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The Surprising Incidence of Tax Credits for the Toyota Prius[†]

By James M. Sallee*

This paper estimates the incidence of tax incentives for the Toyota Prius. Transaction microdata indicate that both federal and state incentives were fully captured by consumers. This is surprising because Toyota faced a binding production constraint, which suggests that they could have appropriated the gains. The paper proffers an explanation based on an intertemporal link in pricing that stems from search frictions, which has the unconventional implication that statutory burden influenced economic burden. The paper develops a bounding estimator to account for endogenous selection into preferential tax regimes that may be useful in other contexts. (*JEL* H22, H24, L11, L62)

Federal and state governments in the United States have responded to rising concerns about the consequences of petroleum consumption in part by introducing tax subsidies for new vehicles that feature fuel-efficient technologies, including gas-electric hybrids. These policies aim to reduce oil consumption in the personal transportation sector, which accounts for 40 percent of gasoline consumption and 20 percent of greenhouse gas emissions (U.S. Environmental Protection Agency 2007). At the federal level, the Energy Policy Act of 2005 introduced a substantial personal income tax credit for hybrids. At the state level, thirteen states have passed tax incentives for hybrids, and many others have considered similar actions. In this paper, I determine who benefits from tax incentives for hybrids using transaction level microdata to estimate the incidence of existing subsidies for the Toyota Prius.

I find that consumers capture nearly all of the benefits of tax subsidies. Transaction prices for the Prius did not change following changes of up to $2,650 in subsidy value. Since consumers receive the subsidy directly from the government after their purchase, constant transaction prices imply that consumers captured the benefits of government intervention. This finding has implications for the evaluation of existing and future policies. It also has broader implications for the study of tax incidence

*The Harris School, University of Chicago, 1155 E. 60th Street, Chicago, IL 60637 (e-mail: sallee@uchicago.edu). The author would like to thank two anonymous referees, Soren Anderson, Rebecca Blank, John Bound, Charlie Brown, Brian Cadena, Dave Cole, Lucas Davis, Dhammika Dharmapala, Wei Fan, Jim Hines, Ben Keys, Brian Kovak, Kai-Uwe Kühn, Jim Levinsohn, John List, Walter McManus, Alex Resch, Caroline Sallee, Dan Silverman, Joel Slemrod, Jeff Smith, Chad Syverson, Sarah West, Marina Whitman, Rob Williams, Martin Zimmerman, and seminar participants at numerous universities and conferences. Special thanks are due to Wei Fan for significant assistance with the data. The author also gratefully acknowledges fellowship support from the National Science Foundation and the National Institute for Child Health and Development.

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because the result, when combined with additional facts about the market for the Prius, runs counter to economic intuition.

Through much of the sample period, the Prius was in excess demand, and consumers had to wait in a queue for several weeks to make a purchase. Given these wait lists, it is difficult to construct a conventional model in which consumers might capture the whole benefit of a tax subsidy. Instead, we should expect Toyota to capture the whole subsidy, since it is operating on a vertical segment of the supply curve. I argue that the most compelling explanation is that Toyota believed that charging higher prices while the subsidy was in place would reduce demand for hybrids in the future, because high past prices cause consumers to expect high future prices and search frictions prevent consumers from learning true prices when mistaken. These frictions can cause automakers to choose current prices based partly on how they will influence future demand. This can explain the observed incidence, but only if consumers base expectations on pre-tax prices, which implies that the statutory incidence of a tax will influence economic incidence.

The paper also makes a methodological contribution. The federal tax credit introduced sharp changes in the value of the subsidy, and the date of these changes was known in advance. This induced a timing response among consumers, who moved their purchases into the more favorable tax windows, creating "bunching" on tax preferred dates. If consumers who adjust their timing in response to a tax change differ systematically from those who do not, then a comparison of prices before and after the tax change will give a biased estimate of tax incidence. To account for this, I employ a novel procedure to estimate an upper bound on price changes based on assuming the worst about the nature of this selection. Public economists have recently kindled interest in this type of bunching around kinks and notches.[1] When agents can endogenously determine their position relative to such discontinuities, observational analyis—including regression discontinuity designs—are confounded. But, when such bunching occurs and individual outcome data are available, the worst case approach used here can be useful in placing bounds on the behavioral effects of interest, even when a strategy for isolating exogenous variation is unavailable.

This paper also contributes to a quickly growing literature on hybrid vehicles. Matthew E. Kahn (2007) documents the correlation between Prius ownership and Green Party registration and other signals of environmental preferences. Kelly Sims Gallagher and Erich Muehlegger (2011) and David Diamond (2009) study the effect of state incentives for hybrids on sales in the United States, and Ambarish Chandra, Sumeet Gulati, and Milind Kandlikar (2010) perform a similar estimation of the effect of provincial incentives for hybrids on sales in Canada. Garth Heutel and Muehlegger (2010) model how network effects and government policy influence hybrid adoption. Arie Beresteanu and Shanjun Li (2011) estimate the sales effect of federal and state incentives using a structural approach. None of these papers utilize price data, and, with the exception of the work by Kahn (2007), all of them

[1] See Joel Slemrod (2010) for an overview of tax notches; Sallee and Slemrod (2010) for an empirical analysis of notches in the vehicle market; Emmanuel Saez (2010) for a study of bunching around the Earned Income Tax Credit; and Raj Chetty et al. (2009) for a study of income bunching in Denmark.

are restricted to analysis of aggregate data. Only Beresteanu and Li (2011) estimate incidence. Their results are at odds with the conclusions reached here, which is not surprising given that they assume market clearing prices, which is pivotal for the Prius.

Finally, this research fills a gap in policy evaluation. Through the third quarter of 2007, the hybrid vehicle tax credit cost the federal government about $785 million, with $394 million going to Priuses.[2] Knowing who benefited from these subsidies is necessary not only to evaluate the current policy, but also to inform future legislative action. Many politicians have called for an increase in the federal hybrid credit; there is a new $7,500 federal credit for electric or plug-in vehicles; many states continue to consider similar measures (16 states debated a bill in 2007 alone); and Canada and France have recently passed similar subsidies for efficient vehicles. Clearly, this remains an active policy area, and the unconventional conclusions regarding tax incidence of this research could inform policy design.

This paper is focused on the question of incidence, but the effect of the policy on quantities and hence on the externalities related to fuel economy are also important. In the case of the Prius, since the tax policy was at its height when Priuses were in excess demand, it is likely that the policy had no environmental benefit at all. More generally, such tax credits are a crude method of pricing vehicle externalities because they induce a "rebound effect" by indirectly lowering the cost of driving (Kenneth A. Small and Kurt Van Dender 2007) and they target a specific technology instead of fuel economy generally (Gilbert E. Metcalf 2009).

I. Tax Incentives for Hybrid Vehicles

The Toyota Prius is a parallel gas-electric hybrid car. The federal government has subsidized such hybrids through the individual income tax system for several years. Before 2006, the clean fuel vehicle deduction provided hybrid buyers a $2,000 "above the line" deduction, claimable even regardless of itemization. The Energy Policy Act of 2005, passed in August 2005, replaced the deduction for tax year 2006 with a more generous hybrid vehicle tax *credit*, worth up to $3,400, based on estimated fuel savings. Not every person who purchases a hybrid is eligible to receive the benefit because the credit does not offset Alternative Minimum Tax obligations and because the credit is non-refundable, as was the prior deduction.

The act included a phase-out provision, triggered when a manufacturer sells 60,000 eligible vehicles. The credit is unchanged in the quarter in which the 60,000th vehicle is sold and in the next quarter. The credit then falls to 50 percent of its original value for the next 2 quarters, then 25 percent for another half year, and then expires completely. The phase-out was allegedly designed to prevent foreign automakers from benefiting more than domestic automakers over the life of the program (David Leonhardt 2006). Toyota hit the 60,000 mark in the second quarter of 2006, triggering a credit reduction effective October 1, 2006. The benefit fell again on April 1, 2007, and it expired completely on October 1, 2007. Honda's

[2] Author's calculations, based on sales data from *Automotive News*, assuming an 85 percent take-up rate.

Table 1—Variation in Federal Tax Incentives for the Toyota Prius

Date effective	Tax incentive
January 1, 2001 to December 31, 2005	$2,000 deduction (up to $700 value in 2005)
January 1, 2006 to September 30, 2006	$3,150 credit
October 1, 2006 to March 31, 2007	$1,575 credit
April 1, 2007 to September 30, 2007	$787.50 credit
October 1, 2007 forward	no credit

phase-out began on January 1, 2008. As of early 2009, no domestic automaker had reached the cap.

The sharp changes provide a natural experiment for the analysis of the influence of the tax policy on the hybrid market. As summarized in Table 1, a Prius purchased on or before December 31, 2005 was eligible for the $2,000 deduction (worth $500 for households in a middle income bracket and up to $700 for the highest income individuals in 2000). A Prius purchased between January 1, 2006 and September 30, 2006 was eligible for a $3,150 credit. A new Prius purchased between October 1, 2006 and March 31, 2007 was eligible for $1,575. A Prius sold between April 1, 2007 and October 1, 2007 garnered a credit of only $787.50.

II. A Description of the Data

This paper uses data from J. D. Power and Associates' Power Information Network (henceforth PIN), which collects transaction data directly from a representative sample of dealers comprising 15 percent of all new car sales to final consumers. PIN data include the price of each vehicle sold, the exact date of the sale, transaction details, and the truncated Vehicle Identification Number (VIN). The data also include age, sex, and state of residence of purchasers.[3] The version of the PIN data released to me suppresses the personal identifying information of consumers and the dealer information that would allow identification of specific dealerships, to preserve confidentiality. I restrict the sample to non-fleet, purchased (not leased) vehicles with complete price information between fall 2002 and May 2007.[4]

The VIN of the new vehicle identifies the make, model, model year, engine type (displacement, cylinders), transmission, doors, body type, and trim level, but it does not detail all available options (e.g., sunroofs or stereo systems). All factory and dealer installed options, advertising fees and delivery fees, are reflected in a measure called the dealer cost, which is what the dealer pays the manufacturer for the vehicle. Also important is the "days to turn"—the number of days that a vehicle was on the dealer's lot before being sold. The data also include information on trade-ins and transaction details, including service contracts, interest rates, and other loan details. In addition to the PIN data, I also use weekly tax inclusive national retail gasoline prices provided by the Energy Information Administration.

[3] Sex is imputed from first names, and ambiguous first names are thus missing.
[4] Less than 3.5 percent of Priuses in the sample were leased.

The effective price to a consumer depends on not only the price that appears on the contract, but also on manufacturer incentives and the trade-in allowance. Manufacturers lower transaction prices by offering cash rebates and low-interest financing. The trade-in over or under allowance—the difference between the amount listed for the trade-in on the contract and the dealer's actual estimated cash value, which is used for entering the vehicle into their own inventory—also influences effective price. Some price movements, which I control for with date variables, are cyclical, including a significant gradual decline in prices over the model year (Adam Copeland, Wendy Dunn, and George Hall 2005) and modest price declines on weekends and days at the end of the month (Meghan Busse, Jorge Silva-Risso, and Florian Zettelmeyer 2006).

The measure of price used in this paper, called the incentive adjusted price, is the transaction price inclusive of factory installed options (but exclusive of taxes, fees, service contracts, and aftermarket options), net of trade-in allowance, cash rebates, and financing incentives. For financing incentives, I assume a 4 percent annual discount rate and estimate the difference in the present value of the loan observed in the data (given the loan amount, interest rate, and term) to that same value using the Federal Reserve's 48-month car loan interest rate.[5]

Dealers pay manufacturers the "dealer invoice price" for a vehicle, which is used in this paper to control for installed options.[6] This is valid if Toyota did not change the invoice price around the policy. Invoice prices tend to be constant both across space and over time within the model year. Franchise law dictates that dealers be charged the same price for identical goods, so invoice prices vary little across dealerships.[7] Generally, instead of changing invoice prices, manufacturers use dealer cash incentives (and direct consumer incentives) to affect transaction prices. I verify this stability for the Prius by checking an industry source that details dealer incentives.[8] The data indicate that prices to dealers were steady through the first two tax changes. The Toyota dealers that I spoke with indicated that they did not receive dealer cash incentives on the Prius. In addition, the distributions of costs in the sample show evidence of changes in the proportion of different options packages, but generally support the notion that Priuses with the same options package cost the same around each of the first two tax windows (see online Appendix for details). Around the third tax change, however, Toyota changed both the dealer invoice and

[5] This price adjustment methodology follows Carol Corrado, Dunn, and Maria Otoo (2006).

[6] Price variation due to installed options can be large. For example, the 2007 Prius has a base MSRP of $22,175, but the premium options package adds $6,350 to the retail sticker price. If the number of Priuses that have this options package changes from week to week, average prices will change, even if the prices of identical vehicles do not.

[7] Manufacturers do create some price dispersion through the use of bonuses and rewards for meeting certain benchmarks and through variation in advertising and delivery fees across localities.

[8] Specifically, I examined the "Dealer Incentives" table in *Automotive News*, which indicates no Prius dealer incentives in December 2005, January 2006, September 2006, October 2006, or March 2007 (*Automotive News* 2005–2007). *Automotive News* does report an incentive in April 2007, but this may have indicated either the "economic savings bonus" or a reimbursement for the costs of using a Prius as a loaner vehicle if the customer later bought a Prius (Mark Rechtin 2007), in which case the incentive would have influenced a tiny fraction of vehicles. In a personal correspondence, an *Automotive News* employee indicated that they believe no dealer cash incentive existed in April 2007, though they were unable to explain the report.

Table 2—Descriptive Statistics of Final Sample

Model year	Last two weeks December 2005	First two weeks January 2006	Last two weeks September 2006	First two weeks October 2006	Last two weeks March 2006	First two weeks April 2006	Total
Sample size							
2003							2,381
2004							7,776
2005	80	82					20,897
2006	**433**	**925**	723	69	2	2	15,715
2007			**460**	**383**	**1,777**	**729**	16,561
Touring 2007			20	43	125	43	1,376
Total	513	1,007	1,203	495	1,904	774	64,706
Mean incentive adjusted price, exclusive of tax subsidy							
2003							21,068
2004							24,438
2005	25,190	24,163					25,110
2006	**26,855**	**26,340**	25,914	25,683	24,854	23,300	26,397
2007			**26,811**	**26,497**	**23,832**	**24,512**	24,748
Touring 2007			27,917	28,208	26,081	26,378	26,663
Total	26,595	26,163	26,290	26,532	23,981	24,613	25,134
Mean incentive adjusted markup, exclusive of tax subsidy							
2003							611
2004							2,279
2005	1,999	1,465					2,270
2006	**2,489**	**2,366**	2,296	2,217	408	−456	2,362
2007			**2,558**	**2,554**	**59**	**253**	804
Touring 2007			2,771	2,595	199	933	1,330
Total	2,412	2,293	2,404	2,511	69	289	1,837

Note: Bold font indicates the model year and trim level used in the primary estimation for each tax change.

the MSRP, necessitating a price adjustment to compare similarly equipped vehicles. This adjustment is discussed below and detailed in the online Appendix.

Table 2 shows the sample size, incentive adjusted price and price over invoice for the final sample of 64,706 Priuses by model year. The 2007 model year features a premium trim level "Touring" package, which is listed separately. The last column shows totals for the entire model year. The main estimates in this paper focus on transactions within two weeks on either side of each tax change. Table 2 shows summary statistics for each of these windows. The first two tax changes are close to changes in model years. The 2006 model year Prius began selling in November 2005, and the 2007 vintage began selling in September 2006.

Since it is critical to compare identical vehicles before and after each tax change, I estimate the effect of the first tax change using only 2006 model year Priuses, and I estimate the effect of the other two changes using only 2007 Priuses with the base trim level. These samples are indicated by bold font in Table 2. Thus, for example,

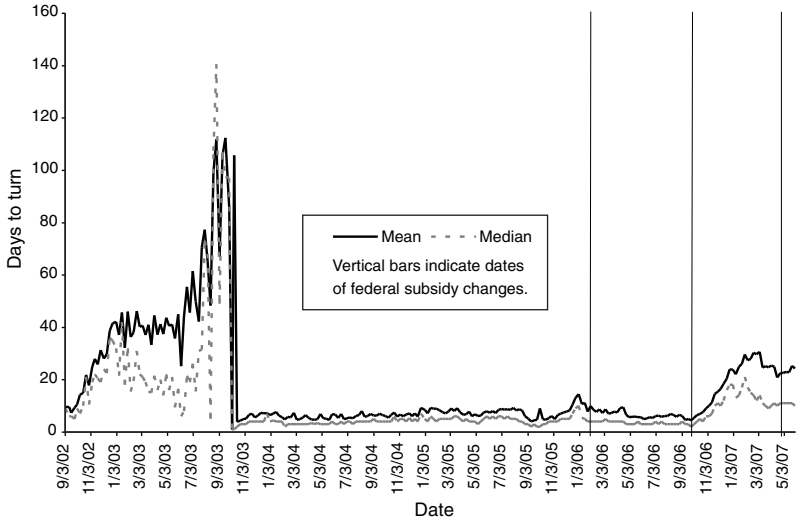

FIGURE 1. AVERAGE DAYS TO TURN OF TOYOTA PRIUS

there are 513 Priuses in the sample in the last two weeks of December 2005 and 1,007 in the first two weeks of January 2006, but I use only 433 from December and 925 from January in the principal analysis to restrict comparisons to a single model year. Except where noted, analysis of all the model years and trim levels together does not change any result significantly. I move now to a description of how consumers responded to these tax incentives.

III. Responses to Changes in the Federal Tax Credit

Transactions surrounding each federal tax-credit change show, that transactions shifted into tax-preferred time periods, and Priuses purchased in higher subsidy periods, cost about the same price as Priuses purchased in lower subsidy periods. This bunching implies consumer awareness of the policy, and, since consumers receive the tax break later, constant transaction prices imply that consumers captured the subsidy. Before turning to this analysis, I briefly discuss wait lists, which are critical to the interpretation.

A. *Wait Lists for the Prius*

The Prius was introduced on a small scale in the United States in model year 2001. The second generation Prius, which introduced the current, distinctive body style, debuted in model year 2004. The second generation Prius was in excess demand from its introduction until the 2007 model year, which was produced on a greater scale. Toyota originally planned to sell 20,000 Priuses per year in the United States. In 2007, they sold over 180,000.

Excess demand led to wait lists. The data do not measure wait times, but average days to turn should be inversely related to wait lists. Figure 1 shows the mean and median days to turn of the Prius for the 2003 to 2007 model years. The first

generation Prius sold slowly, but turnover for the second generation was remarkable. Between the end of 2003 and the fourth quarter of 2006, the median turnover time for a Prius was between three and five days, whereas the market median is over 60. The 2007 model year began at this same low rate, and rose, starting in November 2006, perhaps indicating that wait lists ended at that time.

Auxiliary data confirm this end date. Media reports declaring the end of wait lists also began appearing in November 2006 (e.g., Chris Woodyard 2006). Also, HybridCars.com invited hybrid buyers to report their date of purchase, wait times, and transaction prices in a public forum. The average wait time in these data fell to about 2 weeks in September and October 2006, and then went to zero starting in November. The timing of the dissipation of wait lists is important because it implies that the first two federal policy changes took place during periods of excess demand, whereas the final change did not.

B. *Agents Shifted Transactions Into High Subsidy Time Periods*

The sales distributions surrounding the policy changes reveal strategic shifting that creates transaction bunching on tax preferred dates. Shifting is limited, however, to the two weeks before and after each change. Figure 2 shows the distribution of sales over December 2005 and January 2006 for both the Prius and for non-hybrid Toyota sedans. Priuses purchased in December were eligible for the deduction (worth up to $700), and January purchases were eligible for the credit (worth up to $3,150). Relative to Toyota sedans, the Prius was sold more heavily at the beginning of January than at the end of December, suggesting that transactions were shifted into January, where the subsidy was greater. This pattern does not appear in the same months in the prior year when there was no tax change, as shown in Figure 3. Figures 4 and 5 show the corresponding distributions for September and October. As expected, Prius sales were abnormally high in September 2006, just before the tax credit phased out, but not in the prior year when there was no tax change. Figures 6 and 7 provide the same information for March and April 2007 and lead to the same conclusion.

In total, about 15 percent of monthly sales are shifted on average over the three events, with larger shifts around the larger tax changes, and the bunching is evidently limited to the two-week window surrounding changes. This bunching implies that many consumers were well informed, eligible for the subsidy, and sophisticated enough to time their transaction strategically. It also implies that a before-after comparison of prices around a tax change will not hold constant consumer characteristics that dictate who strategically times their transaction and who does not. This endogenous selection is addressed directly below.

C. *Average Prices Were Non-Responsive*

Straightforward before-after regression comparisons of transaction prices show no evidence of a statistically significant, positive relationship between prices and

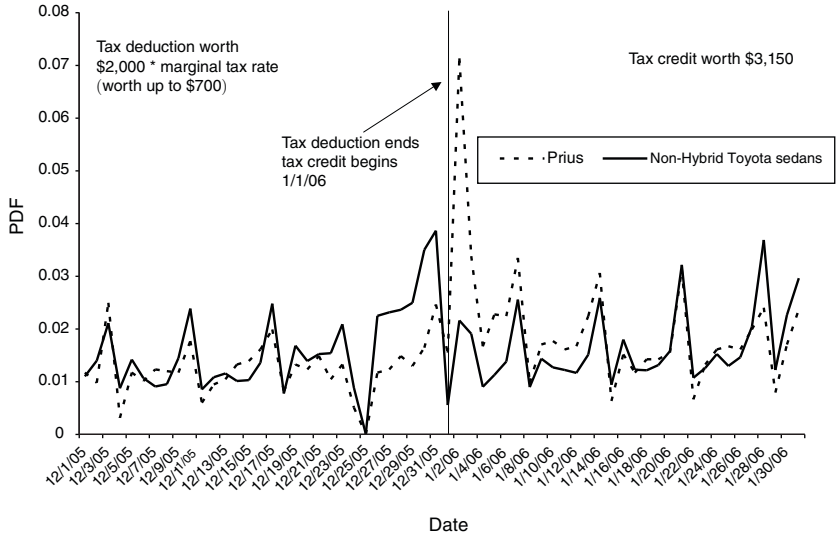

FIGURE 2. DISTRIBUTION OF SALES IN DECEMBER 2005 AND JANUARY 2006, PRIUS AND NON-HYBRID TOYOTA SEDANS (FIRST TAX CHANGE)

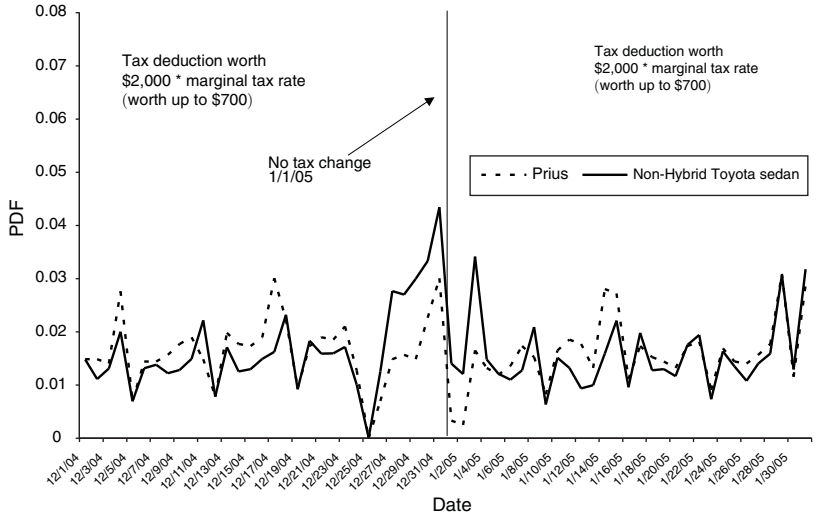

FIGURE 3. DISTRIBUTION OF SALES IN DECEMBER 2004 AND JANUARY 2005, PRIUS AND NON-HYBRID TOYOTA SEDANS (NO TAX CHANGE)

the value of the federal tax subsidy. Before-after estimates are obtained from regressions of the form:

$$P_{ij} = \alpha_1 H_1 \Delta_1 + \gamma_1 W_1 + \alpha_2 H_2 \Delta_2 + \gamma_2 W_2 + \alpha_3 H_3 \Delta_3 + \gamma_3 W_3 + \mathbf{X}_{ij}\boldsymbol{\beta} + \boldsymbol{\mu}_j + \varepsilon_{ij}, \tag{1}$$

where i indexes an individual and j a model year and trim level interaction, the other subscripts denote the three tax changes (1 = December 2005 and January 2006;

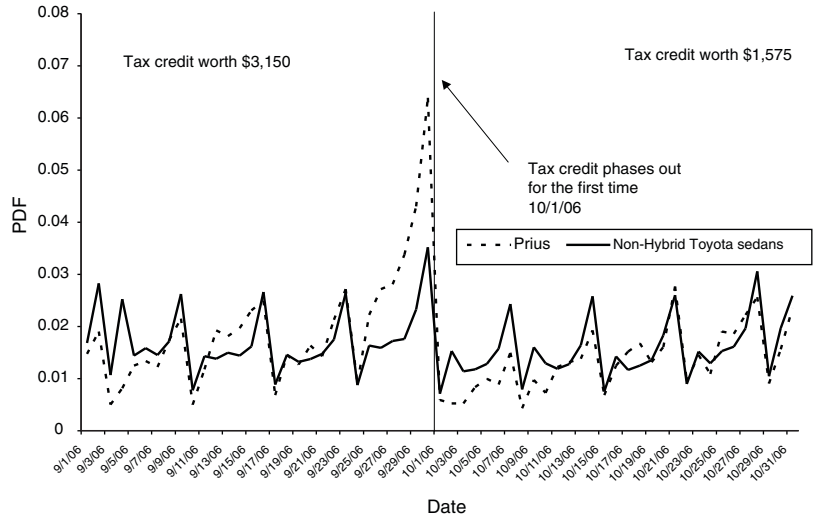

FIGURE 4. DISTRIBUTION OF SALES IN SEPTEMBER 2006 AND OCTOBER 2006, PRIUS AND NON-HYBRID TOYOTA SEDANS (SECOND TAX CHANGE)

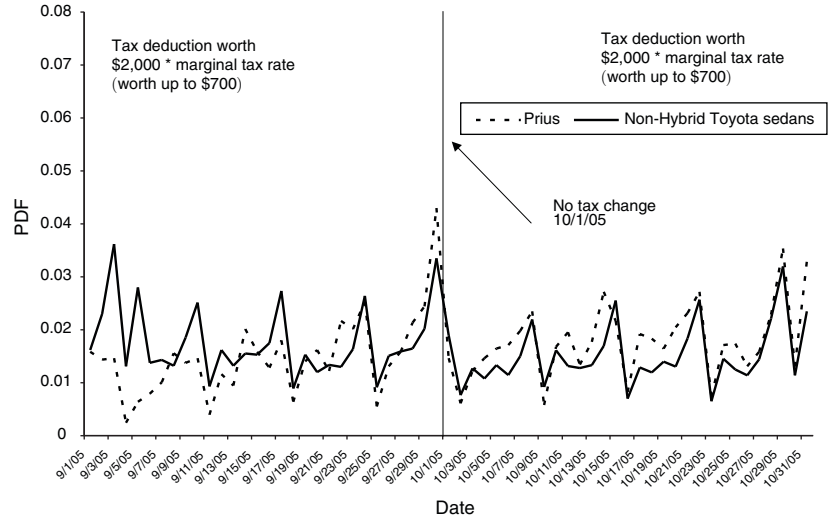

FIGURE 5. DISTRIBUTION OF SALES IN SEPTEMBER 2005 AND OCTOBER 2005, PRIUS AND NON-HYBRID TOYOTA SEDANS (NO TAX CHANGE)

2 = September 2006 and October 2006; 3 = March 2007 and April 2007), P is price, Δ is the dollar amount of the tax change in each case—defined to be *positive* in all cases for consistency of interpretation, H is a dummy equal to 1 if the transaction is for the analyzed model year (see Table 2) and occurs in the *high* subsidy side within two weeks of a change, W is a dummy equal to 1 if the transaction occurs on either side within two weeks of the change, $\mathbf{X}$ is a vector of controls, $\boldsymbol{\mu}$ is a vector of dummies for each model year and trim level, and ε is an error term.[9]

[9] Here, and elsewhere, the i and j subscripts on the H and W dummies are suppressed to avoid clutter.

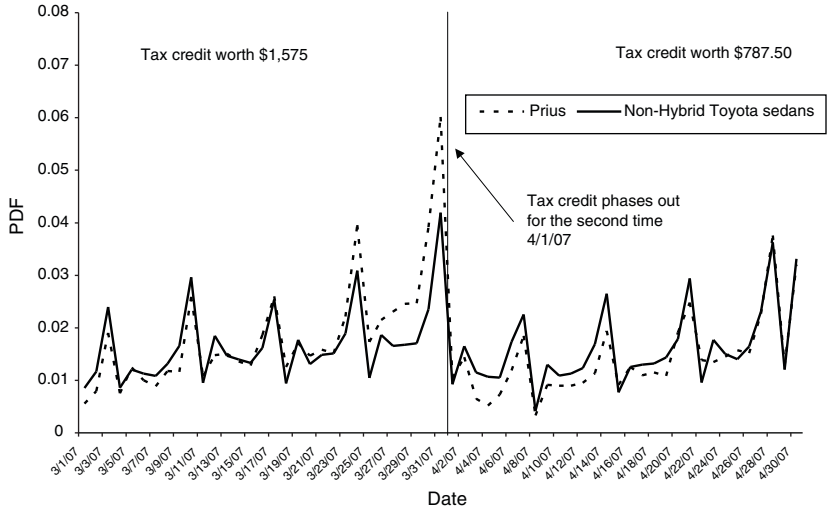

FIGURE 6. DISTRIBUTION OF SALES IN MARCH 2007 AND APRIL 2007, PRIUS AND NON-HYBRID TOYOTA SEDANS (THIRD TAX CHANGE)

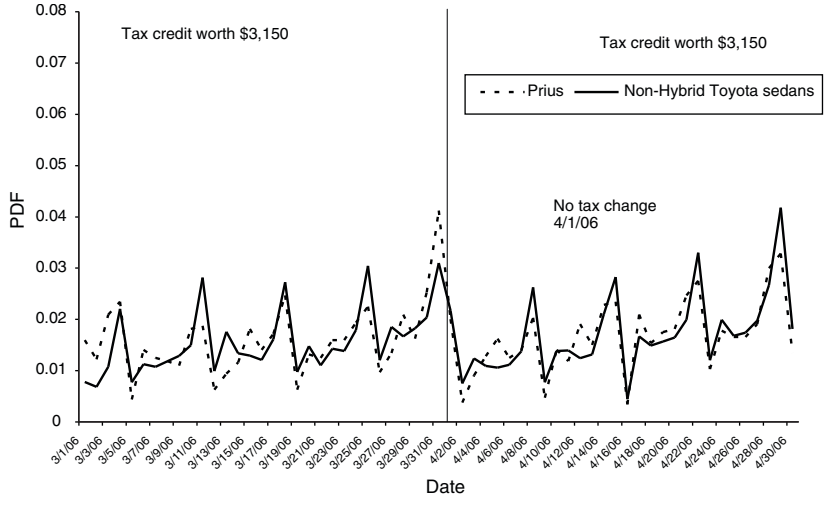

FIGURE 7. DISTRIBUTION OF SALES IN MARCH 2006 AND APRIL 2006, PRIUS AND NON-HYBRID TOYOTA SEDANS (NO TAX CHANGE)

For example, H_1 is coded as 1 if and only if the transaction is for a model year 2006 Prius sold in the first two weeks of January 2006. W_1 is coded as 1 if and only if the transaction is for a model year 2006 Prius sold in either the last two weeks of December 2005 or the first two weeks of January 2006.[10] The dependent variable is the incentive adjusted price paid by consumers. The sample includes all model year 2003 to 2007 Priuses (N = 64,706).

[10] Results are robust to an alternative specification that includes all model years and trims and separately estimates incidence parameters for each. See online Appendix.

The αs are the coefficients of interest. Since the high subsidy period dummies are divided by the dollar amount of the tax change, the αs represent the *change in price per dollar of tax change*. The standard tax incidence model predicts that $0 \leq \alpha \leq 1$. In calculating the size of each tax change, I use the maximum credit amount (\$3,150, \$1,575, and \$787.50), and I assume a 25 percent marginal tax rate for the deduction, making the deduction worth \$500.[11]

Table 3 reports the α coefficients for several specifications in the odd numbered columns, labeled "Before-After." (The even numbered columns are explained below.) The first column contains only the model year and trim level dummies as controls.[12] It is, therefore, simply the difference in means of vehicle prices in the 2 weeks with a higher subsidy value and the 2 weeks with a lower subsidy value, adjacent to each tax change, scaled by the change in subsidy value. Column 3 adjusts for options package composition by including the dealer's cost as a regressor. Column 5 adds day of the week dummies, a dummy for the last five days in a month, state dummies, the retail price of gasoline, and a quadratic trend in the length of time that type of Prius was on the market.[13] Column 7 adds transaction characteristics.

As an example of how to interpret the coefficients, consider the estimate in the first row and third column: controlling for dealer cost, prices fell by 3.7 cents for every increased dollar of subsidy when the tax credit was introduced in January 2006. Given the standard error of 2.4 cents, this is not statistically distinguishable from zero.

Controlling for vehicle composition (via dealer cost) influences the results. As argued in Section II, invoice prices were constant, so controlling for dealer invoice is appropriate and removes compositional effects.[14] Controlling for cost, the price effects for the first two changes are tightly estimated and statistically indistinguishable from zero. The subsidies did not generate a statistically significant upward price movement in the consumer price, so one cannot reject the hypothesis that consumers captured the entire subsidy.

In contrast, the estimates suggest that prices *rose* when the credit fell in April 2007, but this may be misleading because both dealer cost and financing incentives changed. In February and March of 2007, Toyota offered low-interest financing. These incentives were eliminated on April 2, 2007, just as the tax credit dropped from \$1,575 to \$787.50, but Toyota simultaneously announced an "economic savings bonus" for the Toyota Prius effective on the same day. This program lowered the price of each options package by as little as \$600 or as much as \$2,000. The dealer invoice for each options package was also changed.

[11] The 25 percent tax rate fits the most appropriate income range. For example, married couples filing jointly with adjusted gross income between \$59,400 and \$119,950 have a marginal tax rate of 25 percent. This range includes the mean self-reported income of Prius buyers in 2005 (\$87,500) from marketing research data (CNW Research 2007). In 2005, 21 percent of all tax filers had a marginal tax rate of 25 percent, and only 6.4 percent had a marginal rate above this (see Table 3.4, 112, Internal Revenue Service 2007). The top marginal rate in 2005 was 35 percent, which would raise the value of the subsidy to \$700.

[12] Coefficients for model year and trim level dummies are included in the online Appendix.

[13] Results are similar if a time trend is used in addition to or in place of vehicle specific time controls.

[14] One might be concerned that Toyota appropriated gains mainly by shifting the composition of options packages sold towards the more profitable types in response to higher subsidies. Surrounding the tax changes, Toyota sold cheaper configurations in January 2006 (high subsidy), October 2006 (low subsidy), and March 2007 (high subsidy), which does not point towards a consistent pattern (see online Appendix). Thus, it seems likely that the composition shifts are unrelated to the tax changes.

TABLE 3—ESTIMATED BOUNDS ON THE INCENTIVE ADJUSTED PRICE CHANGE OF PRIUSES PER DOLLAR OF TAX CHANGE IN FOUR WEEK WINDOW SURROUNDING TAX CHANGE

	No controls		Compostion adjusted		Controls		Extra controls	
	Lower bound (Before-after) 1	Upper bound (Hetero-geneity) 2	Lower bound (Before-after) 3	Upper bound (Hetero-geneity) 4	Lower bound (Before-after) 5	Upper bound (Hetero-geneity) 6	Lower bound (Before-after) 7	Upper bound (Hetero-geneity) 8
Discount rate = 4 percent								
Jan06–Dec05	−0.194	0.622	−0.037	0.249	−0.022	0.270	−0.006	0.280
	(0.054)	(0.076)	(0.024)	(0.034)	(0.024)	(0.033)	(0.023)	(0.032)
Sep06–Oct06	0.199	0.581	−0.010	0.127	−0.059	0.082	−0.040	0.105
	(0.106)	(0.182)	(0.047)	(0.064)	(0.047)	(0.065)	(0.045)	(0.063)
Mar07–Apr07	−0.864	2.043	−0.207	1.311	−0.195	1.264	−0.090	1.263
	(0.118)	(0.173)	(0.075)	(0.109)	(0.074)	(0.107)	(0.069)	(0.098)
Discount rate = 7.23 percent								
Jan06–Dec05	−0.194	0.612	−0.036	0.234	−0.021	0.253	−0.005	0.264
	(0.053)	(0.052)	(0.022)	(0.019)	(0.022)	(0.019)	(0.021)	(0.018)
Sep06–Oct06	0.226	0.603	0.017	0.149	−0.022	0.112	−0.006	0.133
	(0.057)	(0.059)	(0.035)	(0.032)	(0.034)	(0.031)	(0.032)	(0.028)
Mar07–Apr07	−0.554	2.336	0.103	1.501	0.102	1.438	0.180	1.409
	(0.208)	(0.281)	(0.084)	(0.081)	(0.084)	(0.084)	(0.082)	(0.084)

Notes: Dependent variable is the incentive adjusted transaction price of a new Prius, exclusive of tax subsidies. Heteroskedasticity robust standard errors in parentheses. Upper bound standard errors are from a nonparametric boostrap with 5,000 repetitions. Jan–Dec estimated based on 2006 Prius, Sep–Oct and Mar–Apr coefficients based on 2007, base trim Prius. "No Controls" includes only model year and trim level dummies. "Composition Adjusted" adds vehicle cost as a regressor. "Controls" adds vehicle cost, day of week dummies, state dummies, end of month dummy, price of gasoline and quadratic vehicle trend. "Extra Controls" adds sex, age, total after-market options, a dummy for an APR above the buy rate, a dummy for life insurance, a dummy for an APR above the buy rate, a dummy for life insurance, a dummy for the presence of a trade-in, trade-in actual value, trade-in vintage, and a dummy for a service contract. $N = 64{,}706$ overall, with the following sample sizes in each tax window: 433 (December), 925 (January), 460 (September), 383 (October), 1,777 (March), and 729 (April).

To determine whether consumers paid more or less for similarly equipped vehicles in April 2007, as compared to March, we need to know how much each April Prius would have cost dealers had it been sold in March. To do this, I use dealer invoice prices for each options package from Edmunds.com to identify which package each vehicle most likely had. I then adjust for the change in invoice price so that a Prius with the same options package in March 2007 and April 2007 have the same dealer cost. Details are in the online Appendix. Costs adjusted for the invoice price change are used throughout.

Because Toyota changed financing incentives around the policy date, many more vehicles received low-interest financing and many more customers chose to finance their vehicle through the dealership in March than in April. As a result, estimates are sensitive to the discount rate used to translate the lower monthly payments into a present value.[15] Using the pure discount rate of 4 percent, as suggested by Corrado, Dunn, and Otoo (2006), produces a perverse price estimate. The bottom panel in

[15] Other modifications of the calculation are discussed and additional results are presented in the online Appendix.

Table 3 shows an alternative estimate, using a discount rate of 7.23 percent, which is the mean market interest rate during the sample period. This higher discount rate lowers the present value of financing subsidies, and the results indicate a small positive price effect of the tax credit. It is not clear which discount rate is most appropriate; 4 percent is chosen as an estimate of pure time preference. If consumers are in debt, the appropriate rate might be the interest rate on their outstanding balance. Alternatively, someone with net savings might compare the cash flow difference to a safe asset's return. The discount rate has minimal impact on the estimates surrounding the first two tax changes, as anticipated.

In sum, according to before-after price comparisons, transaction prices surrounding the first two tax changes did not respond to tax changes, indicating that consumers captured the subsidy benefit. Price changes surrounding the third tax change are less clear because results are sensitive to the discount rate. What is clear is that Toyota took action to modify incentives surrounding the third tax change, and the fact that they lowered the MSRP is suggestive of their taking action to compensate for the elimination of the tax credit.[16] The next section moves beyond the before-after price comparisons to account for strategic timing.

IV. Accounting for Heterogeneity in Bunching

Before-after incidence estimates may be biased if consumers who reacted to tax changes by moving their date of purchase differ systematically from those who did not. Below, I argue that this bunching creates downward bias in before-after estimates. To overcome this, I develop an upward-biased estimate of tax incidence by making a worst-case assumption about the bias stemming from heterogeneous bunching. Together, the downward-biased estimate and the upward-biased estimate represent bounds on the true parameter of interest. For two out of three tax changes, these bounds are tight and confirm that consumers capture a significant majority of the tax benefits.

This bounding methodology is kept deliberately simple to provide transparency and generality. The intertemporal bunching analyzed here is but one example of a broad class of endogenous selection that occurs around discrete changes in policy treatment in time, space, and characteristics. The approach taken here to estimate the effect of the policy in the face of endogenous selection may prove useful in these other contexts; bounds may often provide useful information even when the exact structure of strategic bunching is unknown.

A. *A Model of Heterogeneous Bunching*

To understand the implications of bunching, suppose that there are two types of buyers, "movers" and "stayers." Individuals have some random ideal date of purchase drawn from a distribution common to both types. A mover with an ideal date of purchase near a tax change will move their transaction into the high subsidy period. In contrast, a stayer will purchase on the ideal date, regardless of pending tax

[16] Another possibility is that consumers systematically underestimate the value of financing subsidies, as suggested by Busse, Duncan I. Simester, and Zettelmeyer (2010).

changes. Consequently, both movers and stayers buy cars in the high subsidy period, but only stayers buy in the low period.

This composition difference may bias the before-after estimator. To see how, let m denote movers and s stayers, and let τ^H denote the high subsidy time period and τ^L the low subsidy time period. Denoting the price as p, the before-after estimate of tax incidence is:

$$(2)\quad E[p\,|\,\tau^H] - E[p\,|\,\tau^L] = \rho E[p\,|\,\tau^H, m] + (1-\rho)E[p\,|\,\tau^H, s] - E[p\,|\,\tau^L, s]$$

$$(3)\quad = \rho(E[p\,|\,\tau^H, s] - \zeta) + (1-\rho)E[p\,|\,\tau^H, s] - E[p\,|\,\tau^L, s]$$

$$(4)\quad = \theta - \rho\zeta,$$

where ρ is the percentage of those in the high subsidy period that are movers, $\theta \equiv E[p\,|\,\tau^H, s] - E[p\,|\,\tau^L, s]$ is the treatment effect of the tax on stayers and $\zeta \equiv E[p\,|\,\tau^H, s] - E[p\,|\,\tau^H, m]$ is the difference in mean price paid between stayers and movers in the high subsidy period. If movers are better negotiators (argued below), then $\zeta > 0$ and the before-after estimate is a downward-biased estimate of θ.

B. *Heterogeneous Bunching Creates Downward Bias*

There are many transaction details available in the data which can be used to see if high and low subsidy buyers are different. Table 4 shows mean characteristics of transactions in the policy windows and estimates of the mean difference in characteristics between high and low subsidy transactions from regressions of the form:

$$(5)\quad Characteristic_{ij} = \beta H + \gamma_1 W_1 + \gamma_2 W_2 + \gamma_3 W_3 + \varepsilon_{ij},$$

where i indexes individual consumers and j model year and trim level interactions, H is a dummy equal to one if the Prius is the model year analyzed and the transaction is within two weeks of a tax change on the high subsidy side, and W are dummies equal to one if the vehicle is the model year analyzed and the transaction is within two weeks of a tax change.[17]

Table 4 shows that high subsidy consumers were less likely to trade in a vehicle, required a smaller down payment, were less likely to buy a service contract, generated less service contract income for dealers, paid lower total expenses on after-market options, taxes and fees, and were less likely to accept an interest rate that exceeded the buy rate—as compared to low subsidy consumers.[18] High and low subsidy transactions

[17] All results are very similar if the high and low transactions are all grouped together without separate dummies; Prius transactions from a previous year with no tax change are used to construct a difference-in-differences estimate; or non-hybrid Toyota sedans are used as a comparison group to construct a difference-in-differences estimate.

[18] The buy rate is the rate that the financing agency quotes to the dealership. Dealers sometimes sign a contract for a higher rate than the buy rate, allowing them to make income on the difference.

Table 4—Evidence of Heterogeneous Timing Response, Mean Consumer and Transaction Characteristics Surrounding Each Tax Change

Demographics		High subsidy Prius buyers within 14 days of change	Low subsidy Prius buyers within 14 days of change	Difference	Standard error
Percent female	Jan–Dec	39.0	36.1		
	Sept–Oct	41.0	41.1	1.07	(1.62)
	Mar–Apr	40.3	39.9		
Age (years)	Jan–Dec	50.7	51.4		
	Sept–Oct	50.3	50.1	−0.67	(0.46)
	Mar–Apr	49.8	50.8		
Trade-in vehicles					
Percent with trade-in	Jan–Dec	28.3	30.3		
	Sept–Oct	22.2	30.0	−5.41	(1.45)
	Mar–Apr	37.5	43.9		
Trade-in vintage (year)	Jan–Dec	2,000.2	1,999.9		
	Sept–Oct	2,001.2	2,000.9	−0.13	(0.21)
	Mar–Apr	2,000.6	2,001.1		
Trade-in actual cash value ($)	Jan–Dec	9,379	8,736		
	Sept–Oct	9,713	10,121	−609	(358)
	Mar–Apr	7,783	8,950		
Contract details					
Total down ($)	Jan–Dec	5,359	6,421		
	Sept–Oct	6,070	5,843	−482	(238)
	Mar–Apr	4,511	4,988		
Amount financed ($)	Jan–Dec	24,387	24,379		
	Sept–Oct	24,400	24,565	3	(272)
	Mar–Apr	23,347	23,299		
Percent purchased service contract	Jan–Dec	37.7	45.0		
	Sept–Oct	42.4	48.6	−4.95	(1.53)
	Mar–Apr	45.9	49.0		
Service contract profit for dealers ($)	Jan–Dec	586	738		
	Sept–Oct	771	871	−94	(27)
	Mar–Apr	779	838		
Percent purchased life insurance	Jan–Dec	0.54	0.23		
	Sept–Oct	0.00	0.26	−0.01	(0.24)
	Mar–Apr	0.45	0.55		
Total cost of after-market options and fees($)	Jan–Dec	2,816	3,177		
	Sept–Oct	3,592	3,455	−124	(50.83)
	Mar–Apr	3,067	3,160		
Percent with buy rate < APR	Jan–Dec	13.6	14.5		
	Sept–Oct	13.7	17.5	−8.38	(1.13)
	Mar–Apr	3.5	18.0		

Notes: The difference estimate is a coefficient on a dummy equal to one if the transaction is within 2 weeks of a tax change, on the high subsidy side, from a regression with dummy variables for each 4 week window. Sample sizes in each window are as follows: 433 (December), 925 (January), 460 (September), 383 (October), 1,777 (March), and 729 (April).

are statistically indistinguishable from each other in age, sex, trade-in vintage, trade-in value, amount financed and whether the transaction included life insurance.

Thus, high subsidy car buyers differ from their counterparts in ways that suggest they are better negotiators. Intuitively, consumers with excellent knowledge of the car market are less likely to allow dealerships to generate income through service contracts, interest rate markups and after-market options. Also, research has

found that consumers who trade in a vehicle pay more for their new car (Fiona Scott Morton, Zettelmeyer, and Silva-Risso 2001).[19]

This suggests that $\zeta > 0$, which implies that before-after estimates are downward biased. Note that if the opposite were true, and the before-after results were upward biased (or unbiased), then the main conclusion of the paper—that consumers captured subsidy benefits—would still hold. I next develop an *upward*-biased estimate in order to bound the truth.

C. *Upper-Bound Estimates of Incidence*

An upward-biased estimate of incidence can be constructed by making a worst-case assumption about the nature of heterogeneity in bunching. Together with the downward biased before-after estimate, these two estimates should bound the true parameter.

Three assumptions identify an upper bound.[20] First, assume that the lowest price obtained by a stayer is higher than the highest price paid by a mover:

CONDITION 1: $\min[p \mid \tau^H, s] \geq \max[p \mid \tau^H, m]$.

This is an extreme assumption meant to represent a worst-case for the potential difference between types, given the observed outcomes. Critically, under this assumption, the stayers in the high subsidy period can be identified because they are the ones who pay the *highest* prices. To separate the stayers and movers in the high subsidy period, one just needs to know how many stayers there are.

A plausible estimate of the number of stayers in the high-tax period is the number of stayers in the low-tax period, which follows if stayers purchase vehicles at a constant rate within four weeks of each tax change. This motivates Assumption 2:

CONDITION 2: $n_s^H = n_s^L$,

where n is used to denote the size of the sample in each period. Together, Assumptions 1 and 2 enable a researcher to distinguish between movers and stayers in the high subsidy period where both are present. Stayers are simply the n_s^L highest prices.

[19] To further explore this relationship, I ran regressions of Prius prices on each of these characteristics and a set of controls equivalent to column 3 in Table 3, limiting the sample to transactions outside of the two-week windows surrounding each tax change. These regressions confirm that Prius buyers who have a trade-in, who have a service contract or life insurance, who pay more for aftermarket options, or who have a buy rate above the interest rate pay more on average for their vehicles.

[20] This methodology is an example of partial identification in the presence of corrupted data analyzed by Joel L. Horowitz and Charles F. Manski (1995). The corrupted data model supposes that the observed data are a mixture of true data and noise. Given information about the probability of noise, Horowitz and Manski (1995) show how to construct bounds on various parameters. In the present case, prices observed in a tax-favored window are a mixture of true data (prices paid by stayers) and noise (prices paid by movers). This methodology has not been previously applied to the literature on tax incidence, and, more generally, has rarely been applied (an important exception is V. Joseph Hotz, Charles H. Mullin, and Seth G. Sanders 1997). The estimator is also related to the bounding technique developed in David S. Lee (2009). Lee (2009) addresses a situation where the effect of a randomly assigned treatment is only partially identified because some outcome data are missing. In contrast, the approach developed here addresses a situation where selection into the treatment group may occur on unobservable characteristics, but all outcomes are observed.

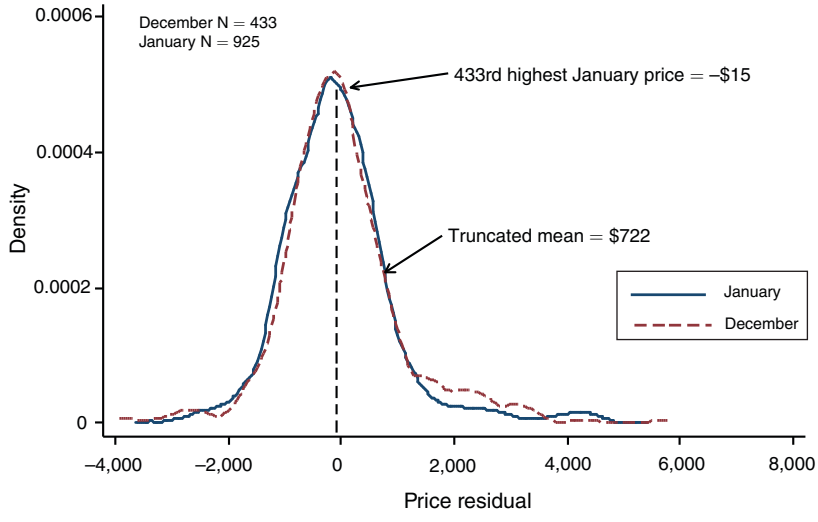

FIGURE 8. ILLUSTRATION OF UPPER-BOUND ESTIMATOR: DECEMBER 2005 AND JANUARY 2006, PRICE RESIDUALS

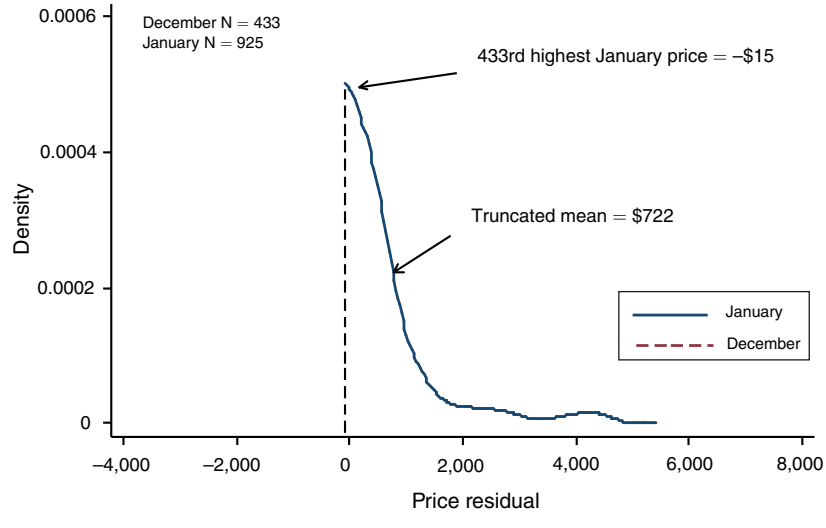

FIGURE 9. ILLUSTRATION OF UPPER-BOUND ESTIMATOR: DECEMBER 2005 AND JANUARY 2006, TRUNCATED "STAYER" JANUARY PRICE RESIDUALS

Given the extremity of Assumption 1, the mean of the n_s^L highest prices in the high subsidy period will *overestimate* the true mean price of stayers in that period.

Figure 8 and 9 illustrate this with data from the first tax change. The solid line is the distribution of price residuals from column 5 of Table 3 in January 2005, and the dashed line is December 2005. (Note that the distributions are very similar, which is why the mean price difference is roughly zero.) The bounding estimator takes the n highest January prices—the right tail—and assumes they are the prices paid by stayers. The upper-bound estimate is then the difference between the mean of the

truncated January distribution (shown in Figure 9) and the full December distribution, scaled by the size of the tax change. Note that the upper-bound estimate is therefore directly a function of the *variance* in high subsidy period prices.

The first two assumptions identify a bound on the treatment effect on stayers. If the tax effect on stayers is the same as the tax effect on movers, then this identifies both.

CONDITION 3: $E[p \mid \tau^H, m] - E[p \mid \tau^L, m] = E[p \mid \tau^H, s] - E[p \mid \tau^L, s]$.

This is a natural assumption if mean prices are determined by overall supply and demand and negotiating ability acts as an orthogonal error term in determining individual prices.

D. *Upper-Bound Estimation Results Confirm Consumer Gains*

Table 3 shows results from this exercise in the even numbered columns. The upper bound is constructed by collecting the n highest residuals from the high subsidy period from the regression that generates the lower bound, where n is the number of observations in the corresponding low subsidy window. The mean of these residuals is scaled by the size of the tax change and added to the lower bound to generate the upper bound. Given the scaling, both the lower and upper bound may be interpreted as dollar price changes per dollar of tax change. The standard error on the upper bound is obtained via nonparametric bootstrap.

Even under this extreme assumption regarding heterogeneity, large price responses to the tax change can be ruled out in the first two tax changes, because the variance in prices is low. According to column 6, the upper-bound point estimates for the first two tax changes indicate that consumers got *at least* 73 percent of the gains around the first tax change and 92 percent of the gains around the second. The estimated bounds for the third tax event, however, have little bite. Bound estimates suggest only that the incidence was between negative 20 and positive 126 cents on the dollar, which is a wider range than theory predicts. Mechanically, this is due to the larger price variation observed in April 2007. The data fail to rule out large price responses to the third tax change (when there were no wait lists), but it places sharp bounds on price changes during the first two tax changes (when there were wait lists).

Several robustness considerations warrant mention. One issue is the exact calculation of financing incentives. Results using additional discount rates are reported in the online Appendix, along with adjustments that account for the possibility that market interest rates are a poor counterfactual for Prius buyers, which turn out not to affect the estimates.

Another issue is that the first two tax changes occur near seams in the model year. Above, I use only one model year and trim level to avoid conflating tax effects with model year seam effects. This will influence the upper-bound estimate if it influences the relative number of observations in each period. Table 5 shows results that use an alternative assumption, which estimates ρ using all model years and trims. This has a very small impact on two of the tax changes, but the upperbound on the second tax change rises noticeably after this adjustment, though it remains well below 1.

Table 5—Estimated Bounds on the Incentive Adjusted Price Change of Priuses Per Dollar of Tax Change Using Sample Proportions to Reflect All Model Years and Trim Levels

	No controls		Compostion adjusted		Controls		Extra controls	
	Lower bound (Before-after) 1	Upper bound (Hetero-geneity) 2	Lower bound (Before-after) 3	Upper bound (Hetero-geneity) 4	Lower bound (Before-after) 5	Upper bound (Hetero-geneity) 6	Lower bound (Before-after) 7	Upper bound (Hetero-geneity) 8
Jan06–Dec05	−0.194	0.551	−0.037	0.224	−0.022	0.244	−0.006	0.256
	(0.054)	(0.075)	(0.024)	(0.032)	(0.024)	(0.032)	(0.023)	(0.030)
Sep06–Oct06	0.199	1.767	−0.010	0.550	−0.059	0.518	−0.040	0.516
	(0.106)	(0.120)	(0.047)	(0.078)	(0.047)	(0.077)	(0.045)	(0.073)
Mar06–Apr06	−0.864	2.067	−0.207	1.324	−0.195	1.277	−0.090	1.274
	(0.118)	(0.172)	(0.075)	(0.110)	(0.074)	(0.108)	(0.069)	(0.099)

Notes: Dependent variable is the incentive adjusted transaction price of a new Prius, exclusive of tax subsidies. Heteroskedasticity robust standard errors in parentheses. Upper-bound standard errors are from a nonparametric bootstrap with 5,000 repetitions. Jan–Dec estimated based on 2006 Prius, Sep–Oct and Mar–Apr coefficients based on 2007, base trim Prius. "No Controls" includes only model year and trim level dummies. "Composition Adjusted" adds vehicle cost as a regressor. "Controls" adds vehicle cost, day of week dummies, state dummies, end of month dummy, price of gasoline and quadratic vehicle trend. "Extra Controls" adds sex, age, total after-market options, a dummy for an APR above the buy rate, a dummy for life insurance, a dummy for an APR above the buy rate, a dummy for life insurance, a dummy for the presence of a trade-in, trade-in actual value, trade-in vintage, and a dummy for a service contract. $N = 64{,}706$ overall, with the following sample sizes in each tax window: 433 (December), 925 (January), 460 (September), 383 (October), 1,777 (March), and 729 (April).

Finally, one potential concern with the overall approach is the possibility that macroeconomic shocks to the economy caused price movements that happened to coincide with tax policy changes. The online Appendix provides analysis demonstrating that the prices of comparable non-hybrid vehicles showed no significant price movements around the policy dates, mitigating this concern. These results can be used to construct difference-in-difference estimates of the incidence of the tax using other vehicles as a control. These results would be very close, within 1 or 2 cents per dollar of tax change, to the estimates reported above.

V. Estimation of the Effect of State Incentives

In several states, federal tax subsidies for hybrid vehicles were supplemented by state policies. In this section, I use a state panel research design to estimate the incidence of state tax incentives for the Prius. I find that, as in the case of the federal tax credit, consumers captured nearly all of the benefits from state tax incentives.[21]

State policies do not lend themselves to analysis of narrow windows around tax changes because sample sizes are too small. Since state laws change at different times, however, a state panel research design is possible. This has two significant advantages that complement the federal analysis. First, the estimating equation can include general time period effects, which alleviates concern about macroeconomic shocks. Second, given credible time period controls, it becomes more palatable to use observations further away from the tax change. Below, I use data from the entire life of each tax program.

[21] If the law of one price prevails across states, then the effect of a state tax incentive might be different than a federal tax incentive. States demonstrate significant differences in prices, however, suggesting that prices are not smoothed completely across borders.

TABLE 6—STATE TAX INCENTIVES FOR THE TOYOTA PRIUS

State	Type	Amount	Start date	End date	Sample size: Subsidy off	Sample size: Subsidy on
Colorado	Income tax	$3,150 to $4,622	7/1/00	—		
Connecticut	Sales tax	6% ($1,500)	10/1/04	—	47	100
District of Columbia	Sales tax	7% ($1,750)	4/15/05	—	32	65
Lousiana	Income tax	2% ($500)	1/1/91	—		
Maine	Sales tax	2.5% ($625)	1/1/97	12/31/05	169	193
Maryland	Sales tax	$1,000 max	7/1/00	7/1/04	2,487	509
New Mexico	Sales tax	3% ($750)	7/1/04	—		
New York	Income tax	$2,000	1/1/01	12/31/04	1,305	214
New York	Sales tax	$240	1/1/01	2/28/05		
Oregon	Income tax	$1,500	1/1/98	—		
Pennsylvania	Rebate	$500	3/25/05	—	525	1,574
South Carolina	Income tax	$630	1/1/06	—	46	182
West Virginia	Income tax	$3,150 to $3,750	7/1/97	6/30/06	27	11
Total					4,638	2,848

Note: For sales tax exemptions, the value of the exemption on a $25,000 car is included in parentheses, for ease of comparison.

Table 6 lists the states with tax incentives, along with the type of incentive, the amount and the effective dates. Twelve states have Prius tax incentives. Connecticut, the District of Columbia, Maryland, Maine, New Mexico, and New York had or have a full or partial sales tax exemption. Colorado, Louisiana, New York, Oregon, Pennsylvania, South Carolina, and West Virginia had or have a state income tax credit.[22] Colorado and West Virginia had the largest incentives, which were often worth more than the federal credit. Sales tax exemptions in Connecticut and the District of Columbia, as well as the credits in Oregon and New York, were also worth more than a thousand dollars for most Priuses.[23]

Table 7 reports regressions of the form:

$$Price_{ist} = \lambda\tau_{st} + \mathbf{X}_{ist}\boldsymbol{\beta} + \gamma_t + \delta_s + \mu_j + \varepsilon_{ist}, \tag{6}$$

where i indexes an individual, s a state and t a time period, *Price* is the incentive adjusted price, τ is the state tax incentive, $\mathbf{X}$ is a vector of controls, γ are week dummies, δ are state dummies, μ are model year cross trim level dummies, and ε is the error term. The parameter of interest is λ. The controls include dealer's cost, day of the week dummies, a dummy for the five days at the end of a month and a quadratic

[22] Utah had a credit for which the Prius did not qualify because it does not have a non-hybrid version.

[23] Some states also passed laws that allow hybrids to use car pool (e.g., high-occupancy vehicle (HOV)) lanes, regardless of the number of passengers. Estimating the effect of these incentives on Prius prices directly is challenging, because such a policy should affect prices as soon as it is *expected* to pass. The states with HOV policies were not the same as the states with tax incentives, so HOV policies pose a problem only to the extent that they introduce bias into the estimation of time effects. The most concerning case is California, which is large enough to impact regression estimates. Results reported below are robust to including separate dummies for California for when the HOV policy was active and when it was not.

Table 7—Incentive Adjusted Price Change of Priuses Per Dollar of State Tax Incentive Change

	1	2
State tax incentive	0.072	0.011
	(0.073)	(0.090)
Dealer cost		1.062
		(0.011)
State fixed effects	X	X
Week fixed effects	X	X
Observations	64,706	64,706
Adjusted R^2	0.25	0.80

Notes: Dependent variable is the incentive adjusted transaction price of a new Prius, exclusive of tax subsidies. Heteroskedasticity robust standard errors, clustered on state, are in parentheses. Controls include dealer cost, day of week dummies, a quadratic in vehicle time and model year and trim level dummies.

trend in time on the market.[24] The regressions are run on the same sample of Priuses used above.

In these regressions, the coefficient on the tax variable is identified by states that experience a policy change in the sample period. Of the twelve states with tax incentives for the Prius, only eight experienced a change in the sample period: Connecticut, the District of Columbia, Maine, Maryland, New York, Pennsylvania, South Carolina, and West Virginia. The final two columns of Table 6 show how many Priuses are in the sample when the state policy was in effect and when it was not.[25]

Table 7 reports the results. The point estimate in column one says that, for a one dollar increase in a state tax incentive, the price of a Prius rises by seven cents. Whether or not the cost of the Prius is included as a regressor, the point estimate is indistinguishable from zero. The upper edge of the 95 percent confidence interval is 0.21. These results are robust to the exclusion of any of the states with tax incentives, ensuring that the result is not driven by a single state with a large sample or a big tax change.

The estimates in Table 7 are small enough that a zero effect cannot be ruled out, but precise enough to rule out large price movements. This corroborates the conclusions drawn from the federal policy: consumers captured the vast majority of tax credits for the Prius. Having established that consumers captured all, or nearly all, of both state and federal tax incentives for the Prius, I move now to a theoretical interpretation of this empirical result.

VI. Why Did Prius Prices Not Respond to Tax Changes?

The price consumers paid for the Prius was not responsive to changes in tax subsidies. In the standard static, competitive tax incidence model, this implies that

[24] Note that a variable indicating the value of the federal tax credit would be perfectly collinear with time period dummies, since the federal credit only varies over time.

[25] New Mexico's policy changed in the time window, but there are no New Mexico Priuses in the sample before the change.

supply is quite elastic, relative to demand. This cannot be, however, since Toyota was capacity constrained, which implies perfectly *inelastic* supply and suggests Toyota should have appropriated *all* of the gains. The implications of perfectly inelastic supply go beyond the static, perfectly competitive tax model, and it is unlikely that the observed incidence stems from dynamic or imperfectly competitive roots. To explain the case of the Prius, such a model would also need to provide an explanation for wait lists, which indicate that the seller can raise prices without lowering quantities. The empirical result of this paper is, therefore, a puzzle.

One plausible solution to this puzzle, popular among industry watchers, is that Toyota believed *future* demand for hybrids would be diminished if they charged market clearing prices for the Prius in early years. If so, then price is not determined solely by current demand and a temporary tax credit might logically have no effect on current price.

Why were there capacity constraints? As discussed in Section III, demand for the second generation Prius greatly outstripped Toyota's expectations. Toyota could not meet this high demand in the short run by increasing production because they faced capacity constraints. Automakers face large fixed costs, and there are potential supply bottlenecks, especially if a vehicle needs an uncommon part like a hybrid battery that is produced by only one supplier. Prior to the 2007 model year, all Priuses were assembled at one plant, the Tsutsumi Plant in Toyota City, Japan. When Toyota ramped up production significantly for model year 2007, it required the opening of a new assembly line in a different plant. This suggests that there were large capital investments that could not be quickly changed to accommodate demand.

Given capacity constraints, why did Toyota not increase prices to clear the market? One conventional explanation among industry watchers is that Toyota kept prices low because, in the long run, they wanted the hybrid drivetrain to be viewed as an affordable, mainstream technology. As the symbol of hybrid technology, the Prius was especially important, and Toyota wanted the Prius to be compared to the Camry and Accord, not to low-end luxury sedans. In other words, Toyota believed that if prices rose to clear the market during the period of excess demand, then demand for hybrids in the future would be lower.

Toyota is heavily invested in hybrid technologies. Toyota sold its one millionth hybrid in June 2007 and announced plans to sell one million a year by 2010 (James R. Healey 2007). Hybrids already represented 10 percent of Toyota's US sales in 2007. In 2003, Toyota pledged to introduce a hybrid version of all of their vehicles. Toyota also holds over 1,000 patents on hybrid-related technologies, and Nissan, Ford, and Fuji Heavy Industries have agreements to use Toyota's techniques (Ian Rowley 2006). If Toyota did believe that higher Prius prices in 2005 would jeopardize hybrid demand in later years, then they had a lot at stake.

Why would future demand depend on current prices? Car shopping involves significant search costs. Consumers do not know the exact price of a particular vehicle, or how much they will like it, until they invest time researching, test driving, obtaining price quotes, or even haggling. Before searching, consumers have expectations regarding valuations and prices, which they use to select which models to search

over. Having investigated several models, consumers choose their favorite car, given the realized valuation and price.[26]

The key implication of search costs is that demand for a model is determined by not only actual prices, but also *expected* prices. Higher expected prices will move a vehicle further down the search queue. Since consumers who achieve a good realization from an early search will stop searching and purchase a vehicle, demand is lower for vehicles with higher *expected* prices, conditional on actual prices. This effect could be quite large because average consumers consider only about three models before making a purchase (Ratchford and Narasimhan Srinivasan 1993; Moorthy, Ratchford, and Talukdar 1997; Ratchford, Myung-Soo Lee, and Talukdar 2003).

Past prices can also determine future demand if they change vehicle classification. Market clearing prices for the Prius in 2004 and 2005 might have been high enough to cast the Prius as a low-end luxury vehicle, inviting comparisons to superior vehicles. Reviews might have deemed Prius to have low "in class" quality, hurting its reputation.

Can a link between current prices and future demand explain incidence estimated here? If Toyota believed that high current prices would decrease future demand, this could explain wait lists. But, if the current price that mattered was the *net of tax* price, then Toyota would have kept this net of tax price constant by capturing the entire subsidy. It is only if consumers base future price expectations on the pre-tax price that a link between current price and future demand could lead to the observed incidence of a temporary tax credit. (The online Appendix develops a heuristic two-period model to further illustrate this conclusion.)

Why might pre-tax prices matter? Even if *current* car buyers know about subsidies, *future* car shoppers may not know that they should adjust past observed prices for the tax break. If future car shoppers are not informed of past policies, or if they do not understand tax incidence, they might forecast future prices based on pre-tax past prices. Furthermore, prices posted on internet chat rooms, Edmunds.com, Consumer Reports and elsewhere are almost always tax exclusive. Individuals occasionally report "out the door" prices that include taxes and fees, but, since sales taxes vary locally, reporting services typically report tax exclusive prices. These agencies did not subtract out the hybrid tax credit when reporting prices.

If consumer pre-tax prices matter, then the incidence of a tax will depend on statutory incidence. If Toyota had received a production credit, it could have kept the pre-tax consumer price constant (at the desired level for signaling future prices) by keeping the gains. If true, this has obvious import for policy design.

Why did Toyota not raise list prices to clear wait lists and then lower them and advertise heavily once production rose? In the search framework, both current and past prices determine demand. Increased advertising might reduce the role of past

[26] This description is closely related to the model of Martin L. Weitzman (1979), who describes the optimal search pattern and stopping rule. In his model, an agent faces a set of alternatives, which each have a unique search cost and a distribution of possible outcomes. After searching among several alternatives, the agent chooses the best option. The optimal search algorithm describes the order in which the options are searched and the optimal stopping rule. Sridhar Moorthy, Brian T. Ratchford, and Debabrata Talukdar (1997) also suggest that the Weitzman (1979) model is a good model of car shopping.

Table 8—Price Sensitivity of the Prius with and without a Wait List

	Incentive adjusted price of Prius
End of month	−8.07
	(7.51)
End of month, no wait list	−108
	(22)
Price of gasoline	1.19
	(0.67)
Price of gasoline, no wait list	5.78
	(1.12)
Weeks on market	−0.35
	(0.10)
Weeks on market, no wait list	−9.85
	(0.64)
Observations	64,706
R^2	0.80

Notes: Dependent variable is the incentive adjusted transaction price of a new Prius, exclusive of tax subsidies. Heteroskedasticity robust standard errors, clustered on state, are in parentheses. Controls include dealer cost, day of week dummies, a quadratic in vehicle time and model year and trim level dummies. Model year 2004, 2005, and 2007 Priuses are coded as having a wait list.

prices, but the intuition of the search framework will still apply if past prices have any influence on current demand.[27]

A. *Prices Were Generally Non-Responsive During Wait Listing*

The explanation proffered above implies that incidence depends on wait lists, and the data support this claim in several ways. First, in the case of the Prius, the difference between the first two and the final tax change is consistent with incidence differing in the presence of wait lists. Second, Prius prices were more sensitive to other price determinants when there was not a wait list, which indicates that prices were inflexible during the 2004 to 2006 model years. Table 8 shows results from regressions of Prius prices on the end of the month dummy, the price of gasoline and weeks that the model has been available, all interacted with a wait list dummy.[28] All three interactions are statistically significant; Prius prices were insensitive to various demand shifters during the wait list period.[29] Finally, the online Appendix includes

[27] Also, automakers may be reluctant to lower *list* prices because this may be viewed as a signal of reduced quality by consumers, who may view list prices as reflecting vehicle attributes and transaction prices as reflecting supply and demand. Automakers prefer to affect a price reduction via consumer rebates, rather than changing list prices. (This is in fact what Toyota did in April 2007.) It is likely that Toyota felt changing the sticker price by five or ten thousand dollars would have led consumers to believe that the vehicle's attributes had changed.

[28] I assume that 2004–2006 model year Priuses were on a wait list, and 2002 and 2007 models were not.

[29] If prices were constant but taxes changed demand, vehicle turnover should change. But, during the wait list period, median turnover was only a few days, which is plausibly a lower bound. Regressions analyzing whether or not turnover was faster in higher tax regimes using both federal and state variation are consistent with this hypoth-

an analysis of the estimated incidence of tax credits for other hybrid vehicles, some of which faced tight supply conditions and some of which did not. Broadly, the pattern of incidence is consistent with tightly supplied vehicles having limited pass through to manufacturers, but the bounds are broad and sample sizes low, making the exercise speculative.

B. *The Role of Dealers*

Even if Toyota wanted low prices, what stopped dealers from raising prices? Toyota is legally prohibited from setting retail prices, but they nevertheless have enormous leverage. According to dealers I spoke with, Priuses were allocated based on how fast previous Priuses had been sold. If a dealership raised the price of a Prius to clear the local market, increasing turnover time, the dealership would receive fewer future Priuses, which were grossing far more than comparable models. More generally, dealers risked Toyota's ire.

This explains why dealers did not make large price adjustments. There is an institutional reason they did not make small adjustments (that might escape Toyota's notice). It is illegal to sell a car for more than the MSRP, unless the dealer modifies the sticker to include an "additional dealer markup" or "additional dealer profit." Dealers occasionally do post additional markups on vehicles in high demand, but individual salespeople cannot negotiate a price above MSRP unless management has changed the sticker. The posting requirement also makes it transparent to consumers that the markup is pure dealer profit. Dealers I spoke with said that such markups created public relations problems. If dealerships experience a discrete reputation cost when charging above the sticker price, this may have prevented small price adjustments around tax changes.[30]

C. *Alternative Explanations*

Gradual Price Changes.—The empirics above focus on short time periods surrounding tax changes in order to minimize the chance that unobserved factors are shifting prices. If Toyota did in fact gain a large share of the federal subsidy, but it captured these gains by increasing prices slowly over several weeks (perhaps to avoid political backlash), the main estimates may miss this. If this were the case, however, we should see this price movement in the state panel regressions, which use transaction prices from the entire sample period, not just windows around each change.

esis, with the exception that turnover was lower during the low value tax deduction regime, which was the height of the wait list period. Details available upon request from the author.

[30] Daniel Kahneman, Jack L. Knetsch, and Richard Thaler (1986) asked consumers whether or not it was fair for an automobile retailer to raise prices by $200 in response to a shortage for a popular model. In one version, respondents were told that the dealer had been selling the car at the list price; the increase would therefore require charging more than the sticker price. In a second version, respondents were told that the dealer had been selling the car at $200 below list price; the increase would therefore lead to a new price equal to the list price. In the first case, 71 percent of respondents said the price increase was unfair, but only 42 percent thought the price increase was unfair in the second case. The difference is statistically significant.

Awareness.—If consumers were unaware of the policy or ineligible for the subsidy, the policy might not impact demand. Figures 2 to 7 demonstrate that many consumers were aware of the tax policy, but it does not imply that all were aware. Dealers themselves, however, were certainly aware of the policy and could have informed potential buyers.[31] It is therefore illogical to conclude that sellers failed to capture a portion of the subsidy because consumers were uninformed; dealers would have chosen to inform consumers if they stood to benefit.

Eligibility.—It is also unlikely that tax ineligibility could have driven the results, because relatively few Prius buyers would have been ineligible. Few buyers in a position to pay $25,000 for a new car have no tax liability, so non-refundability was probably of limited import. Prius buyers who pay the alternative minimum tax (AMT) are also ineligible, but the AMT affected relatively few people in aggregate—around 3 percent of all filers in 2005 and 2006. Prius buyers are, however, wealthier and more likely to live in high AMT states than average tax filers.

Unfortunately, the US Treasury Department has no available information about hybrid tax incentive claimants, so estimates must rely on private marketing surveys. CNW Research estimated that the mean household income of 2006 Prius buyers was $88,750, but they provide only the mean, not a distribution. Scarborough Research provides a breakdown of 2007 hybrid buyers into several income categories: under $50,000, between $50,000 and 75,000, and over $100,000. They estimate a mean income of $112,000, which is considerably higher than the CNW estimate, but this is for *all* hybrid cars, not just Priuses.

To get a high end estimate of the number of Prius buyers who might have paid the AMT, I do the following. First, I use the Prius sales share across states from the transaction data and the higher Scarborough values (assuming the national income distribution applies in all states) to estimate the number of Prius buyers in each income category in each state. Then, I calculate the estimated number of AMT payers for each cell using IRS estimates of 2005 state-specific AMT rates by income category. The IRS reports AMT rates for filers between $100,000 and $200,000, and for $200,000 and over, whereas Scarborough reports simply $100,000 and over. The difference is vast—the AMT rate for those making between $100,000–$200,000 is 14 percent, whereas the rate for those over $200,000 is 61 percent. To get an upper-end estimate, I apply the higher $200,000 rate to *all* of those making over $100,000, which yields an estimate that 29.7 percent of Prius owners paid the AMT. Alternatively, if only one-half earn above two-hundred thousand (still likely a significant overestimate given the distribution of tax returns), the number falls to 18.4 percent.

Under these high-end estimates, most Prius buyers were eligible, but a sizable minority were not. If many Prius buyers were knowingly ineligible, then states with lower AMT rates should have less bunching and a bigger estimated incidence. Auxiliary regressions (not shown) relating the amount of bunching and the state specific incidence against several measures of AMT prevalence show no statistically or

[31] The Toyota dealers I spoke with confirmed that Toyota distributed information about the tax credit several weeks prior to each change, which ensures that dealers were informed.

economically significant relationship between these outcomes and the AMT, which suggests the AMT plays a small role.

Furthermore, many consumers affected by the AMT were evidently surprised to find that they could not benefit from the credit. The discussions at PriusChat.com and Edmunds.com indicate that most people who posted comments on the AMT were surprised that they did not receive the full credit because of the AMT, and mention of the AMT in popular media did not appear until well after the first two tax changes. A law firm even filed a class action suit claiming that AMT payers were misled to expect a credit.

If some fraction of Prius buyers in the tax windows were knowingly ineligible, but this did not influence the degree of bunching, then the main results in this paper must simply be scaled up to reflect the fact that average tax changes were smaller than anticipated. Using the extreme 30 percent number to adjust the preferred specification in column 5 of Table 3 would raise the first event 27 cent upper bound to 39 cents, the second event 8 cent upper bound to 11 cents, and the (uninformative) third event $1.26 upper bound to $1.80. This will boost the upper-bound estimates some, but even these aggressive calculations hardly change the overall story.

Hot Cars.—An alternative explanation for rationing is that Toyota decided wait lists signaled that the Prius was a "hot car" and created valuable publicity.[32] If Toyota had decided that wait lists were good for publicity, this might explain their reluctance to raise prices to clear the market, but it does not explain why they did not appropriate the gains from the tax credit. If Toyota picked an optimal queue length based on publicity benefits, they could have preserved that queue length with higher pre-tax prices after the tax credit was introduced. This hot car theory is a plausible explanation of rationing, but it does not explain incidence.

Information and Incidence.—Busse, Silva-Risso, and Zettelmeyer (2006) find that the incidence of direct to consumer manufacturer rebates differ substantially from direct to dealer ones in the car market. They posit that this is due to asymmetric information because the party with greater information stands to capture more of the rebate in a bilateral bargaining game. This might help explain the present results if consumers have more information about their eligibility for the credit than dealers.[33] This is unlikely to drive the main results, however, because a large fraction of buyers were eligible for the credit, minimizing dealer uncertainty. Note also that the main policy conclusion—that statutory incidence matters—would also be true under this alternative view.

Political Games.—A final possibility is that price changes would have been noticed by Congress, who might subsequently punish Toyota. But, this explains neither wait lists nor the state panel results. Furthermore, it may also lead to tax asymmetry. If Congress had provided a production credit to Toyota, it probably would not have expected Toyota to pass on the entire savings to consumers during

[32] Special thanks to an anonymous referee for this suggestion.
[33] Special thanks to an anonymous referee for this suggestion.

a capacity constraint. It may be that legislators, equipped with only a naïve understanding of tax incidence, believe they can dictate economic incidence through statutory incidence. If so, then they may have signaled to the market that they wanted to provide a subsidy to consumers rather than producers. In a non-competitive market with large, visible players, firms may understand these expectations and choose to meet them.

VII. Discussion and Conclusions

This paper uses transaction level data on new vehicle purchases to assemble several pieces of evidence that indicate that consumers captured the significant majority of the benefits from tax subsidies for the Toyota Prius. The federal tax credit for hybrids created three sharp changes in the value of federal tax subsidies. Incidence estimates based on comparing transaction prices just before and just after each tax change show that subsidy exclusive transaction prices moved very little, if at all, which implies that consumers captured the bulk of the subsidy. An analysis of the incidence of state tax incentives corroborates this result. The paper also develops a method of bounding the effect of the tax in the presence of heterogeneous timing responses. This methodology yields informative upper bounds on the first two tax changes and verifies the conclusion that consumers captured the majority of the subsidy.

The proffered explanation—that Toyota did not raise prices because it was concerned about how current prices influenced future demand—implies that statutory incidence might influence economic incidence if consumers use pre-tax current prices to forecast future prices. The economics of taxation has recently shown renewed interest in cases where tax equivalence theorems fail, and several rationales have been offered. One possibility is that avoidance opportunities and enforcement costs may depend on who remits a tax (Wojciech Kopczuk and Slemrod 2006; Slemrod 2007). Another possibility is that bounded rationality gives rise to different responses to tax inclusive and tax exclusive posted prices (Chetty, Adam Looney, and Kory Kroft 2009). In the car market, Busse, Silva-Risso, and Zettelmeyer (2006) argue that information asymmetry generates differences in the incidence of manufacturer incentives, which act like a subsidy, depending on whether incentives are rebated to consumers or dealers. The case of the Prius highlights conditions under which remittance matters, not because of avoidance, bounded rationality, or information asymmetry, but instead because search frictions cause tax exclusive prices to influence future demand. Note that the prediction that statutory incidence may determine economic incidence is common to other markets out of equilibrium—the incidence of a wage tax in a market with a binding minimum wage depends on incidence.[34] The Prius is an interesting case of this phenomenon.

The bounding methodology developed in this paper could help researchers in many contexts to identify behavioral parameters in spite of strategic bunching. The tax literature has recently taken interest in bunching around kinks and notches,

[34] Special thanks to an anonymous referee for pointing out this example.

and economists of all types are studying discontinuities. When strategic bunching occurs, which is when regression discontinuity fails, something can often still be learned about behavior. Bounding approaches may be one useful tool, particularly when the variance of outcomes is not too great.

The Prius was a new product, with uncertain costs, sold in a market with search frictions and capital investments that led to capacity constraints. Other products subsidized by the government may share these features. Obvious examples are other advanced technology vehicles, such as plug-in hybrids, hydrogen fuel cell vehicles, clean diesels, and electric cars. Many of these advanced technology vehicles qualify for existing subsidies, and, if they become popular, it is reasonable to expect that subsidies will accrue to consumers without influencing quantities. For example, Tesla Motors began producing a high-priced electric vehicle in 2008 and pre-sold the entire first year's production before assembly began. It was later determined that the vehicle qualified for a $7,500 federal tax credit, which was realized as a windfall for consumers. The $7,500 tax credit also applies to the all-electric Leaf from Nissan, which has reportedly already received orders in excess of their first year's supply, and to plug-in hybrids like the Chevy Volt, which should be available during the 2011 calendar year, but only in small numbers. More broadly, other new "green" products, be they solar panels, personal wind turbines, or new technology appliances, are likely to share many of the market features of the Prius and be subsidized by governments. Lessons from the Prius may be useful in predicting the effects of government intervention into these markets. Specifically, it is likely that tax credits given to consumers will have different incidence implications than production credits given to manufacturers.

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