

Beginning in the early 1990s, a new form of short-term, high-interest credit became available – the payday loan. Over the past few decades, the number of payday storefronts has grown dramatically; today, there are more payday lending outlets in the United States than there are McDonald's and Starbucks restaurants combined (Zinman (2010)). In spite of the growing popularity of payday loans, several states have recently passed laws restricting their use through bans or binding interest rate caps. Standard economic theory suggests that increasing access to credit unambiguously improves consumer welfare. However, supporters of these restrictions argue that psychological biases prevent consumers from adequately budgeting for the very high interest rates associated with payday loans – the standard payday loan has an APR of approximately 400 percent – and argue that payday loans catch many of their users in a debt trap.

Despite the attention paid to payday loan regulations in recent years, the policy discussion has been limited by a lack of empirical research. In particular, limited research exists on how consumers adjust their lending in response to these regulations. Customers may decrease overall borrowing after payday loans are no longer available. Alternatively, they may borrow from other credit institutions, either in the traditional banking system or credit sources in the alternative financial services (AFS) industry,[83] other than payday loans. Our paper seeks to answer the question: how do state-level payday loan restrictions affect borrowing behavior?

To answer this question, our paper uses a new data set, the Federal Deposit Insurance Corporation's National Survey of Unbanked and Underbanked Households, a supplement of the Current Population Survey (CPS), to examine the effect of these recent regulations of payday loans on consumer borrowing. This survey is one of the first nationally-representative data sets that contains information on the use of a wide variety of alternative financial services. The survey asks participants about their use of payday loans, pawnshops and rent-to-own stores in the past year.

Using a difference-in-differences strategy, we find that the use of payday loans in the previous year significantly decreased after a ban on payday loans was implemented, suggesting that these regulations are binding and properly enforced. However, we observe that, in response to the ban on payday loans, the use of pawnshop loans increased. This finding suggests that when payday loans are no longer available, pawn loans may serve as a viable substitute source of credit for payday customers. In terms of magnitudes, we find that the decline in payday loan use is almost entirely offset by the increase in pawnshop use – the effect of the ban on the use of any alternative financial services is small and statistically insignificant. If policymakers believe that pawn loans, which are also subject to extremely high interest rates, are equally harmful to consumers as payday loans, policies that ban payday loans while leaving other high-interest credit sources available will be ineffective.

In addition to information on the use of alternative financial services, the CPS supplement surveys users of alternative financial services about the purpose of the loan and why the individual chose to use these credit products rather than a traditional bank loan. This information sheds light on the way in which consumers make use of pawnshop loans following a payday loan ban. Although the proportion of customers using any AFS credit products did not change following

[83]This term refers to financial service products operating outside the traditional banking systems, such as payday loans, pawnshop loans, or rent-to-own credit agreements.

payday loan bans, those borrowers were less likely to report that they had used the loan to cover basic living expenses. At the same time, the ban had no effect on the proportion of customers using alternative financial services to cover emergency expenses, such as car repairs or medical costs. Similarly, in states where payday loans were banned, we find an increase in the number of borrowers who report using pawnshops because they did not qualify for traditional bank loans or because banks did not offer small-dollar loans. In contrast, there was no effect on the proportion of customers using pawnshops out of ease or convenience. These results suggest that the customers who switch to pawnshops when payday loans become unavailable are customers who need a loan for temporary, unexpected expenses and resort to using alternative financial services because they lack access to traditional forms of credit.

The paper is structured as follows. Section I provides background on various forms of alternative financial services. Section II describes various models that explain the demand for payday loans. Section III reviews state regulations of these credit products. Section IV provides a literature review of the effect of payday loan access on financial well-being and the use of alternative financial services. Section V describes the data sources used in the empirical analysis. Section VI provides estimates of the effect of payday regulations on borrowing behavior. Section VII concludes.

I. Background on Alternative Financial Service Products

Alternative financial service (AFS) is a term used to describe credit products and other financial services operating outside the traditional banking systems. Many of these services are outlets for the use of high-interest credit products such as payday loans, pawnshops, and rent-to-own. The following section describes these products.

A. Payday Loans

Payday loans are unsecured, small-dollar, short-term, consumer loans. Following a series of federal laws that gave lenders the right to ignore state usury interest laws, the payday lending industry emerged in the early 1990s. The industry grew dramatically in the next decade from an estimated 2,000 payday stores in 1996 to 24,000 in 2007 (Prager (2009)). Currently, customers spend an estimated $7.4 billion each year on payday loans (Stephens (2011)). While online payday lending has increased in the past decade, the majority of customers still obtain their loans from traditional retail storefronts.[84]

To obtain a loan, customers provide lenders a post-dated check for the principal amount plus a fixed-dollar fee or use a delayed automatic debit agreement. The date of the loan maturity is pre-determined with a standard loan length of two or four weeks, often corresponding with the customer's next "payday". The majority of loans range from $100 to $500 with an average loan amount of $375 (Stephens (2011)). Typical loans have a financing charge of $15 for each $100 borrowed over a two-week period, which translates to an APR of just under 400 percent. If a customer is unable to pay back the loan at the agreed-upon date, he may rollover the loan for an additional fee, take out a new loan to cover the previous loan, or default on the loan. While payday loans are marketed to be short-term credit solutions, and rates are displayed accordingly, the average customer holds a payday loan for five months (Pew (2012)).

Payday lenders rarely evaluate customers on their creditworthiness when obtaining a loan, making these loans a source of credit for individuals who may not have access to a traditional bank loan. However, customers must provide verification of income and have a checking account in order to obtain the loan. So while payday may be higher-risk customers than users of traditional bank loans, they are not without connection to employment and traditional banking systems.

[84] A survey by Pew Charitable Trusts indicates that only 16 percent of payday users shop for their loans exclusively online (Pew (2012))

B. Pawnshops

Pawnshops have been a source of credit for centuries, but have seen a steady increase in recent decades. The number of pawnshops in the United States increased from around 5,000 in 1985 to 9,000 in 1992 (Caskey (1994)) and is currently at just over 12,000 storefronts (Carter (2012)).

Pawnshop loans are also small-dollar, short-term loans, but unlike payday loans, these loans are secured by physical collateral. Customers provide the lender with tangible personal property, such as electronics or jewelry, and, in return, receive a cash loan based on the value of the collateral. The loan issued is typically only for a fraction of the assessed value of the collateral, guaranteeing that the loan is more than fully secured.[85] Therefore, pawnshop lenders do not need to assess the creditworthiness of the customer, making pawnshop loans accessible to a wider population than payday loans.

The average pawn loan is around $100, much smaller than the average loan received from a payday lender. These loans usually have a term of one month with an average fee of $20 for each $100, which translates to an APR of about 250 percent (Avery and Samolyk (2011); Drysdale and Keest (2000)), though these rates can be much lower depending on state regulations (Prager (2009)). If a pawnshop customer cannot repay his loan, he forfeits the pawned item to the lender who can then resell the item, typically for a large profit.

C. Rent-to-Own

Unlike payday or pawn loans, rent-to-own stores allow customers to purchase items on credit rather than providing cash loans. The customer receives the item, typically, electronics, furniture, or appliances, for immediate use from one of the 8,000 stores around the country (Czerwonko (2013)), but in return pays a much higher price for the item than if he was to buy it all at once. These rates have been estimated to be as low as 57 percent APR (Czerwonko (2013)) and as high as 230 percent (Zikmund-Fisher and Parker (1999)). If a customer misses a payment, the lender

[85] Prager (2009) cites that the loan amount offered ranges between 25 and 65 percent of the estimated resale value of the collateral provided by the customer.

has the right to repossess the item.

II. Models of Demand for Payday Loans

A. Neoclassical Explanations for Payday Loan Use

Standard economic theory suggests that individuals will only use a payday loan when the costs associated with forgoing the loan are large enough to justify the high interest rates. Nonetheless, payday loans are a very expensive source of credit. While cheaper forms of credit, like traditional bank loans or credit cards, may be available for many consumers, low-income, high-risk consumers may not qualify for these less expensive loan options. Several studies suggest that a high proportion of payday loan customers are indeed liquidity-constrained. Logan and Weller (2009) report that one third of payday loan customers had recently been denied access to credit. Agarwal, Skiba and Tobacman (2009) find that close to half of payday loan customers do not have a credit card. Carter, Skiba and Tobacman (2011) found that 70 percent of payday customers had no available line of credit. Therefore, payday lenders provide the ability to borrow to customers with few alternative credit options.

Additionally, consumers who have access to traditional forms of credit may chose not to use these options for reasons other than the price of the loan. The high volume and location choices[86] of payday lenders may make obtaining these loans more convenient for certain consumers. Similarly, payday lenders offer borrowers cash on the spot, while traditional banks may take longer to be approved. If the benefits associated with the speed and convenience of obtaining a payday loan are large, consumers may rationally choose to borrow from a payday lender rather than using more traditional sources of credit.

Be it for lack of alternative credit access or preferences for payday loans over traditional bank loans, neoclassical models may support the choice to use payday loans for certain types of expenses

[86]Payday lenders are often located in working class neighborhoods with large minority populations (Stegman (2007)).

in spite of the high interest rates. Customers may need a loan to adjust for shocks to their income or to pay for unexpected expenses to avoid more extreme financial difficulties. For example, an individual who drives to his job may prevent taking time off work if he can receive a short-term loan for car repair. Supporters of the payday lending industry argue that the total fees associated with the loan may be quite small compared with the benefits of having the ability to smooth consumption in times of need, especially if these customers anticipate paying back their loans quickly. A recent study shows that 37 percent of payday borrowers would have taken out the loan at any terms offered (Pew (2013)), suggesting that many payday customers borrow out of desperation.

B. Behavioral Explanations for Payday Loan Use

While some of these customers may rationally choose to borrow at high interest rates, others may exhibit psychological biases that cause them to over-borrow and end up in a debt trap. If customers have time-inconsistent preferences (Laibson (1997)) or exhibit self-control problems (O'Donoghue and Rabin (1999)), they may borrow more than they can afford to repay. Similarly, customers may be overly optimistic about their ability to repay the loan in a timely fashion and be forced to rollover the loan when it matures (Ausubel (1991)). Alternatively, payday lenders may target financially unsophisticated consumers or frame the interest rates in terms of deceptively short time periods given the average length of a loan (Bertrand and Morse (2011)). For example, payday interest rates are often marketed as fees for a two-week loan (e.g., a $15 fee per $100 for a two-week period), rather than the corresponding APR of 391 percent.[87]

The likelihood of accruing large amounts of debt increases when customers borrow to cover recurring expenses or sustained income loss rather than a temporary shock to income or consumption. For example, a customer who uses a payday loan to cover his monthly car payment will likely face the same problem affording his car payment next month and will also need to pay off

[87] While 86 percent of payday customers interviewed in the Pew Charitable Trusts survey reported that the terms and conditions of the loan were clear, several customers showed evidence that they could not accurately compare rates of different credit products or focused solely on their ability to pay back the interest fees rather than the entire repayment (Pew (2013)).

the previous month's loan. This may cause him to enter a cycle of debt. In contrast, if a customer borrows from a payday lender to meet a one-time expense, like a car repair, he will eventually pay off the loan as long as he can afford to make monthly payments larger than the interest fees. In response to these perceived problems, several state and local regulators require payday lenders to post or distribute materials warning customers that these loans are not meant to be used to meet long-term financial needs.[88]

However, in practice, some evidence suggests that payday customers do not use these loans merely as short-term, temporary fixes. A recent report by the Consumer Financial Protection Bureau finds that the average payday loan customer was in debt for 196 days out of the 12-month study period (CFPB (2013)). Data from Pew Charitable Trusts shows that the average payday loan customer can afford $100 each month toward paying off their payday loan while still being able to pay other bills and expenses; while this is enough to pay off the average monthly interest payment, it is not enough to pay off full cost of the average loan (Pew (2013)). While some of these customers may rationally choose to hold their loan for long periods of time in spite of the high costs, the behavioral biases discussed above may cause others to be in debt for longer than anticipated.

III. State Regulations of Alternative Financial Service Credit

A. A Rationale for Payday Loan Regulation

The primary policy concern regarding payday loans is that the availability of these loans leads to chronic indebtedness (Stegman (2007)). These concerns have caused several states to pass regulations restricting the use of payday loans. However, these regulations may exacerbate or alleviate a customer's financial difficulties depending on the customer's need for immediate cash and his ability to repay the loan.

[88] For example, payday lenders in the state of Virginia are required to provide a pamphlet in 24-point font reading "WARNING: A payday loan is not intended to meet long-term financial needs. It is recommended that you use a payday loan only to meet occasional or unusual short-term cash needs." (10 VAC 5 200)

On the one hand, if customers suffer from the psychological biases mentioned above, bans on payday loans may discourage unsustainable spending at high interest rates or encourage consumers to switch to traditional banks or credit cards that have much lower interest rates. This may be particularly important when these customers borrow to cover recurring expenses which can quickly result in customers entering a debt trap. Data from Pew Charitable Trusts suggests that payday customers found that it was “too easy” to borrow from a payday lender and that they could not resist the temptation of taking out the loan (Pew (2013)). This same data shows that 72 percent of customers favor more regulation of payday loans even though 62 percent of payday customers would likely take out another payday loan in the future. So while payday customers are aware of their own likelihood of repeat use, these data suggests a potential desire for regulations to serve as a commitment contract to limit future use.

On the other hand, if customers are rationally borrowing from payday lenders, either due to lack of access to alternative credit sources or because they simply prefer the service provided by payday lenders to that of traditional banks, banning access to payday loans will reduce consumer welfare. If payday loan customers do not have access to other forms of credit, they will reduce their total borrowing when payday loans are banned. Alternatively, customers may be forced to switch to using other forms of credit – that, for one reason or another, was less appealing in the absence of the regulation – once denied access to payday loans.

In reality, it is likely that both types of consumers exist. Evaluations of the change in welfare due to regulating payday loans rely on the proportion of the two types of consumers as well as the size of the utility gain from preventing customers suffering from behavioral biases from borrowing irresponsibly relative to the utility loss from denying rational borrowers access to their preferred form of credit. While these calculations are beyond the scope of this paper, we will provide evidence of the effect of recent regulations on how customers shift between credit products and the types of consumers who continue to borrow in the absence of payday loans.

B. Recent Regulations of Payday Loans and Other High-Interest Credit

State regulations of payday lenders take a variety of forms. While some states explicitly ban payday loans through usury laws or racketeering violations, many other states created effective bans on payday loans through plausibly binding loan rate caps.[89] For example, after Oregon passed a law limiting the fees associated with loans under $50,000 to $10 per $100, less than a quarter of the payday lending outlets in the state remained a year later (Zinman (2010)). As of January 2008, the start of the period covered by the first round of the CPS supplement, twelve states and Washington, D.C. banned or limited the use of payday loans.[90]

In recent years, several states changed their policies regarding the regulation of payday loans. In November 2008, the Arkansas Supreme Court ruled that the Check Cashers Act of 1999, which originally allowed payday lenders to charge high fees for loans in place of interest, violated the state constitution's interest rate cap of 17 percent. In 2010, Arkansas residents voted to implement a cap of 17 percent APR on all consumer credit and the state legislature repealed the Check Cashers Act in 2011. In March 2009, New Hampshire passed a law that limited rates on payday loans to 36 percent APR. Arizona originally exempted payday lending from the state's 36 percent APR interest rate cap; however, this law was allowed to sunset on making payday loans illegal as of July 2010. In November 2010, voters in Montana approved a ballot initiative that capped interest rates on payday loans at 36 percent APR.

Pawn loans are also subject to state regulations on the length of the loan and the amount of interest that can be charged. Many states have no fee limits, while other states have limits as low as $2 per $100 for a two-week loan; however, pawn shops continue to operate in states with even the most restrictive policies. Rent-to-own stores are often able to avoid state regulations

[89] Data on the location of payday storefronts indicates that the number of lending outlets significantly decreases after payday loan regulations are implemented, suggesting that these laws are strictly enforced and that the interest rate caps on payday loans are binding (Zinman (2010)).

[90] These states include Connecticut, Georgia, Massachusetts, Maryland, Maine, North Carolina, New Jersey, New York, Oregon, Pennsylvania, Vermont, and West Virginia. Additionally, in 2008 Ohio passed a law capping payday loan interest rates at 28 percent APR; however, payday lenders in this states have continued to offer loans at higher rates (Pew (2012)). Therefore, we drop Ohio from our sample. Including Ohio in our analysis does not qualitatively alter our results.

on APR disclosure requirements or interest rate caps since the contracts signed by the customers are terminable-at-will. Several states have passed legislation regulating labeling on rent-to-own merchandise including the cash price and the total cost to own (Czerwonko (2013)); however, the reported prices often do not represent the true market price (Drysdale and Keest (2000)).

IV. Literature Review

A. The Effect of Payday Loan Regulations on Payday Loan Use

While data on the use of payday loans is limited, a small number of papers estimate the effect of payday loan bans on usage rates. For example, Zinman (2010) finds that Oregon residents were almost 30 percent less likely to use a payday loan after the state began regulating fees associated payday lending. Carter (2012) uses cross-sectional variation in laws regulating payday loan rollovers and finds that these restrictions decrease the likelihood that an individual will use a payday loan. The Pew Charitable Trusts data shows that payday loan usage rates are more than twice as high in states that do not regulate payday loans than in states that ban them. However, payday loan use does not decrease to zero in restrictive states – 2.9 percent of individuals in restrictive states used a payday loan in the past five years compared to 6.6 percent in permissive states (Pew (2012)).[91] This suggests that some individuals are able to obtain loans through online lenders, illegal storefronts, or by traveling to other states to receive a loan.

B. The Effect of Payday Loan Access on Economic Well-being

As mentioned in Section III, payday loan use may exacerbate or alleviate financial difficulties, depending on the model of demand. A number of papers find evidence that payday loan access eases customers' financial problems, supporting the neoclassical view that individuals use payday loans only when the immediate financial need outweighs the high costs associated with this type of borrowing. For example, Morgan and Strain (2008) find that payday loan access is associated

[91] "Permissive" states are defined as states that allow single-repayment loans with 391 APR or higher.

with lower rates of bankruptcy. Similarly, Morse (2011) suggests that individuals are less likely to foreclose on their homes if they have access to payday loans.

In contrast, several papers find that access to payday loans exacerbates an individual's financial difficulties, which suggests that payday loan customers do exhibit some of the psychological biases discussed above and that restricting access to these types of credit could, in fact, increase economic well-being. Skiba and Tobacman (2009) show that payday loan access increases the likelihood of declaring bankruptcy. Carrell and Zinman (2008) find that a law that restricted access to payday loans among military personnel led to an increase in job performance among Air Force members. Melzer (2011) shows that having access to payday loans causes individuals to be worse off on a variety of measures of economic hardship including difficulty paying bills, food security, and postponing medical care due to costs. Lastly, Zinman (2010) finds that Oregon residents were more likely to experience a worsening of their financial situation after the ban.

C. The Effect of Payday Loan Regulations on Other Types of High-Interest Credit

Due to limited data availability on other types of borrowing, one question that has received less attention in the literature is: how do payday loan customers alter their borrowing behavior when their access to payday loans is regulated? If denied access to these loans, payday customers may be forced to seek out less desirable forms of credit, such as other alternative financial services, that may have even higher interest rates or less desirable collection policies.[92] However, if these customers do not have access to other forms of credit or if other available credit sources are undesirable, these individuals will reduce their total borrowing when payday loans are banned and suffer the potential consequences of forgoing these expenses.

The following studies suggest that when payday loan access is restricted, customers are more likely to use other costly forms of borrowing. Zinman (2010) finds that individuals were more

[92]Skiba and Tobacman (2007) report that 58 percent of customers cannot pay back their loan and forfeit their collateral which often has sentimental value and may be less preferrable than defaulting on a payday loan.

likely to bounce a check or pay bills late after payday loans were banned in Oregon. Similarly, Morgan and Strain (2008) find that Georgia residents were more likely to bounce a check as a source of credit after the state banned payday loans. Skiba and Tobacman (2007) show that individuals who were denied a payday loan due to a low credit score were more likely to take out a pawn loan within the next two days; however, they did not find any evidence suggesting that these customers were more likely to use a pawn loan in the future. While these finding might suggest that payday customers do not have access to bank loans or credit cards and are, therefore, forced to substitute to other forms of high-interest credit when denied a payday loan, Agarwal, Skiba and Tobacman (2009) find that many individuals use a payday loan even when their credit card liquidity exceeds the amount of the payday loan. However, they show that credit card liquidity among these customers had been declining in the period prior to taking out the payday loan.

In contrast, a few papers suggest that other forms of high-interest credit, namely pawn loans, may be complements to payday loans rather than substitutes. Carter (2012) finds that residents of states with fewer restrictions on payday loan rollovers are more likely to use pawnshops and payday loans together, suggesting customers may be more likely to use a pawn loan to pay off the interest on the payday loan and rollover that loan for another period rather than default. Carter and Skiba (2011) argue for this with evidence that payday loan customers who take out a pawn loan within one day of their payday loan due date are more likely to rollover the payday loan.

V. Data

The primary data source for this paper comes from the FDIC's National Survey of Unbanked and Underbanked Households.[93] This survey was conducted by the U.S. Census Bureau as a supplement to the Current Population Survey (CPS). To date, two rounds of the survey have been collected, one in January 2009 and one in June 2011.[94] The supplement contains a nationally-

[93] Unbanked households are defined as households without a checking or savings account. Underbanked households are those with a traditional bank account that also use alternative financial services.

[94] The CPS interviews each sample household for four consecutive months, waits eight months then interviews the household for a final four months. Because of this structure, no individual household appears in both supplements.

representative sample of 46,547 households in the 2009 survey and 45,171 households in the 2011 survey.

The supplement questionnaire contains questions regarding a household's connection to traditional banking systems, use of alternative financial services, and reasons for being unbanked or underbanked. Survey participants were asked whether anyone in the household had used a payday loan, sold items at a pawn shop, or leased merchandise from a rent-to-own store in the past year.[95] For the 2009 supplement, we categorize a household as having used a payday loan in the past year if they responded to the question "How many times in the last 12 months did you or anyone in your household use payday loan or payday advance services?" with a non-zero answer. Similarly, we categorize a household as having used a pawnshop or rent-to-own in the past year if the response to the question "How often do you or anyone in your household sell items at pawn shops/[do business at a rent-to-own store]?" was "At least a few times a year" or "Once or twice a year". In the 2011 supplement, a household is recorded as having used one of these alternative financial services products is they responded affirmatively to the question "In the past 12 months, did (you/or anyone in your household) have a payday loan/[pawn an item because cash was needed]/[have a rent-to-own agreement]?".

Unlike many other data sets that have been used to report patterns of borrowing behavior, the CPS supplement asks participants not only about use of high-interest credit, but also about their reasons for using these alternative financial services. Participants who reported using payday loans in the past year were asked why they chose to use these loans rather than a traditional bank loan; a similar question was asked of pawnshop users. In addition, customers who reported using any alternative financial service credit product in the past year were asked about the purpose of the loan.

The survey includes information on the demographic characteristics of the sample households. The demographic data used in this paper pertains to the household's interview reference person. These variables include the individual's gender, race, education, marital status, income, and em-

[95] Additionally, participants were asked about their use of refund anticipation loans; however the time period referenced in the survey question was not consistent across years, so cannot be used in our main analysis.

ployment. These demographic variables are used as controls in the regression analysis.

The CPS also includes data on the geographic location of each household. We use this geographic data to link the survey data to data on local economic conditions. Data on population and real state income per capita comes from the Bureau of Economic Analysis and data on unemployment rates comes from the Bureau of Labor Statistics.

VI. Empirical Analysis

The following section examines the effect of the recent payday loan regulations described in Section III. Using data before and after the policy changes, we compare borrowing behavior in states that changed regulations to states that did not to determine the impact of these laws on a wide variety of outcomes.

A. Summary Statistics

1. Use of Alternative Financial Services

Table 3.1 reports descriptive statistics on the use of alternative financial services from the CPS supplement data. Of the survey participants in the full sample, 4.2 percent of participants used a payday loan, 6.8 percent used a pawnshop, and 4.4 percent purchased merchandise at a rent-to-own store while 11.9 percent used at least one of these three AFS products. Column 2 reports statistics of use of the same credit products in the past twelve months. The table shows that 2.6 percent of all participants used a payday loan in the past year, suggesting that more than half of individuals that ever used a payday loan did so in the past year. A similar proportion of participants used either pawnshops or rent-to-own in the past year – 2.5 and 1.7 percent, respectively. Overall, 5.8 percent of participants used one of the AFS products in the past year suggesting that, while there is some overlap of the use of the different products, that overlap is limited.

2. Demographic Characteristics

Table 3.2 compares the characteristics of users of AFS credit products to other survey participants, where use is defined as having ever used the product in the past year. Users of AFS products are more likely to be female, single, black, and young; these characteristics look very similar when comparing across users of different types of AFS products. AFS users are also more likely to be socioeconomically disadvantaged in terms of income, education, and employment status; however, these characteristics do vary across the type of product used. Payday loan users, while still economically disadvantaged when compared to individuals who do not use and AFS products, have more education than pawnshop or rent-to-own users and are less likely to be unemployed. This is likely due to the fact that payday loan customers are required to show proof of employment to obtain a loan; however, since the survey asks about payday loan use in the previous year, we may observe some currently unemployed participants reporting use of payday loans. Additionally, while the highest income individuals are less likely to use payday loans, payday loan usage is not concentrated among the lowest income individuals as with pawnshop and rent-to-own usage. Again, this is likely due to the differences in income requirements across the different products.

3. Reasons for Using AFS Credit Products

Alternative financial service credit products are often marketed to be used as short-term solutions for emergency cash needs among liquidity-constrained individuals. This may be due to shocks to income, such as job loss or decreased income, or to shocks in consumption needs. We define these "emergency" expenses to be temporary, unexpected costs such as car or home repairs or medical expenses. Table 3.3 presents the reasons for using these credit products in the past year as reported by AFS users. The most common reason cited for using a loan was not to meet these emergency needs, but to cover expected, plausibly recurring expenses. Almost half of AFS users, 43 percent, reported using these credit products to cover basic living expenses with an additional five percent reporting using the loans for luxury goods. Nineteen percent of customers used the loans to make up for lost income, while only a small proportion reported using these products for temporary emergencies – 13 percent of customers used the loan to cover car or home repairs and 2 percent used the loan for medical expenses.[96]

4. Reasons for Using AFS Credit Products versus Bank Loans

Traditional banks offer much lower interest rates on consumer loans than either payday lenders or pawnshops. However, payday lenders and pawnshops typically serve a low-income, high-risk population that may not be eligible for traditional bank loans and are, therefore, forced to use these high-interest loans due to lack of alternative forms of credit. Alternatively, these customers may have access to cheaper forms of credit, but find using payday lenders or pawnshops more appealing due to other factors such as convenience or ease of use. Column 1 of Table 3.4 presents the main reason payday loan customers reported using the loan instead of a traditional bank loan.[97] Over half of customers report using a payday loan because the loan was easier of faster to obtain or because the storefronts had more convenient hours or location than traditional banks. Only 15 percent of customers reported that they did not qualify for a bank loan and 20 percent of customers used a payday loan because banks do not give small dollar loans. Column 2 shows that pawnshop customers reported very similar reasons for using a pawn loan rather than a bank loan. These findings suggest that the majority of payday and pawn loan customers may not actually be credit-constrained, but rather prefer using AFS products to traditional bank loans for other reasons in spite of the high interest rates.

B. The Effect of Payday Loan Regulations

This section examines whether recent changes in the regulation of payday loans had an impact on the use of payday loans and other types of high-interest credit. As mentioned in Section IV, many

[96]These estimates are very similar to those found in the Pew Charitable Trust Small Dollar Loans data. That study found that 16 percent of payday loan customers used their first loan to cover unexpected expenses (such as car repair or medical expenses), while 69 percent used the loan to cover recurring expenses, including rent, groceries, utilities, car payments, and credit card debt (Pew (2012)).

[97]This table includes data from 2011 only, since the available categories for reasons a customer used a payday loan rather than a traditional bank changed across waves. The categories were consistent across waves for a similar question regarding reasons for using pawnshops; including data from 2009 yields qualitatively similar results.

papers that estimate the effect of these regulations use cross-sectional variation in state policies to determine the impact on the use of payday loans and alternative forms of high-interest credit. However, because variation in state regulations is not random, this approach may conflate the effect of payday loan access with other state-level characteristics. For example, Figure 3.1 shows that the majority of states that restrict the use of payday loans are clustered on the East Coast. If individuals in different regions of the country have different propensities to use payday loans regardless of state regulations, estimates from a model that compares banned states to legal states will be biased. A more convincing set of papers compares borrowing behavior in a state that banned payday loans to a neighboring state before and after the policy change (Morgan and Strain (2008); Zinman (2010)). However, in order to extrapolate these results to the effect of a ban more generally, we must assume that customers in these states are representative of a broader population.

As mentioned above, four states changed their policies on payday loans between the two waves of the CPS data. This allows us to observe the level of use of these three credit products before and after these new regulations in states that experienced a policy change and in states that did not. As long as there are no unobserved differences in the trends in AFS credit use between the states that changed their policies in recent years and those that did not, we can use a difference-in-differences strategy to provide us an unbiased estimate of the effect of laws regulating access to payday loans. While this empirical strategy is subject to some of the same criticisms as the previous studies that use policy changes to identify the effect of regulations on borrowing, the states in which we observe a policy change are quite geographically and demographically diverse, making our result more plausibly generalizable than studies considering a policy change in only one state.

Our empirical model takes the following form:

$$y_{ist} = \beta_1 Ban_{st} + \beta_2 Post_t + \delta_s + \gamma X_{ist} + \pi Z_{st} + \varepsilon_{ist}$$

The unit of observation is an individual i in state s in time period t. The dependent variable, y, is an indicator variable for having used a certain type of credit product in the last year, *Ban* is an indicator variable which takes a value of one if the individual lives in a state where payday loans were regulated in the period he was surveyed, *Post* is an indicator variable for being interviewed in the 2011 survey, δ is a set of state fixed effects, X is a set of individual-level covariates, and Z is a set of state-level controls.

1. Payday Loan Use

Table 3.5 presents the results of the difference-in-differences analysis of the effect of these regulations on the use of payday loans.[98] Column 1 presents a model that includes controls for time period, state, and whether the individual's state of residence restricts the use of payday loans. Using these limited controls, the model shows that payday loan usage is 2.4 percentage points lower in states that ban payday loans. Column 2 adds individual-level demographic characteristics to the model including gender, race, marital status, education, age, income, and employment status. After controlling for these demographics, the size of the ban coefficient increases to 2.8 percentage points. Finally, if payday loan use is correlated with the business cycle, it is important to control for local economic conditions. Column 3 includes controls for state unemployment rate, personal income per capita, and population, which only slightly reduces the estimated effect of the ban to 2.5 percentage points. This final model is our preferred specification. Overall, regardless of specification, our model shows a large decrease in the proportion of individuals using a payday loan after access to payday loans is restricted. This suggests that, not only were these regulations effective at decreasing the availability of payday storefronts, but also that payday loan customers did not shift their business to online payday lenders that are not covered by state regulations. In fact, less than one percent of residents living in states that recently passed payday loan regulations continued to use payday loans after the ban.

2. The Use of Other AFS Credit Products

The following section examines how payday loan restrictions affected the use of pawn loans and rent-to-own agreements. If these other forms of high-interest credit are substitutes for payday

[98] We estimate demand for payday loans using a linear probability model; however, a probit model yields qualitatively similar results.

loans, we would expect that individuals who previously used payday loans would switch to using one of the other AFS products after payday loans were banned. However, if these other forms of credit are complements to payday loans, for example, if payday loan customers take out a pawn loan to avoid defaulting on the original loan as suggested in Carter (2012), then we would expect to see a decrease in the use of pawn shops and rent-to-own. In Table 3.6, column 1 presents estimates of the effect of payday loan regulations on the use of pawnshops using our preferred specification from Table 3.5 and column 2 presents the effect of these regulations on the use of rent-to-own. We find that, in states where payday loans are banned, individuals are significantly more likely to use pawnshops – 1.7 percentage points more likely – than in states that ban payday loans, suggesting that pawnshops and payday loans are substitutes. However, we find no effect of payday loan regulations on the likelihood of using rent-to-own. The difference in substitutability between payday loans and these two alternative forms of credit may not be surprising since payday lenders and pawnshops both offer customers cash loans while rent-to-own outlets only offer credit for the purchase of very specific items. If payday customers use their loan for reasons other than the purchase of electronics, appliances, or furniture, then a rent-to-own agreement will be an unlikely substitute.

While the above results suggest that payday customers shift toward the use of pawnshops once payday loans are no longer available, bans on payday loans may still reduce the overall use of high-interest credit products. Column 3 of Table 3.6 shows the effect of payday loan regulations on the use of any AFS product, defined as having used payday loans, pawnshops, or rent-to-own in the past year. We see that the estimate of the effect of banning payday loans has a small and insignificant effect on the total use of AFS credit products, suggesting that the decrease in payday loan use is almost entirely offset by the increase in the use of pawnshops and rent-to-own. So while payday loan regulations have the intended effect of reducing the use of payday loans, they are not effective at reducing the total use of high-interest credit since these policies target only one form of high-interest loans leaving the others unregulated.

3. Reasons for Using AFS Credit Products

The previous section showed that payday loan regulations reduced the use of payday loans, but that many of these customers simply substituted to using pawnshop loans after the regulation was passed. While the proportion of customers using high-interest credit did not significantly decrease, customers who shifted from using payday loans to pawn loans may use the loans to cover different types of expenses. For example, if customers are hesitant to risk losing personal items to a pawnshop, they may only use this form of credit in times of extreme need. Alternatively, the average pawnshop loan is only a quarter of the size of the average payday loan, so may only be useful for covering small expenses.

Table 3.7 provides estimates of the effect of payday loan regulations on the reason an individual reports for using an AFS credit product.[99] We find that when payday loans are banned, the proportion of customers who use AFS products to meet basic living expenses significantly declines. However, these regulations have no effect on the proportion of customers using AFS products for unanticipated expenses like car repairs or medical costs. This suggests that, the customers who continue to use AFS products after the ban are more likely to use them as suggested by the state-mandated informational materials mentioned above: for short-term, emergency expenses rather than long-term financial needs.

4. Reasons for Using AFS Credit Products versus Bank Loans

As shown in Table 3.4, some individuals report using payday loans rather than traditional bank loans because they are unable to obtain a loan from a bank, either because they do not qualify for a loan or because the loan amount they need is too small to be provided by a bank. If payday loans were no longer available, these customers would be forced to seek out other high-interest forms of credit, like pawnshops, or forgo borrowing the money altogether. However, the majority of customers in our data set report using payday loans simply because they prefer the ease or

[99] Again, we restrict this analysis to customers who reported using payday loans, pawnshops, or rent-to-own in the past year.

convenience of using a payday lender compared to a bank. Customers who use payday loans due to preference rather than necessity may be less likely to substitute to pawnshops once payday loans are banned because they may have access to other credit opportunities outside of the AFS sector.

Unfortunately, the potential reasons participants could cite for choosing a payday loan over a bank loan were not consistent across the two waves of the survey.[100] This prevents us from determining if the decrease in payday loan use seen in Table 3.5 was driven by customers who used these loans due to a lack of alternatives, customers who preferred payday lenders to traditional banks, or both. However, we do not experience the same data issue with the question regarding the reason for using pawnshops versus traditional banks – participants chose from the same set of responses in both surveys. This allows us to use our difference-in-differences analysis to measure the effect of the policy change on pawn loan utilization by reason for use. Table 3.8 presents estimates of the effect of a payday ban on the reason that pawnshop customers preferred a pawn loan to a traditional bank loan. This table shows that the customers who substituted to using a pawn loan after payday loans were banned reported doing so because they did not qualify for a bank loan or were unable to receive a small dollar loan from a bank. In contrast, there was no increase in the proportion of customers using pawnshops out of ease, comfort, or convenience. This suggests that customers who switched from using payday loans to pawn loans were those who most likely did not have other borrowing alternatives.

C. Robustness Checks

1. Distance to the Border

While our difference-in-differences identification strategy used above improves upon that of many other papers that simply use cross-sectional variation in states policies to identify the effect of payday loan bans on borrowing behavior, there may be reason to believe that these estimates are also biased. If trends in the use of payday loans or other forms of high-interest credit are different

[100] The categories in the 2011 survey are the same for both payday and pawn users; however, in the 2009 survey, participants did not have the option of responding that they used a payday loan due to the fact that banks do not offer small dollar loans – a response that is not an obvious subcategory of one of the available responses in the 2009 survey.

in states that change payday loan regulations versus those that do not, our identifying assumption is not valid. As a robustness check, we use a different identification strategy taken from Melzer (2011). This empirical method defines access to payday loans not by the regulations in an individual's state of residence, but by the distance from the individual's home to a neighboring state that permits payday loans. In other words, within a state that bans payday loans, we compare the borrowing behavior of individuals who live close to a border of a state that does not regulate payday loans to those that do not. As long as individuals do not choose where to live based on the regulations of neighboring states and that state regulators decision to ban payday loans is not affected by the behavior of residents of neighboring states, this will be a valid identification strategy.

To determine the measure of payday loan access for each household, we must measure the distance from that household the nearest payday-allowing state. While state of residence is available for our entire sample, the majority of households have finer geographic data: 73 percent of the sample has data on the household's Metropolitan Statistical Area (MSA) of residence and, in addition, 40 percent of the sample has data on county of residence. We are able to use these data to determine the distance of a household to the border of states with different regulatory policies. We define the distance between a household and a neighboring state as the distance between the centroid of the household's county and the centroid of the closest county in the neighboring state. For observations that have data on a household's MSA but not county of residence, we define the distance to a neighboring state as the population-weighted average of the county-level distances for the counties in that MSA. Observations that only include state-level geographic data are excluded from our analysis.

Our econometric model is as follows:

$$y_{icst} = \beta_1 Access_{cst} + \beta_2 Border_{cs} + \beta_3 PawnAccess_{cs} + \beta_4 X_{icst} + \beta_5 Z_{cst} + \delta_{st} + \varepsilon_{icst}$$

where y is a measure of borrowing behavior for individual i in state s and county c in time period t. *Access* is an indicator variable for living in a state that permits payday loans or living within

20 miles of a payday-allowing state. This variable has both a cross-sectional and a time-series component since regulations of payday loans are changing over time while the distance between states remains fixed. *Border* is an indicator for living within 20 miles of any neighboring state. This controls for the possibility that certain types of households choose to live near borders, which could invalidate our identification strategy. All regressions control for the same individual demographic characteristics and local area economic conditions as in the previous regressions as well as state-time fixed effects. Finally, we allows for the fact that the laws regulating pawnshops in neighboring states may affect borrowing as well.[101] *PawnAccess* is an indicator variable for living in a state with limited pawnshop regulation[102] or living within 20 miles of a state with limited pawnshop regulation.

Results of this analysis are presented in Table 3.9. We find that living close to a payday-allowing state increases the proportion of individuals who use a payday loan by a statistically significant 1.3 percentage points. This estimate is significant, but about half the size of the difference-in-difference estimate of the effect of payday loan access measured in the previous section. This may not be surprising given that, while we define these customers as having access to payday loans, they still must travel to another state to receive the loan. In contrast, we no longer observe that individuals who do not have access to payday loans are more likely to use pawnshops – in fact, we observe a positive, though insignificant, effect of payday loan access on the use of pawnshops. This does not necessarily cast doubt on our findings in the previous section since we are identifying our effect off of a different population – those that travel to receive a payday loan versus those who live in a state that allows payday loans. Potentially, we observe this difference across estimation strategies since individuals who recently had access to payday loans in their state of residence may be accustomed to using these credit products and are, therefore, more willing to substitute to other high-interest credit, while individuals who never had access to payday loans never developed a

[101] During our time period and to our knowledge, there were no changes to laws regulating pawnshops. Therefore, any effect of these regulations would be captured by state fixed effects in the differences-in-differences specification and so were not considered in the previous analysis.

[102] Limited regulation is defined as having no fee restrictions or a fee limit of at least $25 for every $100 in a 30-day period.

taste for any form of high-interest credit. Additionally, since we do not know a household's exact location, our measure of distance is likely to be measured with error.

VI. Conclusion

Our paper analyzes the effect of recent state-level payday loan restrictions on the use of payday loans and on borrowing behavior more generally. Our results suggest that these restrictions are effective at curbing the use of payday loans; on average, approximately three percent of residents used payday loans before the restriction, compared with less than one percent after the policy change. However, we also found that this reduction in payday loan use was accompanied by an increase in the use of pawnshop loans. Overall, we find that the adoption of payday loan restrictions do not appear to meaningfully reduce the fraction of the population that utilizes alternative financial services.

Although the overall proportion of individuals using alternative financial services did not change, we found that following a restriction, fewer customers report using AFS credit to cover basic living expenses. In contrast, the new laws had no effect on the proportion of customers using these services to pay for emergency expenses. Finally, following the restrictions, we documented an increase in the fraction of pawnshop borrowers reporting that they took out the loan because they were unable to receive a loan from a traditional bank.

It is important to note several limitations of our study before concluding. First, our analysis examines the effect of policy changes in only four states. While these states are quite diverse, both demographically and geographically, regulations in other states may have a different impact on borrowing behavior. Second, like other difference-in-difference designs, our results are only valid to the extent that the treatment and control states are not characterized by preexisting trends. Third, our analysis is limited by the types of borrowing that are covered in our data set. These customers may increase their use of less expensive forms of credit, like traditional bank loans, credit cards, or borrowing from family members, or they may substitute to even worse forms of credit, like loan sharks. Finally, our results aim to contribute to the positive rather than normative discussion surrounding payday loans. The fact that state restrictions reduce the usage of payday loans and increase the usage of pawn loans may either be desirable or undesirable, depending on whether one believes such loans are welfare-enhancing in the first place. Such issues raise thorny questions of behavioral welfare analysis that are beyond the scope of this paper.

Our findings, while not definitive, speak to important questions of policy. First, they suggest that the issue of payday loans cannot be addressed in isolation, without considering the availability and desirability of other forms of high-interest credit. If one believes that payday loans are uniquely bad (for example if they prey on particular behavioral biases), but are worried that banning payday loan stores will hamper access to emergency credit, then our results should provide cause for comfort. Additionally, our results speak to the reasons that people use payday loans in the first place, and to what might be an effective way to limit their use to customers in need of short-term funds.

Table 3.1: Summary of Use of Alternative Financial Services

	Ever Used	Used in Past Year
Payday Loan	4.23	2.55
	(20.12)	(15.77)
Pawn Shops	6.81	2.48
	(25.19)	(15.55)
Rent-to-Own	4.41	1.71
	(20.53)	(12.98)
Any AFS	11.94	5.78
	(32.42)	(23.34)
N	88,113	87,927

Column 1: percent ever using each type of credit.
Column 2: percent using each type of credit in past 12 months.
Standard deviations in parentheses.

Table 3.2: Demographic Characteristics by Use of Alternative Financial Services

	All	AFS	Payday	Pawnshop	Rent-to-Own
Male	50.5	45.0	45.1	46.2	42.4
	(50.0)	(49.8)	(49.8)	(49.9)	(49.4)
Married	50.8	37.3	39.0	35.3	37.9
	(50.0)	(48.4)	(48.8)	(47.8)	(48.5)
White	81.3	68.4	66.8	69.1	66.6
	(39.0)	(46.5)	(47.1)	(46.2)	(47.2)
Age	49.9	41.0	41.7	41.0	39.4
	(16.9)	(13.4)	(13.6)	(13.3)	(12.9)
Income	50,049	33,239	38,851	28,495	30,908
	(35,452)	(25,975)	(27,168)	(24,474)	(24,483)
Less than HS	12.0	19.7	14.1	22.4	26.0
	(32.5)	(39.8)	(34.8)	(41.7)	(43.9)
Unemployed	5.6	14.0	10.9	18.7	13.0
	(22.9)	(34.7)	(31.1)	(39.0)	(33.6)
N	87,927	4,799	2,124	2,080	1,434

Table reports mean values of each demographic characteristic by use of credit type.
Credit use is defined as having used in the past 12 months.
Standard deviations in parentheses.

Table 3.3: Purpose for Use of Alternative Financial Services

	Full Sample (1)	Recent AFS Users (2)
Make Up for Lost Income	1.39	18.83
	(11.72)	(39.10)
House/Car Repairs or Buy Appliance	0.97	12.85
	(9.81)	(33.46)
Medical Expenses	0.17	2.42
	(4.16)	(15.37)
Basic Living Expenses	3.46	43.37
	(18.27)	(49.56)
School or Childcare Expenses	0.16	1.76
	(3.94)	(13.17)
Special Gifts or Luxuries	0.50	5.03
	(7.05)	(21.86)
Other Reasons	2.16	15.74
	(14.55)	(36.42)
N	87,792	4,737

Table reports reason for use of payday loans, pawnshops, or rent-to-own among
all participants (column 1) and participants using AFS in the past year (column 2).
Standard deviations in parentheses.

Table 3.4: Reason for Using Payday Lender or Pawnshop versus Traditional Bank

	Payday (1)	Pawnshop (2)
Banks Don't Give Small Dollar Loans	20.3	15.8
	(40.2)	(36.6)
More Convenient Hours or Location	12.5	9.4
	(33.1)	(29.1)
Easier or Faster	43.4	40.4
	(49.6)	(49.1)
Feels More Comfortable	1.5	2.7
	(12.3)	(16.4)
Don't Qualify for Bank Loan	15.1	21.5
	(35.8)	(41.1)
Other	7.1	10.0
	(25.7)	(30.0)
N	738	1,241

Standard deviations in parentheses.
Table reports percent of recent users of payday loans (pawnshops) who report using AFS credit instead of a traditional bank for the following reasons.
Includes data from 2011 survey only.

Table 3.5: Effect of Payday Loan Regulation on the Use of Payday Loans

	(1)	(2)	(3)
Payday Ban	-0.0237*	-0.0283*	-0.0250*
	(0.0128)	(0.0155)	(0.0134)
Post	-0.0140***	-0.0160***	-0.0262**
	(0.0019)	(0.0022)	(0.0115)
Male		-0.0027***	-0.0027***
		(0.0010)	(0.0010)
Married		-0.0048***	-0.0048***
		(0.0014)	(0.0014)
White		-0.0194***	-0.0194***
		(0.0030)	(0.0030)
HS Only		0.0033	0.0033
		(0.0026)	(0.0026)
College		-0.0137***	-0.0137***
		(0.0028)	(0.0028)
Age		0.0004	0.0004
		(0.0003)	(0.0003)
Age^2		-0.0000***	-0.0000***
		(0.0000)	(0.0000)
Unemployed		0.0124***	0.0124***
		(0.0042)	(0.0042)
Income 15-50k		0.0069***	0.0069***
		(0.0021)	(0.0021)
Income 50-75k		-0.0033	-0.0034
		(0.0029)	(0.0029)
Income gt75k		-0.0099***	-0.0099***
		(0.0029)	(0.0028)
Log Population			-0.1177
			(0.1768)
Log Income PC			0.1396
			(0.1176)
Log Unemp Rate			0.0248
			(0.0240)
Dep Var Mean	0.0247	0.0254	0.0254
N	85,980	80,063	80,063

Standard errors clustered at the state level in parentheses.

Outcome variable: probability of using payday loan in the past 12 months.

Table 3.6: Effect of Payday Loan Regulation on the Use of Other Alternative Financial Services

	Pawn Shop	Rent-to-Own	Any AFS
	(1)	(2)	(3)
Payday Ban	0.0165**	0.0019	-0.0013
	(0.0064)	(0.0038)	(0.0062)
Post	0.0048	-0.0018	-0.0221
	(0.0080)	(0.0046)	(0.0142)
Male	-0.0007	-0.0037***	-0.0057***
	(0.0018)	(0.0010)	(0.0018)
Married	-0.0030*	-0.0007	-0.0082***
	(0.0016)	(0.0013)	(0.0018)
White	-0.0097***	-0.0094***	-0.0304***
	(0.0030)	(0.0020)	(0.0046)
HS Only	-0.0096***	-0.0144***	-0.0173***
	(0.0030)	(0.0027)	(0.0051)
College	-0.0231***	-0.0285***	-0.0541***
	(0.0043)	(0.0036)	(0.0064)
Age	0.0007***	-0.0002	0.0008***
	(0.0002)	(0.0002)	(0.0003)
Age2	-0.0000***	-0.0000**	-0.0000***
	(0.0000)	(0.0000)	(0.0000)
Unemployed	0.0466***	0.0129***	0.0604***
	(0.0060)	(0.0033)	(0.0078)
Income 15-50k	-0.0235***	-0.0051**	-0.0172***
	(0.0034)	(0.0023)	(0.0032)
Income 50-75k	-0.0373***	-0.0147***	-0.0456***
	(0.0041)	(0.0024)	(0.0039)
Income gt75k	-0.0419***	-0.0158***	-0.0577***
	(0.0051)	(0.0024)	(0.0052)
Log Population	0.0992	-0.1846**	-0.1040
	(0.1219)	(0.0818)	(0.2602)
Log Income PC	-0.0073	-0.0273	0.0752
	(0.0723)	(0.0535)	(0.1444)
Log Unemp Rate	0.0040	0.0044	0.0328
	(0.0173)	(0.0116)	(0.0315)
Dep Var Mean	0.0262	0.0174	0.590
N	80,063	80,139	79,630

Standard errors clustered at the state level in parentheses.
Outcome variables: probability of using a pawnshop, rent-to-own, or any alternative financial service in the past 12 months.

Table 3.7: Effect of Payday Loan Regulation on the Reason for Using Alternative Financial Services

	Lost Income	Basic	Repairs	Medical	Childcare	Luxury	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Payday Ban	0.0051	-0.0187**	-0.0003	0.0001	0.0001	-0.0002	-0.0061**
	(0.0063)	(0.0082)	(0.0016)	(0.0005)	(0.0006)	(0.0013)	(0.0026)
Post	-0.0107***	-0.0199***	-0.0046	-0.0015	-0.0001	-0.0036	-0.0439***
	(0.0038)	(0.0067)	(0.0051)	(0.0015)	(0.0020)	(0.0034)	(0.0057)
Dep Var Mean	0.0141	0.0345	0.0094	0.0017	0.0015	0.0048	0.0212
N	82,058	82,058	82,058	82,058	82,058	82,058	82,058

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics, state-level economic conditions, and state fixed effects.

Outcome variables: probability of using alternative financial services for (1) making up for decreased income, (2) basic living expenses, (3) house or car repairs or to buy an appliance, (4) medical expenses, (5) school or childcare expenses, (6) special gifts or luxuries, or (7) other expenses.

Table 3.8: Effect of Payday Loan Regulation on Why Pawnshop versus Bank

	No Small Loan	Convenient	Faster	Comfort	Don't Qualify	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Payday Ban	0.0113**	-0.0031	-0.0024	-0.0001	0.0045**	0.0022
	(0.0053)	(0.0021)	(0.0097)	(0.0007)	(0.0021)	(0.0040)
Post	0.0137***	-0.0135***	0.0024	-0.0017	0.0046	-0.0146
	(0.0046)	(0.0042)	(0.0080)	(0.0013)	(0.0039)	(0.0093)
Dep Var Mean	0.0089	0.0102	0.0269	0.0017	0.0101	0.0108
N	82,151	82,151	82,151	82,151	82,151	82,151

Standard errors clustered at the state level in parentheses.

All specifications include individual demographic characteristics, state-level economic conditions, and state fixed effects.

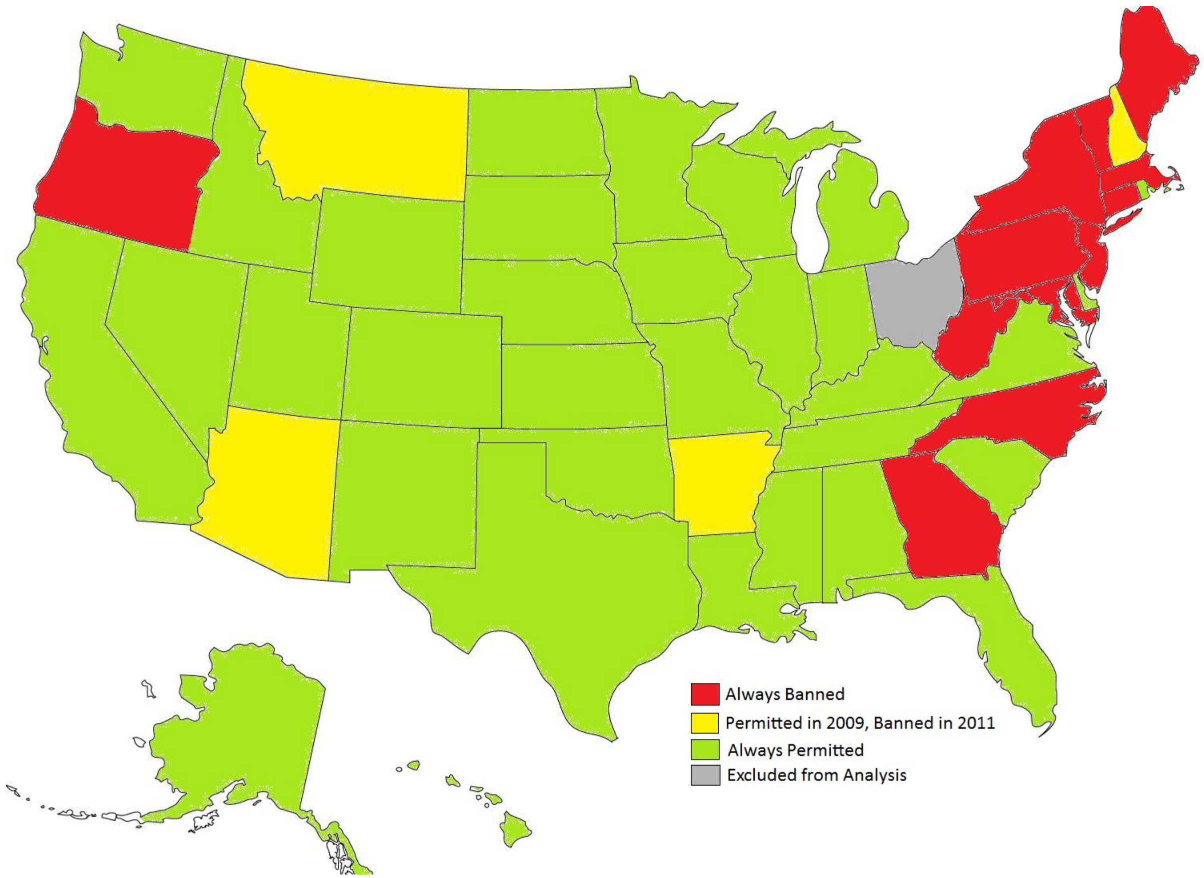
Outcome variables: probability of using a pawn loan rather than a traditional bank because (1) banks do not give small-dollar loans, (2) service is more convenient, (3) easier or faster, (4) more comfortable, (5) do not qualify for bank loan, or (6) other reasons.

Table 3.9: Effect of Payday Loan Regulation on the Use of Alternative Financial Services: Distance to Border

	Payday (1)	Pawn (2)	Rent-to-Own (3)	Any AFS (4)
Payday Access	0.0125***	0.0110	-0.0018	0.0123
	(0.0041)	(0.0106)	(0.0047)	(0.0096)
Border	-0.0037	0.0026	-0.0052	-0.0059
	(0.0033)	(0.0026)	(0.0035)	(0.0037)
Male	-0.0040***	-0.0010	-0.0031***	-0.0066***
	(0.0013)	(0.0020)	(0.0009)	(0.0019)
Married	-0.0058***	-0.0019	-0.0016	-0.0083***
	(0.0017)	(0.0018)	(0.0010)	(0.0023)
White	-0.0228***	-0.0104***	-0.0133***	-0.0371***
	(0.0036)	(0.0030)	(0.0021)	(0.0048)
HS Only	0.0027	-0.0126***	-0.0155***	-0.0207***
	(0.0030)	(0.0042)	(0.0032)	(0.0061)
College	-0.0148***	-0.0255***	-0.0277***	-0.0561***
	(0.0037)	(0.0058)	(0.0042)	(0.0084)
Age	0.0004	0.0007***	-0.0001	0.0009**
	(0.0003)	(0.0002)	(0.0002)	(0.0004)
Age^2	-0.0000***	-0.0000***	-0.0000	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Unemployed	0.0171***	0.0426***	0.0088**	0.0556***
	(0.0047)	(0.0061)	(0.0034)	(0.0081)
Income 15-50k	0.0098***	-0.0241***	-0.0035	-0.0135***
	(0.0027)	(0.0045)	(0.0027)	(0.0039)
Income 50-75k	0.0005	-0.0373***	-0.0116***	-0.0395***
	(0.0035)	(0.0051)	(0.0023)	(0.0051)
Income gt75k	-0.0053*	-0.0428***	-0.0122***	-0.0510***
	(0.0030)	(0.0059)	(0.0023)	(0.0061)
Log Population	-0.0015	-0.0027*	-0.0036***	-0.0057***
	(0.0012)	(0.0014)	(0.0012)	(0.0020)
Log Income PC	-0.0148	-0.0085**	-0.0111*	-0.0302**
	(0.0089)	(0.0034)	(0.0056)	(0.0123)
Log Unemp Rate	-0.0028	-0.0063	-0.0039	-0.0074
	(0.0070)	(0.0073)	(0.0041)	(0.0114)
Dep Var Mean	0.0257	0.0262	0.0177	0.0596
N	58,092	58,107	58,160	57,770

Standard errors clustered at the state-distance level in parentheses.
All specifications include state*year fixed effects and controls for access to pawnshops.
Outcome variables: probability of using a payday loan, pawnshop, rent-to-own, or any alternative financial service in the past 12 months.

Figure 3.1: Map of Payday Loan Regulations



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