Online Appendix to Consumer Myopia in Vehicle Purchases: Evidence from a Natural Experiment

By Kenneth T. Gillingham, Sébastien Houde and Arthur A. van Benthem

Appendix A. Further Data Details

This appendix provides further details on the restatement, further details on the transaction prices reported to the DMV data, and provides a complete list of affected vehicles, and gives an example of a fuel-economy label.

While there have been other fuel-economy restatements for a small number of vehicle models (e.g., Ford restated the fuel economy for six models in 2014, and similar issues arose in 2019), the restatement by Hyundai and Kia was by far the largest in history and the first example of a restatement that affected many models. To make amends after this restatement, Hyundai and Kia provided owners of the affected vehicles purchased prior to the restatement with a lifetime offer of reimbursement based on the difference between the original and restated EPA fuel-economy rating (plus a 15% premium as an apology) (Autoblog.com, 2012). This compensation was announced only after the news about the restatement became public. Buyers were compensated via prepaid debit cards given at dealerships based on odometer readings and the fuel costs in the region where they live.

Through a class-action lawsuit, with a settlement finally approved by the courts on July 6, 2015, a second reimbursement option was added allowing affected customers to receive a single cash lump-sum payment (so customers could avoid having to return to the dealership frequently to have mileage verified) (Consumerwatchdog.org, 2018). An appellate court put this settlement on hold in January 2018, ruling that a lower court had made errors in approving the settlement. As a result, there is still a class-action lawsuit working its way through the courts as of January 2019.[1]

Note that both the initial compensation and any later payments resulting from class-action lawsuits only affected vehicles that had already been sold before the restatement date, and did not affect new vehicle buyers afterwards. As such, the new car transaction prices that we analyze do not involve or include compensation or settlement payments.

[1]Hyundai and Kia also settled with the U.S. EPA and agreed to pay $100 million in civil penalties, the largest such fines in EPA history up to that date, in addition to relinquishing emissions credits worth around $200 million and offering compensation to previous buyers. See U.S. Environmental Protection Agency (2014).

Next we discuss the transaction prices in our data. The original source of our data is the Department of Motor Vehicles (DMV) in each state. To better understand what these data include, we spoke with state employees in both California and Connecticut to confirm our understanding. In both states, the process is identical. When new vehicles are purchased, the final transaction price is sent by the dealer to the DMV for both record-keeping and sales tax purposes. The reported final transaction prices are routinely audited and are considered accurate. The prices reported are the final price shown at the bottom of the 'final tally' sheet given to any new vehicle buyer. They are net of all rebates and incentives by the dealer to the customer (e.g., see page 13 under the 'Discounts' heading of California Department of Tax and Fee Administration (2019) for guidance for California, which was confirmed in a phone call). The final DMV transaction prices also (implicitly) reflect rebates and incentives from the manufacturer to the dealer (i.e., 'factory-dealer incentives'), as these will be passed through into lower transaction prices; this final transaction price that the consumer faces is the relevant one for our valuation analysis. A limitation of the DMV data is that they typically do not include any direct-to-customer manufacturer rebates, sometimes known as "customer cash" or "bonus cash." Such incentives can be relatively substantial (e.g., $1,000-$2,000) and are not uncommon.

Thus, we explored whether direct-to-customer manufacturer rebates might have also changed in response to the restatement. In short, our inquiries suggested that there was no change in direct-to-customer manufacturer rebates due to the restatement. We pursued this question using two avenues. First, we contacted all of the companies we were aware of that have data on direct-to-customer manufacturer incentives. The most well-known data source, which has been used in several previous academic studies, is J.D. Power. Unfortunately, in recent years, J.D. Power has refused to work with or even interact with academics. A second potential source is Autodata Solutions, but this company was recently purchased by J.D. Power. A third potential data source is TrueCar.com. They receive data from their dealer network and third parties, but only publish aggregate data by automaker and do not retain historical data. However, we managed to find and compile historical press releases with the data from TrueCar.com (by doing a search on the subscription service, Dow Jones Factiva, with the search terms 'truecar.com AND incentive' using a date range from January 2010 to May 2014 and source 'PR Newswire - All sources') (TrueCar.com, 2011-2014). The data include forecasts of what the direct-to-customer manufacturer incentives will be from the previous month and actual data. The data for the month just after the restatement—November 2012—show that actual direct-to-customer incentives were $1,358 per vehicle for Hyundai and Kia (on average across both automakers) and the incentives were actually *less* than the forecasted value of $1,488 per vehicle (note that the forecast was made by TrueCar.com before the restatement occurred). We see a similar pattern in December 2012 as well, where the actual incentives were $1,476 per vehicle and the forecasted incentives were $1,573 per vehicle. In contrast, in October 2012, before the restatement, the actual incentives were $1,375 per vehicle, while the forecast was $1,323. These findings are important because they show that Hyundai and Kia did not increase their direct-to-customer incentive spending just after the restatement. More broadly, there was no discernable change in trend in incentive spending around the time of the restatement; incentive spending was on an slightly upward trajectory in the fall of 2012, possibly due to the winter months being the slower months for new vehicle sales. For context, the average change in incentives between two months is $57 (calculated using Excel), which is well below the price changes we observe from the restatement.

The data from TrueCar.com demonstrate that overall incentive spending did not change from the trend due to the restatement. While our results are noisy, we also do not find evidence of declines in sales for either affected or unaffected Hyundai and Kia new vehicles—we only observe the decline in price for affected new Hyundai and Kia vehicles. This alone suggests that it is unlikely that incentive spending was shifted from affected to unaffected models and trims (recall that consumers several months later would not typically know whether a given vehicle trim is affected or unaffected by the restatement). But we also made a series of phone calls or other inquiries to data information services (e.g., WalletHub, Oddity Software, Consumer Financial Protection Bureau, Experian Automotive, Factiva, Nexus Uni, Business Insights) when inquiring for data, and several were willing to speak to whether there was any dramatic change in the composition of Hyundai and Kia direct-to-customer incentive spending at that time, and all said they were not aware of any shifts. While this is only anecdotal, it further supports minimal changes in direct-to-customer incentive spending that might affect our undervaluation calculations. Note that we cannot observe movement in other dealer's margins, such as preferential financing, but our inquiries into any changes on these margins turned up nothing. No mention of changes in financing at Hyundai or Kia right after the restatement came up in extensive web searches and in the discussions with data information services.

Now we move to the list of all of the Hyundai and Kia vehicles affected by the restatement. Table A1 contains a complete list of all of the Hyundai affected vehicles, along with selected vehicle characteristics. Table A2 provides the same information for the Kia affected vehicles. 80,000 of the vehicles sold had their combined (city and highway) rating drop by 3-4 miles-per-gallon, while 240,000 dropped by 2 miles-per-gallon, and 580,000 dropped by 1 mile-per-gallon (MPG) (Autoblog.com, 2012). Note that for some models, the change in the combined miles-per-gallon rating is zero, even if the city or highway ratings changed. In Table B.4 below, we show a robustness check in which we run our primary specifications while excluding such minimally affected models to confirm that they are not affecting our results.

We now move to a discussion of the fuel-economy label. Fuel-economy labels on all new vehicles indicate the combined city/highway fuel economy of the vehicle

Table A1—: Hyundai Affected Models

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
						Original Rating			Restated Rating		
Model	Model Year	Trim	Engine	Drive	Tran.	City miles-per-gallon	Hwy MPG	Comb. MPG	City MPG	Hwy MPG	Comb. MPG
Elantra	2011		1.8L		Automatic	29	40	33	28	38	32
Elantra	2011		1.8L		Manual	29	40	33	28	38	32
Sonata HEV	2011		2.4L		Automatic	35	40	37	34	39	36
Accent	2012		1.6L		Automatic	30	40	33	28	37	31
Accent	2012		1.6L		Manual	30	40	34	28	37	32
Azera	2012		3.3L		Automatic	20	29	23	20	28	23
Elantra	2012		1.8L		Automatic	29	40	33	28	38	32
Elantra	2012		1.8L		Manual	29	40	33	28	38	32
Genesis	2012		3.8L		Automatic	19	29	22	18	28	22
Genesis	2012		4.6L		Automatic	17	26	20	16	25	19
Genesis	2012		5.0L		Automatic	17	26	20	17	25	20
Genesis	2012		5.0L R-Spec		Automatic	16	25	19	16	25	18
Sonata HEV	2012		2.4L		Automatic	35	40	37	34	39	36
Tucson	2012		2.0L	2WD	Automatic	23	31	26	22	29	25
Tucson	2012		2.0L	2WD	Manual	20	27	23	20	26	22
Tucson	2012		2.4L	2WD	Automatic	22	32	25	21	30	25
Tucson	2012		2.4L	4WD	Automatic	21	28	23	20	27	23
Veloster	2012		1.6L		Automatic	29	38	32	27	35	30
Veloster	2012		1.6L		Manual	28	40	32	27	37	31
Accent	2013		1.6L		Automatic	30	40	33	28	37	31
Accent	2013		1.6L		Manual	30	40	34	28	37	32
Azera	2013		3.3L		Automatic	20	30	24	20	29	23
Elantra	2013		1.8L		Automatic	29	40	33	28	38	32
Elantra	2013		1.8L		Manual	29	40	33	28	38	32
Elantra	2013	Coupe	1.8L		Automatic	28	39	32	27	37	31
Elantra	2013	Coupe	1.8L		Manual	29	40	33	28	38	32
Elantra	2013	GT	1.8L		Automatic	28	39	32	27	37	30
Elantra	2013	GT	1.8L		Manual	27	39	31	26	37	30
Genesis	2013		3.8L		Automatic	19	29	22	18	28	22
Genesis	2013		5.0L R-Spec		Automatic	16	25	19	16	25	18
Santa Fe	2013		2.0L Turbo	2WD	Automatic	21	31	25	20	27	23
Santa Fe	2013		2.4L	2WD	Automatic	22	33	26	21	29	24
Santa Fe	2013		2.0L Turbo	4WD	Automatic	20	27	22	19	24	21
Santa Fe	2013		2.4L	4WD	Automatic	21	28	23	20	26	22
Tucson	2013		2.0L	2WD	Automatic	23	31	26	22	29	25
Tucson	2013		2.0L	2WD	Manual	20	27	23	20	26	22
Tucson	2013		2.4L	2WD	Automatic	22	32	25	21	30	25
Tucson	2013		2.4L	4WD	Automatic	21	28	23	20	27	23
Veloster	2013		1.6L		Automatic	29	40	33	28	37	31
Veloster	2013		1.6L Turbo		Automatic	25	34	29	24	31	28
Veloster	2013		1.6L		Manual	28	40	32	27	37	31
Veloster	2013		1.6L Turbo		Manual	26	38	30	24	35	28

Source: Hyundai Motor Company (2011-2014). MPG denotes miles-per-gallon.

Table A2—: Kia Affected Models

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
						Original Rating			Restated Rating		
Model	Model Year	Trim	Engine	Drive	Tran.	City MPG	Hwy MPG	Comb. MPG	City MPG	Hwy MPG	Comb. MPG
Optima HEV	2011		2.4L	2WD	Automatic	35	40	37	34	39	36
Rio	2012		1.6L	2WD	Automatic	30	40	33	28	36	31
Rio	2012		1.6L	2WD	Manual	30	40	34	29	37	32
Sorento	2012	GDI	2.4L	2WD	Automatic	22	32	25	21	30	24
Sorento	2012	GDI	2.4L	4WD	Automatic	21	28	23	20	26	22
Soul	2012		1.6L	2WD	Automatic	27	35	30	25	30	27
Soul	2012		1.6L	2WD	Manual	27	35	30	25	30	27
Soul	2012		2.0L	2WD	Automatic	26	34	29	23	28	25
Soul	2012		2.0L	2WD	Manual	26	34	29	24	29	26
Soul	2012	ECO	1.6L	2WD	Automatic	29	36	32	26	31	28
Soul	2012	ECO	2.0L	2WD	Automatic	27	35	30	24	29	26
Sportage	2012		2.0L	2WD	Automatic	22	29	24	21	28	24
Sportage	2012		2.4L	2WD	Automatic	22	32	25	21	30	25
Sportage	2012		2.4L	2WD	Manual	21	29	24	20	27	23
Sportage	2012		2.0L	4WD	Automatic	21	26	23	20	25	22
Sportage	2012		2.4L	4WD	Automatic	21	28	24	20	27	23
Optima HEV	2012		2.4L	2WD	Automatic	35	40	37	34	39	36
Rio	2013		1.6L	2WD	Automatic	30	40	33	28	36	31
Rio	2013		1.6L	2WD	Manual	30	40	34	29	37	32
Rio	2013	ECO	1.6L	2WD	Automatic	31	40	34	30	36	32
Sorento	2013	GDI	2.4L	2WD	Automatic	22	32	25	21	30	24
Sorento	2013	GDI	2.4L	4WD	Automatic	21	28	23	20	26	22
Soul	2013		1.6L	2WD	Automatic	27	35	30	25	30	27
Soul	2013		1.6L	2WD	Manual	27	35	30	25	30	27
Soul	2013		2.0L	2WD	Automatic	26	34	29	23	28	25
Soul	2013		2.0L	2WD	Manual	26	34	29	24	29	26
Soul	2013	ECO	1.6L	2WD	Automatic	29	36	32	26	31	28
Soul	2013	ECO	2.0L	2WD	Automatic	27	35	30	24	29	26
Sportage	2012		2.0L	2WD	Automatic	22	29	24	21	28	24
Sportage	2012		2.4L	2WD	Automatic	22	32	25	21	30	25
Sportage	2012		2.4L	2WD	Manual	21	29	24	20	27	23
Sportage	2012		2.0L	4WD	Automatic	21	26	23	20	25	22
Sportage	2012		2.4L	4WD	Automatic	21	28	24	20	27	23

Source: Kia Motors America, Inc. (2011-2014). MPG denotes miles-per-gallon.

in large block letters, include an estimate of the projected annual fuel cost from running that vehicle in large letters, include a dollar value savings (or spending) in fuel costs over the next five years relative to the average new vehicle, and also provide the vehicle's tailpipe greenhouse gas rating and a smog rating.[2] The EPA-rated fuel economy on the labels is also presented on websites widely used by car buyers, such as FuelEconomy.gov and Edmunds.com. In any comparison between vehicles, the EPA-rated fuel economy values will play prominently.

In May 2011, the Environmental Protection Agency and National Highway Traffic Safety Administration updated the label and it became widely used by nearly all automakers starting with model year 2012. It was mandatory starting with model year 2013. Figure A1 provides an example of the post-2011 fuel-economy label required to be posted on all new vehicles at the dealership. The fuel economy listed on the label for each affected Hyundai or Kia vehicle was updated immediately at the beginning of November in 2012.

There is a growing literature on the extent to which consumers pay attention to labels about the energy efficiency of products. For example, Newell and Siikamaki (2014) find that the EnergyGuide label for appliances that provides simple information on the monetary value of energy savings appears to come close to guiding cost-efficient decisions. Davis and Metcalf (2015) show that more precise information from EnergyGuide labels can lead to significantly better choices. Houde and Myers (2019) also show heterogeneity in the response to energy information in appliance purchases. In one of the few papers on fuel-economy labels, Alberini, Bareit and Filippini (2016) find that discrete fuel-economy grades (A-G) on mandatory labels for new vehicles in Switzerland influence equilibrium prices. This literature allows us to hypothesize that a large change in the listed fuel economy on the labels will influence equilibrium outcomes in the new vehicle market.[3] Moreover, in our context, it is not just the label that changed, but actually the EPA fuel-economy rating, which affects everywhere that fuel economy is mentioned.

[2]The combined city/highway fuel-economy estimate is based on U.S. EPA test ratings. The annual fuel cost estimates and fuel savings estimates are based on on-road fuel economy and an assumed 15,000 miles driven annually.

[3]The fact that Allcott and Knittel (2019) show that interventions to provide information about fuel economy (in addition to the fuel-economy labels) have little effect on behavior casts some doubt on the effectiveness of informational interventions, but is still consistent with consumers basing their beliefs on the rated fuel economy posted on the vehicle and found on websites and in manufacturer brochures.

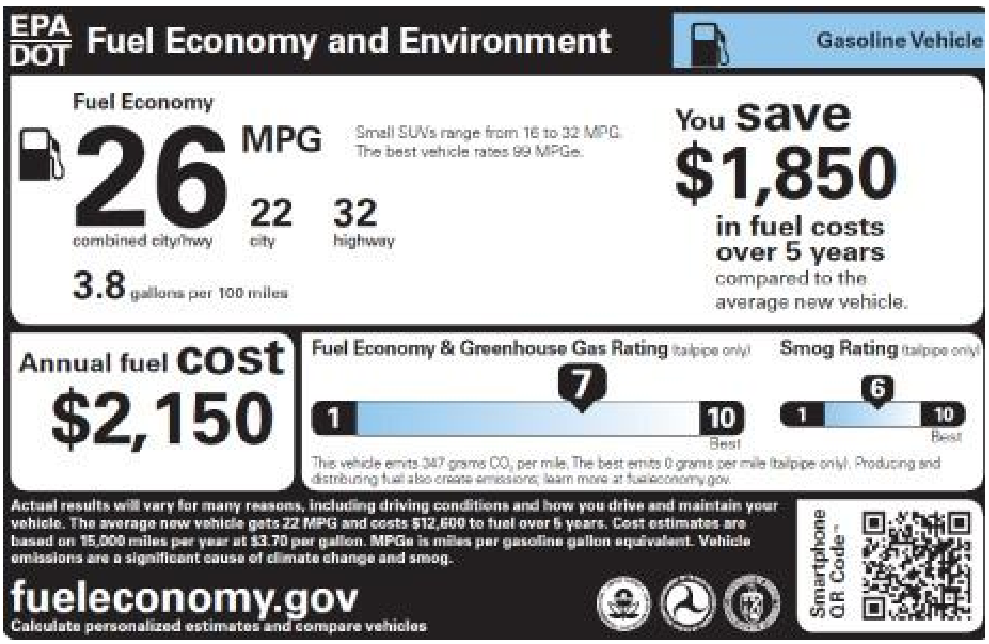

Figure A1. : An Example of a Fuel-Economy Label

Notes: Source: National Highway Traffic Safety Administration (2011).

Appendix B. Robustness Checks

This section provides a series of results to explore the robustness and heterogeneity in our primary findings. We begin by focusing on several different sets of fixed effects, which slightly change the variation being used to identify our coefficients. Table B.1 provides the first set of robustness results by including different sets of fixed effects for month-of-sample interacted with vehicle class. Specifically, we change the definition of a vehicle class to be finer than the one used in our main specification, where we do not distinguish luxury and non-luxury brands. In this robustness test, we use the exact segment definition proposed by R.L. Polk, which distinguishes luxury and non-luxury brands (which we label "finer class fixed effects"). We also use a coarser set of class fixed effects, which combine compact, mid size and full size crossover utility vehicles (into "crossover"); compact, mid size and full size sport utility vehicles (into "SUV"); subcompacts and compacts (into "small cars"); and mid size and full size (into "large cars"). These checks slightly change the variation being used, which amounts to effectively changing how we control for relative time trends in the price of affected and non-affected vehicles across segments. We find that our results are highly robust to these alternative specifications.

Table B.1—: Robustness Checks with Alternate Class Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Logs			**Levels**	
$1(Post\ Restatement)_t \times 1(Affected\ Model)_j$	-0.012	-0.011	-0.011	-294	-283	-240
	(0.003)	(0.004)	(0.004)	(91)	(93)	(90)
Year-Month × Class FE	Y			Y		
Year-Month × Coarser Class FE		Y			Y	
Year-Month × Finer Class FE			Y			Y
Year-Month × Make FE	Y	Y	Y	Y	Y	Y
VIN10 FE	Y	Y	Y	Y	Y	Y
DMA FE	Y	Y	Y	Y	Y	Y
1(Post Restatement) × DMA FE	Y	Y	Y	Y	Y	Y
R-squared	0.95	0.95	0.95	0.96	0.96	0.96
N	1.52m	1.52m	1.52m	1.52m	1.52m	1.52m

Notes: Dependent variable is log or level of the transaction price (in dollars). Columns 1 and 4 are our primary specification. Columns 2 and 5 use a coarse definition of vehicle classes where we only distinguish: small car, large car, minivan, crossover, SUV, and pickup. Columns 3 and 6 use a finer definition of vehicle classes, relative to the main specifications, where luxury and non-luxury vehicles are distinguished. The definition of vehicle classes in those specifications closely follows R.L. Polk nomenclature. An observation is a year-month-DMA-VIN10. VIN10 refers to the VIN prefix, which is a trim-engine combination. DMA refers to a Nielsen designated market area, which is an area covering several counties. Class refers to the vehicle class. *Post Restatement* refers to the year-month being during or after November 2012. All estimations are weighted by monthly sales. Standard errors clustered by VIN10.

Table B.2 provides further robustness results by including quarter-of-age by make fixed effects to capture the cyclicality in the vehicle market that depends on the time since a vintage of a vehicle was introduced to the market.

We also perform a further set of robustness checks. First, we perform a series of

Table B.2—: Robustness Checks with Quarter-of-Age Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Logs			**Levels**	
$1(Post\ Restatement)_t \times 1(Affected\ Model)_j$	-0.012	-0.012	-0.011	-294	-294	-276
	(0.003)	(0.003)	(0.003)	(91)	(92)	(89)
Year-Month × Class FE	Y	Y	Y	Y	Y	Y
Year-Month × Make FE	Y	Y	Y	Y	Y	Y
VIN10 FE	Y	Y	Y	Y	Y	Y
DMA FE	Y	Y	Y	Y	Y	Y
1(Post Restatement) × DMA FE	Y	Y	Y	Y	Y	Y
Quarter-of-Age FE		Y			Y	
Quarter-of-Age × Make FE			Y			Y
R-squared	0.95	0.95	0.95	0.96	0.96	0.96
N	1.52m	1.52m	1.52m	1.52m	1.52m	1.52m

Notes: Dependent variable is log or level of the transaction price (in dollars). Columns 1 and 4 are our primary specification from Table 2. An observation is a year-month-DMA-VIN10. VIN10 refers to the VIN prefix, which is a trim-engine combination. DMA refers to a Nielsen Designated Market Area, which is an area covering several counties. Class refers to the vehicle class. *Post Restatement* refers to the year-month being during or after November 2012. Quarter-of-age refers to the number of quarters since the introduction of a new VIN10. All estimations are weighted by monthly sales. Standard errors clustered by VIN10.

checks relating to decisions we made in creating our dataset. We see what happens if we do not drop vehicles with transaction prices below $5,000 (3,203 additional vehicles are retained, or 0.02% of observations). We view transaction prices less than $5,000 with suspicion, as they are likely miscoded. We also examine the effect of excluding price outliers by only including vehicle transactions within a price ratio around the mean price for that model-trim over the whole sample period between 0.67 and 1.5. Finally, we restrict the sample to include Hyundais and Kias only, allowing us to focus only on variation between affected and non-affected models for these two automakers. In Table B.3 we see some minor differences, but by-and-large, we find that our results are robust across these specifications.

We also run all of the primary specifications after excluding affected models where the change in the rated fuel economy is minimal (defined as only changes in city and/or highway ratings, but no change in the combined rating). One might be concerned that these skew our results. Table B.4 excludes these minimally treated models from the sample. Again, the results are remarkably similar.

One may also be interested in automaker heterogeneity. Table B.5 examines the heterogeneous treatment effect on transaction prices by automaker. The point estimates suggest a slightly larger effect for Hyundai than Kia, but the difference in the effect between the two is not statistically significant.

In Table B.6, we examine heterogeneous effects on transaction prices by vehicle class. We observe a larger effect for large cars than small cars. For vehicles in the crossover and sport classes, the effect is not statistically significant. Our take-away from this is that large cars and small cars are the dominant force behind the equilibrium price change, which could correspond to consumers interested in these car classes being sensitive to fuel-economy information.

Table B.3—: Further Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
		Logs			**Levels**	
$1(Post\ Restatement)_t \times 1(Affected\ Model)_j$	-0.016	-0.010	-0.011	-295	-279	-336
	(0.005)	(0.003)	(0.004)	(92)	(89)	(81)
Year-Month × Class FE	Y	Y	Y	Y	Y	Y
Year-Month × Make FE	Y	Y	Y	Y	Y	Y
VIN10 FE	Y	Y	Y	Y	Y	Y
DMA FE	Y	Y	Y	Y	Y	Y
1(Post Restatement) × DMA FE	Y	Y	Y	Y	Y	Y
Include prices <= $5,000	Y			Y		
Exclude price outliers		Y			Y	
Hyundais and Kias only			Y			Y
R-squared	0.86	0.98	0.92	0.96	0.98	0.93
N	1.52m	1.48m	0.14m	1.52m	1.48m	0.14m

Notes: Dependent variable is log or level of the transaction price (in dollars). The "exclude price outliers" specification excludes outliers less than 67% of the mean price and greater than 150% of the mean price. An observation is a year-month-DMA-VIN10. VIN10 refers to the VIN prefix, which is a trim-engine combination. DMA refers to a Nielsen Designated Market Area, which is an area covering several counties. Class refers to the vehicle class. *Post Restatement* refers to the year-month being during or after November 2012. All estimations are weighted by monthly sales. Standard errors clustered by VIN10.

Table B.4—: Robustness Check Excluding Minimally Treated Observations

	(1)	(2)	(3)	(4)	(5)	(6)
		Logs			**Levels**	
$1(Post\ Restatement)_t \times 1(Affected\ Model)_j$	-0.010	-0.010	-0.011	-147	-253	-286
	(0.004)	(0.004)	(0.004)	(84)	(97)	(94)
Year-Month × Class FE		Y	Y		Y	Y
Year-Month × Make FE	Y	Y	Y	Y	Y	Y
VIN10 FE	Y	Y	Y	Y	Y	Y
DMA FE	Y		Y	Y		Y
1(Post Restatement) × DMA FE	Y		Y	Y		Y
R-squared	0.95	0.91	0.95	0.96	0.95	0.96
N	1.51m	1.51m	1.51m	1.51m	1.51m	1.51m

Notes: Dependent variable is log or level of the transaction price (in dollars). An observation is a year-month-DMA-VIN10. VIN10 refers to the VIN prefix, which is a trim-engine combination. DMA refers to a Nielsen Designated Market Area, which is an area covering several counties. Class refers to the vehicle class. *Post Restatement* refers to the year-month being during or after November 2012. All estimations are weighted by monthly sales. Standard errors clustered by VIN10.

Table B.5—: Heterogeneous Effects on Transaction Prices by Automaker

	Primary		**Automaker**	
	(1)	(2)	(3)	(4)
	Logs	Levels	Logs	Levels
$1(\textit{Post Restatement})_t \times 1(\textit{Affected Model})_j$	-0.012	-294		
	(0.004)	(91)		
$1(\textit{Post Restatement})_t \times 1(\textit{Hyundai Affected Model})_j$			-0.014	-365
			(0.005)	(123)
$1(\textit{Post Restatement})_t \times 1(\textit{Kia Affected Model})_j$			-0.010	-212
			(0.004)	(114)
Year-Month × Class FE	Y	Y	Y	Y
Year-Month × Make FE	Y	Y	Y	Y
VIN10 FE	Y	Y	Y	Y
DMA FE	Y	Y	Y	Y
1(Post Restatement) × DMA FE	Y	Y	Y	Y
R-squared	0.95	0.96	0.95	0.96
N	1.52m	1.52m	1.52m	1.52m

Notes: Dependent variable is log or level of the transaction price (in dollars). An observation is a year-month-DMA-VIN10. VIN10 refers to the VIN prefix, which is a trim-engine combination. DMA refers to a Nielsen Designated Market Area, which is an area covering several counties. Class refers to the vehicle class. *Post Restatement* refers to the year-month being during or after November 2012. All estimations are weighted by monthly sales. Standard errors clustered by VIN10.

Table B.6—: Heterogeneous Effects on Transaction Prices by Vehicle Class

	(1) **Logs**	(2) **Levels**
$1(Post\ Restatement)_t \times 1(Small\ Car\ Affected\ Model)_j$	-0.013	-320
	(0.005)	(123)
$1(Post\ Restatement)_t \times 1(Large\ Car\ Affected\ Model)_j$	-0.025	-702
	(0.004)	(134)
$1(Post\ Restatement)_t \times 1(Crossover\ Affected\ Model)_j$	-0.007	-190
	(0.004)	(98)
$1(Post\ Restatement)_t \times 1(Sport\ Affected\ Model)_j$	-0.002	239
	(0.005)	(220)
Year-Month × Class FE	Y	Y
Year-Month × Make FE	Y	Y
VIN10 FE	Y	Y
DMA FE	Y	Y
1(Post Restatement) × DMA FE	Y	Y
R-squared	0.95	0.96
N	1.52m	1.52m

Notes: Dependent variable is log or level of the transaction price (in dollars). An observation is a year-month-DMA-VIN10. VIN10 refers to the VIN prefix, which is a trim-engine combination. DMA refers to a Nielsen Designated Market Area, which is an area covering several counties. Class refers to the vehicle class. *Post Restatement* refers to the year-month being during or after November 2012. All estimations are weighted by monthly sales. Standard errors clustered by VIN10.

Appendix C. Effect of Restatement on Other Outcomes

C1. Effect on Quantities

In this appendix, we estimate several models exploring the effect of the restatement on sales. Such estimations are likely to provide little useful evidence, since automobile sales are very noisy. For example, model-trims have highly variable temporal phase-in and phase-out patterns and there are niche model-trims that are rarely sold, leading to large month-on-month relative changes in sales.

Table C.1 confirms our intuition that automobile sales are very noisy. In Panel A, we estimate a model aggregated at the VIN10-DMA-year-month level and regress the sales of each model on $1(Post\ Restatement)_t \times 1(Affected\ Model)_j$ and the same set of fixed effects that we include in the price regressions in Tables 2-6, which are year-month by class fixed effects, year-month by make fixed effects, VIN10 fixed effects, DMA fixed effects, and post restatement by DMA fixed effects. This rich set of fixed effects in combination with noisy data may make it difficult to detect a change relative to the various time trends, but such trends are necessary nevertheless for our identification strategy.[4] As explained above, we need to address model years sold during months in which they are being phased in or phased out (and thus showing large percentage changes in sales) or the possibility of niche models that are rarely sold unduly affecting our results. Accordingly, we focus on the specifications in columns 2 through 5, which present the results where we exclude observations if the monthly sales are less than some percentage of average monthly sales for a particular model-trim. In column 2, that percentage is 25% of monthly sales, in column 3 it is 30%, in column 4 it is 40%, and in column 5 it is 50%. Column 1 includes all the outlier phase-in and phase-out months and is therefore not a suitable specification. Panels B and C show the estimates from regressions with fewer fixed effects, analogous to columns 1 and 2 in Table 2. The point estimates are very similar.

The coefficients in Table C.1 are all *positive*, suggesting that the restatement *increased* sales, which may appear to be a counter-intuitive result. However, they are all imprecisely estimated (results from column 1 are borderline significant at the 5% level, but as discussed above, this specification including the outliers is problematic and not suitable). The precision of the estimates improves somewhat as we apply a stricter exclusion criterion; we favor column 5 for that reason. We recognize that the lack of a statistically significant effect (either positive or negative) may be due to a lack of power, although all estimations include over three million observations. Note that the highly variable phase-in and phase-out patterns make estimating an effect on quantities especially challenging when there

[4]When we estimate a less flexible specification (replacing the year-month by make fixed effects with separate year-month and make fixed effects), our estimates change little. The preferred estimate in column 5 of Table C.1 becomes 0.02 with a s.e. of 0.03; even closer to zero than in the more flexible specification.

is not a strong signal in the data.[5] As the estimates in Table C.1 do not allow us to rule out either sizable positive or negative quantity effects, we discuss the implications of negative or positive quantity effects on estimates of the valuation of fuel economy in Section 5.2. We find our conclusions about undervaluation to be robust to a wide range of quantity effects.

Table C.1—: Effect of Restatement on Sales

	(1) Incl. Outliers	(2) <25%	(3) <30%	(4) <40%	(5) <50%
Panel A: Most Flexible Specifications					
$1(\textit{Post Restatement})_t \times 1(\textit{Affected Model})_j$	0.15	0.06	0.05	0.04	0.05
	(0.08)	(0.05)	(0.05)	(0.05)	(0.04)
Year-Month × Class FE	Y	Y	Y	Y	Y
Year-Month × Make FE	Y	Y	Y	Y	Y
VIN10 FE	Y	Y	Y	Y	Y
DMA FE	Y	Y	Y	Y	Y
1(Post Restatement) × DMA FE	Y	Y	Y	Y	Y
Panel B: Without Year-Month × Class FE					
$1(\textit{Post Restatement})_t \times 1(\textit{Affected Model})_j$	0.19	0.07	0.06	0.05	0.06
	(0.08)	(0.05)	(0.05)	(0.04)	(0.04)
Panel C: Without DMA FE and 1(Post Restatement) × DMA FE					
$1(\textit{Post Restatement})_t \times 1(\textit{Affected Model})_j$	0.11	0.06	0.05	0.04	0.05
	(0.06)	(0.04)	(0.04)	(0.04)	(0.04)
R-squared	0.22-0.46	0.23-0.51	0.24-0.53	0.24-0.53	0.24-0.53
N	4.01m	3.75m	3.70m	3.62m	3.53m

Notes: Dependent variable is log of sales. Panels A-C correspond to the identical fixed-effect structure as in Table 2. Columns 2-5 present the results eliminating outliers by excluding observations if the monthly sales are less than some percentage of average sales, as given in the heading. An observation is a year-month-DMA-VIN10. VIN10 refers to the VIN prefix, which is a trim-engine combination. DMA refers to a Nielsen designated market area, which is an area covering several counties. Class refers to the vehicle class. *Post Restatement* refers to the year-month being during or after November 2012. The R-squared row shows the range across the three panels. Standard errors clustered by VIN10.

C2. Effect on Advertising

In this subsection, we examine adjustments in advertising by the two affected automakers. For example, the automakers could have increased advertising expenditures to make up for the bad publicity. To examine this, we use data from Kantar Media on advertising expenditures by automaker (Kantar Media, 2006-2015). In the two figures below, we find no evidence of changes in either advertising expenditures or the number of advertisements by Hyundai and Kia after the restatement. We have also run simple regressions and find no statistically significant effects, with the point estimate quite close to zero. We thus conclude that the quantity of advertising did not change after the restatement.

[5]We also ran a robustness check to test if sales of all Hyundai and Kia models could have decreased as a result of the restatement. To assess this, we ran the specification for Figure 2 in the main text—but now with sales instead of price as the dependent variable. The treatment variable is $1(\textit{Post Restatement})_t \times 1(\textit{Hyundai or Kia})_j$. The estimate is 0.01 with a s.e. of 0.02, which does not suggest much of an overall effect on Hyundai and Kia sales post-restatement.

Of course, Hyundai and Kia are required by law to update any advertisement that specifies the fuel economy of the vehicle, so the content of advertisements must change at least somewhat. This is analogous to the change in advertising around fuel economy during gasoline price shocks, underscoring that our estimated effect is an equilibrium effect in the same way that the rest of the literature is estimating an equilibrium effect.

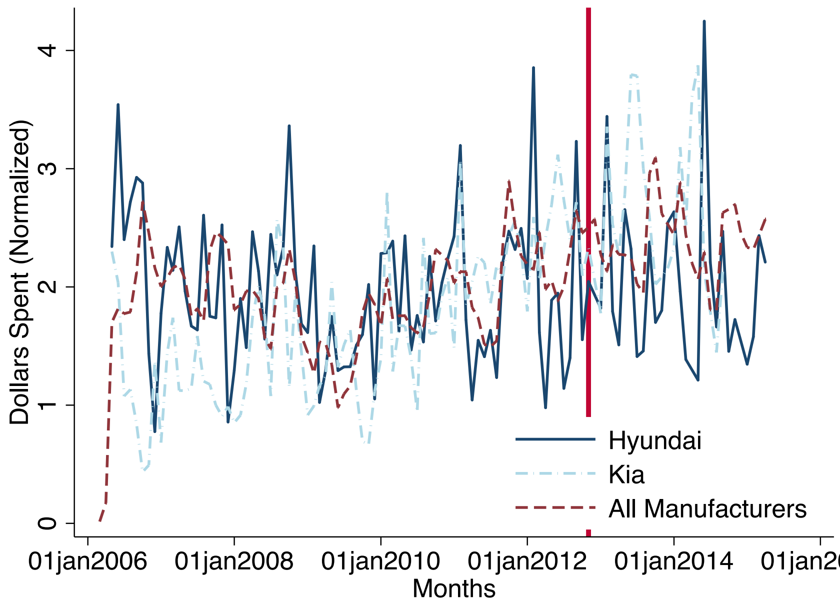

Figure C.1. : Spending on Advertising by Different Automakers

Notes: The red line is the date of the restatement.

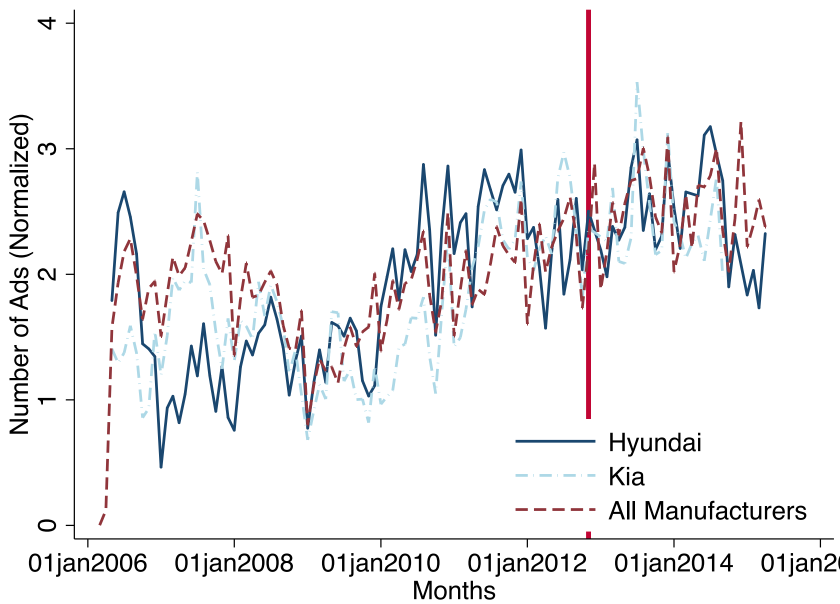

Figure C.2. : The Number of Advertisements by Different Automakers

Notes: The red line is the date of the restatement.

Appendix D. Further Details on the Valuation Calculations

D1. Motivation from a Discrete Choice Model

This subsection motivates Equation (2) from a discrete choice model. For this, we closely follow Allcott and Wozny (2014). The starting point is a random utility model, where the alternative-specific indirect utility of product j at time t, U_{jt}, is a linear function of income (Y), the purchase price (P_{jt}), discounted fuel operating costs (G_{jt}), other controls (X_{jt}), and unobservables ($\tilde{\xi}_{jt}$):

$$U_{jt} = \delta(Y - P_{jt} - \eta G_{jt}) + X_{jt}\beta + \tilde{\xi}_{jt}.$$

With the assumption of an i.i.d Type I extreme value error $\xi_{jt} \equiv \tilde{\xi}_{jt} + \delta Y$, we have a multinomial logit specification, implying that

$$s_{jt} = \frac{e^{U_{jt}}}{\sum_k e^{U_{kt}}},$$

where s_{jt} is the average probability of purchase of the representative consumer, or the market share. Further, under this assumption of the errors, we have the standard identity:

$$log(s_{jt}) - log(s_{0t}) = -\delta P_{jt} - \theta G_{jt} + X_{jt}\beta + \xi_{jt},$$

where we define $\theta \equiv \delta\eta$. Then in this framework, the definition of the valuation parameter is the ratio θ/δ. This parameter quantifies the tradeoff between how consumers value an extra dollar spent on the upfront purchase price (through δ) and a dollar spent on expected future fuel costs (through θ).

To directly estimate this valuation parameter, Allcott and Wozny (2014) invert the market share equation as follows:

$$P_{jt} = \gamma G_{jt} + X_{jt}\tilde{\beta} + \epsilon_{jt}, \tag{D1}$$

where $\gamma \equiv -\theta/\delta$ is the quantity of interest and the structural error term is $\epsilon_{jt} = \frac{1}{\delta}(log(s_{0t}) - log(s_{jt}) + \xi_{jt})$. Similarly, define $\tilde{\beta} \equiv \frac{1}{\delta}\beta$. Note that X_{jt} here contains various controls required for identification, including a variety of fixed effects. In our context, we include year-month by class fixed effects ($\rho_{t\times Class_j}$), year-month by make fixed effects ($\mu_{t\times Make_j}$), region fixed effects (η_r), and their interaction with an indicator for the post restatement period ($\eta_r \times 1(Post\ Restatement)_t$), and VIN10 fixed effects (ω_j). With these fixed effects included, Equation (D1) is effectively the same as Equation (2).

Interpreting the estimate of γ as an estimate of the valuation of fuel economy requires that the structural error term is not correlated with the regressors. As defined above, ϵ_{jt} includes the market share at time t for product j. In our setting the identification of γ thus requires that the contemporaneous market shares for each product j should not be correlated with the change in discounted fuel costs induced by the restatement. This is true when supply is completely inelastic and fixed. If supply cannot change, then the market shares are exogenous. (Note that, if supply responses do occur, we need to adjust the estimate of our valuation parameter. We do this in Section 5.2 and conclude that our finding of undervaluation of fuel economy is robust a wide range of quantity adjustments.)

Another difference between our estimation and the empirical strategies that have recently been used in this literature is that our estimating equation, unlike Equation (D1), does not use the level of discounted fuel costs as a regressor (captured by the variable G), but the *difference* in G induced by the restatement. We define our estimating equation as:

$$P_{jt} = \gamma \Delta G_{jt} + X_{jt}\tilde{\beta} + \epsilon_{jt}, \tag{D2}$$

to put the emphasis that we exploit variation in G induced by the restatement. The variable $DeltaG_{jt}$ corresponds to the change in fuel operating costs for affected vehicles only. It is thus equal to zero for all non-affected vehicles and it is also equal to zero in the pre-restatement period for affected vehicles. Because the restatements were known and salient, measurement error in ΔG is not a major concern in our setting. This is in contrast to Allcott and Wozny (2014) and Sallee, West and Fan (2016), whose empirical strategy essentially requires constructing the average G that each consumer faces, which will be a noisy estimate of its true value. Allcott and Wozny (2014) address the issue using an instrumental variables strategy. Despite not explicitly including an estimate of G in their estimation, Busse, Knittel and Zettelmeyer (2013)'s empirical strategy is also prone to measurement error due to the fact that they must impute the average gasoline price that each consumer faces. They show that this issue is not important in their setting by using different levels of aggregation in average gasoline prices.

Our natural experiment and approach allow us to circumvent the measurement error issue to a certain extent by focusing on estimating the behavioral response to a change in G induced by the restatement and publicized by the EPA, which is perfectly observed. Note that the size of the change in G that each consumer faced is, of course, a function of the gasoline prices consumers paid, driving behavior, and other assumptions required to construct G. We show, however, that our estimates of the valuation parameter are robust to these assumptions (Table D.1).

We also use ΔG rather than G, as in Allcott and Wozny (2014) (and incidentally Sallee, West and Fan, 2016) because in our setting it is important that we limit the variation in G coming from gas prices. The validity of the estimating equation in Equation (D2) requires minimal quantity adjustments due to the fact that it is an inverted market share equation and sales are in the error term. Allcott and Wozny (2014)s exclusion restriction is that gas prices (and thus the level of G) are not correlated with sales in the used car market. As Allcott and Wozny

(2014) and Busse, Knittel and Zettelmeyer (2013) pointed out, this exclusion restriction is unlikely to hold in the new car market given that new vehicles sales adjust strongly to variation in gas prices. Our natural experiment is appealing because there was little room for supply-side adjustment to the restatement for MY 2012 (for MY 2013 we use a bounding analysis) in the new car market. But in estimating Equation (D2), we need to exploit variation in G that comes primarily from the restatement rather than gas prices.

D2. Sensitivity Analysis of the Valuation Parameter

To estimate the valuation parameter, we need to construct the discounted change in future fuel costs of each vehicle model in our sample. This requires making assumptions about how consumers discount the future, drive their vehicles, forecast gasoline prices, and how long they expect their vehicles to last. Table D.1 outlines various sensitivity tests we have conducted, data sources, and comparisons with other studies. We find that the discount rate is the variable having the most important effect on valuation. We consider different data sources for gasoline prices. We further consider different scenarios where expected gasoline prices are being held constant in real terms at the levels at the time of purchase. This martingale assumption implies that consumers use today's price as a forecast of future prices for the entire lifetime of their vehicle. We consider the average price at the annual-national level, annual-state level, month-national level, and at the month-national level without seasonal trends. We also consider a scenario where we remove all variation in gasoline prices and use the gasoline price for the years 2012, 2013, 2014, the average of 2012 and 2013, or the average of 2012, 2013 and 2014 as the constant gasoline price that consumers use in their forecasting. Finally, we consider a scenario where consumers are able to make a perfect forecast of future gasoline prices, where we use realized prices up to 2017 and then the Energy Information Administration's forecasted gasoline prices for the other future years. Compared to previous studies, our different scenarios about expectations of gasoline prices broadly cover the range of assumptions that has been used. For instance, Busse, Knittel and Zettelmeyer (2013) and Sallee, West and Fan (2016) both use the martingale assumption. Allcott and Wozny (2014) use the martingale assumption, but also consider a scenario where consumers base their expectations on oil futures.

For vehicles' survival probabilities, we estimate the results separately using data from Jacobsen and van Benthem (2015) (source: R.L. Polk (1993-2009)) and Busse, Knittel and Zettelmeyer (2013), the latter of which were derived from the National Highway Traffic Safety Administration (NHTSA). We also estimate the result using vehicle survival probabilities specific to Hyundai and Kia (which are somewhat higher than for most other brands), using data from R.L. Polk (1993-2009). Data for vehicle miles traveled come from National Highway Traffic Safety Administration (2018). We compare results using NHTSA publications from 2006 and 2018.

Table D.1—: Sensitivity Analysis: Valuation Parameters

Discount Rate	Gasoline Prices	VMT	Survival Probability	Ratio of Means	Valuation Parameter	Valuation Parameter: 2012 Model Year Only
4%	Year-US	NHTSA 18	JvB	No	0.173	0.395
1%	Year-US	NHTSA 18	JvB	No	0.144	0.329
12%	Year-US	NHTSA 18	JvB	No	0.255	0.582
4%	2012-US	NHTSA 18	JvB	No	0.169	0.389
4%	2012-2014-US	NHTSA 18	JvB	No	0.174	0.402
4%	Month-US	NHTSA 18	JvB	No	0.171	0.407
4%	Year-State	NHTSA 18	JvB	No	0.201	0.384
4%	Year-US	NHTSA 06	JvB	No	0.148	0.337
4%	Year-US	NHTSA 18	BKZ	No	0.181	0.412
4%	Year-US	NHTSA 18	Hyundai/Kia	No	0.198	0.453
4%	All	NHTSA 06/18	BKZ/JvB	No	[0.169-0.201]	[0.384-0.417]
4%	Year-US	NHTSA 18	BKZ	Yes	0.438	0.908

Notes: Valuation parameters presented for different assumptions pertaining to the construction of the discounted fuel costs. Different levels of aggregation are considered for gasoline prices. "Year" refers to annual data. "US" refers to national-level data. "State" refers to state-level data. The row with "2012-US" uses the average U.S. nationwide gasoline price for the year 2012: 3.68 USD/gallon. Similarly, the row with "2012-2014-US" uses the average U.S. nationwide gasoline price, where the average is taken over the years: 2012, 2013 and 2014: 3.56 USD/gallon. In those two scenarios, there is no variation in discounted fuel costs induced by gasoline prices. The VMT estimates are based on the data from National Highway Traffic Safety Administration (NHTSA). We use the data from the years 2006 and 2018. For the survival probabilities, we use data from Jacobsen and van Benthem (2015) (JvB; source: R.L. Polk (1993-2009)). We also consider the NHTSA data as reported by Busse, Knittel and Zettelmeyer (2013) (BKZ). For the scenario labelled "Hyundai/Kia," we use the survival probabilities specific to Hyundai and Kia calculated from JvB's data. In the last row, we report the valuation parameters using the approximation that relies on the ratio of the mean change in prices over the mean change in discounted fuel costs. This approximation has a large impact on the valuation parameter and leads to an upward bias. The discount rate and whether we solely rely on the 2012 model years are the two dimensions that induce the most variation in the results. The data source for the VMT, survival probabilities, and the level of aggregation in the gasoline prices have little effects on the results.

D3. Imperfect Competition

Our calculations of the valuation parameter are based on the implicit assumption that the equilibrium prices were set in a competitive market. This assumption was also used in most of the other recent studies in Table 8, including Busse, Knittel and Zettelmeyer (2013). However, the automobile market is traditionally modeled by economists as a market with differentiated products, where automakers can exercise some market power (Berry, Levinsohn and Pakes, 1995). In Panel D of Figure 3, we present the case with market power and upward-sloping supply to provide intuition for how market power may affect our valuation parameter estimate.[6]

In this section, we present a stylized analytical model to provide further intuition for how market power influences the calculation of the willingness-to-pay. For illustrative purposes, we focus on comparing the two extreme cases of perfect competition and monopoly. This provides easily accessible intuition for the broader case of market power in a market with multiple firms; as such a market with imperfect competition would fall in between the two extremes.

Our main finding is that the equilibrium price effect under monopoly is (weakly) greater than the equilibrium price effect under perfect competition. Further, if we have elastic and upward-sloping supply (as in Panel D of Table 8), then the gap between the willingness-to-pay and the equilibrium price change is smaller when the market is a monopoly than under perfect competition. In other words, market power implies that an upward-sloping supply curve would affect our valuation calculations less.

Preliminaries. — Consider the case of (locally) linear demand. This is a reasonable assumption given that we find relatively small price changes. To keep the exposition simple, we also focus on the single-product case where demand is given by:

$$P(Q) = \alpha^0 - \frac{Q}{\delta},$$

where α^0 and $\delta > 0$. We model the effect of the restatement as a reduction in the overall willingness-to-pay for the product by all consumers in the market.[7] Formally, this implies a downward parallel shift in demand:

$$P'(Q) = \alpha^1 - \frac{Q}{\delta},$$

[6] A full model of the strategic pricing response of a firm with multiple closely related products in a multi-product oligopoly is beyond the scope of this paper, but the simple framework presented here nevertheless contains basic intuition for the market power case.

[7] For larger shifts, it is possible that there is a rotation of the demand curve, but the parallel shift assumption is reasonable as a local approximation. It is also supported by our robustness checks suggesting that there is little evidence to support a compositional effect.

where $0 < \alpha^1 < \alpha^0$, and the change in willingness-to-pay for fuel economy equals $\Delta WTP = \alpha^1 - \alpha^0$.

Finally, we assume that supply is elastic and upward-sloping:

$$MC(Q) = \beta + \frac{Q}{\sigma},$$

where $\beta^0 > 0$ and $\sigma > 0$.

Competitive Pricing. — In the competitive case, the equilibrium price, before or after the restatement, is determined by the intersection of demand and supply: $P(Q) = MC(Q)$. Solving for quantities before and after the restatement, the change in equilibrium price ($\Delta P = P' - P$) is:

$$\Delta P = (\alpha^1 - \alpha^0)\frac{\delta}{\sigma + \delta} = \Delta WTP \frac{\delta}{\sigma + \delta}$$

It can also be useful to re-express this expression in terms of demand and supply elasticities. Using the linear case as an approximation of the demand and supply relationships, the demand elasticity is given by $\epsilon^D = -\delta \cdot P/Q$ and the supply elasticity is given by: $\epsilon^S = \sigma \cdot P/Q$. Replacing these two expressions in the expression above, we have:

$$\Delta P = \frac{-\epsilon^D}{\epsilon^S - \epsilon^D}\Delta WTP. \tag{D3}$$

This expression formalizes the intuition in Figure 3. When supply is upward-sloping and elastic, i.e., $\epsilon^S > 0$, and demand is downward-sloping $\epsilon^D < 0$, the change in equilibrium price will always underestimate the change in willingness-to-pay given that $\frac{\epsilon^S - \epsilon^D}{-\epsilon^D} > 1$. When supply is perfectly inelastic, i.e., $\epsilon^S = 0$, the change in equilibrium price is exactly the change in willingness-to-pay: $\Delta P = \Delta WTP$.

Monopoly. — In the case of a monopolist, the equilibrium price is determined by the intersection of the marginal revenue and the marginal cost: $MR(Q) = MC(Q)$. When we again assume (locally) linear demand, the marginal revenue curve is given by:

$$MR(Q) = \alpha - \frac{2Q}{\delta}.$$

Solving for quantities before and after the restatement, the change in equilibrium price is now:

$$\Delta P = \Delta WTP \frac{\delta + \sigma}{2\sigma + \delta},$$

which can be expressed as:

$$\Delta P = \frac{\epsilon^S - \epsilon^D}{2\epsilon^S - \epsilon^D} \Delta WTP. \tag{D4}$$

Comparing the change in equilibrium price under both market structures, the following inequalities can easily be verified: $\frac{-\epsilon^D}{\epsilon^S - \epsilon^D} < \frac{\epsilon^S - \epsilon^D}{2\epsilon^S - \epsilon^D} < 1$ if $\epsilon^S > 0$ and $\epsilon^D < 0$. This implies that under imperfect competition, the change in equilibrium price implied by the restatement will always be larger than under the competitive case, but still it will be less than the full change in willingness-to-pay if supply is elastic ($\epsilon^S > 0$). Put simply, under imperfect competition, the firm has a greater ability to adjust prices to capture the consumer surplus associated with the valuation of fuel economy relative to a perfect competitive setting. Nonetheless, the firm cannot fully capture the surplus, unless supply is completely inelastic.

Bias from Ignoring Imperfect Competition. — If we are in a setting with imperfect competition, but we calculate the change in willingness-to-pay assuming perfect competition (i.e., using the demand and supply parameters), the estimate for ΔWTP is given by re-arranging Equation ((D3)):

$$\Delta WTP^{biased} = \frac{\epsilon^S - \epsilon^D}{-\epsilon^D} \Delta P, \tag{D5}$$

where, with a slight abuse of notation, the superscript *biased* is a mnemonic that indicates that the change in willingness-to-pay is calculated using the wrong assumption about the underlying market structure.

If imperfect competition is at play and we are actually in a monopoly setting, then the true (i.e., unbiased) change in willingness-to-pay, which we denote ΔWTP^*, should be calculated using Equation ((D4)):

$$\Delta WTP^* = \frac{2\epsilon^S - \epsilon^D}{\epsilon^S - \epsilon^D} \Delta P. \tag{D6}$$

The bias from ignoring imperfect competition is simply the difference between the two expressions:

$$Bias = \Delta WTP^* - \Delta WTP^{biased} = \frac{(\epsilon^S)^2}{\epsilon^D(\epsilon^S - \epsilon^D)} \Delta P. \tag{D7}$$

The bias is thus proportional to the size of the change in price and a scaling term that is always less than zero for $\epsilon^S > 0$ and $\epsilon^D < 0$. In our context, given that $\Delta P < 0$, the bias would be positive. This means that by ignoring imperfect competition, we are *overestimating the reduction in willingness-to-pay induced by the restatement.* Note that in this single-product case, the valuation parameter is simply the ratio of the change in willingness-to-pay over the change in expected fuel costs. An upward bias (in absolute value) in calculating ΔWTP thus biases the valuation parameter toward one (and more generally upward). Therefore, this simple illustration shows how ignoring imperfect competition can lead to an overestimate of the willingness-to-pay for fuel economy.

The basic logic here generalizes to other forms of imperfect competition besides monopoly. The key point for our setting is that under imperfect competition, upward-sloping supply is less influential in biasing our valuation parameter based on inelastic demand than in the perfect competition setting.

D4. Bias from the "Ratio of the Means" Approximation

With the setup based on a discrete choice model presented in Section D.D1 above, it is easier to understand the ratio of the means issue referred to in the main text. Before moving to the equations, it is illustrative to begin with a simple example to fix ideas. Suppose that two different vehicle models were subject to a restatement in fuel economy: Model A, which has a price of \$50,000, and Model B, which has a price of \$10,000. Also, suppose that both models are equally popular, so we can ignore their relative market shares in this example. When the unexpected restatement occurs, this changes consumer expectations about the future fuel costs of each of the two vehicles. Suppose the restated EPA fuel-economy ratings correspond to a change in discounted lifetime fuel costs of \$5,000 for Model A and \$1,000 for Model B. We are then interested in how the equilibrium prices and quantities change. Suppose that sales are held constant. And further suppose that the restatement leads to heterogeneous changes in equilibrium prices: \$5,000 for Model A, but only \$100 for Model B.

The valuation parameter implied by this illustrative event is \$5,000/\$5,000 = 1 for Model A and \$100/\$1,000 = 0.1 for Model B. The mean of the valuation ratio is thus the average of 1 and 0.1, which equals 0.55. This is the exact valuation parameter when both models are equally popular. Now consider the approximation, which is the ratio of the mean of the changes in prices over the mean of the changes in future fuel costs: \$2,550/\$3,000 = 0.85. What we see is that the naive approximation puts too much weight on changes in the numerator or denominator that are large in absolute value.

The intuition for the issue should be clear: the ratio of the means is not necessarily the same as the mean of the ratios. Houde and Myers (2019) analyze the appliance energy efficiency context and show the conditions under which we would expect a bias more generally, and what the sign of the bias might look like. The insights from the appliance energy efficiency context carry over to our setting as well. To see the issue mathematically, note that the goal in estimating Equation (D1) is to consistently estimate the true γ. Consider a case where there is heterogeneity over vehicles in γ, so that we can write the parameter as

γ_j.[8] Our simple example above was one case where there was heterogeneity in γ across vehicles, and we showed in Table B.6 that there is heterogeneity across car classes, so we know that empirically there is indeed heterogeneity in γ across vehicles in our context. We are interested in the mean effect, or $E[\gamma_j]$, where the mean is taken over the population of vehicles. However, by definition, this is the mean of a ratio: $E[\gamma_j] = E[\theta_j/\delta_j]$.

To see how this true value (a mean of a ratio) relates to the approximation (a ratio of means), consider the second-order Taylor expansion:

$$E[\theta_j/\delta_j] \approx E[\theta_j]/E[\delta_j] - cov(\delta_j, \theta_j)/E[\delta_j]^2 + Var(\delta_j)E[\theta_j]/E[\delta_j]^3.$$

Thus, the value of interest $E[\gamma_j]$ (the mean of the ratio) is only equal to $E[\theta_j]/E[\delta_j]$ (the ratio of the means) when the covariance and variance terms in the equation are equal to zero (this is a slightly weaker condition than assuming no heterogeneity in γ_j). Our results indicate that there is heterogeneity in γ_j and our calculations showing a difference in the results between the two approaches suggest that the higher order terms in the approximation are important.

Note that several papers in the literature that aim to estimate $E[\gamma_j]$ report a ratio that corresponds to $E[\theta_j]/E[\delta_j]$, as they separately estimate $E[\theta_j]$ and $E[\delta_j]$. This is true for studies that rely on reduced-form methods (Busse, Knittel and Zettelmeyer, 2013; Leard, Linn and Zhou, 2018) and a similar issue could arise using structural methods (Grigolon, Reynaert and Verboven, 2018). A key point is that when there is heterogeneity across the population and a correlation between the response in upfront purchase price and the response in future fuel costs, this correlation will lead the exact measure of undervaluation to deviate from the approximation. If there is a positive correlation between θ and δ (e.g., vehicles for which consumers really do not like a change in upfront purchase price are more likely to be vehicles for which consumers really do not like a change in future fuel costs), then this equation would predict that the approximation would be biased upwards in terms of the valuation.[9]

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[8]Houde and Myers (2019) consider heterogeneity over consumers, so it is γ_i being considered, but the same logic follows as here.

[9]It is an upward bias in the valuation because we subtract off the covariance term, so the coefficient becomes more negative, which means less undervaluation (recall -1 means full valuation, while zero means not valuing future fuel costs at all).

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