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Do Household Finances Constrain Unconventional Fiscal Policy?*

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Abstract

When the zero lower bound on nominal interest rate binds, monetary policy makers may lack traditional tools to stimulate aggregate demand. We investigate whether "unconventional" fiscal policy, in the form of pre-announced consumption tax changes, has the potential to meaningfully shift durables purchases intertemporally and how it is affected by consumer credit. In particular, we test whether car sales react in anticipation of future sales tax changes, leveraging 57 pre-announced changes in state sales tax rates from 1999-2017. We find evidence for substantial tax elasticities, with car sales rising by over 8% in the month before a 1% increase in the sales tax rate. Responses are heterogeneous across households and sensitive to supply of credit. Consumers with high credit risk scores are most able to pull purchases forward. At the same time, other effects such as customer composition and attention lead to an even larger tax elasticity during recessions, despite these credit frictions. We discuss policy implications and the likely magnitudes of tax changes necessary for any substantive long-term responses.

JEL Classification: D12, E21, G01, G11, H31

Keywords: counter-cyclical fiscal policy, credit market frictions, consumer durables.

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1 Introduction

The prolonged period of low demand and low interest rates during the Great Recession prompted economists to consider new policies to stabilize the business cycle. The constraint of the zero lower bound (ZLB) on nominal interest rates, in particular, prevented the use of short-term interest rate reductions to stimulate consumption and investment. To overcome this problem, macroeconomic theorists (Feldstein 2002, Hall 2011, Correia et al. 2013) proposed an "unconventional" fiscal policy: a commitment to raise consumption taxes in the future. The anticipation of higher consumption taxes can promote intertemporal substitution just as traditional monetary policy does, by raising the price of future consumption relative to current consumption. A credible commitment to raise sales taxes may therefore be used to stimulate consumer spending during a recession, particularly when paired with coincident lump sum transfers or income tax reductions to offset any sales tax-related decline in real income.[1]

Unconventional fiscal policy holds promise in theory but may not be effective in practice. Intertemporal substitution may be muted because consumers are inattentive and unaware of future tax changes. Recent literature in public economics argues that sales taxes are not fully salient (e.g., Chetty et al. 2009). Intertemporal substitution may also be muted by financial frictions. In order to shift expenditures forward in time, households need wealth or credit access, both of which typically decline during recessions. Financial frictions may therefore impede spending even among consumers who are attentive to future sales tax increases. Lastly, there is a question of whether demand elasticity changes over the business cycle, leading to either stronger or weaker responses during recessions. There is some evidence that (compensated) product demand is more price sensitive during recessions, particularly for luxury goods (Gordon et al. 2013). On the other hand, Berger and Vavra (2015) show that durable purchases adjust less frequently during recessions, as households prefer to reduce their consumption of durables and do so through depreciation and reduced purchases because of adjustment costs.

We evaluate these issues by studying the response of vehicle purchases to state sales tax changes in the United States between 1999 and 2017. We measure the number of financed vehicle purchases, at monthly frequency in each zip code, using the FRBNY Consumer Credit Panel/Equifax Data. During the sample period we observe 57 changes in sales taxes at the state

[1] Announcing temporary investment tax credits would have similar effects on business investments (House and Shapiro 2006).

level. The majority of these changes—more than 70%—are sales tax increases and the mean size of the change is 0.5 percentage points. We supplement this analysis with additional data on car registrations by zip code from Experion's AutoCount database.

We focus on vehicle purchases for two reasons. First, large durable purchases such as cars are particularly relevant for countercyclical policies. They are the type of long-lasting good for which *expenditures* can be shifted in time without outsized variation in *consumption* and the category of consumption that varies most through the business cycle. For example, during the Great Recession, the decline in durable goods was about triple that in nondurables. Second, vehicle sales taxes depend on the location of the vehicle registration rather than the location of the purchase. This feature proves crucial for predicting the effect of a national sales tax increase on expenditures. For other goods and services that are taxed based on the location of purchase, consumers can avoid state taxes by shopping across state borders. The local spending response in these categories therefore incorporates adjustments to shopping patterns in addition to intertemporal substitution due to taxes. By contrast, vehicle spending responses isolate the intertemporal substitution that is relevant to evaluating a binding national sales tax.

We estimate the elasticity of vehicle purchases to sales taxes by regressing the (log) change in the number of purchases on the change in (log one plus) the sales tax rate. We address the concern that sales taxes are endogenous to economic conditions in two ways. We use the staggered nature of sales tax changes to absorb common national variation in purchases, including business cycle fluctuations, with year-by-month fixed effects. We also examine high-frequency (monthly) *changes* in purchases precisely around the date of the sales tax changes so as to avoid bias from lower-frequency co-movement in tax rates and economic conditions. We present four key findings.

First, consumers respond strongly to anticipated changes in future sales tax rates. We estimate a tax elasticity of 8—that is, vehicle purchases increase by 8% in the current month for each 1 percentage point increase in the subsequent month's tax rate. The anticipatory response is concentrated in the month prior to the tax increase, consistent with a model in which consumers are attentive to future tax changes and accelerate their purchases by just enough to avoid paying higher sales tax. The tax elasticity of vehicle purchases is about 6 times larger than that of other retail spending in Baker et al. (2018), as one would expect based on the high durability of vehicles and the inability to avoid tax increases through cross-border or online shopping.[2]

[2] Using data from retail merchandisers, Baker et al. (2018) show that the tax elasticity of spending is highest for storable or durable consumer retail goods that households can stock prior to a tax increase.

Second, more creditworthy consumers respond more strongly to future sales tax changes. Consumers in the highest quintile of credit or risk scores (either Equifax Risk Score or VantageScore provided by Experian) are twice as responsive as those in the lowest quintile. This heterogeneity in tax elasticity is consistent with a model in which consumers rely on credit to accelerate their vehicle purchases and that credit frictions dampen the intertemporal substitution required for unconventional fiscal policy.[3]

Third, the tax elasticity of vehicle spending varies through the business cycle and, surprisingly, is largest during recessions. In particular, we find that car purchases are twice as responsive during recessions as during non-recessions. This finding holds independent of whether we define recessions nationally or in a state specific manner. The relevance of credit frictions in the cross-section of consumers predicts the opposite effect, namely that the average tax elasticity might be particularly low in recessions, when loan delinquency rates rise (see Figure 1) and lenders reduce credit supply (Benmelech et al. 2017), particularly for less creditworthy consumers (Agarwal et al. 2018). What might the countervailing force be? Some of this increased responsiveness may be driven by increased attention paid to price changes or sale opportunities in a recession. We find that consumers search more intensively on Google for information about sales tax changes during recessions relative to expansions. This increase in search attention comes despite the fact that news coverage of tax changes remains unchanged during recessions. We also find that the composition of buyers changes in recessions. That is, the month before a sales tax increase during a recession sees more of an increase in age, credit score, and credit quality than before a tax increase during non-recessions.

[Figure 1 about here]

Fourth, the sales tax changes that we observe induce substitution over a very short horizon. In our sample, the incremental purchases in advance of tax increases fully reverse in the month of the tax changes, implying substitution over a one-to-two month horizon. Though such rapid reversal would present a problem for unconventional fiscal policy, we are hesitant to conclude that the effects of unconventional fiscal stimulus would reverse so quickly. The reason we remain circumspect is that the sales tax changes in our sample are small, leading to small savings from accelerating purchases, whereas the proposed consumption tax increase in the calibrated New

[3] Two-thirds of car purchases in the U.S. are financed with credit (Board of Governors of the Federal Reserve System 2016)

Keynesian model of Correia et al. (2013) is 10 percentage points. Accelerating the purchase of a $25,000 new vehicle would save $2,500 under the proposed scenario but only $125 for the average tax increase of 0.50 percentage points in our sample. With modest savings at stake, it makes sense that consumers accelerate their purchases by only a few months. A simple back-of-the-envelope calculation illustrates this point. The consumer should compare the sales tax savings to the expense of scrapping an old car earlier than planned. This expense is roughly the monthly depreciation rate of the old car times the number of months by which the purchase and trade-in is accelerated. If we assume straight-line depreciation and a vehicle life of 11 years, we find that consumers should accelerate purchases by one month if the sales tax rate changes by at least 0.6 percentage points, consistent with our findings. Extrapolating to a 10 percentage point increase in taxes, we estimate that consumers should pull future car purchases forward by 16.5 months. Hence, such a policy *could* shift consumer spending substantially, though we are unable to evaluate such large changes directly in our sample.

The paper is organized as follows. Section 2 discusses the related literature. Section 3 describes our data. Section 4 provides institutional details about sales taxes. Section 5 describes our methodology to study the effect of sales tax rate changes on car purchases. Section 6 discusses the results of our analysis. Section 7 concludes.

2 Related Literature

Our paper relates to the literatures on the dynamic effect of consumption tax changes, on financial constraints faced by households, and on the purchase of durable goods during recessions.

There is a growing empirical literature that studies the effect of consumption taxes on consumer spending. Most evidence is based on event studies of one-time changes in value added taxes (VAT) in different countries (e.g., Crossley et al. 2014, Cashin 2014, Cashin and Unayama 2016, Büttner and Madzharova 2017, D'Acunto et al. 2018). However, only few papers study sales tax changes in the U.S. One important difference between VATs and sales taxes in the U.S. is that sales taxes are not included in posted prices and hence could be much less salient to consumers than changes in price-inclusive VATs.

Agarwal et al. (2017) show that a substantial fraction of consumers responds to temporary state sales tax holidays and Baker et al. (2018) show similar results to sales tax rate changes. Most of the response is in the form of stockpiling of consumer goods, with larger responses for more storable and durable goods. However, these studies are limited in scope by either the policy's coverage of a small set of products (e.g., in the case of sales tax holidays in Agarwal et al. 2017) or limitations of the spending data used to study broader sales tax changes (e.g., retail spending covered by AC Nielsen scanner data in Baker et al. 2018).

A related literature in empirical industrial organization shows that it is important to account for anticipated price changes and consumer inventory behavior when estimating dynamic demand elasticities (e.g., Hendel and Nevo 2006, Coglianese et al. 2017).

We contribute to this literature by exploring the role of credit frictions in the transmission of policy changes to aggregate demand. Credit frictions have received increased attention by researchers in the aftermath of the recent financial crisis (e.g., Gross and Souleles 2002, Mian and Sufi 2009, 2011, Mian et al. 2013, Mondragon 2014, Agarwal et al. 2018).

Related to our analysis of the stimulative effect of tax policy on car sales, Mian and Sufi (2012) evaluate the effect of the Car Allowance Rebate System program of 2009 (CARS or "Cash-for-Clunkers"), finding a relatively large demand effect that is reversed within a few months after the end of the program (i.e., evidence of intertemporal substitution).

Green et al. (2018) extend this analysis by studying the role of credit frictions in this context. The large demand response is puzzling considering both credit constraints and transaction fixed costs, which should lower the response. However, the study shows that the large program take-up was mainly driven by the fact that the subsidy provided liquidity at the time of purchase (which is different from other similar programs such as the First Time Homebuyer Credit, where the subsidy is delayed through a tax credit in the following year; e.g., Berger et al. 2014). Hence, financial frictions played a much smaller role for the "Cash-for-Clunkers" program because consumers were able to use the rebates for down payments on new car loans.

Finally, our paper is also relates to research studying whether the effect of a policy depends on the state of the economy (e.g., Auerbach and Gorodnichenko 2012, Ramey and Zubairy 2018, Eichenbaum et al. 2018). As mentioned above, and related to our focus on the demand for durables, Berger and Vavra (2015) argue that more households would like to downsize durables during recessions independent of credit frictions, but adjustment costs prevents them from actively adjusting their stock of durables. Instead, these households let the stock of durables further depreciate, leading to fewer transactions during recessions and cash transfers having a smaller effect on durable purchases than during normal times.

This result is noteworthy since previous work surveyed in Jappelli and Pistaferri (2010, 2017) has shown that the marginal propensity to consume *nondurables* out of income (MPC) is larger among credit-constrained consumers. Hence, the average MPC of nondurables increases in recessions as more households are credit constrained (e.g., Gross et al. 2016). The buffer stock model—the canonical model of intertemporal consumption behavior—also predicts that the MPC is larger for credit-constrained consumers with low levels of liquid assets and low debt capacity (e.g., Carroll et al. 2017). Our findings suggest that while the sensitivity of durable purchases to cash flows might be lower during recessions, the price sensitivity is higher. Hence, policies that affect relative prices might be particularly effective during recessions.

3 Data

The main data on motor vehicle sales comes from the FRBNY/Equifax Consumer Credit Panel and is supplemented with the Experian AutoCount database.

3.1 Sales Tax Data

The main analysis uses hand-collected state sales tax rate changes for 1999-2017. In previous research, we also used zip code level sales tax rate changes. These come from CCH Wolters Kluwer and cover the period 2003-2015. For each zip code-month observation, we have information on all levels of sales taxes imposed in the zip code. These sales taxes stem from four levels of tax jurisdiction: state, county, city, and special tax districts (often comprised of jurisdictions relating to water districts, school districts, water districts, etc.). In addition, we know the combined rate, which may differ somewhat from the sum of the component rates due to local sales tax offsets or statutory maximum tax rates in a jurisdiction.

The local sales tax data cover 41,250 ZIP codes across all 50 states as well as Washington DC. Sales taxes vary significantly across both states and over time, ranging between 0% and 12%. A number of states have no sales tax (Delaware, Montana, New Hampshire, and Oregon), while others are generally high (such as Washington, Louisiana, Tennessee, and California having rates consistently above 8%). A majority of a given zip code's total sales tax rate is driven by the state sales tax. State sales taxes average approximately 5.5%, while local sales taxes total about 1.3%. Distributional data regarding the sales tax changes in our sample — for both state and overall sales tax changes— are detailed in Table 1.

[Table 1 about here]

However, as discussed below, there appears to be significant reporting issues with the car sales data at this granular level, which is the reason we use state-level tax changes in our main analysis.

[Figure 2 about here]

Figure 2 demonstrates some of the considerable variation in state tax rates, both in the cross-sectional and over time. The top panel shows the highest sales tax rate ever applicable to a state during our sample period. The bottom panel maps out the number of sales tax changes during the same period.

Because of persistent differences in car sales and leasing behavior across geographic areas, we focus our analysis primarily on changes in car purchases in the months surrounding *changes* in sales taxes. During our sample period, there were 57 state-level sales tax changes (and more than 2,000 distinct local-level tax changes). The size of a given state sales tax change (in absolute value) varies widely: from less than a tenth of a percentage point to over one percentage point. During our sample period, sales taxes have been rising, on average. Of the 57 state sales tax rate changes that we observe, 42 are positive, with average sales tax rate increase of about 0.61% and the average decrease about 0.46%.

While these sales tax changes are not uniformly distributed across months in a year, they do exhibit a significant amount of variation in timing. For instance, while tax changes in January and July (the beginning of most state Fiscal Years) are the most common months, neither month makes up more than 20% of total sales tax changes. In addition, the first month of each quarter (January, April, July, and October) sees slightly more sales tax changes than the following months.

3.2 FRBNY Consumer Credit Panel/Equifax

We obtained panel data on the number and dollar amount of newly initiated vehicle loans from the FRBNY Consumer Credit Panel/Equifax (CCP) tradeline database.

The CCP is populated with a 5% random sample of individuals who have an Equifax credit report and whose credit report includes their social security number. The panel we use covers the years 1999 to 2017 and contains both unsecured and secured loans/lines of credit for a given borrower. This consists of car loans, but also the multitude of other borrowing such as mortgages and HELOCs. In addition, we observe the borrower's Equifax credit risk score.

For each auto loan taken out by sample members, the CCP tracks the loan amount, origination date, and prepayment or maturity date. We use this auto "tradeline" information to measure the number of newly initiated auto loans in each month, by zip code and credit score grouping. The CCP does not identify financed purchases per se, but we believe the initiation of a loan is a close proxy for a financed purchase since automobile loan refinancing is quite rare. The CCP therefore allows us to analyze financed purchases, but not outright purchases and leases.

3.3 Experian AutoCount

To analyze such outright purchases and leases, we turn to Experian AutoCount data. This panel data reports the number of new and used vehicle purchases by zip code, month and financing status, covering years 2005-2015.

Experian constructs the AutoCount database from two sources: vehicle registration records at state Departments of Motor Vehicles (DMV) and buyer credit reports at Experian's credit bureau. The data are a census of vehicle transactions in the 46 states (and Washington DC) in which the DMV makes registration records available.[4] On a monthly basis, Experian collects the following vehicle transaction information from each state DMV: the locations of the buyer and seller (to the zip code level), vehicle manufacturer, model and model year, vehicle status (new or used), financing status (outright purchase, debt-financed purchase or lease), and month of purchase. For financed purchases (but not outright purchases and leases), Experian then merges in the buyer's credit record at the individual level.

The final database provides aggregate measures of the number of transactions by zip code, month, vehicle status and financing status. We further stratify financed purchases by the buyer's credit score to test whether sales tax elasticities vary by buyer creditworthiness. Details of these variables are noted in Table 1. Car sales in a zip code-month range from 0 up to over 1,000 sales, with the average zip code seeing about 80 total car sales in a month. Leases are significantly more skewed and are highly concentrated in just the top 5% of zip codes.

For our monthly analysis of sales tax changes, it is important to note that the AutoCount data include measurement error in the timing of purchase. Experian does not observe the precise

[4] Delaware, Oklahoma, Rhode Island, and Wyoming do not provide information to Experian AutoCount.

transaction date, but instead identifies the month of purchase as the month in which the transaction first appears in its database. Since the database is updated at a monthly frequency (at least), there can be a one-month lag in the timing of purchase. In particular, when the purchase either occurs or is reported to the DMV subsequent to Experian's monthly update, it will be dated one month after the actual purchase. We discuss this issue in more detail below when interpreting the short-run dynamics around sales tax changes.

4 Sales Taxes, Auto Sales, and Leases

For the purposes of the taxability of automobile sales and leases, the applicable tax jurisdiction is the location in which the vehicle is registered (e.g., if a car is purchased in Gary, Indiana but registered in Chicago, Illinois, the combined state and local sales tax rate in Chicago is the applicable rate). In accordance with this fact, we organize our auto sales and lease data according to where the vehicles were registered. Customers who trade in a vehicle as part of their purchase or lease of a new vehicle will almost always be responsible for sales taxes as applied to the net purchase price, receiving a sales tax credit for their traded-in vehicle.[5]

For a given tax jurisdiction, automobile sales and leases are generally subject to simply the prevailing state and local sales taxes. However, there are a number of deviations from this general trend. Some states, such as Iowa and New Mexico, have a motor vehicle excise tax (e.g., 3%) that is not linked to the state-wide sales tax. We exclude such states from our primary analysis.

The treatment of sales taxes on auto leases varies significantly across states. Arkansas, Maryland, Illinois, Oklahoma, Texas, and Virginia were states that charged sale taxes on the full cost of the vehicle rather than just on down-payments and monthly lease payments. This could be the difference between paying 8% on the car's $40,000 price rather than on a $2,000 down-payment and 24 monthly payments of $300 each.

Other states, such as New Jersey, New York, Minnesota, and Georgia, while only charging sales taxes on down-payments and lease payments, require that all of the future sales taxes be paid up front (e.g., the sales tax applicable to the down payment and sum of all future monthly payments).

One other persistent difference across states is that, for some states, sales taxes may be applied

[5]Exceptions to receiving the tax credit for trade-ins are California, Hawaii, Kentucky, Maryland, Michigan, Montana, and Virginia.

to the full vehicle price rather than the actual price paid by the customer. That is, the sales taxes due may not take into account any dealership or manufacturer rebates or incentives that are applied to the sales price. Thus, a location with a 5% sales tax may see a customer paying an effective tax of 6% or more on their final vehicle purchase price.

While there is variation in both the tax treatment of leases and discounts across states, there is little variation of these policies within states over time and what variation we do see is uncorrelated with changes in sales tax rates.[6]

5 Method

We measure the response of vehicle transactions to sales taxes by regressing monthly changes in vehicle purchases or leases on (leads and lags of) monthly changes in sales taxes. We estimate the regression model:

$$\Delta \ln(cars_{jt}) = \sum_i \beta_i \, \Delta \ln(1 + \tau_{s,t+i}) + \gamma_t + z_{st} + \varepsilon_{jt}, \tag{1}$$

where $cars_{jt}$ is the number of vehicle purchases or leases in zip code j in state s and month t, τ is the sales tax rate, γ_t are time fixed effects, and z_{st} are additional time-varying covariates. We cluster standard errors by state.

The coefficients β_i measure the elasticity of the number or value of vehicle purchases to changes in the sales tax rate. In further specifications, we also conduct sub-sample analysis to test for heterogeneity in the tax elasticity of spending. We focus in particular on the role of credit supply in moderating or amplifying spending responses, splitting by credit risk scores and other measure of creditworthiness.

The economic interpretation of our empirical results depends crucially on whether the tax changes were anticipated. If tax changes are anticipated, then the spending changes around the tax changes reflect substitution effects, while income and wealth effects take place at the time households learn about an upcoming tax change. As mentioned above, such income and wealth effects could be neutralized either over the business cycle or by offsetting changes to labor income or capital taxation as in Correia et al. (2013).

[6]For instance, on January 1, 2015, Illinois shifted its sales tax regime for car leases. Previously, individuals leasing a car would pay sales taxes on the entire capitalized cost of the vehicle. After the change, sales taxes are only due on the down payment and monthly lease payments.

Using newspaper articles, Baker et al. (2018) show that the typical sales tax rate change has a "fiscal inside lag" of 6 to 9 months, i.e., the difference between when the policy is announced or voted on and when it is implemented. Sales tax changes passed by ballot propositions tended to have lower lags in implementation, but still generally give at least 2-3 months of potential anticipation before a change takes effect. The study also documents the fact that searches on Google containing the term "sales tax" tended to spike *before* the new tax rate is implemented, suggesting that households were aware of upcoming changes and were seeking information about them.

6 Results

6.1 Retiming of Car Purchases Around Sales Tax Changes

Table 2 shows our main results estimating equation 1 using the Equifax data. We find that consumers pull car purchases forward by one month in anticipation of a state sales tax increase. Even though sales taxes are not included in posted prices and are therefore potentially less salient than price-inclusive consumption taxes such as excise taxes or VATs, consumers seem to pay attention to the tax changes. Table 8 discussed in Section 6.3 below supports this finding by showing that Google searches increase substantially in the months before tax rates change.

[Table 2 about here]

In column 1, we report a large tax elasticity, over eight, in anticipation of the tax change. This sizable increase in purchases is then offset by a large decline in the month when the increase is implemented. In the month following the initial month of tax increase, purchases increase from the low level of the month of implementation to approximately the level that existed prior to tax increase. This dynamic suggests that some purchases from the period of the tax change are moved forward by approximately one month and then the trend in auto sales largely reverts.

6.1.1 Tax Increases, Decreases, and Size of Tax Change

Columns 2 to 4 show similar results restricting the sample of tax changes to either large changes (at least 0.5 percentage points in absolute value) or if we separate the changes into increases in tax rates and decreases. The results for increases are modestly larger suggesting that households might be more likely to pull purchases forward in response to future tax increases than delay them in response to an anticipated decline. However, as shown in Table 1, the great majority of tax changes in our sample are increases implying that the increase results rely on a broader set of observations.

6.1.2 Longer-Term Spending Dynamics

Column 5 adds local-level sales tax changes to the analysis. For this column, the change in tax is the change in total sales taxes at the zip code level, as compared to the state only changes in columns 1 to 4. The overall pattern of a pre-implementation increase followed by an implementation month decline persists. However, all results are muted. There are two potential explanations for this attenuation. First, local sales tax changes may be less salient than state sales taxes because consumers are less aware of these changes and hence less responsive to them. This may be partially due to the fact that local changes tend to be more modest. The challenge with this explanation is that in previous research with retailer-specific and household-specific data, no difference in responsiveness between local- and state-level changes was seen.

An alternative explanation is that local-level taxes are not correctly applied or accounted for during car sales. This could introduce substantial measurement error. Another potential manifestation of this is that many households are not aware that these taxes are applied based on the owner's place of residence and as a result believe that non state-wide sales tax increases can be avoided. In support of this conjecture we note that the AutoCount data confirm that the majority of car sales occur in a zip code different from the one where the owner resides, while nearly all occur in the state of residence. In addition, cars are among the only items where sales tax is applied based on place of residence rather than based on the point of sale.

Finally, column 6 shows longer term dynamics, adding more leads and lags, and estimating in log-levels. This column displays a similar pattern, but also shows that there is potentially a weak (and insignificant) negative pre-trend. Hence the short-run response we see in the surrounding months are on top of a negative trend, which might be the reason for the sales tax increase.[7]

[7] As with the previous columns, column 6 finds a much larger response in spending on automobiles surrounding a tax change than seen in Baker et al. 2018 utilizing Nielsen retail spending data. Relative to the log-level of spending in months -4 to -2, auto sales are 7.2% higher in month -1, 2.4% lower in month 0, and indistinguishable from 0 in the following months (coefficients of 0.66 and -1.11). In contrast, using the same specification, Nielsen retail sales were 1.1% higher in month -1, 0.8% lower in period 0, and indistinguishable from 0 in the following months (coefficients of 0.22 and 0.17). We posit that the starkly larger responses surrounding the tax changes are likely driven by the differences in durability between automobiles and Nielsen retail goods, making it rational to

[Table 3 about here]

In Table 3, we show a similar results as in column 1 of Table 2, but now using AutoCount data. The AutoCount data are generated via a different process and, as a result, cover a somewhat different set of transactions. In particular, the AutoCount data cover car purchases independent of whether they were financed, but lack data on financed and non-financed transactions that were not purchased from dealers (in most states). We also know whether a car was new or used when purchased, which is important since we would expect new car sales to have a much larger impact on aggregate demand than used car transactions.

As noted earlier, the AutoCount data also have modest, but potentially important, mismeasurement in the timing of transactions for two reasons. First, a car enters the data set at the time of registration rather than at the time of purchase. There is generally a small delay between purchase and registration. This delay is likely to be more severe for used cars not purchased from dealers, which are not included in the data set. Second, the month of the data set is the month when the data were updated, rather than the actual date of the registration.

While the data are pulled late in the month, some registrations may not have entered the database by the time the data are pulled and may end up being measured in a subsequent month's transactions. As a result, some sales that are attributed to month t in the AutoCount data may have actually occurred in month $t-1$. This is particularly problematic if the dynamics around sales tax changes are very short run and there is a substantial increase in purchasing in the last couple days before the increase as these are the transactions that are most likely to be attributed to the subsequent month.

Column 1 shows the results for total sales. We see a substantial increase in purchasing in the month prior to an increase, an insignificant increase in the month of the tax hike and a decline in the month following the increase.

6.1.3 New vs. Used Car Sales and Financing Status

Columns 2 to 4 show a similar pattern based on a breakdown by financing status - the pattern in total sales is quite similar to the patterns in financed and non-financed sales as well as leases. Columns 5 to 8 trace the responses separately for used and new car purchases. New car purchases seem to respond particularly strongly to sales tax changes, as purchases rise both prior

engage in a larger degree of 'stocking up' on cars than on things like groceries and sundries.

and contemporaneous with the tax change, and the cumulative increase—the sum of these two coefficients—exceeds that of used purchases by 50%.

There are three takeaways from this table. First, the timing issue appears to be large and meaningful when comparing these results to those in Table 1 with more precise measurement of transaction month. The continued (though sometimes insignificant) growth in purchasing during the month of the tax increase likely reflects the combination of growth in the last few days before implementation and decline following implementation. The month after implementation then represents the first month of data when all transactions are subject to the new tax rate. Second, the response of purchases to the tax change is very similar across the eight columns in the table. Importantly for our analysis, the results for financed sales are very similar to the results for total sales. This suggests that the patterns for the financed sales in the Equifax data are representative of overall sales. Third, new vehicle purchases respond, if anything, more strongly to sales tax changes than do used purchases. Sales tax policy therefore matters for the level and timing of vehicle production rather than just the timing of trades within the existing vehicle stock.

6.2 Consumer Credit and the Responsiveness of Car Sales

In Table 4, we divide individuals into quintiles of the Equifax Risk Score distribution to investigate whether tax changes affect these different groups in substantially different ways. Note that the 1st quintile denotes the largest credit risks and the 5th quintile denotes the lowest risk group. We find that the general pattern of the response is comparable across the different Equifax Risk Score groups, with sales increasing in advance of a tax decrease and decreasing after the tax is increased.

[Table 4 about here]

However, individuals in the highest two quintiles are nearly twice as responsive as individuals in the bottom quintile in both the month of and the month prior to a tax change. This is consistent with credit constraints playing a factor in aggregate responsiveness of car sales to foreseen price/tax changes. Households with higher credit risk scores are more able to obtain financing to purchase a car earlier than they would have in the absence of a tax change. Households with the lowest credit risk scores may be unable to obtain financing or be unable to save up the necessary down-payment for a car purchase with one or two fewer months to save. Overall, these results suggest substantial distributional effects across households, with higher credit risk score households making up a greater proportion of car sales in the month prior to a tax increase. We test this prediction directly in Table 7.

[Table 5 about here]

Table 5 performs a similar analysis using the AutoCount data and dividing households into quintiles of the VantageScore distribution to measure credit availability. We also divide the analysis into total, new and used sales. We find that the response of new vehicle purchases to sales taxes seems particularly sensitive to credit availability. In particular, we observe the lowest levels of responsiveness for new purchases among those at the bottom of the credit risk score distribution. By contrast, there is no meaningful gap in the responses of households at the top and bottom of the VantageScore distribution for used car purchases. Part of the reason for this disparity may be that financing is more important for the purchase of new, higher-priced cars.

6.3 Car Sales Responsiveness During Recessions

One interpretation of the results in Tables 4 and 5 is that sales tax changes might be an ineffective policy tool during recessions or financial crises when credit is tight. Credit access might serve to dampen the response to unconventional fiscal policy at the time when it is most needed. In Table 6 we investigate this conjecture directly, showing that responses during recessions are not depressed because there are other effects that go in opposite direction. These effects tend to dominate the dampening caused by growing credit frictions.

[Table 6 about here]

In columns 1 and 2 we show breakdowns in the responsiveness to tax changes based on whether the economy is in recession. We find that the elasticity is more than twice as large during recessionary periods as in normal times. Here, we define recessions at a national level (using the timing from the NBER dating committee), because we might expect aggregate shocks to the credit environment to be most important during national recessions. Columns 3 and 4 show that we continue to find much larger effects during recessions if we define a recession using state specific economic measures. In particular, we define a state as being in recession if the value of the State Coincident Index developed by the Federal Reserve Bank of Philadelphia is below its reading from three months prior.

This larger response during recessions suggests that other factors that work in opposite direction during recessions more than offset the dampening effect of credit frictions. We explore two such potential mechanisms, differences in the composition of consumers and differences in attention to taxes.

6.3.1 Differences in Buyer Composition During Recessions

In columns 5 to 9 we investigate whether there are different reactions to recessions within each Equifax Risk Score quintile by adding recession interactions to our Equifax Risk Score based regressions in Table 4. These columns demonstrate that while all consumers are more responsive in recessions, households with high Equifax Risk Scores are much more responsive. This pattern is suggestive of composition effects driving part of the larger response in recessions. In particular, the sales tax rate changes during recessions might draw in more buyers with higher credit risk scores. This could occur if Equifax Risk Score borrowers are a larger fraction of the pool during recessions or if they become more responsive due to the condition of the economy.

[Table 7 about here]

Table 7 approaches this question in a different way. Here, we test whether the average characteristics of car buyers is more sensitive to sales tax changes during recessions than in normal times. We find substantial evidence that this is the case.

Most importantly, columns 1 and 2 show that credit risk scores are higher in the month before the tax change, suggesting that high credit risk score buyers do take advantage of purchasing before a sales tax increase more so than lower credit risk scores. This effect is much stronger during recessions than during non-recessionary periods. Similarly, we find equivalent patterns in columns 3 to 8.[8]

The likelihood of a car buyer also having a mortgage (proxy for being wealthier and more credit-worthy) is much higher in the month preceding a sales tax increase. Car buyers tend to buy more expensive cars, reflected in the increase in loan amounts, in the month before a sales tax increase. And car buyers are disproportionately young in the month following a sales tax increase. All of these indicators suggest that richer, more credit-worthy, and more sophisticated

[8] The sample sizes in this table are smaller than in the other tables. In the other tables we are able to impute missing values with a 0 if there was no purchase in a given zip code-month. In Table 7 however, we need at least one purchase in each zip code-month in order to have an existing value for the dependent variable (Equifax Risk Score, mortgage, etc.).

car buyers are the ones who respond most strongly. Moreover, these households seem to be particularly sensitive during recessions.

In addition, these short-term swings are large relative to the differences in average car purchaser composition during recessions and non-recessions. For instance, during recessions, a 1% sales tax increase is associated with an approximate 8 point swing in purchaser credit risk score from the month prior to the increase to the month of the increase. In normal times, this swing is only 3 points.

We should note that all our estimates that find larger responsiveness during recessions are based on growth rates. It may be true that these larger elasticities during recessions represent similar level changes in car sales if there is a sufficiently smaller base during recessionary periods. In Table 6 we find that the responsiveness in recessions is more than double the response during non-recessions. For the level changes to be similar, we would need the base level of car sales during non-recessions to be about double the level in recessions. While car sales are substantially lower during recessionary periods, they are only about 30% lower in our data so a lower base is insufficient to reverse the higher responsiveness during recessions.

[Table 8 about here]

6.3.2 More Attention to Taxes in Recessions

Columns 5-9 of Table 6 show that the differential response in recessions and normal times to a pre-announced sales tax change remains even conditional on the Equifax Risk Score (i.e., within quintiles of the score). This suggests that while composition effects can explain some of the difference in tax elasticities in recessions and normal times, there are other factors that lead these elasticities to differ.

Table 8 presents evidence of one such other factor explaining why households may be more sensitive to upcoming price changes during recessions than during normal times. In this table, we look at how sales tax changes affect two indicators of news and attention. Google Trends provides one way of measuring what households are searching for online over time. We can track this at a city- or state-level with a monthly frequency. In columns 1 and 2, we show that Google searches that contain the term "sales tax" increase substantially in the month before and month of a tax change. In column 3, we see that this spike is especially large during recessions, suggesting that households may be more sensitive to a potential price change that they may be able to take advantage of.[9]

In columns 4 to 6, the same exercise is repeated but for the fraction of news articles that mention the term "sales tax", as measured at a state-month level. We wish to test whether the increase in Google searches during recessions is just a function of a greater degree of news coverage of the tax changes themselves. Here, we document a similar increase in news coverage leading up to the tax change, but no differential increase in coverage during recessions. If anything, there seems to be an insignificant relative decrease in coverage of sales tax changes during recessions.

Consistent with the findings of Gordon et al. (2013), these results suggest that households may be more sensitive to price changes during recessions and that households may expend somewhat more effort to take advantage of lower prices where they can. Such an effect would ameliorate the increase in financing and liquidity frictions that are likely to be present during recessions and make it more likely that unconventional fiscal policy could still achieve significant results.

6.4 Duration of the Stimulus

Our findings indicate that household bring spending forward to the months before sales tax rates increase and that this intertemporal substitution is off-set within one or two months. Hence, one might be worried that the effect of such unconventional fiscal policy is too short-lived to be relevant in practice. We now use a simple back-of-the-envelope calculation to show that this is not necessarily the case. The reason is that the observed tax rate changes are small relative to the size of the sales tax rate change that would have been called for during the Great Recession.

Let T denote the economic life of a car (in months) and g the monthly growth rate of the value (or price) of a new car. Hence, for a consumer who replaces his old car, the value of the new car, V_{new}, and the value of the previous car when purchased new T months ago, V_{old}, is $V_{new} = (1+g)^T \times V_{old}$.

Pulling forward the car purchase by one month is worth it if the amount of tax saving exceeds the amount of value lost by scrapping the car one month earlier than planned, i.e., if

$$\Delta\tau \times V_{new} \geq \frac{V_{old}}{T}, \tag{2}$$

assuming straight-line depreciation.

[9] Results are equivalent if we utilize the Google search term "tax" rather than "sales tax". See, for instance, Baker and Fradkin (2017) for a more thorough discussion of the data.

The average new car during our sample period had an economic life of about years ($T = 132$). If the value of new cars grows at the annual rate of inflation of about 2% ($g = 0.17\%$), the break-even tax rate change that leaves the consumer indifferent between purchasing the car in the month of the tax increase versus one month before is 0.61%, which is close to the average state sales tax change (in absolute value) of 0.55% observed over our sample period.

Calibrating a New Keynesian macroeconomic model to the U.S. economy in a recession at the ZLB, Correia et al. (2013) find that consumption taxes would need to increase by about 10 percentage points (from 5% to 15%) and potentially by even more if one takes into account the relatively small tax base of sale taxes in the U.S. Hence, the policy intervention would need to be about 18 times larger than the typical tax change observed in the data. Extrapolating our findings to such a large tax change suggests that the effect might last for about 16.5 months after the tax increase. For comparison, the Great Recession lasted 18 months according to the NBER recession dating committee.

7 Conclusion

In the face of the zero lower bound, monetary policy authorities may be unable to utilize their typical countercyclical policy tools. As one policy alternative, a number of researchers have proposed "unconventional" fiscal policies. These would take the form of stimulus payments financed by pre-announced changes in consumption taxes.

This paper evaluates one measure of the impact of this unconventional fiscal policy on the magnitude of intertemporal substitution component of aggregate demand stimulus, namely car purchases. We address four main questions regarding the effectiveness of such a policy:

1. Are sales tax changes salient enough for consumers to respond significantly?
2. Do credit frictions dampen the response of large durable purchases?
3. Is the response especially low during recessions, precisely when we need it the most?
4. Will the effect of the stimulus last long enough to be policy relevant in practice?

Utilizing high-frequency data surrounding 57 changes in state sales tax rates, we provide answers to these questions. In prior research we show that households actively search for information about upcoming sales tax changes using Google search data, suggesting that sales tax changes are sufficiently salient in practice. In this paper we show that this fiscal foresight also translates into significant effects on durable purchases (in addition to the effects on retail spending documented in Baker et al. 2018). Consumers exhibit significant levels of intertemporal substitution in car purchases, financing, and leasing. Car sales rise (fall) dramatically in the months just prior to a sales tax increase (decrease) and return to trend in the months after the tax change takes effect.

As many consumer durables are purchased with consumer credit, we seek to better understand the importance of credit constraints when households face incentives to pull forward purchases. We demonstrate that credit constraints are indeed an important driver of both the heterogeneous effects across households and the overall size of the intertemporal substitution we observe, with more constrained households exhibiting significantly lower car purchase elasticities. Given the typical decline in credit supply during recessions, it is likely that any unconventional fiscal policy will have a more muted impact than in periods with abundant credit.

These results reinforce the importance of the direct stimulus or liquidity provision component of "unconventional" fiscal policy. Such direct payments to households prior to the onset of any consumption tax increase would alleviate some of the credit and liquidity constraints that likely prevented many households from purchasing a car to take full advantage of the lower sales taxes in the present, as shown by by Green et al. (2018) for the "Cash for Clunkers" program.

Despite these credit constraints having a dampening effect on the stimulus, we find that the average response to a sales tax rate change is actually larger during recessions. We show that this can largely be explained by composition effects and by increased price sensitivity during recessions. Specifically, we find that while most consumers are more price sensitive during recessions, this is especially true for consumers with higher creditworthiness. We show that there is a systematic shift during recession toward consumers with higher credit risk scores, which leads to a substantial increase in the average response.

Going beyond this composition effect, we show that one mechanism which leads to higher price sensitivity during recession may be increased attention. We find that Google searches for the term "sales tax" increase much more during recessions than during expansions, even though news coverage of tax changes is not higher in recessions.

Finally, we show that while the small historical sales tax rate changes we observe in the data lead to relatively short-lived responses, we can reasonably expect the much larger sales tax changes that would be called for during a deep recession to have a much longer-lived effect on demand.

There are several caveats for using our estimates to obtain a complete picture of the effectiveness of unconventional fiscal policy. First, we only study one market. While car purchases are an important component of consumer durables, further research is needed to extrapolate our results to other important markets subject to credit constraints, most importantly residential investment in housing and non-residential fixed investment.[10]

Second, the sales tax rate changes we study are small such that the observed effect on durable expenditures dies out quickly. Studying the large tax changes needed in plausibly calibrated macroeconomic models to respond to a shock that can explain the Great Recession as in Correia et al. (2013) therefore involves a substantial amount of extrapolation. It is possible that there are important non-linearities that our analysis is unable to identify that would affect such a dramatic change in tax rates. Moreover, one might expect substantial supply-side effects and pre-tax price responses to such an unusual policy.

There are also important practical issues that need to be resolved. First and foremost, the U.S. currently does not have a federal consumption tax. Second, there are important questions about time-inconsistency of such policies, which are discussed in Auerbach and Obstfeld (2004, 2005) and Correia et al. (2013). These committment issues are potentially more serious for the federal government than for states, which have balanced-budget requirements that make such budget-neutral tax policies more credible.

Finally, an important open question for future research is how to schedule the temporary tax increase in practice. Policymakers face a subtle trade-off. On the one hand, they would ideally like to start increasing sales taxes as soon as possible to ensure that the increase in demand occurs at the trough of the business cycle. On the other hand, consumers also need enough foresight to pull purchases forward before tax changes increase. Moreover, it is important that households and firms understand that this policy is budget-neutral. An unexpected and uncompensated increase in sales taxes would decrease aggregate demand and potentially worsen the recession.

[10]Here, the unconventional fiscal policy would be a pre-announced property tax increase.

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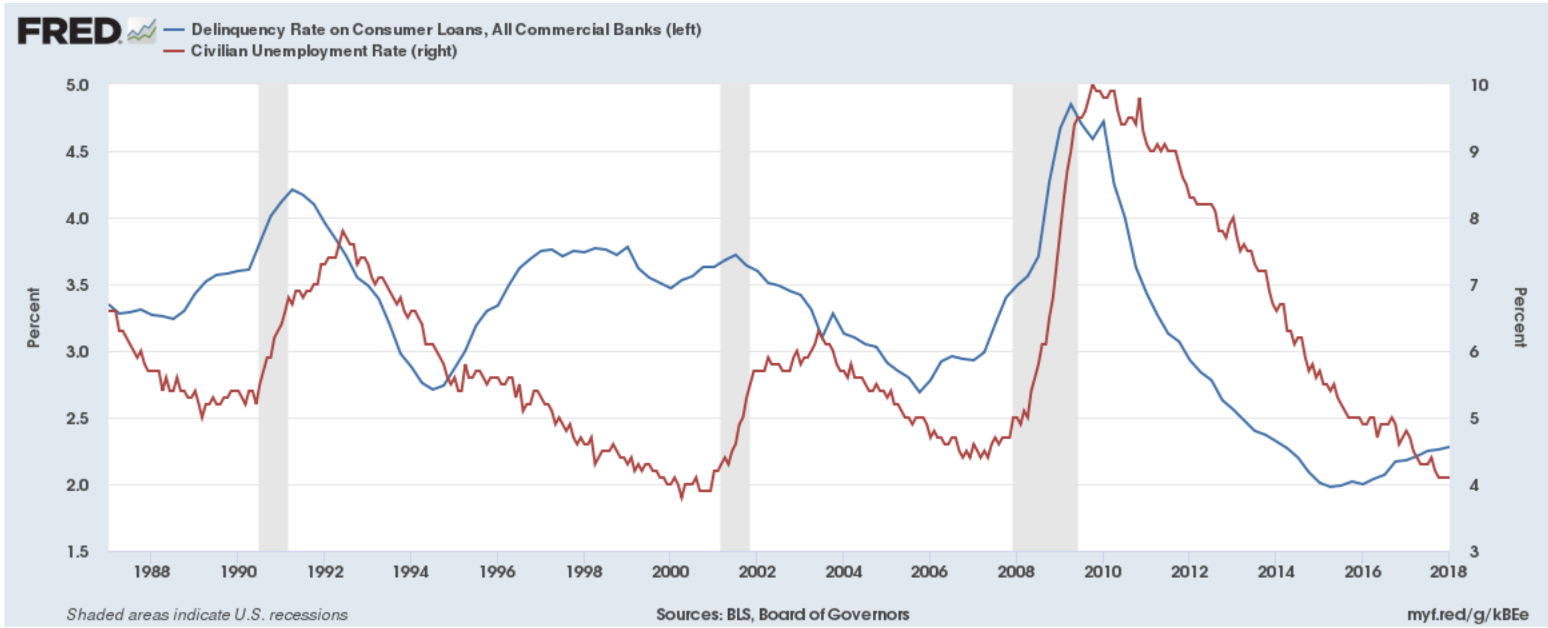
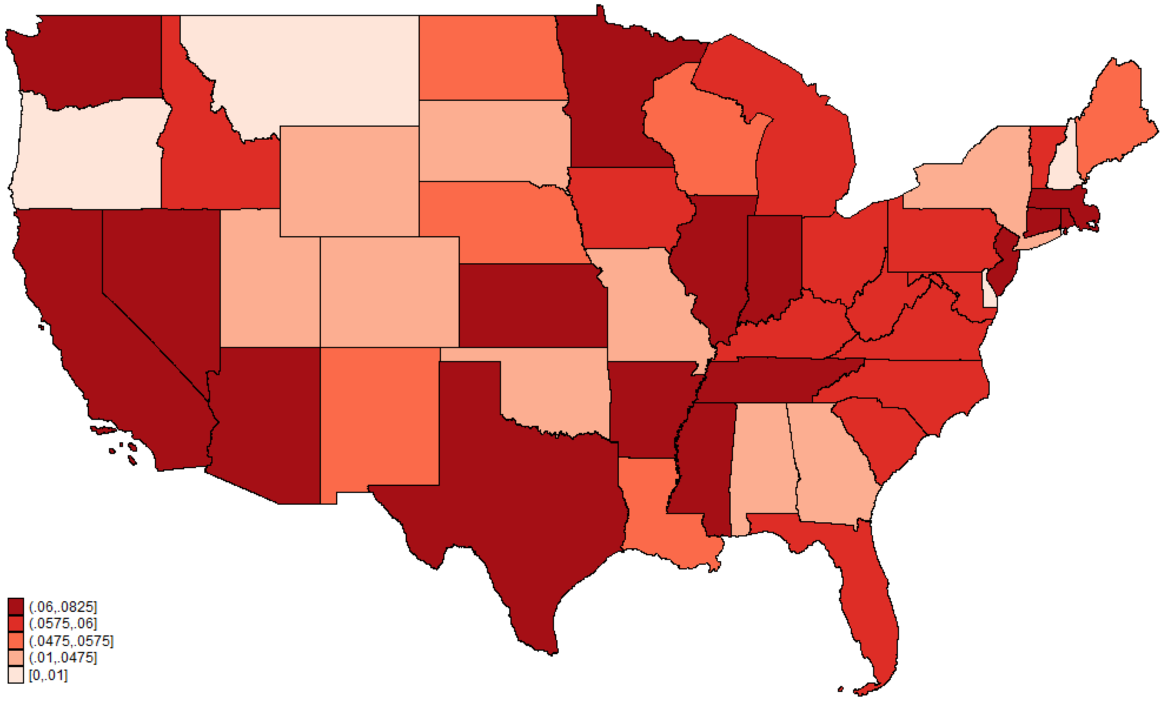
Figure 1: Delinquencies Over the Business Cycle



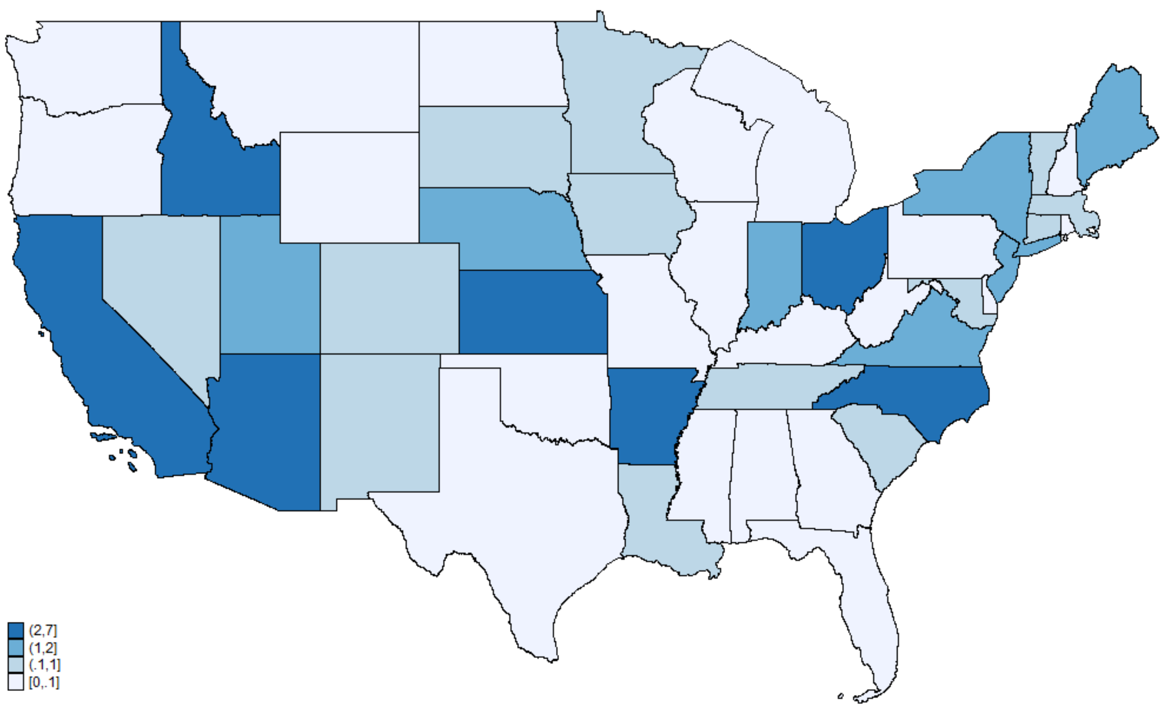
Notes: Graphs display the delinquency rate on all covered consumer loans at commercial banks and the quarterly U3 unemployment rate in the United States. Both series cover 1985-2018.

Figure 2: Sales Tax Rate Variation

(a) Maximum state sales tax rates, 1999-2017



(b) Number of tax rate changes by state, 1999-2017



Notes: Maps plot the maximum level of state sales tax rates for years 1999-2017 (a) and number tax changes for each state in this period (b). Sales tax rates are expressed in percentages.

Table 1: Summary Statistics

	Mean	Min	Percentiles					Max
			5^{th}	25^{th}	50^{th}	75^{th}	95^{th}	
Panel A: CCH Sales Tax Data								
State Sales Tax	5.6%	0	4.0%	4.75%	6.0%	6.25%	7.25%	8.25%
Total Sales Tax	6.8%	0	5.0%	6.0%	7.0%	8.0%	9.2%	12.0%
ΔState Sales Tax	0.55%*	-1.0%	-1.0%	0.25%	0.25%	1.0%	1.0%	1.25%
Panel B: FRBNY Equifax Consumer Credit Panel								
Total Purchases (Unconditional)	3	0	0	0	1	4	14	193
Total Purchases (Conditional >0)	5	1	1	1	3	7	17	193
Equifax Risk Score	680	0	555	638	685	729	789	840
Average Loan Size (in $1,000)	13.9	0	4.7	9.5	12.9	16.7	25.8	100
Credit Card Utilization	0.46	0	0.02	0.23	0.41	0.61	0.98	1.00
Panel C: Experian AutoCount Data								
Total Car Sales	82.8	0	2	10	33	114	317	1,903
New Car Sales	23.1	0	1	2	8	30	95	1,137
Used Car Sales	59.7	0	2	7	24	78	230	1,517
Non-Financed Sales	34.5	0	1	4	13	41	134	1,478
Financed Sales	48.3	0	1	5	17	65	194	1,248
Leases	5.9	0	0	0	0	2	27	5,935

Source: Authors' calculations based on FRBNY Consumer Credit Panel/Equifax data from 1999-2017 and Experian AutoCount data from 2005-2015. For purposes of summary statistics, an observation is a zip code-month. For changes in sales taxes, months with no change are omitted in this table. Statistics are weighted by Census 2000 population.

* The average state change is the average absolute value change.

Table 2: Response of Car Purchases to State Sales Tax Changes, Equifax CCP Data

	All Tax Changes	Large Changes	Increases	Decreases	Total Sales Tax	Levels
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta\log(1+\tau)$, leads 2-4						-2.287
						(1.422)
$\Delta\log(1+\tau)$, lead 1	8.247***	8.832***	8.120***	8.620***	5.493**	4.954**
	(2.782)	(2.625)	(2.832)	(2.998)	(2.203)	(2.318)
$\Delta\log(1+\tau)$	-9.645***	-10.73***	-11.18***	-5.499***	-6.763***	-4.713***
	(1.949)	(2.001)	(2.718)	(1.739)	(2.443)	(1.105)
$\Delta\log(1+\tau)$, lag 1	3.068***	3.142***	1.851***	6.271***	2.010*	-1.630
	(0.805)	(0.713)	(0.668)	(1.401)	(1.142)	(1.095)
$\Delta\log(1+\tau)$, lags 2-4						-3.400***
						(1.120)
$\Delta\log(1+\tau)$, lags 5-8						-3.842**
						(1.537)
Year-by-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,996,086	5,933,599	5,962,468	5,926,423	4,397,986	5,996,086
R-squared	0.024	0.025	0.024	0.024	0.023	0.790

Source: Authors' calculations based on FRBNY Consumer Credit Panel/Equifax data. The dependent variable is log monthly car purchases financed with credit. Specifications are listed in the heading of each column. Changes in columns (1) to (5) and levels in column (6). Regressions are weighted by average annual zip code population from the U.S. Census Bureau. Robust standard errors in parentheses are clustered at the state level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table 3: Response of Car Purchases to State Sales Tax Changes, Experian AutoCount Data

	Total Sales	Financed	Non-financed	Leases	Used	New: All New	New: Financed	New: Non-fin.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δlog(1+τ), lead 1	9.053***	8.862***	7.221***	7.858**	9.391***	7.003***	6.656**	6.111***
	(2.039)	(2.959)	(1.764)	(3.225)	(2.123)	(2.476)	(2.806)	(1.835)
Δlog(1+τ)	3.695	2.836	4.067	4.931	1.032	8.493**	8.172*	9.838***
	(3.784)	(3.934)	(3.667)	(4.808)	(3.261)	(4.155)	(4.358)	(3.225)
Δlog(1+τ), lag 1	-8.986***	-8.668***	-9.924***	-10.538***	-6.039***	-13.867***	-12.327***	-17.605***
	(2.391)	(2.251)	(2.664)	(2.503)	(2.130)	(3.000)	(2.963)	(2.734)
Δlog(1+τ), lag 2	-4.759	-7.952*	-3.192	7.425***	-4.769	-6.952	-8.800**	-5.066
	(5.045)	(4.011)	(5.410)	(2.542)	(5.111)	(4.190)	(4.019)	(5.172)
Year-by-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,356,223	2,356,223	2,356,223	2,356,223	2,356,223	2,356,223	2,356,223	2,356,223
R-squared	0.206	0.166	0.115	0.037	0.187	0.134	0.124	0.041

Source: Authors' calculations based on Experian AutoCount data. Regressions are weighted by average annual zip code population from the U.S. Census Bureau. Dependent variables are listed in the heading of each column. Robust standard errors in parentheses are clustered at the state level. *** p < .01, ** p < .05, * p < .1.

Table 4: Differential Response Across Credit Scores, Equifax CCP Data

	Equifax Risk Score Quintiles (from highest to lowest risk)				
	1st	2nd	3rd	4th	5th
	(1)	(2)	(3)	(4)	(5)
$\Delta\log(1+\tau)$, lead 1	3.966***	3.399**	4.963**	7.537***	6.460**
	(0.920)	(1.559)	(1.996)	(2.003)	(2.602)
$\Delta\log(1+\tau)$	-4.140***	-5.030***	-5.544***	-8.179***	-7.301***
	(1.458)	(0.977)	(0.708)	(1.591)	(2.000)
$\Delta\log(1+\tau)$, lag 1	1.170	1.454	0.963	3.330***	3.209***
	(0.891)	(1.052)	(0.760)	(0.815)	(0.980)
Year-by-month FE	Yes	Yes	Yes	Yes	Yes
Observations	5,989,936	5,989,936	5,989,936	5,989,936	5,989,936
R-squared	0.007	0.008	0.009	0.010	0.010

Source: Authors' calculations based on FRBNY Consumer Credit Panel/Equifax data. The dependent variable is the change in log monthly car purchases financed with credit. Regressions are weighted by average annual zip code population from the U.S. Census Bureau. Robust standard errors in parentheses are clustered at the state level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table 5: Differential Responses Across Credit Scores, Experian AutoCount Data

	Experian VantageScore Quintiles (from highest to lowest risk)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Total Sales	1st	2nd	3rd	4th	5th
$\Delta\log(1+\tau)$, lead 1	8.611***	7.497**	7.686***	7.988***	9.251***
	(2.955)	(2.976)	(2.369)	(2.416)	(3.217)
$\Delta\log(1+\tau)$	2.177	1.252	1.926	3.422	4.672
	(4.579)	(3.509)	(4.010)	(4.207)	(4.211)
$\Delta\log(1+\tau)$, lag 1	-6.505**	-6.315***	-7.748***	-10.071***	-11.122***
	(2.414)	(2.342)	(2.566)	(2.484)	(2.393)
$\Delta\log(1+\tau)$, lag 2	-4.691	-7.835*	-7.654*	-8.539**	-7.639**
	(4.390)	(4.114)	(3.917)	(4.172)	(3.562)
R-squared	0.084	0.085	0.071	0.064	0.057
Panel B: New Sales	1st	2nd	3rd	4th	5th
$\Delta\log(1+\tau)$, lead 1	-2.359*	4.688**	4.639*	6.041**	8.045***
	(1.228)	(2.281)	(2.591)	(2.540)	(2.976)
$\Delta\log(1+\tau)$	2.251	4.537	8.613*	9.220**	10.281**
	(2.248)	(3.675)	(4.484)	(4.449)	(4.509)
$\Delta\log(1+\tau)$, lag 1	-3.544**	-5.967*	-10.357***	-13.136***	-14.890***
	(1.584)	(2.967)	(3.385)	(3.005)	(3.102)
$\Delta\log(1+\tau)$, lag 2	-3.035	-8.300**	-7.786**	-9.344**	-6.245*
	(2.456)	(3.949)	(3.337)	(3.785)	(3.139)
R-squared	0.012	0.044	0.055	0.058	0.056
Panel C: Used Sales	1st	2nd	3rd	4th	5th
$\Delta\log(1+\tau)$, lead 1	9.549***	8.530**	8.943***	8.025***	8.562***
	(3.086)	(3.278)	(1.996)	(2.088)	(2.768)
$\Delta\log(1+\tau)$	1.690	0.339	-1.704	-2.176	-1.890
	(4.536)	(3.346)	(3.478)	(3.390)	(3.373)
$\Delta\log(1+\tau)$, lag 1	-5.912**	-5.993**	-5.178**	-5.799***	-4.384**
	(2.261)	(2.253)	(2.273)	(2.065)	(2.141)
$\Delta\log(1+\tau)$, lag 2	-4.143	-7.175*	-7.106*	-5.928	-8.057*
	(4.297)	(4.187)	(4.085)	(4.063)	(4.133)
R-squared	0.083	0.068	0.039	0.025	0.019
Year-by-month FE	Yes	Yes	Yes	Yes	Yes
Observations	1,460,260	1,460,260	1,460,260	1,460,260	1,460,260

Source: Authors' calculations based on Experian AutoCount data. Regressions are weighted by average annual zip code population from the U.S. Census Bureau. Dependent variables for all columns is the change in log car sales for households within a given credit score quintile, aggregated to the zip-month level. Panel A displays results for changes in total car sales, Panel B does the same for only new car sales, and Panel C does the same for only used car sales. Robust standard errors in parentheses are clustered at the state level. *** p < .01, ** p < .05, * p < .1.

Table 6: Response in Recessions vs. Normal Times

	by NBER Recession Dates		by State Coincident Index		by Equifax Risk Score Quintile				
	Recession	Non-Recession	Recession	Non-Recession	1st	2nd	3rd	4th	5th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δlog(1+τ), lead 1	15.08***	6.328***	13.51***	5.923**	3.913***	1.662	4.175**	6.436***	4.142*
	(2.286)	(2.217)	(2.558)	(2.635)	(1.006)	(1.675)	(1.749)	(1.539)	(2.269)
Δlog(1+τ)	-17.48***	-7.098***	-16.82***	-6.631***	-2.664*	-4.027***	-4.475***	-5.780***	-3.847***
	(2.623)	(1.301)	(2.039)	(1.354)	(1.347)	(0.980)	(0.905)	(1.126)	(1.334)
Δlog(1+τ), lag 1	3.708***	2.867***	3.917***	2.746***	0.477	2.384*	1.095	3.751***	2.244*
	(1.175)	(0.923)	(0.863)	(1.009)	(1.294)	(1.205)	(0.878)	(1.252)	(1.114)
Recession x Δlog(1+τ), lead 1					0.164	5.333***	2.419	3.386	7.116***
					(1.122)	(1.607)	(1.889)	(2.197)	(1.618)
Recession x Δlog(1+τ)					-4.527**	-3.075	-3.281**	-7.356***	-10.60***
					(1.890)	(1.958)	(1.351)	(2.244)	(1.681)
Recession x Δlog(1+τ), lag 1					2.132	-2.864	-0.409	-1.296	2.963**
					(1.712)	(1.917)	(1.109)	(1.656)	(1.185)
Year-by-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	524,157	5,471,929	1,129,625	4,866,461	5,989,936	5,989,936	5,989,936	5,989,936	5,989,936
R-squared	0.017	0.025	0.036	0.022	0.007	0.008	0.009	0.010	0.010

Source: Authors' calculations based on FRBNY Consumer Credit Panel/Equifax and Haver Analytics/Federal Reserve Bank of Philadelphia data. The dependent variable is the change in log monthly car purchases financed with credit. Regressions are weighted by average annual zip code population from the U.S. Census Bureau. Robust standard errors in parentheses are clustered at the state level. *** p < .01, ** p < .05, * p < .1.

Table 7: Composition Effects in Recessions vs. Normal Times

	Equifax Risk Score		Mortgage		ln(Loan Value)		Age	
	Recession	Non-Recession	Recession	Non-Recession	Recession	Non-Recession	Recession	Non-Recession
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta\log(1+\tau)$, lead 1	238.2**	120.4	1.514*	0.297	3.481***	0.0222	5.697	-7.903
	(99.50)	(78.87)	(0.891)	(0.465)	(0.493)	(0.806)	(12.50)	(12.16)
$\Delta\log(1+\tau)$	-595.1***	-196.0**	-3.062***	0.111	-1.966**	0.330	-88.86***	-17.09
	(70.11)	(81.13)	(0.599)	(0.257)	(0.802)	(0.756)	(23.63)	(12.88)
$\Delta\log(1+\tau)$, lag 1	281.5***	172.5**	1.835***	0.00962	1.357***	1.319**	46.02	16.30
	(82.16)	(75.84)	(0.487)	(0.350)	(0.438)	(0.570)	(34.15)	(10.13)
Year-by-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	257,048	2,875,428	256,942	2,840,889	257,048	2,875,428	256,938	2,838,616
R-squared	0.003	0.004	0.002	0.002	0.002	0.002	0.001	0.002
Mean of dep. variable	678.66	675.59	0.47	0.42	9.46	9.54	44.32	44.36

Source: Authors' calculations based on FRBNY Consumer Credit Panel/Equifax data. Dependent variables are listed in the heading of each column. Regressions are weighted by average annual zip code population from the U.S. Census Bureau. Robust standard errors in parentheses are clustered at the state level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table 8: Google Searches and Newspaper Articles during Recessions

	Google Searches			Newspaper Articles		
	(1)	(2)	(3)	(4)	(5)	(6)
Δlog(1+τ), lead 4	-2.368			16.06*		
	(4.774)			(8.006)		
Δlog(1+τ), lead 3	-4.599			35.18***		
	(5.926)			(12.31)		
Δlog(1+τ), lead 2	8.103			36.59***		
	(4.902)			(6.628)		
Δlog(1+τ), lead 1	44.48***	50.37***	38.76***	44.45***	42.34***	45.92***
	(7.178)	(9.819)	(5.943)	(10.20)	(6.146)	(6.870)
Δlog(1+τ)	15.03***			9.749		
	(5.355)			(5.904)		
Recession x Δlog(1+τ), lead 1			57.66***			-16.68
			(13.36)			(15.02)
Year-by-month FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,886	4,814	4,814	6,171	7,293	7,293
R-squared	0.727	0.745	0.729	0.636	0.608	0.599

Robust standard errors in parentheses are clustered at the state level. *** p < .01, ** p < .05, * p < .1.