KDIS Working Paper 24-06

Do Expenditure Cascades Exist? Evidence from Bush Administration Tax Reforms

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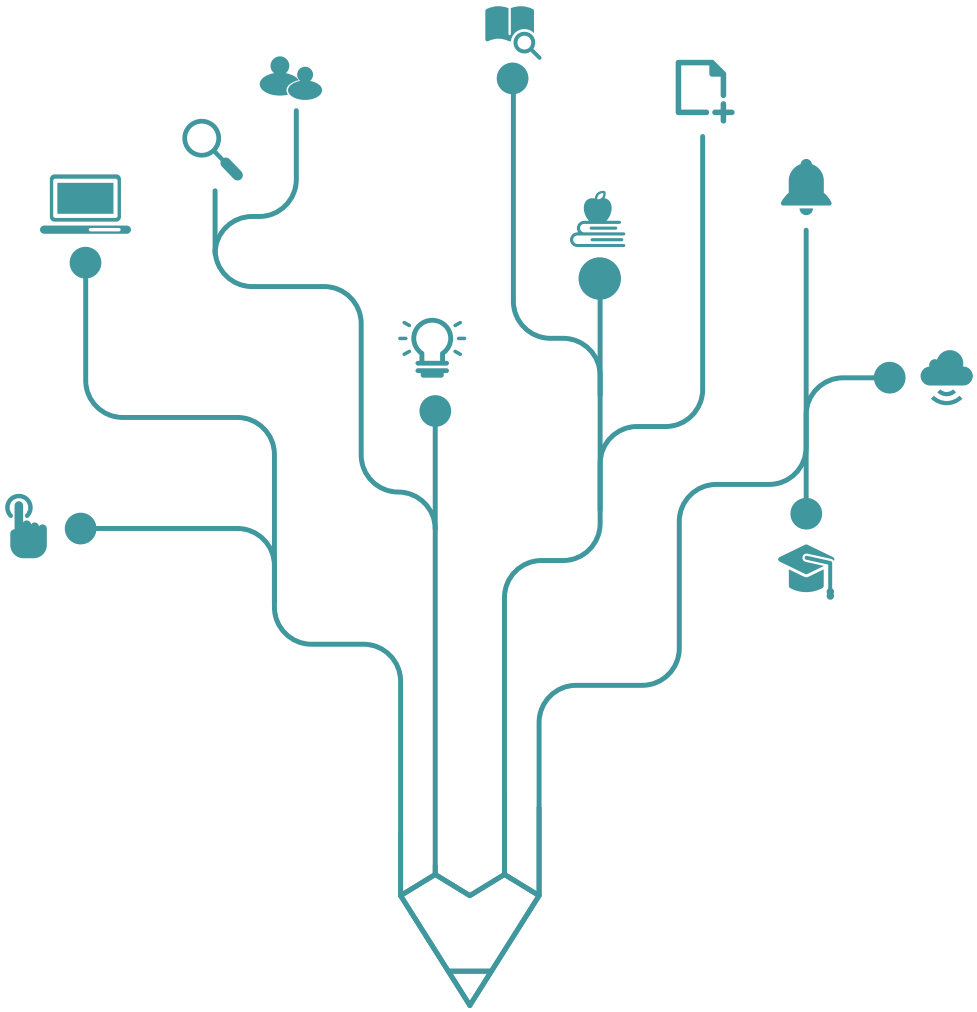

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Do Expenditure Cascades Exist?
Evidence from Bush Administration Tax Reforms*

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September 2024

Abstract

A series of tax reforms made during the Bush Administration reduced the tax burden of upper-income households more substantially than lower-income households, thereby changing the distribution of after-tax income exogenously. This paper exploits variation in the timing of the reforms and the state share of upper-income households to explore whether there exist consumption externalities trickling down along the income distribution. Using the Consumer Expenditure Survey data ranging from 1994 to 2007, I find robust evidence that upper-income households increased their consumption after the reforms, especially for nondurables. However, I do not find evidence that the degree of potential exposure to upper-income consumption affects lower-income consumption.

Keywords: Expenditure cascades, consumption externalities, trickle-down consumption, Bush tax reforms

*I am grateful to Donald Bruce, Marianne Wanamaker, and Celeste Carruthers for their guidance throughout this project. I would like to thank William Fox, LeAnn Luna, Sen Ma, Sumit Agarwal, Jungmin Lee, and seminar participants at the University of Tennessee, Jinan University, China Meeting of the Econometric Society, the University of Nottingham-Ningbo, Asian and Australasian Society of Labour Economics Conference, Seoul National University for helpful feedback and comments. All errors are my own.

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I Introduction

One remarkable feature of recent growing income inequality in the United States is a sharp increase in real income of the top one percent in contrast to stagnant median income (Piketty and Saez, 2003; Piketty et al., 2018). Robert Frank argues that the top income earners would increase their consumption, especially on visible goods and services, as their income soars. This in turn would raise the standard of consumption to which lower-income people refer when they decide how much to spend. The trickling-down process would continue until it reaches the people located at the bottom of the income distribution. This type of consumption externalities, which spread from the upper part of the income distribution to its lower part, is what Frank calls expenditure cascades (Frank, 2005, 2007; Frank et al., 2014).

This paper explores whether the expenditure cascades exist using a unique quasi-experiment in the United States. A series of tax reforms during the Bush Administration in the early 2000s changed the purchasing power of U.S. households exogenously. The reforms reduced federal marginal tax rates (MTR) for households whose MTRs were equal to or greater than 28 percent by at least three percentage points while leaving MTRs for those in the bottom bracket unchanged. As a result, the after-tax income distribution of U.S. households has become widened. Given the nature of the tax rate changes, which have been incremental since the Tax Reform Act of 1986, I believe that the Bush tax reforms provide a unique setting to examine whether greater exposure to upper-income consumption increases lower-income consumption.

The key empirical challenge is to measure the extent to which lower-income households are exposed to upper-income consumption. One natural candidate is to use a direct measure of upper-income consumption. For example, Drechsel-Grau and Schmid (2014) use the average consumption of households whose income levels are higher than own household in a given year to construct reference consumption in Germany. Bertrand and Morse (2016) use the 80th/90th percentile consumption in a given state-year as reference consumption in the United States. Although this approach is intuitive, regressing own consumption on reference consumption might be subject to empirical challenges such as omitted variable bias and reverse causality.[1]

[1] To address these concerns, Drechsel-Grau and Schmid (2014) show that their findings are robust to the inclusion of state by year fixed effects and the exclusion of reference households living in the same state as own households. Bertrand and Morse (2016) show that their findings are robust to state-specific time trends and state unemployment rates.

Alternatively, I employ a difference-in-differences design where I define potential exposures to the reference consumption as the state share of households whose MTRs were reduced significantly as a result of the tax reforms. Duesenberry (1949) acknowledges that frequent contact with people who consume high-quality goods would intensify the desire to improve quality of life by upgrading their consumption bundles. This is called *the demonstration effect*. Similarly, my identification strategy relies on the idea that lower-income households living in regions with a greater share of upper-income households would have a greater chance of observing upper-income consumption, and thus be subject to stronger consumption externalities on average.

Assuming that the differences in lower-income consumption across states remain constant in the absence of the tax reforms, I can identify the causal effects of potential exposure to upper-income consumption on lower-income consumption. The assumption would not be valid if there exists substantial heterogeneity in the growth rate of lower-income consumption. To address potential concerns, I control for an extensive set of state-level time-varying characteristics as well as state fixed effects. I also estimate event-study specifications, which provide suggestive evidence regarding the validity of the parallel trends assumption.

Using the Consumer Expenditure Survey (CEX) data ranging from 1994 to 2007, I show that quarterly consumption of households with high tax liabilities went up by $900 after the tax reforms. The response was mainly driven by increased nondurable consumption. Next, I examine whether greater exposures to the increased upper-income consumption affects lower-income consumption, thus generating the expenditure cascades. In particular, I employ two state-level intensity measures to approximate potential exposures. The first measure is the state share of households whose pre-reform MTRs were 31 percent or greater (MTR31 share). The second measure incorporates population density into the MTR31 share to further account for the differences in the chance of observing upper-income consumption across states. In both measures, I do not find that the lower-income households with a greater risk of being exposed to upper-income consumption consumed more than their counterparts after the tax reforms. Lastly, I examine whether the lower-income households located closer to the MTR31 households on the income scale spent more than those located further away, as the expenditure cascades hypothesis implies. Again, I do not find evidence of the expenditure cascades.

This paper makes two distinct contributions to existing studies on household consumption. First, this study relies on a quasi-experiment to study the existence of the consumption externalities from upper-income households to lower-income households. There exists a large body of literature that documents the empirical evidence of consumption externalities (Grinblatt et al., 2008; Angelucci and De Giorgi, 2009; Brown et al., 2011; Kuhn et al., 2011; Roth, 2014; Petach and Tavani, 2021; Agarwal et al., 2021). However, upward-looking consumption is distinct from any peer effect on consumption in that the reference group consists of households that make a relatively higher income than own household. This distinction is important because such consumption behavior would raise distributional concerns in the age of rising economic inequality, and thus have very different policy implications.

A few studies examine whether the consumption of richer households affects the consumption of others who make less income. Drechsel-Grau and Schmid (2014) find suggestive evidence that upper-income consumption, even when it includes households who are located far from own households on the income distribution, affects lower-income consumption in Germany. Quintana-Domeque and Wohlfart (2016) find that dining out among upper-income households is positively associated with spending on food away from home among lower-income households in the United Kingdom, while there is no relationship between food at home consumption between the two groups. This paper is close to Bertrand and Morse (2016), who explore the same research question using the CEX data covering almost three decades. Using state variation in the 80th/90th percentile income over time, the authors document robust evidence of trickle-down consumption.

While these studies rely on variation in observed income over time, I exploit variation in the timing of the Bush tax reforms and potential exposures to upper-income consumption across states. This setting allows me to document causal evidence of the expenditure cascades. The use of quasi-experiment is also useful in evaluating the validity of the parallel trend, which is the key identifying assumption in the standard two-way fixed-effect model (de Chaisemartin and D'Haultfœuille, 2020). I show that in my dataset, the assumption seems to be valid only when time-varying state characteristics are included. More critically, these differences in empirical design lead to a different conclusion about the effects of upper-income consumption on lower-income consumption.

This study also speaks to the literature examining the economic impact of tax reforms. In particular, the distributional effects of the Bush tax reforms have garnered attention from both

media outlets and academia (Elmendorf et al., 2008; Hungerford, 2011; Horton, 2017). My findings suggest that the potential welfare losses from the expenditure cascades may not be an important consideration at least when the changes in the MTRs are modest.

The paper proceeds as follows. Section II provides institutional background on the Bush tax reforms and explains how I identify consumption externalities from upper-income taxpayers to lower-income taxpayers around the reforms. Section III describes the CEX data used in the study and Section IV presents the estimation strategy and results. Section V concludes.

II Research Design

II.A Background on Bush tax reforms

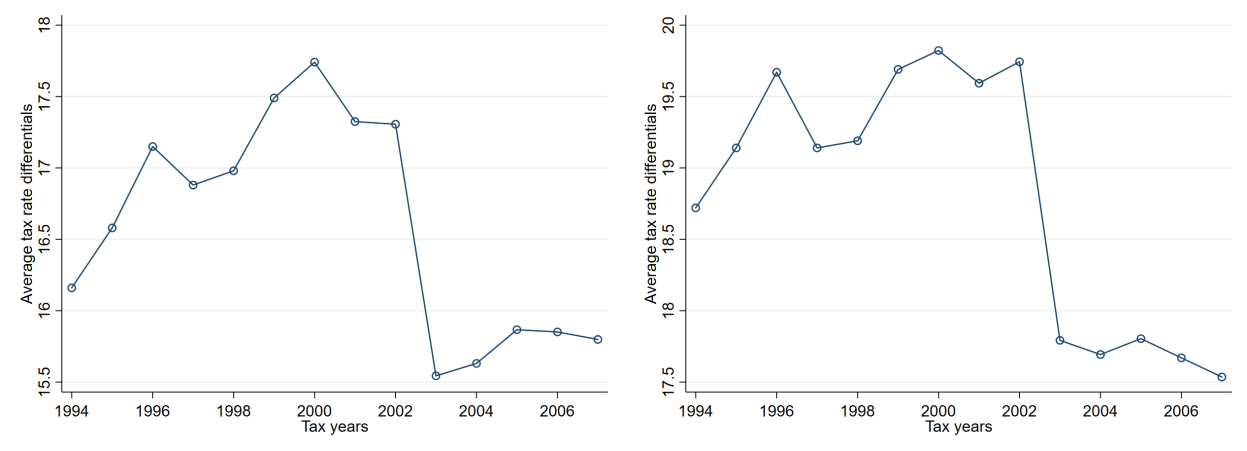
A series of tax reforms, the Economic Growth and Tax Relief Reconciliation Act of 2001 (EGTRRA) and the Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA), was implemented during the Bush Administration. EGTRRA and JGTRRA were designed to provide economic incentives to work and invest by reducing the distortion created by a high tax burden (Auten et al., 2008). EGTRRA and JGTRRA contained several tax provisions, including changing retirement plan rules, raising the standard deduction for joint returns, raising the child tax credit from $500 to $1000, gradually repealing estate taxes, lowering taxes on dividends and capital gains, and so on.[2] Among the provisions, the most noticeable feature of the reforms is a reduction of MTRs.

Table 1: Marginal federal income tax rates for joint returns

2000			2001			2002			2003		
MTR(%)	Taxable income		MTR(%)	Taxable income		MTR(%)	Taxable income		MTR(%)	Taxable income	
	Lower	Upper		Lower	Upper		Lower	Upper		Lower	Upper
						10.0	0	15,011	10.0	0	17,118
15.0	0	57,305	15.0	0	57,456	15.0	15,011	58,419	15.0	17,118	69,450
28.0	57,305	138,460	27.5	57,456	138,874	27.0	58,419	141,169	25.0	69,450	140,184
31.0	138,460	210,989	30.5	138,874	211,647	30.0	141,169	215,100	28.0	140,184	213,608
36.0	210,989	376,828	35.5	211,647	377,978	35.0	215,100	384,103	33.0	213,608	381,426
39.6	376,828		39.1	377,978		38.6	384,103		35.0	381,426	

Note: All figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars.
Source: www.taxfoundation.org

[2]Table A2 in Auten et al. (2008) provides a summary of EGTRRA and JGTRRA provisions.



(a) Top 10 percent VS bottom 50 percent

(b) Top 5 percent VS bottom 50 percent

Figure 1: Average tax rate differentials over time, 1994-2007

Note: The average tax rate differential is defined as the difference in the average tax rate between upper-income tax units and lower-income tax units. Upper-income tax units refer to top 10 percent tax units (among tax units whose adjusted gross incomes were positive) in Panel (a) and top 5 percent tax units in Panel (b). Lower-income tax units refer to bottom 50 percent tax units The average tax rate is the ratio of the total income tax to the total positive adjusted gross income.
Source: Individual Income Tax Return (Form 1040) Statistics provided by Internal Revenue Service

Table 1 shows the tax brackets for joint returns and associated MTRs from 2000 to 2003. The MTRs were stable before and after the tax reforms. When the scheduled changes in the MTRs were fully phased in, however, the MTRs were reduced by at least three percentage points for households who faced the top four income tax brackets, while the reforms did not change the MTR for households whose taxable income belonged to the lowest tax bracket in the pre-reform period.[3] Throughout the paper, I define households in the former group as *MTR28* households as their pre-reform MTRs were 28 percent or above and households in the latter group as *MTR15* households for a similar reason.

Figure 1 summarizes the evolution of the average tax rate differentials between high-income households and lower-income households from 1994 to 2007. Panel (a) shows that the average tax rate for the top 10 percent households decreased by 3.73 percentage points from the tax year 2000 to 2004, while the average tax rate for the bottom 50 percent households decreased by 1.62 percentage points during the same period. Panel (b) compares the average tax rate of the top 5

[3]The tax reforms also introduced a new 10 percent bracket, which affected all households whose taxable income was positive.

percent households to that of the bottom 50 percent households, confirming that the Bush tax reforms reduced the tax burden for upper-income households more substantially.

Existing studies also show that the Bush tax reforms mostly benefited top-income households. Elmendorf et al. (2008) conclude that only households in the top quintile received substantial benefits from the tax reforms and the majority of households were made worse off. Similarly, Sherman and Aron-Dine (2007) estimate that the average after-tax income of the top one percent increased by $146,000 (or 20 percent) from 2003 to 2004, compared to a $1,700 (or 3.6 percent) increase for the median households.

II.B Identification

The main objective of this paper is to empirically examine whether the increased consumption of upper-income households trickled down to lower-income households. Non-experimental variation in the household purchasing power can be correlated with unobserved household characteristics such as preferences (Johnson et al., 2006), which in turn would affect household consumption behavior. The Bush tax reforms, which have widened the after-tax income distribution of U.S. households exogenously, provide a unique setting that one can use to test the expenditure cascades hypothesis.

Because of the introduction of a new 10 percent bracket and other changes in tax provisions, all households were effectively treated by the reforms. This makes the construction of an appropriate counterfactual challenging. I overcome this challenge by exploiting state heterogeneity in the share of upper-income households. The idea is based on the notion of the *demonstration effects* (Duesenberry, 1949), implying that frequent exposures to those who consume high-quality goods than one's own encourage households to imitate their consumption behavior. Therefore, lower-income households living in states where there are relatively many high-income households would be exposed to stronger consumption externalities.

I employ two different measures of potential exposures to consumption externalities. The first intensity measure is the state share of households whose MTRs were equal to or greater than 31 percent ("MTR31 share") as of 2000. The MTR31 share measures the potential risk of being exposed to upper-income consumption in a given state. However, it would not capture the true extent of

consumption externalities if a state is sparsely populated.[4] To explore the extent to which the MTR31 share fails to reflect the true externality intensity, I adjust the first measure by weighting the MTR31 share using the state population density.[5]

Figure 2 shows that there exists substantial variation in the state share of the MTR31 households. In general, the MTR31 share and the adjusted MTR31 share are strongly correlated, but the two measures diverge significantly in some cases. For example, the MTR31 shares for New Jersey and New Hampshire are 6.68 and 6.7, respectively. The land size of the two states is also similar. The difference is that the population of New Jersey is approximately 6.8 times larger than the population of New Hampshire as of 2000. I will provide further details regarding the construction of intensity measures in the next section.

III Data

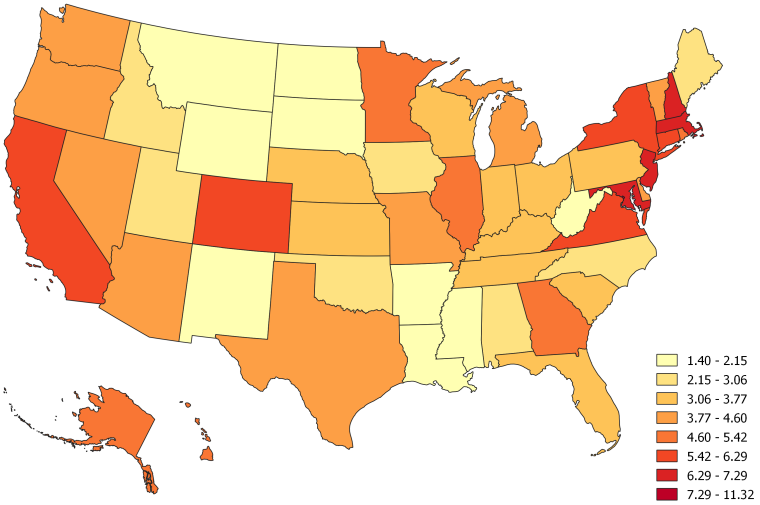
I mainly rely on the Consumer Expenditure Survey (CEX) microdata, which is by far the most comprehensive data on household consumption in the United States. The CEX consists of the Interview survey and the Diary survey. In this study, I use data from the FMLI file of the Interview survey from 1994 to 2007. In the first through fourth interviews, the CEX collects detailed information on expenditures from the survey participants. The CEX also collects information on demographics and household characteristics in the first and fourth interviews.

The total quarterly expenditure variable in the CEX comprises household spending on food, alcohol and beverage, housing, apparel, transportation, health, entertainment, personal care, reading, education, tobacco, miscellanea, cash contribution, and personal insurance. Following the existing literature using the CEX data (Meyer and Sullivan, 2013; Bertrand and Morse, 2016), I convert expenditures on vehicle and housing purchases into service flows by combining the dataset created by Meyer and Sullivan (2013) to the original CEX data. [6] The primary consumption measure is total household consumption, although I analyze household consumption for durables and nondurables separately as well.

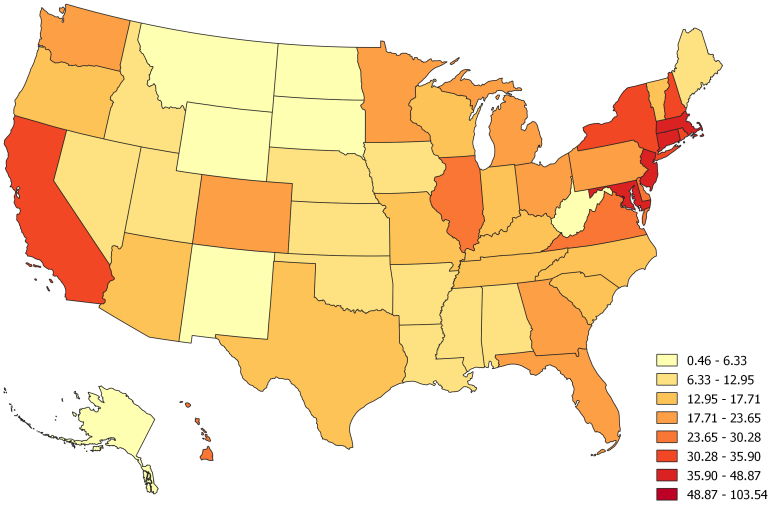
[4] A similar concern arises when one considers mounting evidence that the United States is highly segregated by socioeconomic characteristics such as income or race (Chetty et al., 2014).

[5] Because the population density is highly skewed to the right, I use the log of the density instead.

[6] Meyer and Sullivan (2013) estimate the values of the vehicles and their annual depreciation rates using the information on the makes and models of the vehicles. Then the authors calculate vehicle service flows using those estimates. The authors also impute the rental equivalent for households residing in public or subsidized housing.



(a) MTR31 share



(b) Adjusted MTR31 share

Figure 2: Externality intensity measures

Note: The MTR31 share is the state share of households whose pre-reform MTRs were 31 or greater. The adjusted MTR31 share is the interaction between the MTR31 share and state population density.
Source: Current Population Survey March microdata, 1995-2008.

For information on household income, I use both before-tax income and after-tax income to control for any income effects that may arise as a result of the reforms. Before-tax income includes wage and salary, gain/loss from business, Social Security and Railroad Retirement income, benefits, workers' compensation and veterans' payments, unemployment compensation, public assistance/welfare, interest, child support, alimony, food stamps, income from dividends, royalties, estates, trusts, and other income. After-tax income is obtained by subtracting federal and state/local income taxes, property taxes, and other taxes from the before-tax income.

To identify MTR28 and MTR15, I need information on taxable income and tax filing status of households, which are not available in the CEX for the period that I study.[7] Therefore, I impute household tax filing status and MTRs based on family characteristics and the total amount of federal tax paid. In particular, I define the total federal tax paid as a sum of an annualized federal income tax deduction from earnings and additional federal income tax paid, less than an additional federal income tax refund. For example, if the imputed tax filing status of a household is married filing jointly and the total amount of federal income tax paid is larger than \$9,562, which is the maximum upper bound for the federal tax liability at the 15 percent bracket during the estimation window, the household is defined as MTR28.

A nontrivial number of observations are dropped for several reasons. First of all, I drop households without state information.[8] I also drop households that reported non-positive before-tax income, non-positive after-tax income, or total quarterly consumption less than \$1,000.[9] In addition, I exclude households that might be considered MTR28 in some year and MTR15 in another year due to changes in bracket endpoints.[10] Lastly, I drop households whose before-tax income is too high to be MTR15 or too low to be MTR28. Appendix A provides more details on the CEX data management.

Table 2 provides summary statistics for the estimation sample used in empirical analysis. The total quarterly expenditure of the MTR28 households is higher than the average quarterly consumption of the MTR15 households by \$9,545. Note that the average total consumption and the average total expenditure are similar to each other. Not surprisingly, the annual pre-tax income

[7] Tax filing status data in the CEX becomes available in 2013.

[8] Observations from Mississippi, Montana, New Mexico, North Dakota, and Wyoming do not have state identifiers.

[9] Bertrand and Morse (2016) also drop households with zero income or with zero food consumption.

[10] For example, the MTR of households whose real taxable income were between \$58,419 to \$69,450 in 2002 was reduced from 27 percent to 15 percent in 2003 (see Table 1).

Table 2: Summary statistics

	MTR15		MTR28	
	Mean	SD	Mean	SD
Total expenditures	11210.14	(9108.03)	21021.60	(15710.25)
Total consumption	12632.36	(7708.44)	22177.38	(13851.90)
Income before taxes	51337.19	(39662.04)	125838.53	(81436.22)
Income after taxes	50821.09	(39564.85)	105462.95	(72546.67)
Family size	2.58	(1.56)	2.58	(1.42)
Number of earners	1.21	(0.99)	1.75	(0.84)
Home ownership	0.66	(0.47)	0.82	(0.39)
Age of reference person	50.81	(17.87)	45.64	(11.65)
Male	0.52	(0.50)	0.65	(0.48)
White	0.82	(0.38)	0.89	(0.32)
Single household	0.30	(0.46)	0.28	(0.45)
High school grads	0.30	(0.46)	0.16	(0.37)
Some college	0.20	(0.40)	0.19	(0.39)
College grads	0.24	(0.43)	0.40	(0.49)
Graduate school	0.08	(0.27)	0.22	(0.41)
Head of household	0.09	(0.29)	0.03	(0.18)
Single household	0.30	(0.46)	0.28	(0.45)
Observations	153236		36685	

Note: All monetary figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars.
Source: Consumer Expenditure Survey data, 1994-2007, and the author's calculation.

of the MTR28 households is larger than twice the annual pre-tax income of the MTR15 households. While family size is not very different across the two samples, the MTR28 households tend to have more income earners, which partially explains the income gap between the two groups. The head of the MTR28 households is almost five years younger, more likely to be male and white, and more likely to have a college degree. The shares of married couples and homeowners are also higher in the MTR28 households.

IV Empirical Estimation and Results

This section discusses the details of empirical analysis and main results. An exogenous increase in disposable income caused by the tax reforms would raise the consumption of the MTR28 households as long as consumption is normal and the credit constraint of MTR28 households is binding.[11]

[11]The tax reforms would not change the consumption behavior of upper-income taxpayers if their credit constraints were not binding. However, there is evidence that the credit constraint of upper-income households is also binding (Kaplan and Violante, 2014).

If the tax reforms did not boost MTR28 consumption substantially, the demonstration effect from the upper-income group to the lower-income group would be negligible, and thus consumption externalities would not exist in the first place. Using a difference-in-differences framework, I first examine whether upper-income taxpayers increased consumption as a result of the tax reforms. In the second part of this section, I estimate the effects of upper-income consumption on lower-income consumption.

IV.A Did MTR28 households increase their spending?

I begin by plotting the average consumption for upper-income households and lower-income households in Figure 3. Each red triangle represents the average quarterly consumption of the MTR28 households for a given year and each hollow circle represents the average quarterly consumption of MTR15 households. To make the comparison easier, I remove the initial gap in the average consumption between the two types of households from the average MTR28 consumption, and plot the trend with grey triangles.

During the pre-reform period, the two consumption paths are parallel to each other. However, the average MTR28 consumption rose noticeably compared to the MTR15 consumption after the initial reforms (EGTRRA) in 2001. The gap in the average consumption between the two groups enlarged over the years following EGTRRA, became the largest in 2005, and remained substantial until the end of the estimation period. On average, upper-income consumption net of lower-income consumption increased by $1,602 after the tax reforms.[12]

Although Figure 3 shows suggestive evidence regarding the effect of the Bush tax reforms on upper-income consumption, it does not tell exactly how much of the change is precisely attributable to the tax reforms. For example, the relative increase in the average consumption of the MTR28 households might reflects the income effect not induced by the tax reforms if the incomes of those households grew much faster than the MTR15 households. To tease out the effects of other factors that might drive the change in the MTR28 consumption, I estimate the following difference-in-differences model:

[12]The difference in the average consumption between two types of households is $8,651 in the pre-reform period and $10,253 in the after-reform period.

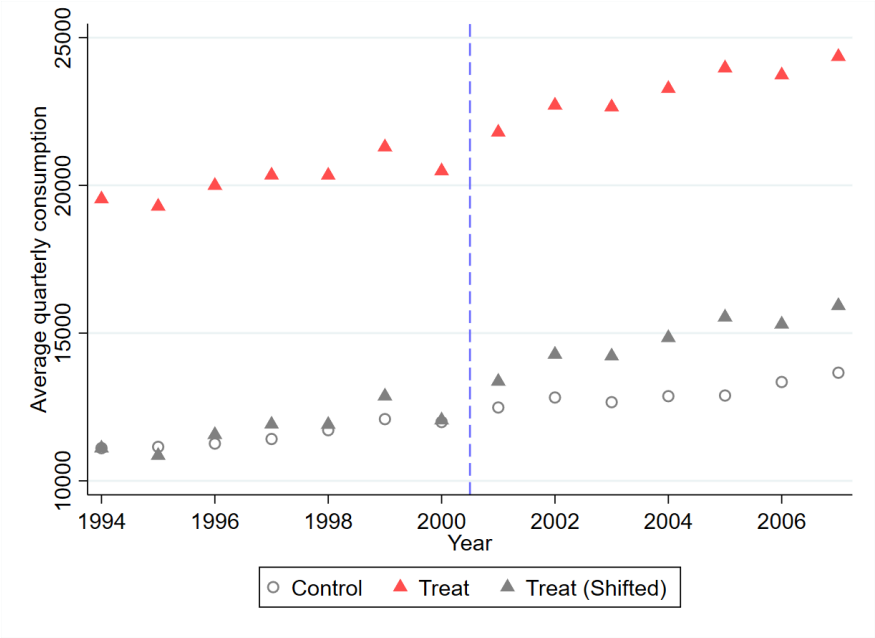


Figure 3: Trends in the average total quarterly consumption

Note: The series of the average total quarterly consumption for control (i.e., MTR15 households) and treatment (i.e., MTR28 households) groups are represented by hollow circles and red triangles, respectively. For comparison, I also present the series of the averages MTR28 consumption minus the average MTR15 consumption for year 1994 in grey triangles. All figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars.

Source: Consumer Expenditure Survey, 1994-2007.

$$C_{i,s,t} = \delta MTR28_i + \beta Post_t \times MTR28_i + \gamma X_{i,t} + \varepsilon_{i,s,t} \tag{1}$$

where $C_{i,s,t}$ is a measure of consumption of household i living in state s in year-quarter t. $Post$ is a dummy variable that takes 1 if consumption took place after 2001, and 0 otherwise. $MTR28$ takes 1 for the MTR28 households, and 0 the MTR15 households. X is a vector of household characteristics including family size, the number of earners, homeownership status, age of a reference person, the number of survey participation, and dummy variables that indicate whether the reference person is male, white, or single. X also includes four dummies for the reference person's education as well as dummies for tax filing status. All specifications include state fixed effects and year-quarter fixed effects. I weight each observation using the CEX weight variable and cluster standard errors by state to account for heteroskedasticity and any form of within-state correlation.

The coefficient of interest is β, which captures the effect of the tax reforms on the consumption of the MTR28 households. The identifying assumption is that the trends in the average consumption

of the MTR28 and MTR15 households would have been similar in the absence of the tax reforms. While it is not possible to formally test the validity of the parallel assumption, the graphical evidence presented in Figure 3 suggests that the assumption is likely to be valid in this context.

Table 3: The effect of Bush tax reforms on consumption of the MTR28s

	(1)	(2)	(3)	(4)	(5)
Post × MTR28	829.46***	919.08***	876.84***	871.77***	809.60***
	(192.89)	(194.03)	(201.60)	(205.57)	(179.42)
Pre-tax income squared	No	Yes	Yes	Yes	Yes
State characteristics	No	No	Yes	Yes	Yes
State specific time trends	No	No	No	Yes	No
Income group specific time trends	No	No	No	No	Yes
Observations	192587	192587	192587	192587	192587
R^2	0.532	0.534	0.534	0.535	0.535

Note: All specifications control for pre-tax household income, family size, the number of earners, homeownership status, age of a reference person, the number of survey participation, dummy variables for male, white, single, educational attainment, and tax filing status. State fixed effects and year-quarter fixed effects are also included in all models. All monetary figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars. Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results are summarized in Table 3. Column 1 presents the baseline result from estimating equation (1) using a full sample. The estimate implies that, on average, the tax reforms increased quarterly consumption of the MTR28 households by $829.5, and the estimate is statistically significant at the one percent level. The size of the estimate increases by about $90 when I add the square of before-tax income to allow for the nonlinearity of consumption in household income in column 2. While the difference-in-differences framework removes time-invariant characteristics of states, time-varying state characteristics such as state economic indicators can still bias the estimate by affecting the MTR28 households and the MTR15 households differently over time. To reduce potential biases caused by time-varying unobservables, I add a vector of state-level characteristics in column 3 and state-specific linear time trends in column 4.[13] Resulting estimates are comparable to the estimate given in column 2. Lastly, in column 5, I divide households into ten groups based on their pre-tax income and add linear time trends specific to each income group to account for any effect common in households making similar incomes. The estimate is reduced to 809.6, but it remains significant.

[13]Specifically, I control for state GDP per capita, median income, unemployment rate, housing price index, population, the share of individuals aged 24 or under, the share of individuals aged 25 to 44, the share of the female population, and the share of the white population.

Figure 4: The effect of the Bush tax reforms: total consumption

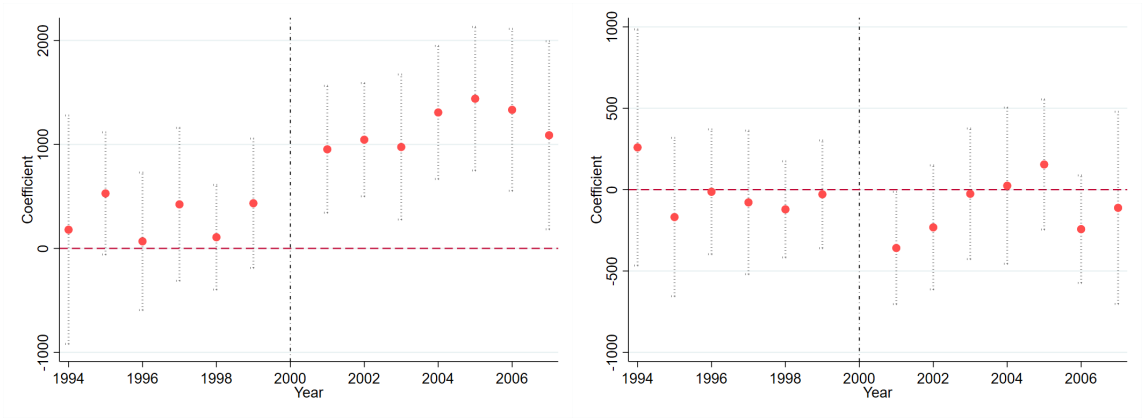
Note: Each circle represents the point estimate for β_k in equation (2) for $k \in \{1994, ..., 2007\}$ and dotted lines are confidence intervals associated with the estimates. The base year is 2000, where the coefficient and confidence intervals are set to zero.

Next, I explore how the effect of the tax cuts on the MTR28 consumption changes over time by estimating the following event study specification:

$$C_{i,s,t} = \delta MTR28_i + \sum_{k=1994}^{2007} \beta_k \{\mathbb{1}(k = y) \times MTR28_i\} + \gamma X_{i,t} + \delta Z_{s,t} + \varepsilon_{i,s,t} \tag{2}$$

where $\mathbb{1}(\bullet)$ is an indicator that equals 1 if k equals year $y \in \{1994, ..., 2007\}$, and 0 otherwise. All else is the same as given in equation (1). This exercise would also provide further evidence for the validity of the parallel trend assumption.

Figure 4 summarizes the dynamics of the treatment effect on the MTR28 consumption. Each circle in the figure represents the estimate for β_k in equation (2) for $k \in \{1994, ..., 2007\}$ and dotted lines are confidence intervals associated with the estimates. Figure 4 indicates that the coefficient estimates are not statistically different from zero prior to EGTRRA and JGTRRA. On the contrary, the estimated effects become statistically significant in the years following EGTRRA, continuing to grow, peaking in 2005. I further examine the effect of the tax reforms on the MTR28 consumption on non-durables (including services) and durables separately in Figure 5. Studies find that household



(a) Nondurable consumption

(b) Durable consumption

Figure 5: The effects of the Bush tax reforms: nondurables VS durables

Note: Each circle represents the point estimate for β_k in equation (2) for $k \in \{1994, ..., 2007\}$ and dotted lines are confidence intervals associated with the estimates. The base year is 2000, where the coefficient and confidence intervals are set to zero. Durable consumption is the sum of spending on vehicle purchases and house furnishings and equipment, and nondurable consumption is total consumption minus durable consumption.

consumption responses vary across types of goods and services that they consume (Johnson et al., 2006; Parker et al., 2013). This is not surprising given that households purchase durables such as vehicles or furniture much less frequently than nondurables. I define durable consumption as the sum of spending on vehicle purchases and house furnishings and equipment. Non-durable consumption is simply the difference between total consumption and durable consumption. Figure 5 suggests that an increase in the MTR28 consumption is mainly driven by spending on nondurables., which account for about 89% of total consumption.

In sum, I find robust evidence that the Bush tax reforms increased the consumption of the MTR28 households compared to that of the MTR15 households by raising their disposable income. In my sample, the average MTR28 household spent $20,461 in the pre-treatment period and $23,564 in the post-treatment period. This indicates that the tax reforms alone account for about 26.1 percent of the overall increase in the consumption of the MTR28 group from 1994 to 2007.

IV.B Did the Bush tax reforms generate consumption externalities from upper-income households to lower-income households?

IV.B.1 Main results

I show that the tax reforms increased consumption of the MTR28s, mainly for non-durables. This section explores whether the increase in upper-income consumption produced externalities to lower-income households, thus increasing their consumption. As I describe in Section II.B, I use the MTR31 share and the adjusted MTR31 share to measure the extent of consumption externalities from upper-income consumption.[14]

I begin by estimating the following difference-in-differences specification:

$$C_{i,s,t} = \beta Post_t \times Intensity_s + \gamma X_{i,t} + \delta Z_{s,t} + \varepsilon_{i,s,t}. \tag{3}$$

where $Intensity_s$ is a measure of treatment intensity for state s. Here I use after-tax income to control for any income effects that might be generated by the tax reforms.[15] All else are the same as equation 1. For this analysis, I restrict the sample to the MTR15 households. The key identifying assumption is that, on average, the MTR15 consumption in state i and state j for all $i \neq j$ would have grown similarly in the absence of the tax reforms. All observations are weighted by the weight provided by the CEX data, and the standard errors are clustered by state.

Table 4 summarizes estimation results. When I use the MTR31 share to measure treatment intensity in Panel A, the estimate is positive, but statistically insignificant (column 1). Incorporating the square of the after-tax income does not change the result substantially (column 2). The size of the estimate becomes much smaller once I control for a vector of state characteristics as described in equation 3 (column 3), and becomes even smaller when income group-specific time trends are added (column 4). In Panel B, I use the adjusted MTR31 share to approximate treatment intensity. The magnitude of the estimates are different because of the difference in the mean values for those intensity measures. The estimates are positive without state time-varying characteristics (column 1 and 2), but becomes negative with those additional controls (column 3 and column 4). Nonetheless,

[14] I prefer to use the MTR31 share over the MTR28 share because the former would be more appropriate to approximate the externality intensity from upper-income consumption. I provide additional results using the MTR28 share in the Appendix.

[15] For example, the use of pre-tax income would fail to capture the effects of an increase in the child tax credit.

Table 4: Consumption externalities on lower-income households: baseline results

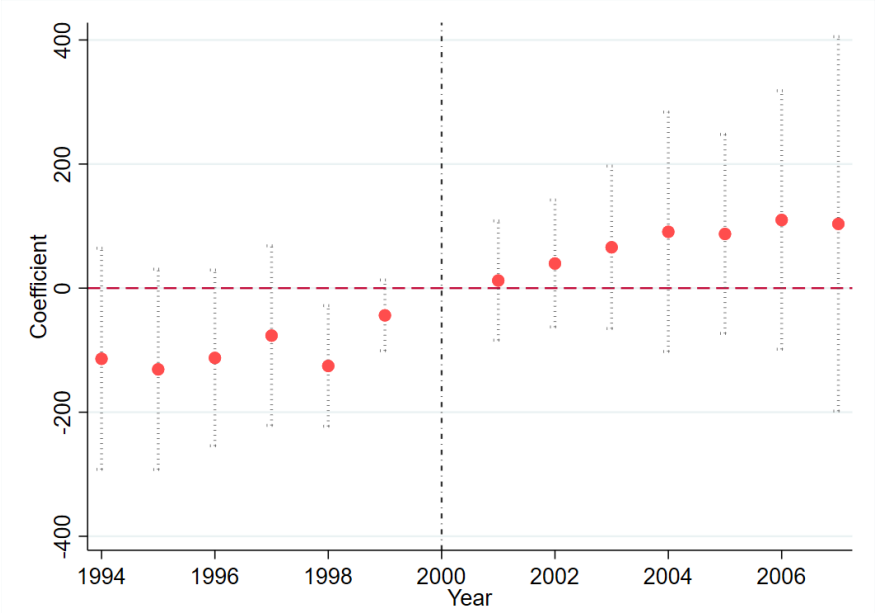
	(1)	(2)	(3)	(4)
Panel A: MTR31 share				
Post × Intensity	153.4	153.6	12.29	9.763
	(98.64)	(99.13)	(52.68)	(52.64)
R^2	0.486	0.486	0.486	0.486
Panel B: Adjusted MTR31 share				
Post × Intensity	11.75	11.78	-5.209	-5.538
	(11.02)	(11.09)	(5.734)	(5.710)
R^2	0.486	0.486	0.486	0.486
Income after tax	Linear	Quadratic	Quadratic	Quadratic
State characteristics	No	No	Yes	Yes
Income group time trends	No	No	No	Yes
Observations	155390	155390	155390	155390

Note: All specifications control for after-tax household income, family size, the number of earners, homeownership status, age of a reference person, the number of survey participation, dummy variables for male, white, single, educational attainment, and tax filing status. State fixed effects and year-quarter fixed effects are also included in all models. All monetary figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars. Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

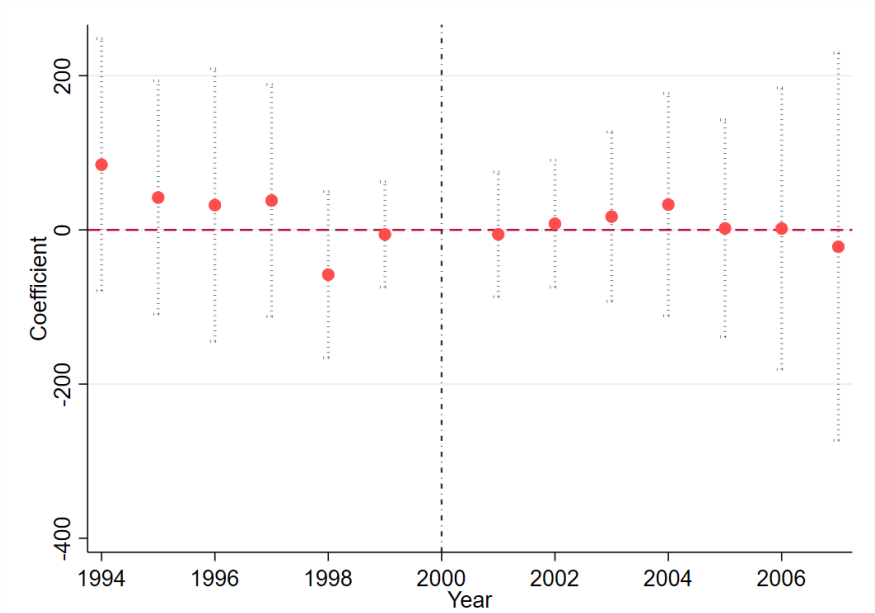
all estimates are not statistically significant. Overall, I do not find evidence that lower-income households who are more likely to be exposed to upper-income consumption spent more.

I further explore the dynamic effects of consumption externalities using the event study framework, similar to equation (2). Figure 6a shows the estimates from the model that includes the same set of control variables as in column 2 of Table 4. When state time-varying characteristics are excluded, the estimates seem to evolve somewhat linearly over the period. The signs of the estimates changed from negative to positive after the initial reform, but none of the estimates in the post-reform period are statistically significant. When I test the hypothesis that the estimated effects are jointly equal to zero for the pre-reform period and the post-reform period separately, the F-statistics are 1.58 (p-value: 0.1869) and 0.32 (p-value: 0.9255), respectively. Alternatively, Figure 6b presents the estimates from the model that accounts for state time-varying characteristics. The seemingly upward trend in the estimates shown in Figure 6a disappears, reducing the concern about the potential violation of the parallel trend. Again, the results do not support the claim that the tax reforms generated consumption externalities from upper-income households to lower-income households.

Although I do not find evidence of consumption externalities in the total consumption of lower-income households, it might mask substantial heterogeneity across different categories of



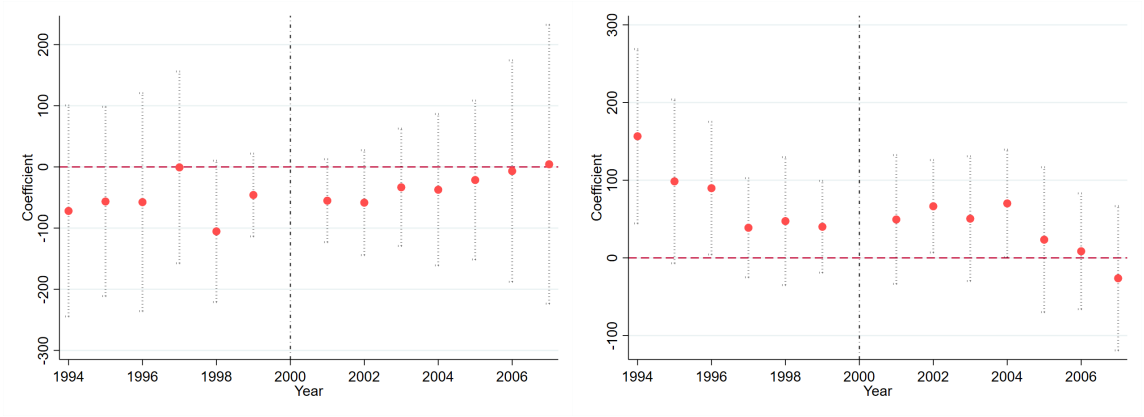
(a) MTR31 share without time-varying state controls



(b) MTR31 share with time-varying state controls

Figure 6: Consumption externalities on lower-income households: total consumption

Note: Panel A presents the estimation results from an event study specification, where I replace $post_t \times intensity_s$ in equation 3 with $\sum_{k=1994}^{2007} \beta_k \{\mathbb{1}(k = y) \times Intensity_s\}$ and exclude time-varying state characteristics. Panel B presents the results from a specification with time-varying state characteristics. Each circle represents the point estimate for β_k, $k \in \{1994, ..., 2007\}$, and dotted lines represent confidence intervals associated with the estimates. The base year is 2000, where the coefficient and confidence intervals are set to zero.



(a) Nondurable consumption

(b) Durable consumption

Figure 7: Consumption externalities: nondurables VS durables

Note: The figure presents the estimation results from an event study specification, where I replace $post_t \times intensity_s$ in equation 3 with $\sum_{k=1994}^{2007} \beta_k \{\mathbb{1}(k = y) \times Intensity_s\}$. Each circle represents the point estimate for β_k, $k \in \{1994, ..., 2007\}$, and dotted lines represent confidence intervals associated with the estimates. The base year is 2000, where the coefficient and confidence intervals are set to zero.

consumption. To explore this possibility, I estimate the effects of potential exposure to upper-income consumption on lower-income consumption on nondurables and durables, separately. For nondurable consumption, the pattern shown in Figure 7a is generally consistent with Figure 6b, confirming the non-existence of consumption externalities. Figure 7b, on the other hand, is difficult to interpret because the pattern indicates that the parallel trend assumption is unlikely to hold in this case. The violation of the parallel trend arises due to state heterogeneity in the growth rate of vehicle rental prices, which accounts for about 43.5 percent of the total durable consumption in the estimation sample. Nevertheless, the pattern shows no indication of the consumption externalities from upper-income households.[16]

IV.B.2 Robustness checks

I examine whether these results are robust in Table 5. Panel A and B present results from the specification without time-varying state controls (column 2 of Table 4), and panel C and D presents results from the specification with the state controls (column 3 of Table 4).

Thus far, I use total consumption as the dependent variable. However, in the context of consumption externalities, the actual amounts of spending may be more appropriate than the service

[16]I present results from specifications that use the adjusted MTR31 share in Figure B.2 in the Appendix.

Table 5: Consumption externalities on lower-income households: robustness checks

	(1) Total expenditure	(2) W/T outliers	(3) Complete participants	(4) Price controls
Panel A: MTR31 share without state controls				
Post × Intensity	111.0	140.5	154.9	39.17
	(89.36)	(94.69)	(93.47)	(81.50)
R^2	0.367	0.525	0.501	0.486
Panel B: Adjusted MTR31 share without state controls				
Post × Intensity	4.232	10.37	11.38	-2.228
	(10.15)	(10.55)	(10.51)	(8.480)
R^2	0.367	0.525	0.501	0.486
Panel C: MTR31 share with state controls				
Post × Intensity	-1.113	6.304	-2.139	-20.12
	(56.15)	(51.55)	(65.22)	(53.11)
R^2	0.367	0.526	0.501	0.486
Panel D: Adjusted MTR31 share with state controls				
Post × Intensity	-9.335	-6.069	-7.727	-7.928
	(5.625)	(5.591)	(6.604)	(5.345)
R^2	0.367	0.526	0.501	0.486
HPI	No	No	No	Yes
Observations	155390	155181	87954	155390

Note: All specifications control for after-tax household income, family size, the number of earners, homeownership status, age of a reference person, the number of survey participation, dummy variables for male, white, single, educational attainment, and tax filing status. State fixed effects and year-quarter fixed effects are also included in all models. All monetary figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars. Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

flows. In column 1, I replace the total consumption variable, which is constructed using the data from Meyer and Sullivan (2013), with the total expenditure variable provided by the CEX. The size of estimates are smaller compared to the results given in Table 4. Although the results in given column 1 are moslty insignificant, the one given in panel D is at the margin of significance (p-value: 0.100).

Column 2 and column 3 test the robustness of my results to a different estimation sample. In column 2, I exclude outliers, which are households with total consumption less than half of the 1st percentile of the total consumption distribution or greater than twice the 99th percentile. For some states with a relatively small number of observations for a given consumption year, the existence of those who spend too much or too little may bias the main results. Column 3 restricts the sample to households that participate in all four CEX surveys to examine whether the results are affected

by nonrandom survey participation. The results in both columns are similar to the ones presented in column 2 of Table 4.

The key identifying assumption in my difference-in-differences estimation is that, on average, lower-income consumption in state i and state j for all $i \neq j$ would have grown similarly in the absence of the tax reforms. This assumption might be violated if local prices in states with a larger MTR31 may grow faster than their counterparts. If there exists heterogeneity in local inflation across states, it would influence household spending differently even when they consume the same amount of goods and services (Roth, 2014; Bertrand and Morse, 2016). Indeed, Cooper (2013) documents that the correlation between real consumption growth and real house price growth is about 0.7 during the 2000s. To control for heterogeneity in local prices, I add the state-level single-family housing price index (HPI) provided by Federal Housing Finance Agency to my preferred specification as a proxy for local prices. The results are given in column 4. In general, this changes the size of the estimates substantially. Nevertheless, none of them are statistically significant. In sum, all the results presented in Table 5 are consistent with the main results, confirming that there is little evidence of consumption externalities from upper-income households.

IV.B.3 Testing the expenditure cascdes hypothesis

The expenditure cascades hypothesis proposed by Frank et al. (2014) not only states that consumption externalities trickle down from the high-income earners to the middle class, but it also suggests that the impact of such externalities should be stronger for those located closer to the reference households who generate the externalities. The effect of upper-income consumption would diminish as it moves further away from the reference households in the income distribution. Now I explore the gradient effect suggested by the expenditure cascade hypothesis by estimating the following reduced-form equation:

$$C_{i,s,t} = \alpha + \sum_{k=1}^{3} \beta_k (Post_t \times Intensity_s \times T_i^k) + \gamma X_{i,t} + \delta Z_{s,t} + \varepsilon_{i,s,t}. \tag{4}$$

where T^k takes 1 if household i belongs to the kth post-tax income tertile in a given year, and 0 otherwise. The expenditure cascades hypothesis implies that $\beta_1 < \beta_2 < \beta_3$.

Table 6: Expenditure cascades

	(1)	(2)	(3)
Panel A: Estimates for βs			
Post × Intensity × Bottom	-9.554	-3.596	-8.591
	(52.40)	(52.64)	(47.74)
Post × Intensity × Middle	18.32	17.44	39.85
	(51.59)	(51.97)	(56.11)
Post × Intensity × Top	30.01	26.21	4.829
	(58.03)	(57.83)	(94.42)
Panel B: Tests of equality across βs			
$\beta_3 = \beta_2$	0.7457	0.6565	0.0432
$\beta_2 = \beta_1$	0.6967	0.1914	0.0060
Intensity-tertile interactions	No	Yes	Yes
Post-tertile interactions	No	No	Yes
Observations	155390	155390	155390
R^2	0.486	0.486	0.486

Note: All specifications control for after-tax household income, family size, the number of earners, homeownership status, age of a reference person, the number of survey participation, dummy variables for male, white, single, educational attainment, and tax filing status. State fixed effects and year-quarter fixed effects are also included in all models. All monetary figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars. Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6 summarizes the results from the analysis using the MTR31 share.[17] Panel A presents the point estimates for each three-way interaction term and Panel B presents p-values for the tests of eqaulity across the βs. Column 1 shows the result from the baseline specification given in equation (4). While the estimated effect increases monotonically from the bottom income tertile to the top income tertile, I fail to reject the hypothesis that all β's are equal to zero. When I control for the interactions between the MTR31 share and each income tertile in column 2, it yields a similar result. Lastly, I additionally control for the interactions between the post dummy and the income tertile dummies. Now the p-value for each equality test given in Panel B becomes less than 0.05, rejecting the null of equality among β's. However, the non-monotonic changes in the estimated effect is inconsistent with the implication of the expenditure cascades hypothesis. Overall, I find little evidence supporting the claim that upper-income consumption trickles down to lower-income households, thereby changing their consumption behavior.

[17] Results from the adjusted MTR31 share is available in Table B.1 in the appendix.

V Discussion

This study investigates whether upper-income consumption affects lower-income consumption using a series of tax reforms during the Bush Administration as a natural experiment. Not surprisingly, I find that the tax reforms raised the quarterly consumption of high-income households whose marginal tax rates were reduced significantly by about $900. However, I do not find that the increase in upper-income consumption trickles down to lower-income households. Finally, I do not find that the overall effect masks heterogeneity across different income tertiles among lower-income households. These results deviate from Bertrand and Morse (2016) who document robust evidence of trickle-down consumption. However, my study is different from Bertrand and Morse (2016) in terms of research design, estimation sample, and other details of empirical analyses. Most importantly, I control for a rich set of state time-varying characteristics, which turn out to be critical for constructing a valid control group in the context of this study.

There are some caveats. First of all, it is difficult to extrapolate my results to a setting where there is a substantial change in the consumption of high-income earners, especially for highly visible goods such as vehicles (Grinblatt et al., 2008). A $300 increase in the monthly consumption for upper-income may not be large enough to trigger the expenditure cascades, but a larger amount of spending may do. Second, while the CEX is the most comprehensive data on household consumption in the United States, the finest geographical unit available in the CEX is a state. Studies use the CEX data to explore consumption externalities (Bertrand and Morse, 2016; Petach and Tavani, 2021). Moreover, a recent study indicates that the impact of others' consumption is not necessarily limited by geography (Bailey et al., 2018). Nevertheless, if consumption externalities primarily occur at the local level (i.e., county-level or below), my empirical analysis might not be ideal to capture such effects. Despite these limitations, my findings suggest that potential welfare losses from consumption externalities suggested by Frank (2005) might not be very large at least when there is only a modest change in the income distribution.

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Appendices

Appendix A Identifying MTR28 and MTR15

The CPS contains information on individual taxable income (TAXINC) and household tax filing status (FILESTAT). To create a household taxable income variable, I sum taxable income of all individuals who have the same household identifier (SERIAL). Next, I convert the nominal household taxable income into 2011 U.S. dollars using CPI-U-RS. Finally, I determine whether a household is MTR28 or MTR15 based on taxable income and tax filing status of the household. If a household had taxable income greater than the maximum upper bound for 15% bracket as of 2000, then the household is MTR28. If a household had taxable income equal to or less than the minimum lower bound for 15% bracket as of 2000, then the household is MTR15. Table A.1 provides lower bounds and upper bounds for each tax bracket by tax filing status from 1994 to 2007.

Table A.1: Upper/lower bounds of federal tax liabilities at the 15% bracket, 1994 to 2007

Year	Married filing jointly	Single	Head of household
1994	8553.88	5121.08	6865.62
1995	8572.56	5132.55	6869.04
1996	8585.80	5138.63	6883.63
1997	8634.75	5166.18	6926.66
1998	8753.57	5239.74	7017.32
1999	8716.44	5213.66	6995.42
2000	8590.54	5142.57	6886.15
2001	8613.21	5154.59	6907.72
2002	8007.46	4866.36	6397.84
2003	9562.92	4781.46	6368.15
2004	9525.59	4762.80	6340.47
2005	9423.98	4711.99	6275.93
2006	9414.83	4707.42	6269.12
2007	9515.79	4757.89	6332.11
Maximum upper bound	9562.92	5239.74	7017.32
Minimum lower bound	8007.46	4707.42	6269.12

Note: All figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars.
Source: www.taxfoundation.org and the authors calculation.

Unlike the CPS, the CEX does not contain information on household tax filing status. Thus, I have to impute tax filing status of households using information on family type (FAM TYPE) and the composition of earners (EARNCOMP). Table A.2 provides description of the variables. I assume that the tax filing status of households was married filing jointly if the family type of a household is coded 1, 2, 3, 4, or 5. Exceptions are the cases where 1) family type code is 3, 4, or 5, and 2) a household member other than the reference person and spouse works. Together with

households whose family type is either 6 or 7, I code tax filing status for those households as head of household. Lastly, I code tax filing status for single-person households as single.

Table A.2: Codes for family type (FAM TYPE) and composition of earners (EARNCOMP)

Code	Family type
1	Husband and wife (H/W) only
2	H/W, own children only, oldest child under 6 years old
3	H/W, own children only, oldest child 6 to 17 years old
4	H/W, own children only, oldest child over 17 years old
5	All other H/W CUs
6	One parent, male, own children only, at least one child age under 18 years old
7	One parent, female, own children only, at least one child age under 18 years old
8	Single persons
9	Other CUs
Code	**Composition of earners**
1	Reference person only
2	Reference person and spouse
3	Reference person, spouse and others
4	Reference person and others
5	Spouse only
6	Spouse and others
7	Others only
8	No earners

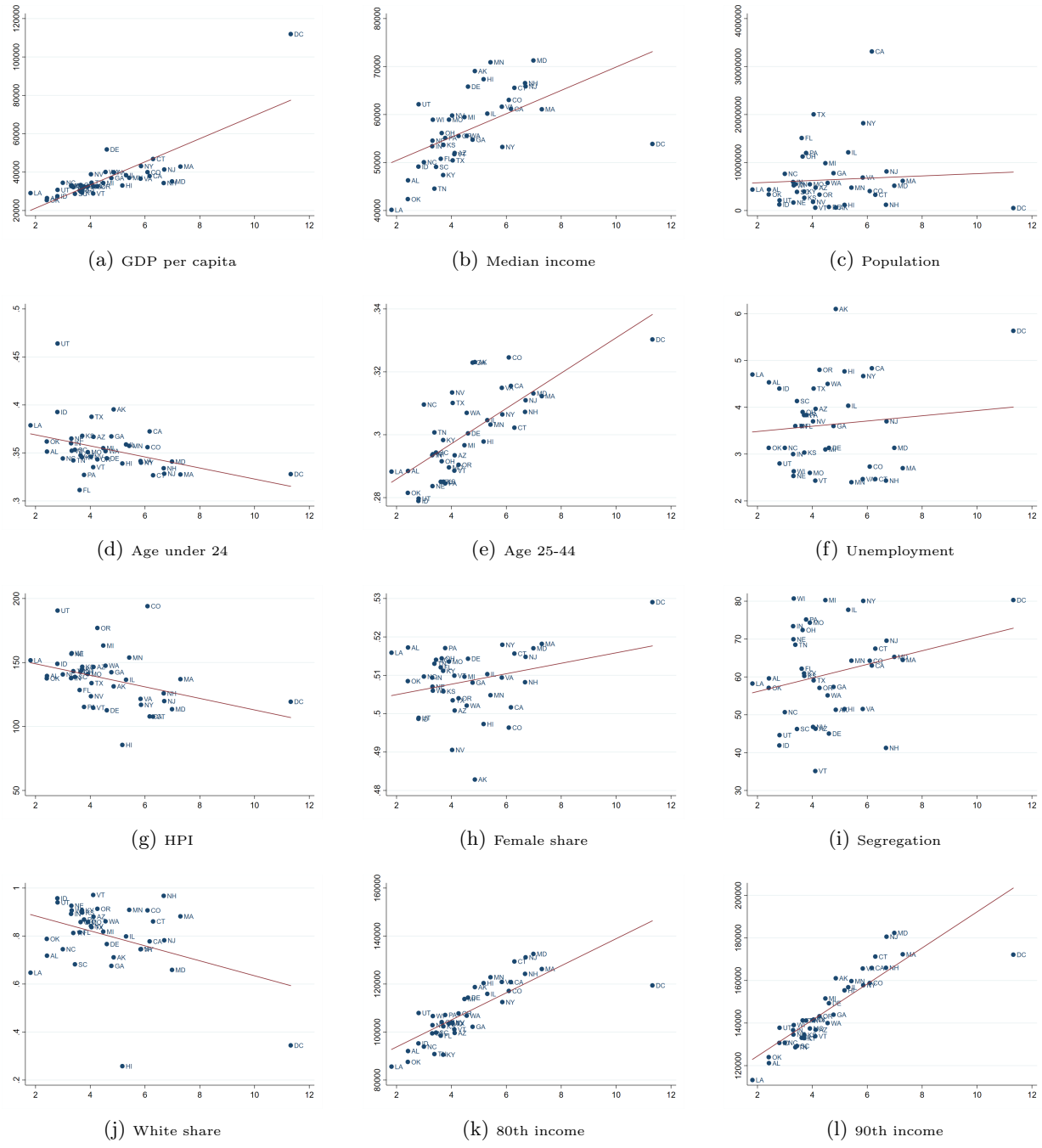
Source: Interview Data Dictionary of the Consumer Expenditure Survey.

Appendix B Additional results

Table B.1: Expenditure cascades: The adjusted MTR31 share

	(1)	(2)	(3)
Panel A: Estimates for βs			
Post × Intensity × Bottom	-7.562	-6.155	6.287
	(5.638)	(5.430)	(5.695)
Post × Intensity × Middle	-3.764	-3.544	1.247
	(5.657)	(5.542)	(6.052)
Post × Intensity × Top	-4.266	-5.654	-23.63**
	(7.199)	(7.361)	(9.388)
Panel B: Tests of equality across βs			
$\beta_3 = \beta_2$	0.7457	0.6565	0.0432
$\beta_2 = \beta_1$	0.6967	0.1914	0.0060
Intensity-tertile interactions	No	Yes	Yes
Post-tertile interactions	No	No	Yes
Observations	155390	155390	155390
R^2	0.486	0.486	0.486

Note: All specifications control for after-tax household income, family size, the number of earners, homeownership status, age of a reference person, the number of survey participation, dummy variables for male, white, single, educational attainment, and tax filing status. State fixed effects and year-quarter fixed effects are also included in all models. All monetary figures are adjusted for inflation using CPI-U-RS and presented in 2011 U.S. dollars. Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$



(a) GDP per capita (b) Median income (c) Population

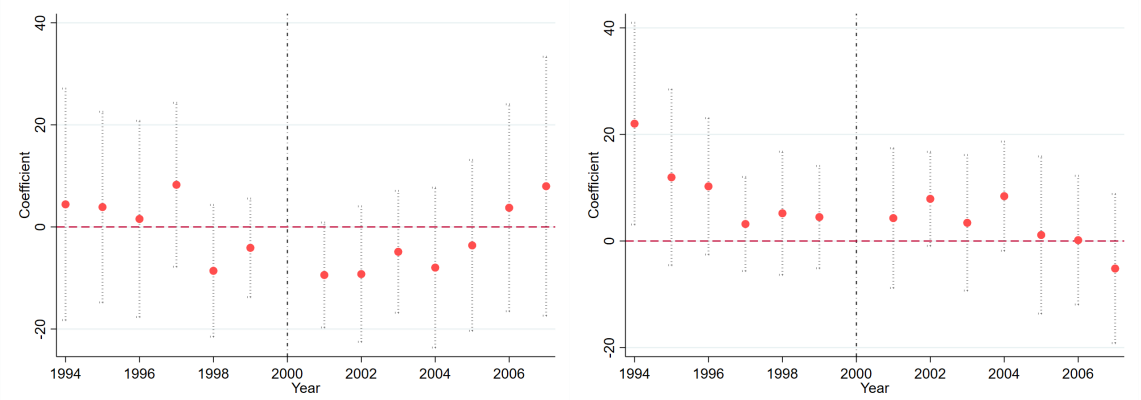
(d) Age under 24 (e) Age 25-44 (f) Unemployment

(g) HPI (h) Female share (i) Segregation

(j) White share (k) 80th income (l) 90th income

Figure B.1: Correlates of the MTR31 share

Note: This figure presents correlations between the MTR31 share and state characteristics as of the first quarter of 2000.



(a) Nondurable consumption

(b) Durable consumption

Figure B.2: Consumption externalities: nondurables VS durables

Note: The figure presents the estimation results from an event study specification, where I replace $post_t \times intensity_s$ in equation 3 with $\sum_{k=1994}^{2007} \beta_k \{\mathbb{1}(k = y) \times Intensity_s\}$. Each circle represents the point estimate for β_k, $k \in \{1994, ..., 2007\}$, and dotted lines represent confidence intervals associated with the estimates. The base year is 2000, where the coefficient and confidence intervals are set to zero.