

The Effects of New-Vehicle Price Changes on New- and Used-Vehicle Markets and Scrappage: Peer Review and Response to Reviewer Comments

The Effects of New-Vehicle Price Changes on New- and Used-Vehicle Markets and Scrappage: Peer Review and Response to Reviewer Comments

Assessment and Standards Division
Office of Transportation and Air Quality
U.S. Environmental Protection Agency

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NOTICE

This technical report does not necessarily represent final EPA decisions or positions. It is intended to present technical analysis of issues using data that are currently available. The purpose in the release of such reports is to facilitate the exchange of technical information and to inform the public of technical developments.

August 2021

MEMORANDUM

SUBJECT: Peer Review for RTI Report, “The Effects of New-Vehicle Price Changes on New- and Used-Vehicle Markets and Scrappage”

In March 2021, EPA contracted with ICF to conduct a peer review of a study conducted by RTI International. The draft study, titled “The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage,” conducted a literature review and developed a method to examine the effects of changes in the prices of new vehicles on new and used vehicle sales and vehicle scrappage.

The peer reviewers selected by ICF were Drs. Ashley Langer of the University of Arizona, Benjamin Leard of the University of Tennessee, and James Sallee of the University of California at Berkeley. EPA would like to extend its appreciation to all four reviewers for their efforts in evaluating this survey. The reviewers brought useful and distinctive views in response to the charge questions.

The first section of this document contains the final RTI response to the peer reviewers’ comments. The second section provides the peer review report conducted by ICF. It documents the peer review process, provides both a summary of the peer review comments and the detailed responses, the peer reviewers’ curriculum vitae, conflicts of interest declarations, and notes from the peer reviewer mid-review meeting.

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To: Gloria Helfand, U.S. Environmental Protection Agency

From: Mark Jacobsen and Robert Beach

Date: July 30, 2021

Subject: Response to External Peer Review Comments on *The Effects of New-Vehicle Price Changes on New- and Used-Vehicle Markets and Scrappage*

The report “*The Effects of New-Vehicle Price Changes on New- and Used-Vehicle Markets and Scrappage*” was submitted for independent, external peer review in April 2021. The external peer reviewers provided their independent responses to EPA’s charge questions. This memorandum documents our responses to the comments provided by three expert reviewers.

We present the individual reviewer comments (verbatim) in response to the 6 peer review charge questions in the following tables, organized by reviewer, along with our responses. In some cases, reviewers provided comments in addition to their direct responses to the charge questions, each of which are also addressed within this memorandum. We have responded to all the detailed comments individually and indicate how the draft report was revised to response to peer reviewer comments in all cases where revisions were made. The peer review report also included a summary of the review comments, but we did not include separate responses to the summary comments because we responded to each of the detailed comments upon which the summary comments are based.

Peer Review Charge Questions

1. Does the Presentation Describe the Data and Methods Sufficiently?
2. Does the Report Miss Relevant Literature; Are the Interpretations of the Elasticities Defensible?
3. Are the Data and Assumptions Appropriate and Objectively Chosen?
4. Are the Methods and Procedures Employed Technically Appropriate and Reasonable?
5. Does the Modeling Analysis Appear to Produce Results Consistent with the Assumptions and Data?
6. Are the Results Sensitive to the Data and Assumptions Used in Model Development; Are There Alternative Assumptions?

Expert Peer Reviewers

An EPA contractor identified and selected three reviewers who met the technical selection criteria provided by EPA and who had no conflict of interest in performing this review:

Ashley Langer, Ph.D.

Assistant Professor, Department of Economics, University of Arizona

Faculty Research Fellow of the National Bureau of Economic Research

Benjamin Leard, Ph.D.

Assistant Professor, Department of Agricultural and Resource Economics, University of Tennessee
Faculty Fellow, Howard Baker, Jr. Center for Public Policy, University of Tennessee

James Sallee, Ph.D.

Associate Professor, Department of Agricultural and Resource Economics, University of California, Berkeley

We thank the reviewers for their very detailed and conscientious expert review of our report. Their queries and recommendations have enabled us to improve the report by correcting oversights, adding new material, and improving clarity.

Comments by Dr. Ashley Langer

CHARGE QUESTION	COMMENTS	RESPONSE
1. Does the presentation describe the data and methods sufficiently to allow the reader to form a general view of the quality and validity of the analysis approach?	Yes. This is the best government report I've ever read. The model is laid out clearly and is in line with the state-of-the-art research in the best economics journals. The authors are extremely clear about where each parameter is coming from and why they made the modeling assumptions that they did. I feel like after reading this report I could, with relatively little additional thought, actually code up the dynamic simulation in the paper and test alternative models. That is a feat in a technical report like this.	We appreciate this positive feedback on the clarity of the report.
2. Does the report miss relevant literature in its review? Are the interpretations of the elasticities in the literature review, and the estimates of elasticities used in the model stemming from	I think that the authors overall have gone well beyond just conducting a literature review. They have calculated elasticities using important estimates from the literature and in some cases have literally gone back to the data and code from the papers and calculated elasticities that are not recoverable from only the published version. They have also used good judgement to choose which elasticities from the literature are	Thank you for the suggestion of additional papers to incorporate in the discussion. We focused specifically on U.S. papers in our assessment of the elasticities available from the existing literature because we felt those were most relevant for characterization of the U.S. vehicle market. However, we agree it makes sense to incorporate the suggested literature to help place our model in the context of dynamic vehicle

the literature review, defensible?	more likely to be applicable to their model and give the reader solid reasoning for why they made the choices they did. That said, I do think that there are a couple of papers that should probably be at least discussed. Schiraldi (2011) and Adda and Cooper (2000) both model dynamic adoption of new and used cars. They aren't based in the U.S., but they may be the closest models to the ones the authors are after here. Putting their model in the context of the dynamic car purchasing literature seems important.	purchase literature. We added footnote 2 referring to and citing these papers.
3. Are the data and assumptions appropriate for the analysis conducted and objectively chosen? If not, do you know of other data or proposed alternative assumptions that might be used in this analysis?	I think that the data and assumptions of the model are appropriate and reasonable. The authors have provided extensive sensitivity testing and their take-aways from the model appear to be extremely generalizable.	We appreciate this positive feedback on the data and assumptions used for this study.
4. Are the methods and procedures employed technically appropriate and reasonable? Please distinguish between cases involving reasonable disagreement in methods as opposed to cases where you conclude that current methods involve specific technical errors.	The methods and procedures are technically appropriate and reasonable. In particular, because the authors are not attempting to defend a specific point estimate, but are more attempting to lay out intuition for how policy affects equilibrium new and used vehicle adoption and pricing, I don't have any real critiques of the approach. The authors are clear about the assumptions of the model and the ways that these assumptions could be relaxed in future work.	We appreciate this positive feedback on the methods and procedures used for this study.
5. Does the modeling analysis appear to	The results are consistent with the assumptions and data used in model	We appreciate this positive feedback on the modeling analysis.

produce results consistent with the assumptions and data used for model development? Do the results presented by RTI follow from the data and assumptions used in the analysis?	development. Beyond following from the data, the results provide intuition for how the results would change with alternative modeling assumptions and the authors are very clear about which assumptions are most critical for changing the results.	
6. In what ways are the results sensitive to the data and assumptions used in model development? Are there alternative assumptions and data that the researchers should consider providing improved analysis?	The authors are very careful to keep the analysis to the adoption and pricing of a single representative vehicle of each age. I do worry slightly that others who read the report may want to use these results to draw conclusions about the effect of things like fuel economy policy on the overall fuel economy of the vehicle fleet over time. I think it might make sense for the authors to add some discussion of the complications that within-vintage vehicle heterogeneity is likely to add in the real world. In particular, a policy like a CAFE standard, which penalizes fuel inefficient vehicles while subsidizing fuel efficient vehicles, will have different long-run effects on the age distribution of fuel inefficient and efficient vehicles. I see this not as something that the authors should do to improve this analysis: adding heterogeneity is very complicated as the authors point out. But I do think that it would be helpful for the authors to be fairly clear about the limits of the analysis they have conducted by explicitly saying that policies that do not uniformly affect the new vehicle fleet will have complicated effects on the long-run age distribution of vehicles.	We agree with the reviewer's suggestion to add more discussion of the potential implications of distributional effects beyond the single representative vehicle used for each vintage in the report. We added discussion of these points and related caveats associated with the findings implied by the current analysis in paragraph 2 of Section 6.2 and in the new Section 10.4.
ADDITIONAL OVERALL COMMENTS PROVIDED (NOT CHARGE QUESTION-SPECIFIC):		

Overall, this is the best report of this type I have ever read. The analysis provides both intuition and a concrete path forward for future analysis. The model rests on reasonable assumptions and is very clear about how things might change under alternative assumptions. The calibration is reasonable and makes choices about which values from the literature to use that are based on the quality of the studies rather than weighting all previous work equally. The authors conduct extensive sensitivity analysis that allows the reader to fully understand which results are robust to alternative assumptions and which depend on the parameterization. Congratulations on great work!	We appreciate this positive feedback on the analysis, model, and report.
ADDITIONAL COMMENTS BY SPECIFIC REPORT CHAPTER:	
<i>Dr. Langer also commented on specific sections of text throughout the report:</i> 3.1, Bullet 4 (“Production of vehicles...”), which discusses long-run changes in vehicle models: Cite Knittel and coauthors here on long-run model changes? Was that paper ever published?	We thank the reviewer for reminding us of this work. We added the citation to Blonigen, Knittel, and Soderbery (2017) on long-run competition and entry.
3.1.1.1, Paragraph 3 (“In the literature estimating...”), which discusses new-vehicle demand systems and the distinction between purchase price and net ownership costs: This paragraph was helpful for explaining what you are trying to get at.	We appreciate the positive feedback on clarity of this text.
3.1.1.2, Paragraph 2 (“Much of the literature...”), which discuss the distinction between average own-price elasticity among individual models of new vehicles and aggregate own-price elasticity: But it’s also important to recognize that the identifying variation behind these estimates may not be well set up to get at these aggregate elasticities.	We added discussion of this caveat on p. 3-4.
3.1.1.3, Paragraph 1, in reference to the sentence: “The derivatives of demand here reflect a world where (at least from the consumer’s perspective) only the price of the vehicle has increased:” Single vehicle?	We clarified this reference on p. 3-4. The point was that they reflected only changes in price, not vehicle quality.
3.1.1.3, Paragraph 2 (“More often...”), which discusses derivatives of demand and consumer willingness to pay for vehicle attributes/regulations that change vehicle attributes: It’s probably worth being clear here that these derivatives are at least theoretically possible to get from the models, if the models include the right vehicle attributes.	We added clarification to this point starting at the top of p. 3-5.

3.1.1.3, Paragraph 3 (starting at “A consumer might...”), which discusses the effects of fuel economy regulations on consumer demand and explains that consumers may perceive fuel economy-related gas savings differently. This doesn’t need to be phrased as heterogeneity in beliefs. It could just be that some consumers drive more and others drive less so increases in fuel economy have different effects for different consumers.	We expanded on our discussion to incorporate the reviewer’s point about differences in driving patterns on p. 3-5.
3.1.1.3, Paragraph 4, in reference to the sentence: “We will refer to the combined change in perceived cost at the time of purchase as a ‘generalized cost:’” Shouldn’t this be something like the compensating variation? Is it really “perceived cost”? and it’s clearly heterogeneous, so it’s likely the mean change in perceived cost?	We expanded on our discussion on p.3-5 to clarify the meaning of “generalized cost” as used in this report.
3.1.2, Paragraph 1, in reference to the sentence: “If assuming perfect competition, supply will in fact be perfectly elastic:” But we wouldn’t do this in the car industry.	We expanded on our discussion of supply in the U.S. vehicle market on p. 3-6.
3.1.2, Paragraph 1, in reference to the sentence: “However, the presence of a used market interacts with the equilibrium system and the new-vehicle demand elasticity is no longer sufficient to measure the effect of a policy, even when new vehicle supply is perfectly elastic:” Related to the above, I would be stronger here that supply isn’t perfectly elastic, even in the long run.	We expanded on our discussion of supply in the U.S. vehicle market to incorporate the reviewer’s point about the vehicle industry not being perfectly competitive on p. 3-6.
3.1.2, Paragraph 2, in reference to the sentence: “When a particular used model’s price rises, more of that model become available (e.g., because scrap dealers, insurance companies, and mechanics decide to repair and sell more of them as vehicles instead of as scrap metal):” Owners don’t also respond? Trade in vehicles rather than keeping them as an extra car for a child or relative?	We added a reference to potential owner response on p. 3-6.
3.1.2, Paragraph 3, which discusses scrappage functions. In response to the phrase “vehicle scrappage depends only on own price of vehicles:” It should depend on the price of other vehicles to the extent that parts for repairs are interchangeable and supply curves for those parts slope up.	We added a footnote identifying the possibility that scrappage could depend on the prices of other vehicles on p. 3-6, though we expect this effect to be small and maintain this assumption.
3.1.2, Paragraph 4, in reference to the sentence: “Empirical estimates will typically be presented as a derivative or elasticity of this function, rather than of the density of underlying shocks that determines the function:” Derivative of the density, no?	We clarified to refer to the derivative of the density on p. 3-7.

3.1.3, Paragraph 1, which discusses how the presence of equilibrium effects in observational data may lead to biased estimates:” Would this be clearer if it were framed as omitted variables bias? So there are omitted variables that are correlated with both the likelihood of scrappage and the cost of repair?	Equilibrium effects can create bias in a number of ways, so we kept the more general text. However, we agree omitted variables are certainly one useful way to view the source of bias and have added footnote 9 in Section 3.1.3 to this effect.
3.1.3, Paragraph 3, which discusses the effects of new-vehicle prices on scrap rates of used vehicles, in reference to the phrase: “a downward shock to the scrap rate will create more competition for new versions of a vehicle (because there are now more used ones entering the market):” Remaining in the market, not entering the market.	We corrected the text on p. 3-8.
3.1.3, Paragraph 3, which discusses the effects of new-vehicle prices on scrap rates of used vehicles, in reference to the phrase: “Estimating the reduced form successfully would require good quasi-experimental variation in new-vehicle prices, but the likely confounders and long time series required have meant that the literature has been unable to find many suitable settings:” And is this even the right elasticity? As the authors say above, this is only for a price change for a single vehicle, and the cross-price elasticities are likely to matter because used car prices are determined in equilibrium.	We added text on p. 3-8 to incorporate the reviewer’s point.
3.2.1, Paragraph 3, in reference to the question: “How do the effects on the composition of the vehicle inventory relate to the initial policy goals?” It seems like somewhere here there also needs to be recognition that it’s not just one “substitution” effect to used cars but that it will depend a lot on which vehicles the new car buyers are substituting toward, and how substitution is happening within the used car market (so 20 year old vehicles may still be scrapped at high rates, but there may be much less scrappage of 10 year old vehicles and this changes the policy impacts).	We have added additional questions to our list starting at the bottom of p. 3-9 in response to the reviewer’s comment and a footnote referencing our assumption that there are differences in substitutability between vehicles that are farther apart in age.
3.2.1, Paragraph 3, in reference to the question: “For the group of consumers that substitutes from new to used, how much does this substitution drive up equilibrium used-vehicle prices?” And WHICH used vehicle prices?	We expanded on this point about differing impacts on different vintage used vehicles in the text.
3.2.2, Paragraph 6, which starts with “A dynamic model...” in reference to the sentence “Assuming the policy shock is long-lived the effects on used-vehicle prices and scrappage evolve, likely strengthening, over time.” Why? Not sure it’s not true, but not completely obvious why it is. Couldn’t producers evolve/adapt to weaken the effects?	We have added text on the mechanical nature of the strengthening: shortages in the used market become more severe with each new vintage directly affected by the policy. However, as pointed out by the reviewer, the size of the direct

	effects could also diminish if technology allows easier compliance over time. This is also of potential dynamic interest, and we added it in footnote 13.
4.1, Paragraph 3, in reference to the sentence “The fact that the elasticities differ in this way is entirely consistent with economic theory; more disaggregated choice sets (in terms of attributes, time of purchase, etc.) mean that the best substitutes for any given good within the choice set will be more similar to it (e.g., more substitution would be expected between sedan models or between makes of sedan than between new and used vehicles or between a personal passenger vehicle and alternative means of transportation).” This is not exactly how I would think about the issue. Substitution happens for many reasons. In particular, since people are heterogeneous, having more products means that there are products that are similar to a vehicle on different dimensions, which means that more heterogeneous people can be on the margin between buying this vehicle and another.	We added a sentence to reflect this point on p. 4-2.
4.2, Table 4-2: List of Papers Included and Parameter Estimates Provided. Busse, Knittel, and Zettlemeyer doesn’t have any relevant elasticities? I guess everything that they’re looking at is responses to gas prices rather than responses to price changes, but isn’t that central to their generalized price measure?	We were not able to calculate the relevant elasticities we were focused on for this review. As the reviewer notes, they focused on responses to changes in gas prices and did not provide enough information to estimate demand elasticities in response to changes in vehicle prices.
5.1, in reference to this sentence about Table 5-1: “The elasticities in the first panel hold constant most other aspects of the equilibrium system, including substitution possibilities to used vehicles.” What does this mean, exactly? Are you just saying that the attributes of used vehicles are held constant? Not sure what a substitution “possibility” means.	Clarified in the text on p. 5-2 that we were referring specifically to their holding used-vehicle prices constant.
5.1.1, which discusses aggregate own-price elasticity of demand for new vehicles with respect to the price of new vehicles: I feel like somewhere in here there needs to be more discussion of the fact that this is not necessarily all substitution to used vehicles but also includes substitution to fewer total vehicles (either not traveling at all, using a used vehicle more, or switching to bike/transit/etc.). But maybe I missed that elsewhere?	We do have some discussion of substitution to the outside good elsewhere in the text but added a sentence making this point at the beginning of Section 5.1 on p. 5-2.
5.1.1, Paragraph 1, in reference to the sentence: “In addition, we made a simple assumption regarding average model-level	Yes, the reviewer is correct. We clarified in the text on p. 5-3 that we

elasticities and average substitution to the outside good.” This is a little confusing, but I think is saying that everything should technically be share-weighted averages instead of simple averages, right?	would ideally calculate share-weighted rather than simple averages.
5.1.1, Paragraph 4, which starts with “Barry, Levinson...” noted that it should say “(2004)” after Pakes in the last sentence.	We added the year to this reference on p. 5-4.
5.1.3, Paragraph 3, in reference to the first sentence: “As expected given the relationship between the new- and used-vehicle markets, the available elasticities of new-vehicle demand when used-vehicle prices can adjust (-0.18 , -0.36) indicate less responsiveness.” What timeframe should we think of these elasticities as being over? In BLP, it’s very clearly a year. But here are they longer-term? Presumably not super long-term though?	We added text indicating that these elasticities are representative of the medium term, allowing for new and used markets to adjust to a new equilibrium.
5.2.2, Paragraph 1, in reference to a sentence about estimates of own-price elasticity for used vehicles, “Their estimate of own-price elasticity for used vehicles ranged from -0.75 to -1.93 with a mean of -1.23 .” Aggregate own price elasticity?	We added a sentence on p. 5-8 clarifying that this estimate is not directly comparable to the aggregate elasticities for new vehicles because it is an average of model-level values rather than an aggregate elasticity.
5.2.2, Paragraph 2, in reference to the sentence “Bento et al. (2009) also reported model-level price elasticities among used vehicles: the average vehicle-level elasticity among all used vehicles was -0.54 .” I guess I’m a bit confused about what a used vehicle own-price elasticity is given that there are consumers on both the supply and demand sides. Given that there are lots of substitutes I would expect an own-price elasticity of a used vehicle to be well above 1 in magnitude. But if the seller is basically going to sell the car one way or another regardless, then does that pull down the elasticity? So if prices go up, the supply of used vehicles goes up and the demand goes down so the number of vehicles TRANSACTED doesn’t change? Is there a way to provide some clarity on this?	We thank the reviewer for raising this point. In our representative agent model, we are considering total demand (irrespective of how many transactions would be needed to reshuffle to a particular level of demand). We have clarified this point on p. 5-8.
5.2.4, but seems to be in reference to section 5.2 as a whole: Does Schiraldi provide an estimate that is useful? Adda and Cooper? They’re not on US data, but it still seems like they’re informative.	We incorporated only U.S. data in this study as most representative of market conditions in the U.S., so we did not use non-U.S. estimates. In addition, these papers do not present estimates of our elasticities of interest.
5.3.1, which discusses the elasticity of aggregate scrappage with respect to the average price of used vehicles, in reference	Moved sentence referenced in the comment below into the first

to the sentence: “In Hahn’s (1995) Table 2, he relates an exogenous “bounty” (subsidy to scrappage) to changes in the absolute number of vehicles scrapped relative to a baseline.” Timeframe seems important to mention here. Short-run, I assume?	paragraph on Section 5.3 on p. 5-9 noting that the studies using bounty programs are focused on short-run responses, which may differ substantially from the long-run response. Also mentioned that Hahn is capturing short-run response on p. 5-10.
5.3.1, Paragraph 3, in reference to the sentence: An important caveat of studies looking at a bounty for scrappage lies in the temporary nature of the program: it may be that a short-lived, salient subsidy will quickly harvest a stock of “almost-scrapped” vehicles, overstating the elasticity. Yes, maybe just move this up front in the discussion?	Moved sentence into the first paragraph of Section 5.3 on p. 5-9 to make this point sooner and clarify the implications of studying scrappage through the simulation of bounty programs.
5.4, Table 5-4. “Time Frame of Data Used” is hard to read.	This text looks the same as for the other tables in our version, but we will check readability in the final report.
Section 6 (overarching statement about the section): I get that this is sort of an obnoxious comment, but I could have used more of a road-map up front on this section. For instance, it wasn’t completely clear up front that the price vector is sort of secondary and just guarantees equilibrium in the system of equations. It’s obvious after reading through, but being a little bit clearer about how this section would be laid out would have been helpful.	We have added a paragraph on p. 6-1 outlining the content of Section 6 and think it was especially useful to give people a road map about the two parallel price vectors (which indeed are equivalent in the long-run section but enter importantly later when we think about dynamics and expectations).
6.1, in reference to “ q_a^D number of vehicles of age a demanded” (as well as the notation for number of vehicles of age a supplied). Demanded in transactions or overall? Ditto with supplied.	These are aggregate quantities, and we added that to the variable description on p. 6-1 to clarify.
6.2, Paragraph 1, in response to the sentence “The elasticities for individual vehicles measured by this approach do not provide clear signals about an elasticity of demand for new vehicles in the aggregate, because the outside good for individual vehicles includes other new vehicle models; thus, demand for an individual model is likely to be much more elastic than demand for a generic new vehicle.” I get what you’re saying here, but wouldn’t it be clearer to just say “the alternative choice for each individual vehicle includes other new vehicle models and the outside good”?	We incorporated the reviewer’s comment into the discussion on p. 6-2.
6.2, Paragraph 2, which starts with “We assumed...” and discusses assumptions about the demand system and the	We appreciate the positive feedback.

electrification of the vehicle market. Good discussion in this whole paragraph.	
6.4, Paragraph 5, which starts with “Importantly, supply and demand...” Related to an earlier comment: this paragraph is super helpful for understanding how everything is coming together and more of this earlier in the section would be helpful to the reader.	Thank you for this useful suggestion: we now also raise the connection between price vectors on p. 6-1 in the introduction to Section 6.
6.4.1, Paragraph 3, in reference to the sentence “Because scrappage occurs when repair costs exceed the value of the vehicle the form of the scrap function (constant elasticity) determines the form of the repair cost density.” Isn’t this backwards? The repair cost density is the primitive and the scrap function follows from it? I appreciate that you are putting an assumption on the scrap function rather than the repair cost density though.	Agreed, the density seems more like the fundamental and we have updated the language to reflect that on p. 6-9. In practice, an assumption on either this density or the scrap function fully specifies the other one.
6.6, Paragraph 6 about β_a (Share of aggregate spending on each vehicle age), in reference to the sentence “Expenditure shares come from combining the age profile above with data on the cost of vehicles of different ages.” I’m fairly confident that you explained this somewhere and I’m just missing it, but why isn’t β_a just calculated in the model from the equilibrium prices and quantities? Why does it need to be treated as an exogenous parameter? It seems very odd given the simulation exercise?	As pointed out by the reviewer, the baseline values are calibrated internally using equilibrium prices and quantities. They are exogenous from the perspective of policy (since underlying consumer preferences need to be the same in the baseline and policy world), and we have clarified these points in the text.
6.6, Figure 6-2: Baseline Price Profile by Age. I think that the “baseline” here just means that you use it for the expenditure shares and then update it in the simulation, yes? A clearer title would be helpful.	We think the use of “baseline” here and elsewhere is clear (i.e., simulated patterns over time based on historical data in the absence of a shock), though we added “vehicle” to the title and added a sentence on p. 6-12, further clarifying what is meant by this baseline price path.
7.1, Paragraph 2, which discusses Table 7-1: Demonstration of Channels of Adjustment in Quantities and Prices When Generalized Cost of New Vehicles Rises by 1%, in reference to the sentence “The 1% increase in generalized cost measures the strength of the policy, hence the term “policy elasticity.” Impact? I’m not really sure what “strength” means here.	We revised the text on p. 7-1 to clarify what was meant here. The idea is that the level of the generalized cost represents the magnitude of the policy. The policy elasticities are then capturing the responsiveness of the vehicle market relative to the magnitude of a policy shock.
7.1, Table 7-1, in reference to the Cross-Price New/Used column: Maybe I missed this earlier, but this is the elasticity to	Yes, this reflects substitution to all used vehicles (summed over ages),

all ages of used vehicles? I get that this might make sense since new buyers switch to newer used vehicles and those buyers switch to older used vehicles and it all works its way through the system, but being explicit about this would probably be helpful.	and we clarify this in the text. We also added text to footnote 26, directing readers to Appendix C where we provide more detail on the role of age difference in substitution.
7.2, Paragraph 5, in reference to the first sentence: “To complete the setting, note that the demand system contains 30 ages, but so far we have only specified three demand elasticities.” Ok, this makes sense re: my above comment. A quick sentence about this earlier where you talk about assumptions would be helpful.	Related to the comment above, we added a sentence describing the assumption of an aggregate used vehicle market at the beginning of Section 7.1 on p. 7-1, as well as the footnote on Table 7-1 to help clarify.
7.4, Paragraph 3, which describes Table 7-2: Policy Elasticities Corresponding to Selected Demand and Scrappage Elasticities, in reference to the sentence: “The first panel (rows A through E) holds the scrappage elasticity fixed at a value of -0.7 (Jacobsen and van Benthem [2015] and also close to the median of values in Table 5-3).” Rows A through E: Not labeled.	We were not sure what you meant by those rows not being labeled, though this comment and the one below may have referred to our lack of showing these as panels despite referring to the first and second panels in the text. We have corrected the text on p. 7-5 to refer to everything by the row labels A through I.
7.4, Paragraph 3, which describes Table 7-2: Policy Elasticities Corresponding to Selected Demand and Scrappage Elasticities, in reference to the sentence: “The second panel (rows F through I) explores changes in this elasticity, spanning most of the range in the literature with values of -0.2 to -1.2).” No second panel.	See response to the comment above.
7.4, Paragraph 4, in reference to the sentences: “Scenarios A through C explore an increase in substitution to the outside good. As we saw in Table 7-1, this is one of the most important elasticities in determining the effect of policy on new-vehicle sales.” It’s probably worth being really clear early in the report that this is critical and perhaps the worst-identified parameter in the modern discrete choice literature.	We added sentences making this point in the Executive Summary and the Introduction.
7.4, Paragraph 11, which starts with “Figure 7-1 displays...” in reference to the sentence “When the scrap elasticity is -0.7 on the horizontal axis, the values on the Y axis reflect the policy elasticities as shown in Table 7-2 for Scenario D.” Instead of Y, vertical. Or X instead of horizontal [for parallel sentence structure].	We revised the text on p. 7-8 to refer to horizontal and vertical axes for consistency.
8.3, Paragraph 3, referring to the sentence “For instance, there is some empirical evidence of increased buying in advance of regulatory changes going into effect, once those changes have	Agreed. We added citations to a couple of papers examining anticipatory responses to transportation policy on p. 8-3.

been announced.” Do you want cites on these types of statements?	
9.1, Paragraph 2, referring to the sentence “This is a small-scale version of changes in real estate prices that can occur even when the announced change is decades away.” Exactly what Holland, Mansur, and Yates get for electric vehicle mandates in their recent AEJ.	We added a citation to Holland, Mansur, and Yate and a couple of sentences on this paper starting at the bottom of p. 9-1.
9.1, Paragraph 2, referring to the word “sales” in the last sentence, “Preannouncement has no effect on the long-run outcome: sales converge to the 0.25% decline in a little over 5 years.” New vehicle sales. Just being clear that the changes in the used vehicle market take longer to get worked out (I assume).	We revised text on p. 9-2 to clarify that this is referring specifically to new-vehicle sales.

Comments by Dr. Benjamin Leard

CHARGE QUESTION	COMMENTS	RESPONSE (INITIAL DRAFT)
1. Does the presentation describe the data and methods sufficiently to allow the reader to form a general view of the quality and validity of the analysis approach?	In general, yes. The document could cite a few citations that have either been updated or published. Citation 1: Dou and Linn (2018), which was a working paper, is now a published paper appearing in the <i>Journal of Environmental Economics and Management</i>. The citation is Dou, X. and J. Linn (2020). How do US passenger vehicle fuel economy standards affect new vehicle purchases? <i>Journal of Environmental Economics and Management</i>. 102: 1-21. Citation 2: Leard (2021) is an updated version of Leard (2020). The current version is available at https://media.rff.org/documents/WP_19-01_rev_2021.pdf. The market price elasticity of demand estimate from the most recent version is -0.37.	We updated these citations throughout the text of the report and in the references. We revised the text to incorporate the latest elasticity estimate from Leard (2021) in the text.
2. Does the report miss relevant literature in its review? Are the	Yes, the report misses several relevant citations.	Our literature searches included grey literature but did not capture the Stock et al. comment on the proposed SAFE rule or the Center

interpretations of the elasticities in the literature review, and the estimates of elasticities used in the model stemming from the literature review, defensible?	Citation 1: James A. Stock et al. Comment on Proposed SAFE Rule, EPA-HQ-OAR-2018-0283-6220 (October 26, 2018). The Stock et al. comment on the proposed SAFE Rule does not report elasticities, but new vehicle market price elasticities in the range of -0.03 to -0.09 can be calculated from their results. The Stock et al. comment also includes an important correction of a spreadsheet error in the Notice of Proposed Rulemaking, resulting in a revised elasticity estimate for that model of -0.07. It is worth mentioning how this citation is different from other papers such as Leard (2021). My understanding of the paper is that it has a model that accounts for general equilibrium effects. It might be useful to include a new column in Table 5-1 that has a binary yes-no variable “Includes general equilibrium effects” since the point of the current document is to account for these effects. Citation 2: The report. Sean P. McAlinden et al., <i>The Potential Effects of the 2017-2025 EPA/NHTSA GHG/Fuel Economy Mandates of the US Economy</i>, Center for Automotive Research, 27 (Sept. 2016), https://www.cargroup.org/wp-content/uploads/2017/02/The-Potential-Effects-of-the-2017_2025-EPANHTSA-GHGFuel-Economy-Mandates-on-the-US-Economy.pdf This is a report that estimates the long-run market-price elasticity of demand for new vehicles to be -0.61. The interpretation of one of the citations is incorrect:	for Automotive Research report. We thank the reviewer for identifying those. We reviewed these references and have added the McAlinden et al. (2016) report to our summary tables and counts that we include in the report. We have also added a citation to the Stock et al. (2018) comment but did not include that reference in our summary tables or reported range because it did not meet our criteria for inclusion in the primary set of studies. While the authors are very highly qualified transportation researchers, their comment is neither peer-reviewed nor grey literature in the form of a working paper series, report issued by a university or independent research institute, or otherwise available as a publicly available output published by an independent research institution, which were the criteria for our searches.
	- Fischer, Harrington, and Parry (2007): This study finds an implied new vehicle market price elasticity of demand equal to -1 by reporting separate elasticities for cars and light trucks. This ignores substitution between the two vehicle categories. This	Fischer, Harrington, and Parry (2007) detail the elasticities in Appendix B. For the set of elasticities that adjusts for changes in the used market, they find -0.79, -0.85, and -0.36 for cars, trucks,

	paper also reports a long-run elasticity for the combined market for new motor vehicles, finding it to be -0.36. This value is reported in the bottom panel of Table 5-1. I am puzzled why the -1 value is reported in the top panel when the combined effect is what is relevant here.	and combined, respectively. We report only the combined value of -0.36 in the lower panel. For elasticities looking at new vehicles without adjustments for the used market, they report (from GM source data) values of -2.25, -0.97, and -1.0 for cars, trucks, and combined, respectively. We report only the combined value of -1.0 in the upper panel.
3. Are the data and assumptions appropriate for the analysis conducted and objectively chosen? If not, do you know of other data or proposed alternative assumptions that might be used in this analysis?	Certain papers that report new vehicle market price elasticities are, in my opinion, inappropriate to cite and use to construct a range of elasticities. The following papers cited in Table 5-1 are inappropriate: Berry et al. (2004): This study <i>assumes</i> a new vehicle market price elasticity of demand for new vehicles equal to -0.4 (in addition to model where they assume it is equal to -1). This elasticity is not estimated in this paper. Therefore, it is inappropriate to cite. Fischer, Harrington, and Parry (2007): This study finds an implied new vehicle market price elasticity of demand based on model simulations, not estimated from data. Generally, these types of “calibration” results should not be included with other studies that estimate the elasticity. BLP (1995), Goldberg (1998), Bento et al. (2009), Knittel and Metaxoglou (2014), Dou and Linn (2020): My issue with citing these papers is that none of them focus on properly estimating the market price elasticity of demand for new vehicles. None of them exploit proper statistical variation necessary to identify this parameter. Take, for example, BLP (1995). This study uses a panel of aggregate sales and price data for vehicle models sold during 1971-1990. The study is almost exclusively focused on obtaining unbiased estimates of the model-specific own-price elasticity of demand	We thank the reviewer for careful review and feedback on the papers included. We agree that there are caveats to many of the studies being used, including the fact that some were not focused on estimation of the market price elasticity of demand. Nonetheless, those are the elasticities that are implied by the universe of available studies. Thus, we feel that it is reasonable to include estimates generated from simulation models (which are denoted as being calculated from calibrated models in the table), as well as those calculated from studies of vehicle demand providing sufficient information to calculate the elasticities implied by those models. While Berry et al. (2004) did not estimate the aggregate price elasticity of demand directly, they estimated a parameter representing price responsiveness in their central case that can be used to calculate the market elasticity. An important implication of our review of the literature is that the availability of appropriate elasticity estimates is quite limited (which we mention in the overview of the literature at the beginning of Section 5). While these available estimates help set our ranges for the cases presented

	(e.g., Toyota Camry). One small part of the paper discusses the implied market-price elasticity of demand. As discussed in Leard (2021), this elasticity is a function of the own-price elasticity and the propensity of new vehicle buyers to leave the new vehicle market. It is not clear how BLP (1995) identify this propensity (they have no discussion of this). BLP (1995) should instead be cited when discussing the model level own-price elasticity of demand, since this is the focus of the paper. The other papers – Goldberg (1998), Bento et al. (2009), Knittel and Metaxoglou (2014), Dou and Linn (2020) – also have alternative focuses and do not discuss much at all how the market price elasticity of demand is identified from the data. Therefore, these papers should not be cited to construct a plausible range of values for the market price elasticity of demand. A paper like Bento et al. (2009), for example, which focuses on the link between vehicle choice and VMT choice, should be cited when constructing a plausible range for the VMT elasticity.	using our simulation model, the model is flexible and allows for the use of alternative assumptions.
4. Are the methods and procedures employed technically appropriate and reasonable? Please distinguish between cases involving reasonable disagreement in methods as opposed to cases where you conclude that current methods	I have a few concerns with the methods. Concern 1: The modeling of vehicle ownership costs (r) and prices (p) is unclear to me. Why is it necessary to model vehicle ownership costs in the first place? Why not just model vehicle prices? The discussion in 6.4.1 seems to be motivated from this distinction. But it is not clear why the model cannot be simplified to only have vehicle prices or vehicle prices plus fuel costs. Ownership costs appear in the vehicle demand function equation (6 – 1). The authors motivate using ownership costs with the following:	(1) This comment was quite helpful in clarifying the way we describe depreciation and survival. We now make the link clearer in the first paragraph of Section 6 and the (new) third paragraph of Section 6.4.1. Specifically, modeling a fixed path of depreciation (i.e., ownership cost being a fixed percentage of asset value at any given age) and modeling a fixed survival path are closely connected. Consider instead what would happen if survival of vehicles became shorter but depreciation were a fixed fraction

involve specific technical errors.	“Modeling ownership cost as the variable of interest to the consumer follows the approach in Bento et al. (2009) and is important when allowing choices to be made between new and used vehicles.” Bento et al. (2009) define ownership costs as the sum of fuel costs and a rental rate, where the rental rate is defined as the sum of Foregone return of the real value of the car, which is proportional to the used vehicle price. Depreciation, which is typically proportional to the used vehicle price (e.g., a typical used vehicle depreciation rate is often cited to be 15 to 20% of the value of a used vehicle). Insurance and registration, which are generally tiny relative to the first two components. Given these features, it seems straightforward to model used car demand as a function of vehicle prices and fuel costs only. This would simplify the model set up and it would be consistent with how supply is defined, which is a function of used vehicle prices.	of asset values: vehicle owners would suffer unexpected losses because they would still be depreciating vehicles that are no longer operational. Or, if vehicle survival became longer, as in many of the cases we examined, there would be windfall gains on vehicles that are fully depreciated but still operational. By recomputing depreciation together with survival we are able to capture equilibrium changes. When the up-front cost of new vehicles increases, the choices owners make to extend survival are linked to (and, in fact, are implicitly motivated by) spreading out the higher vehicle cost over more years. More subtly, consider the intuition in line 1 of Table 7-2. If we made depreciation a fixed fraction of asset value, then demand for new vehicles would fall by 1% (because ownership cost would just be a constant fraction of the now-higher new-vehicle price). This would not be consistent with a zero elasticity to the outside good because new and used car quantities are also linked in that scenario; depreciation needs to adjust to produce an equilibrium.
	Concern 2: It is unclear how a vehicle is defined in the model. Is a vehicle defined by age only? So the model has 31 vehicles: new, 1 year old, 2 years old, ..., 30 years old. This should be more explicitly stated in the first paragraph of Section 6.2.	(2) Yes, the reviewer is correct that a vehicle is defined only by vintage in this aggregate model. We clarified this and added more detail on the limitations of this aggregation in the new second paragraph of Section 6.2.
	Concern 3: Section 6.3.1 New Vehicles states that the price of a new vehicle is taken as given. Is it equivalent to say that the new vehicle market is perfectly competitive? This is in contrast to most of	(3) Consumers taking prices as fixed is not equivalent to assuming the new vehicle market is competitive because the price could, and likely does, include a

	the vehicle market literature (Bento et al. 2009 for example), which models the new vehicle market is imperfectly competitive. It would be useful to include more details on this assumption, why it was adopted, etc.	markup. To the extent negotiating power at a dealership might change, we did abstract from that. We have now clarified Section 6.3.1 to this effect and explain more clearly the role of imperfect competition in new vehicle supply (paragraph 2 of Section 6.3.1). The extent a policy might be expected to increase or decrease the degree of competition would affect the prices faced by the consumer. In the present setting, this would appear as an input in the sense that generalized cost (i.e., total price paid by the consumer, net of attribute changes) would need to be adjusted to account for policy-induced changes to market power. We do not have a strong prior on if policy is likely to increase or decrease market power, so we suggest a neutral assumption that markups remain fixed.
	Concern 4: In Section 6.6, the authors mention that they explore a range of the demand elasticities θ_{aa} and θ_{Na}. It would be useful to provide a table of elasticities assumed. I realize this table is presented in Table 7-2, but it would be constructive to include a table in Section 6.6 with discussion defending the choice of elasticities.	(4) We have now added to Section 6.6 a summary and discussion of the elasticities we chose (we attempted to span most of the range in the literature table). This version of the report also includes a new case, exploring a very small new-vehicle demand elasticity (-0.1) in Appendix C. This expanded limit reflects, for example, a value closer to the Stock et al. (2018) comment on the SAFE rule.
	Concern 5: The authors assume that the own-price elasticities for used vehicles is identical to the new-vehicle elasticity. This is problematic because of how households sort into different vehicle ages. New vehicles are more frequently bought by wealthy households, and a lot of literature has shown that wealthy households tend to be less responsive to price than average or low income households. For this reason, I	(5) Footnote 21 provides some detail on this question, noting that used-vehicle owners may face more limited substitution possibilities to the extent they may be considering public transit, or not traveling, as a substitute. We also fully agree with the possibility in the reviewer's comment that income or other effects could instead make

	would expect the own price elasticities to be larger in magnitude for older vehicles.	elasticities higher among used-vehicle owners. Appendix Table C-1 examines this possibility: in row (3) used vehicles have double the own-price elasticity as new vehicles. We have expanded the text in Appendix C to discuss the rationale mentioned by the reviewer.
	Concern 6: At the end of Section 7.2, the authors state that “Cross-price demand elasticities between individual ages are set to fall at 8% per year of difference in age.” This seems reasonable, but it is not clear where 8% comes from. Is this based on results from a demand system such as Bento et al. (2009)?	(6) There is very little evidence in the literature about the degree of substitutability across ages. We are glad to hear that the reviewer finds the 8% assumption to be reasonable, but we have also added text emphasizing that for this parameter exploring multiple values is especially important. We now do this in Section 7.2 and in Appendix C, where we have added cases ranging from very concentrated substitution all the way to perfectly flat. The overall policy elasticities are not very sensitive to this parameter. Intuitively, it does not appear to matter much if substitution is direct (e.g., from age 5 vehicles directly to ages 6 through 30, which is what can happen when the profile of substitution is very flat) or through an indirect cascade (e.g., substitution from age 5 to 6, which induces others to substitute from 6 to 7, 7 to 8, and so on, which is what happens when the elasticity falls off sharply with age difference).
5. Does the modeling analysis appear to produce results consistent with the assumptions and data used for	Yes. It is clear that the policy elasticities are smaller in magnitude than the new vehicle demand elasticities, which makes sense given the equilibrium response.	We agree with the reviewer that the relative magnitude of the elasticities is reasonable.

model development? Do the results presented by RTI follow from the data and assumptions used in the analysis?		
6. In what ways are the results sensitive to the data and assumptions used in model development? Are there alternative assumptions and data that the researchers should consider providing improved analysis?	Given my comments above, I suggest applying a different range for the market price elasticity of demand for new vehicles. I would also consider adding greater detail to the definition of a vehicle. It seems to me that part of the value of this exercise is to simulate the effects of various policies (for example, CAFE standards). The current structure of the model has a highly aggregated vehicle definition that seems lacking for performing simulations of an actual policy. At a minimum, it would be useful to provide a discussion on how the model could practically be expanded, and the potential calibration and computational challenges involved with such a task.	As discussed in responses to comments above, we added the McAlinden et al. (2016) study to our primary set of elasticities from the literature and updated text to the latest version of Leard (2021) (changing from -0.40 to -0.37), but found the Stock et al. (2018) comment to be outside our criteria for inclusion and continue to believe that the other papers used to develop the range should be retained as representative of the available literature that can be used to estimate the market price elasticity of demand for new vehicles. As noted above, certainly caveats are associated with a number of the available estimates, but we continue to feel that the range specified is appropriate. Along those lines, were an analyst to prefer to focus on alternative elasticity values, they could emphasize specific scenarios in Table 7-2 that align with their preferred specification. In addition, the simulation model was developed with the flexibility to readily use alternative parameters. Adding more detail to the definition of a vehicle is outside the scope of the current study, which is focused on representation of vehicle markets at an aggregate level. Outputs from this model can be linked to more detailed engineering models to capture additional detail

		within those models. Nonetheless, we agree with the reviewer that it would be valuable to expand the model presented in this report in the future (e.g., adding electric vehicles or other advanced fuel vehicle technologies and otherwise capturing within-vintage vehicle differences and characteristics likely to be differentially affected by transportation policy).
ADDITIONAL OVERALL COMMENTS PROVIDED (NOT CHARGE QUESTION-SPECIFIC):		
ADDITIONAL COMMENTS BY SPECIFIC REPORT CHAPTER:		

Comments by Dr. James Sallee

7. CHARGE QUESTION	8. COMMENTS	9. RESPONSE (INITIAL DRAFT)
1. Does the presentation describe the data and methods sufficiently to allow the reader to form a general view of the quality and validity of the analysis approach?	The report is clear. Data and assumptions are documented well. Assumptions and modeling choices are explained thoroughly. Moreover, the results of the analysis are well explained, and every effort is taken to provide basic economic intuition that gives context to the quantitative model output. As a result, readers should be fully able to evaluate the merits of the model and understand both its contributions and its limitations from the information contained in the report. The report lays out the main analysis in a sequence of steps that ensure clarity by adding complexities and features one at a time (e.g., section 7.1). The report also has many sections that seek to consolidate results and illustrate the underlying	We appreciate this positive feedback on the quality and clarity of the report.

	intuition of the results (e.g., the summary in section 7.6). Altogether, the report explains a complex economic model in a concise and effective way. This is not easy to do, and the authors of the report should be commended for the quality of the exposition.	
2. Does the report miss relevant literature in its review? Are the interpretations of the elasticities in the literature review, and the estimates of elasticities used in the model stemming from the literature review, defensible?	The report does an admirable job of surveying the literature and putting the information available from past research into context. I am not aware of other studies that should have been included in the literature review. As the report notes at several points, there are many studies of the automobile market, but attention to the elasticities pivotal for the current analysis are rarely the focus and are often not even calculated. Thus, the report's finding that few existing papers can be used to calculate the key elasticities conforms with my understanding of the literature. All interpretations of the elasticities offered in the report appear appropriate. The use of elasticities from the literature to inform the model is certainly defensible. The report explains clearly why the existing literature offers limited guidance on key values. It therefore emphasizes strongly the need for sensitivity analysis, which is then provided. This is the appropriate stance to take. If anything, I would recommend even more caution in relying on the existing literature (and hence elevate even further the critical role of sensitivity analysis) for two reasons,	We appreciate the positive feedback on the literature review and the constructive suggestions for further enhancement. We have further expanded our discussion of the caveats associated with the parameters available from the literature starting on p. 5-1 (including addition of the excellent points raised by the reviewer regarding study age and consumer income and changes in embedded technology). We agree with the reviewer that many of the study authors were not necessarily focusing on estimation of the elasticities of interest for this study and may therefore not have conducted a thorough evaluation of the implied elasticity. Nonetheless, those are the elasticities implied by the demand systems in the literature, and we believe it is reasonable to use those as part of the assessment of available elasticities. As noted by the reviewer, standard errors are not necessarily available from each study and are not necessarily calculated with consistent methods.

	both of which are noted in the report itself, but I note here for emphasis. First, many of the studies are more than a decade old. Mechanically, vehicles are becoming more durable and lasting longer (this is noted in section 2). At the same time, embedded technology is also changing rapidly, which might make newer and older vehicles less close substitutes. Both factors imply that empirical relationships observed in the automobile more than a decade in the past may be misleading indicators of the vehicle market of the future. Moreover, one might suspect that the elasticity of demand for new vehicles shrinks as consumers get richer. If true, older studies might exaggerate the new vehicle price elasticity. (Some interaction between income and price sensitivity is often estimated in prior studies. Perhaps there is some information in those that would suggest how large might be any trend in the elasticity driven by economic growth. The report does not mention this.) Second, a substantial share of the available studies do not focus on the elasticity parameters that are of interest here. In several cases the authors of the report had to construct an estimate that was not even reported in the original research. This is relevant because researchers have discretion in how to construct and estimate these models. When a parameter (like the substitution to the outside good) is viewed as a byproduct of estimation, the original researchers may not have exerted maximal effort in ensuring that the estimation was reliable.	
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	The report does not discuss standard errors in the original studies. Instead it uses the point estimates from prior studies to form a qualitative sense of the range of estimates across papers. This is a defensible approach, especially because the standard errors are likely calculated with different methods across studies and are thus not always comparable. Even so, where available, it might be useful to have standard errors from the original studies included in the review to more fully characterize uncertainty.	We agree with the reviewer that it would be useful to incorporate standard errors. However, as was noted in the introduction to Section 5 of the report in the paragraph beginning with “Unfortunately, available information from these papers is generally insufficient ...”, there is generally not enough information available from the papers to even identify or calculate a standard error, let alone compare them on a consistent basis. Thus, we have continued to focus on point estimates for the purposes of this report. The model does include flexibility to conduct additional sensitivity analyses.
3. Are the data and assumptions appropriate for the analysis conducted and objectively chosen? If not, do you know of other data or proposed alternative assumptions that might be used in this analysis?	The data and key model parameters used in the baseline analysis and the various scenarios appear appropriate. The data sources cited are all reliable and would be considered standard, objective sources. I do not know of any alternative data sources that I believe to be superior. Likewise, the parameter assumptions are appropriate and appear consistent with an objective analysis. (In answering this question, I interpret “assumptions” to refer specifically to parameters used in the model, rather than assumptions about the structure of the model or analysis, which I interpret as falling under the definition of “methods and procedures” addressed in the next question. Essentially, this answer is about section 6.6 of the report.) The baseline scrappage scale factors (b_a in the notation of the model) are said to simply be “calculated using baseline vehicle survival probabilities.” The relevant paragraph (p. 6-10) adequately explains the source of data, but I was uncertain of	We appreciate this positive feedback on the data and assumptions used in this report. The scale factor in the scrappage equation b_a is indeed a simple algebraic calculation such that baseline prices exactly reproduce baseline scrap rates. We added more detail on the calculation of baseline scrappage scale factors in paragraph 4 of Section 6.6 as suggested by the reviewer.

	whether there was a degree of modeler discretion here, or if each b_a is simply an algebraic calculation based on equation 6-5, where both s_a and p_a are raw data inputs. This is a minor detail that could be easily clarified.	
4. Are the methods and procedures employed technically appropriate and reasonable? Please distinguish between cases involving reasonable disagreement in methods as opposed to cases where you conclude that current methods involve specific technical errors.	The methods and procedures developed and deployed in the report are technically appropriate and reasonable. The model is complex: it deals with thirty vehicle ages while incorporating forward looking expectations. To maintain tractability, it must make simplifications—there is a single composite vehicle model; consumer heterogeneity is not explicated; forward looking beliefs are fully rational; new vehicle supply is perfectly elastic; the model abstracts from the choice of miles traveled; and, there are no income effects. Those assumptions are justified and reasonable. They serve the interest of transparency and clarity, and they are all appealing as a benchmark case. The report is transparent and clear about assumptions and offers caveats where appropriate. For instance, the analysis is built on a model of vehicle purchase, rather than vehicle use. As noted in the report, it is difficult to see exactly how incorporating changes in mileage in response to vehicle prices might play out in the full equilibrium. This decision could perhaps be added to the model at some future date, but even then, the key parameters (e.g., how vehicle price affects mileage, presumably through a change in depreciation) are not well estimated in the literature and thus would require	We appreciate the reviewer's comments generally agreeing that the methods and procedures we used in this report are appropriate and reasonable. As noted by the reviewer, it may be possible to further expand the model in the future to add vehicle-miles traveled, but we agree that the necessary parameters for defining vehicle use relationships are even less available from the literature than the vehicle demand elasticities reviewed in the current study. The reviewer raises a good point about potential changes in substitutability between new and used vehicles related to systematic changes in vehicle characteristics over time. We added discussion of this consideration in footnote 6. In addition, we incorporated discussion of the implications of imperfect competition for interpretation of the model and its inputs in paragraph 2 of Section 6.3.1. The degree to which costs are passed through (and/or subjected to markups) can be embodied in the generalized cost input to the model. We have updated Section 6.3 to relax our assumptions on the supply side. The model here focuses on demand, so to the extent there is information on competition changes, those could also be reflected in generalized cost. From the perspective of the (representative) consumer, the price of new vehicles is given throughout. Finally, we expanded on the discussion of assumed substitution patterns among vehicles of different ages in Section 7.2 and especially in

	additional conjecture. As such, a model focused on ownership seems appropriate. The report offers a generalized interpretation of a cost shock to new vehicles. It suggests that a regulatory change that creates a combination of changes in price and attributes can be interpreted as a tax equal to the “net utility cost.” This is sensible and appealing as a modeling approach. (It is also very important because new vehicle regulations create just this sort of collection of changes simultaneously.) That said, a minor caveat would seem to be that a change in other attributes (as opposed to just a change in price) might change the substitutability between new and used vehicles; that is, a regulation that changes the attributes of new vehicles might change the theta terms in the demand system. This is likely to have a minimal impact in the current setup. But, it could become a more important issue in the future if the model is used to contemplate a transition to electric vehicles or vehicles featuring different levels of automation, in which case attributes affected by the policy could radically change the substitution between new and used vehicles. The report assumes full pass through of cost shocks to consumers: because new vehicle supply is assumed to be perfectly elastic, all regulatory burdens are borne by consumers. The report argues that this is a sensible interpretation for the long run, and it is indeed the most appealing benchmark. That said, I am not certain that we would expect full pass through of costs in a market with	Appendix C. Appendix C also now includes several new cases where we explore a wide range of assumptions, including a very fast rate of falloff that effectively constrains all substitution to within a few years. Rapid falloff further reduces the policy elasticity below the demand elasticity, as might be expected given that it makes substituting away from new vehicles more difficult in the long run. Appendix C also explores a case with entirely flat substitution. This is perhaps less realistic but provides a useful extreme bound on the substitution pattern.
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	imperfect competition. It is perhaps useful then to contemplate how the results would be different if part of a regulatory cost increase is borne by new vehicle sellers, and part is passed on to consumers. Is it possible to simply interpret the cost shock in the model as the portion passed on to consumers? The only method and procedure in the modeling analysis that I had trouble understanding was the assumption about how the demand system was assumed to be structured across used vehicles of different ages. The text describes the assumptions deployed in an intuitive (and appealing) way, but it does not write out algebraic explanations. Furthermore, in some places, the report offers as intuition that older vehicles are less close substitutes for newer vehicles, which is meant to rationalize several patterns. I struggled to follow this reasoning because I could equally imagine a world in which there was only substitution between adjacent ages (e.g., the only cross-price elasticity for a 3-year old car is between 2- and 4-year old cars), but nevertheless a price shock to new vehicles cascades through the entire distribution of ages.	
5. Does the modeling analysis appear to produce results consistent with the assumptions and data used for model development? Do the results presented by RTI follow from the data and assumptions used in the analysis?	The modeling analysis produces results that appear to follow logically from the assumptions and data. As a peer reviewer, I did not attempt to replicate any of the findings or inspect the model code that executes the simulations (nor was I given the material to do so). Instead, my judgment that the results reported follow from the data, model, and assumptions is based on the fact that the pattern of results are consistent	We appreciate the positive feedback on the reasonableness of our modeling results.

	with the economic theory that the analysis purports to embody. The qualitative results that emerge in the baseline model, and the difference across the nine scenarios described in section 7.4 (as well as the additional versions in the appendix), conform with theoretical expectations. These results, and the economic reasoning that aligns with them, are described clearly in the report. I found that discussion sound and convincing.	
6. In what ways are the results sensitive to the data and assumptions used in model development? Are there alternative assumptions and data that the researchers should consider providing improved analysis?	The report explores the sensitivity of key results by generating simulations based on a range of parametric assumptions that span the range of elasticities from the literature. While it is always possible to perform more sensitivity analysis (and future users will apparently be able to do so on their own, which is a great feature of the project), the current report does a nice job overall of showing the likely range of key results. Quantitative values of course vary across scenarios, but several key findings, including the policy elasticity being significantly smaller than the new vehicle elasticity and the overall fleet size shrinking in response to a new vehicle price increase, appear to be robust to a range of inputs and assumptions. This is reassuring and seems to suggest that key qualitative insights that can inform policy are very likely to hold. Section 7.4 presents the core sensitivity analysis by running a variety of scenarios. This section emphasizes variation in three parameters: the new vehicle elasticity, overall substitution to the outside good, and the scrap elasticity. But, it seems there is a fourth crucial elasticity, which is the substitutability	As suggested by the reviewer, we added more discussion of sensitivity analysis at several points in the report. We now look at different values for substitutability between different vehicle vintages in Appendix C. In addition, we added a new section focused on model and parameter uncertainties associated with this model in Section 9.4. Finally, we expanded Appendix C to also include output from both alternative survival data sources noted in the main text (Leard, and Jacobsen and van Benthem).

	between vehicles of different ages. The appendix provides a couple of alternative model runs that seem to address this point. The discussion around these scenarios is quite concise, and as a result I am not fully certain I understand how to interpret those results. The appendix results seem to suggest that key model results are not sensitive to big changes in these assumptions. The report does not feature standard errors. Instead of focusing on standard errors, the report focuses on alternative scenarios. This is a sensible approach, but it does open the possibility that some users may underestimate (or overestimate) uncertainty. To help future users, it might be useful to include a discussion about uncertainty in one section, starting with a bullet point of sources of uncertainty in the results, with some guidance as to what the authors believe are the biggest sources. These would include both uncertainty about key parameters, which might be amenable to some statistical characterization, and model uncertainties, which might be harder to quantify. At a few points, the report simply states that the model results are robust to differences in data inputs. This includes reference to vehicle market data used in a paper by Leard and one by Jacobsen and van Benthem, rather than the AEO report that is the main data source. I fully believe this is true, but it might be useful to report those robustness checks somewhere.	
10. ADDITIONAL OVERALL COMMENTS PROVIDED (NOT CHARGE QUESTION-SPECIFIC):		

The analysis described in this report is a significant step forward and represents a valuable tool for improving regulatory analysis of the car market.

Attempts to incorporate interactions between new vehicle regulations and the used vehicle market that are based on statistical relationships between new vehicle prices and the used and new vehicle markets studied separately are inadequate grounds for policy analysis and highly likely to be misleading. In essence, this model studies long-run general equilibrium interactions, something that is notoriously difficult to study using observed market data. Thus, an understanding of how interactions between new and used vehicle markets dampen, amplify or propagate the effects of new vehicle regulations requires an integrated model that provides internally consistent effects constrained by economic theory. The analysis in this report does exactly that, which is why it is so valuable. Specifically, this new tool should foster improved analysis of the ways in which used vehicle market interactions may alter the net impact of new vehicle regulations on consumers and the environment.

The model described in the report identifies key mechanisms that govern the relationship between new vehicle costs and the used vehicle market. There is substantial uncertainty about key parameters, so care should be taken in interpreting the quantitative output of the model (this is also true, of course, of other models that underly regulatory analysis of automobiles), but the analysis here reveals that certain facts are robust (e.g., the policy elasticity is smaller than the new vehicle demand elasticity). Moreover, it provides a tool that can be used to assess the plausibility of different outcomes (e.g., the total fleet size can increase in response to a new vehicle cost increase only under implausibility limited substitution to the outside good).

A central contribution of the report is to highlight the difference between a new vehicle demand elasticity and what the report refers to as the policy elasticity, the latter of which takes into account the feedback of used vehicle prices into the demand for new vehicles following a cost shock to new vehicles. The analysis demonstrates that the policy elasticity is consistently smaller than the demand elasticity, and that the magnitude of the gap between the two is of importance. This is a critical insight, and it conforms with economic theory. The report explains this relationship effectively, and it

We appreciate the positive comments on the valuable contributions of this report. As suggested by the reviewer, we have added discussion of future research and analyses that we feel are likely to provide the most valuable new information for continuing to refine the use of this model for future policy analyses in a new Section 10.4.

provides a tool that users can customize to perform sensitivity tests and additional analyses. This tool might become even more useful in the future if subsequent studies provide new information about the key parameters that serve as inputs to the model. Having run many iterations of the model (presumably many more than are presented in the report), the authors likely have insights about the most important parameter choices and modeling decisions. A possible addition to the report would be a prioritized list that emphasizes where additional estimates from future research, or additional sensitivity analysis from future users, would provide the most valuable new information	
ADDITIONAL COMMENTS BY SPECIFIC REPORT CHAPTER:	
Included here are a series of comments, many of which are quite minor expositional notes, ordered with respect to each section of the report. Section 3 This section provides a valuable discussion of the key economic forces that link the new and used vehicle markets and the role of scrappage. I suspect that it would be possible to illustrate these relationships further by using a pair of simple supply and demand diagrams to illustrate the steady-state (long-run equilibrium) effects. Initially, a new vehicle cost increase shifts up the supply curve in the new vehicle market. The magnitude of the impact depends on the elasticity of demand. The change in the new vehicle market impacts used vehicle supply (shifted down because there are fewer new cars to become used) and used vehicle demand (more so as buyers view used and new as close substitutes), resulting in a new price and quantity. The size of this change depends on the scrap elasticity, which determines the slope of used vehicle supply. The resulting change in used vehicle price then shifts the new vehicle demand curve, resulting in a new quantity. The role that each of the key elasticities places could be labeled in a heuristic diagram of these two markets. Analytical expressions that show the simple relationship between key elasticities would also be helpful. This is not feasible in the full model with 30 vintages of used vehicles, but a future modeling exercise that reduced the used vehicle market to a single composite might make it possible to	As suggested by the reviewer, we have incorporated a diagram illustrating the supply and demand relationships in the new- and used-vehicle markets. This appears as Figure 3-1, and we provide text explaining the figure in Section 3.2.1. Also as suggested, we can now describe how the slopes of lines in the figures, and therefore the relative size of effects, connect to the core model parameters. Deriving analytical expressions in a two-age world is, we think, best left for a future exercise. We now discuss this in Section 3.2. We think a two-age model would likely not directly inform the results of the present study because the relationships in the more realistic multi-age model here allow many more interactions.

characterize the key comparative statics, showing how they depend on the handful of key elasticities.	
Section 5 The literature synthesis is effective and clear overall. The report offers a rich discussion of some of the studies, but in a few cases the results of prior studies are reported without a lot of commentary about their reliability or applicability to the purpose at hand. For example, my impression is that authors of papers in the tradition of Berry, Levinson and Pakes (1995) are relatively skeptical of the reliability of their own estimates of the elasticity of substitution to the outside good, relative to the other estimates in those systems. I gather that the magnitude of this elasticity can scale with assumptions made by the researcher about what fraction of consumers are shopping for a car in a given period, something which is nebulous and often chosen without hard data. This is not to suggest that there are better estimates available, but rather to emphasize our relative ignorance about the market level elasticities that are central to the analysis here. In some cases, the authors of the report had to do additional calculations to extract an elasticity estimate from a prior paper. To the extent possible, it would be useful to have those steps explicated somewhere, for future reference. (For example, I'm not sure that someone else can replicate the author's interpretation of the elasticity in the Hahn study, or replicate the new calculations from Bento et al. 2009.)	Based on the reviewers' comments, we have incorporated additional text on the reliability and applicability of available elasticity estimates, particularly caveats associated with many of these estimates, starting on p. 5-1. In addition, we added footnote 17 indicating that the underlying spreadsheets used to calculate elasticity parameters for some studies are available from the report authors upon request.
Section 6 It might be helpful to include units (for the prices) in Figure 6-2. The demand system has $(A+2)(A+1)/2$ substitution elasticities, where A is 30 in the present analysis. This is a large number. The calibration feeds in basically three elasticities. All of the rest are filled in via assumption about the structure of cross-price elasticities through the age structure. This assumption (described in sections 7-2 and 7-3, in particular footnote 21) appears reasonable and is an appealing solution to the problem that no estimates of this age substitution pattern exists. But, I was not confident that I could fully reconstruct the age structure of substitution from the description in the text. A small, helpful step would be to add algebraic expressions detailing this procedure and	We added units for the prices in Figure 6-2. We added algebraic expressions detailing the calculation of the substitution elasticities (and discussion of methods for conducting additional sensitivity analyses related to substitution between different vehicle vintages) in Appendix C (footnote 42).

commenting further on how interested users might conduct additional sensitivity analysis (noting that some is provided in the appendix).	
Section 7 Table 7-1 (and several related tables) do not label the main results as the “policy elasticity.” This term is emphasized in the text, but it is not called out in the tables. Adding it might be helpful. It might be helpful to also include some measure of price changes (for used cars this may be an index across ages) in Table 7-2. The price changes are not the object of interest, but they are a key channel, so including them might help the reader. Figure 7-1 should include somewhere (title or note) that the new vehicle elasticity is -0.8 because the main point of the graph is that the policy elasticity is substantially smaller. This is not apparent without referring back to a prior table.	We made edits to Tables 7-1 and 7-2 and Figure 7-1 to more clearly identify the policy elasticities, based on the review comments. We note that the relevant price changes for a selection of used vehicles are reported in Table 7-3.
Section 10 Scrappage plays a key role in the analysis, but scrappage can mean either that a vehicle is exported from the United States or that it is fully decommissioned. The paper does not mention this distinction. It is standard practice for regulatory analysis to focus on emissions in the US fleet, and as such it is logical to ignore this distinction in the present analysis. The distinction is, however, relevant to the true greenhouse gas emissions implications of new standards. This might be a subject of future work. The model built here is free of transaction costs. In reality, transaction costs associated with buying and selling vehicles are substantial. This is unlikely to matter for analysis of long-run effects, but it could have an impact on transition dynamics. Modeling transaction costs is certainly beyond the scope of the current report, but it might be useful to speculate as to whether transaction costs would amplify or dampen the main effects during a transition.	As surmised by the reviewer, we did not distinguish between vehicles being exported or decommissioned. We agree with the reviewer that this could be valuable to explore in future work and now mention this distinction in our discussion of potential future research in a new Section 10.4. We agree with the reviewer that modeling of transaction costs is beyond the scope of the current report but could have effects on transition dynamics for certain scenarios, though we think even these are likely to be small. We added discussion of potential implications of incorporating transaction costs in the new Section 10.4.
Appendices It might be helpful to add further labels to Figure D-1, which is showing two completely different model runs with separate assumptions about expectations, rather than two sets of results that come out of the same model run.	We added labels and a note to Figure D-1 to further clarify the contents.

Some of the figures in appendix D label time just as “year” instead of the more helpful “years since policy effective date.”	We updated the labels to consistently indicate “years since policy effective date,” as appropriate.
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Peer Review of “The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage”

RTI Report 0215574.004.028

EPA Contract No. EP-C-16-020

Work Assignment No. 4-04

June 11, 2021

Submitted to:
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I. Introduction

The U.S. Environmental Protection Agency (EPA) sets emission standards for new light-duty vehicles. Such standards are expected to increase the up-front costs of new vehicles, and thus to affect new-vehicle sales as well as the market for used vehicles by altering substitution between new and used vehicles. Changes in the valuation of used vehicles are, in turn, likely to affect vehicle scrappage rates by reducing the number of old vehicles that are retired each year. Consumer response to changes in vehicle prices induced by regulatory policy can therefore have important implications for the total size and average age of the U.S. vehicle inventory, influencing net impacts of regulatory actions on greenhouse gases, criteria pollutants, vehicle safety, and other outcomes. Being able to estimate the effect of regulations on the new and used vehicle markets will enhance EPA’s ability to examine the economic and environmental effects of new light-duty vehicle emission standards.

The RTI report: 0215574.004.028, “The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage” (referred to as the Report) describes the development of methods to estimate these impacts. The methods involve identifying a relationship between changes in new-vehicle costs and new and used vehicle fleet sizes using a simulation model parameterized with literature estimates of various market and consumer response elasticities.

EPA’s guidelines specify that all highly significant scientific and technical work products shall undergo independent peer review according to specific agency protocols. This process is designed to ensure the use of the highest quality science in its predictive assessments and to assure stakeholders that each analysis/study has been conducted in a rigorous, appropriate, and defensible way. Therefore, EPA submitted the Report for external peer review, seeking independent expert opinion on the methodologies employed and analyses presented in the Report. ICF facilitated this peer review process. This memorandum contains a summary of the peer review results as well as documentation of the process.

The peer review was conducted from March to June 2021 in accordance with the current version of EPA’s *Peer Review Handbook*.¹ At the conclusion of the review process, ICF collected all unedited peer reviewers’ comments and provided them to EPA. This Technical Report contains a brief summary of the reviewers’ comments to EPA’s charge questions, along with the unedited answers presented by each peer reviewer.

Supporting documentation collected from the reviewers, including their curriculum vitae (CV) and conflict of interest (COI) statements, is also provided.

¹ U.S. Environmental Protection Agency, *Peer Review Handbook*, 4th Edition, October 2015. Prepared for the U.S. EPA by Members of the Peer Review Advisory Group, for EPA’s Science Policy Council, EPA/100/B-15/001. Available at <https://www.epa.gov/osa/peer-review-handbook-4th-edition-2015>, including OMB’s Information Quality Bulletin for Peer Review (Handbook, Appendix B) provisions for the conduct of peer reviews across federal agencies.

The following materials are included in this Task 3 Technical Report.

1. Description of the Peer Review Process (Section II)
2. Reviewer Responses to Charge Questions (Section III)
3. Reviewer Supporting Documentation (Appendix A and Appendix B)
4. Notes from mid-review meetings with EPA, ICF, and the contracted peer reviewers (Appendix C).

II. Peer Review Process

ICF conducted the peer review in three stages. First, ICF identified a qualified set of reviewers; second, ICF contracted with the selected peer reviewers and conducted the review; then, ICF collected reviewers’ feedback on the RTI Report. ICF documented the peer review process, consisting of this Technical Report, to submit the assembled information from the peer reviewers to EPA. Ultimately, EPA will convey results of the peer review process to the authors of the RTI Report, who will respond to the comments received. The following sections provide detail on these steps.

1. Selecting Reviewers

ICF first identified a pool of independent subject matter experts from which to select three qualified candidates to form a review panel. Qualifications included two technical considerations. First, candidates could not have actual or apparent conflict(s) of interest or lack of impartiality that would preclude an independent review. Secondly, it was necessary that the combined expertise of the candidates covered the two focus fields of this analysis:

1. Simulation modeling of light duty vehicle price changes and consumer behavior response.
2. Fuel economy policies of light duty vehicles.

ICF identified fourteen potential reviewers for the report based on a combination of individuals originally suggested by EPA and identified through ICF’s research. ICF then contacted each candidate by e-mail to assess reviewer’s expertise in the field, ability to perform the work during the period of performance, and any association with the topic that would preclude an independent review (note that at this stage, the Report was not shared with the potential peer reviewers). ICF also collected a CV or resume from each candidate that expressed availability and interest in participating in the review panel.

Based on all the information gathered during this initial phase, ICF selected three reviewers. While all candidates were highly qualified to act as peer reviewers, ICF sought to select candidates whose combined expertise would cover all technical aspects of the Report while bringing diverse and complementary perspective to the peer review process. ICF suggested the

following reviewers in the April 12, 2021 Peer Review Selection Memo to EPA², here listed in alphabetical order

1. Ashley Langer
Assistant Professor, Department of Economics
Faculty Research Fellow of the National Bureau of Economic Research
University of Arizona, Tucson
McClelland Hall 401
Tucson, AZ 85721
alanger@email.arizona.edu
520-621-0117
2. Benjamin Leard
Assistant Professor, Department of Agricultural and Resource Economics
Faculty Fellow, Howard Baker Jr. Center for Public Policy
University of Tennessee, Knoxville
Morgan Hall 310A
Knoxville, TN 37996
bleard@utk.edu
865-974-5005
3. James Sallee
Associate Professor, Department of Agricultural and Resource Economics
University of California, Berkeley
207 Giannini Hall
Berkeley, CA 94720
sallee@berkeley.edu
510-643-5133

ICF anticipated that this selected group of reviewers would provide extensive and complementary expertise to conduct the peer review, as documented by ICF in the Peer Review Selection Memo. EPA concurred with ICF choices and approved all selected reviewers.

2. Administering the Review and Receiving Comments

ICF composed and delivered a charge letter to the three selected reviewers to send along with the RTI Report “The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage”. The charge letter included EPA’s charge questions to the reviewers, instructions on how to complete the review, a timeline of when comments were due to ICF, and a conflict of interest (COI) form to be returned to ICF along with the comments. ICF sent these materials to each individual reviewer on May 3, 2021.

² Draft peer reviewer selection memo, REVISED (Task 1) Contract EP-C-16-020, Work Assignment No. 4-04: Peer Review of “The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage”, to Elizabeth Miller and Dana Jackson, US EPA OTAQ, from: Paola Massoli, and Sarah Lettes, ICF.

EPA then arranged and hosted a mid-review teleconference on May 6, 2021 with the selected peer reviewers and ICF. The goal of the meeting was to introduce the peer reviewers to the EPA staff and address early questions or concerns. The 1-hour meeting included an overview of the review process, background information on the Report, and a discussion on technical and practical aspects. ICF’s notes from this meeting are included as Appendix C.

ICF requested that the peer reviewers provide comments to the charge questions within two weeks. All peer reviewer comments and completed COI forms were received by May 25, 2021. ICF compiled all unedited peer review comments, charge letter information, and attachments into a peer review report. ICF organized all comments into tables so that the individual comments could be easily grouped and compared for review purposes. ICF prepared and submitted a Draft Peer Review Summary Report that assembled the unedited reviewer comments for EPA review, and delivered the draft report to EPA on June 2, 2021. EPA provided ICF with additional comments and edits, and a revised, final report was delivered to EPA on June 11, 2021.

3. Difficulties Encountered

No significant difficulties were encountered while performing this review.

III. Responses to Charge Questions

Section 1 presents an overview of the peer reviewers’ comments received on the six charge questions. This overview is followed by Section 2, which provides the direct, unedited peer reviewer responses to each of the charge questions. The unedited responses by reviewer appear in a table format. In those tables, the left column lists the EPA’s charge question, and the right column provides the reviewer’s comments.

1. Comment Overview and Summary

The following section summarizes the peer reviewers’ comments to the charge questions. The questions have been abbreviated for easier presentation. These summaries do not rewrite the responses or supersede the unedited comments presented in Section 2.

Overall, all three peer reviewers agree that the modeling analysis presented in the Report produces results consistent with the assumptions and data used for model development. This said, each reviewer had comments and constructive suggestions to improve content or to clarify the literature cited and the methodology used.

Both Dr. Langer and Dr. Sallee also provided extensive additional comments beyond those requested by the six prescribed charge questions. Those are not summarized here but are presented in their entirety in Section 2. In addition, Dr. Langer provided direct edits to the Report

draft document, which was shared with EPA; those comments are included in the Additional Comments section of the table.

1.1 Does the Presentation Describe the Data and Methods Sufficiently?

The reviewers agreed that the report presented the data and methods sufficiently. In fact, Dr. Langer called it the “best government report” that she has read.

Both Dr. Langer and Dr. Sallee noted that the explanations of each assumption, parameter, and component of the model were clear, and that the document was organized in a way that facilitated the understanding of such a complex model.

Dr. Sallee appreciated that the report explained the economic intuition underlying each component of the quantitative model. Dr. Langer noted that the report was so complete that she would have been able to code the simulation to test alternative models. Dr. Leard agreed that the report described the data and methods sufficiently. He pointed two citations that should be revisited to reflect recently updated or published papers and provided the updated citations.

1.2 Does the Report Miss Relevant Literature; Are the Interpretations of the Elasticities Defensible?

The reviewers provided mixed responses to this charge question.

Both Dr. Langer and Dr. Sallee praised the authors of the Report for having extensively reviewed and utilized information available in the literature to extract or calculate elasticities include in their model, while using good judgment on which values to include or exclude based on relevance and data quality. Dr. Langer suggested the citation of two additional studies that model the dynamic adoption of new and used cars and should be included in the report.

Dr. Sallee commented that the report included all the relevant studies of which he was aware of, but also recognized that there are only a few studies in the literature that provide suitable elasticity parameters to characterize demand for new vehicles and other consumer behaviors. He pointed out that many of these studies are dated, and that current substitutability of new and old vehicles may have changed due to changes in embedded technology (i.e., new vehicles are more durable and last longer). He also added that the elasticity of demand for new vehicles is likely smaller as consumers get wealthier, so older studies might exaggerate new-vehicle price elasticity.

Finally, he noted that a substantial share of the available studies does not focus on the elasticity parameters that are of interest in the Report. Hence, while recognizing the overall approach and interpretations of elasticities from the literature as defensible, he suggested adding a sensitivity analysis that incorporates standard errors from the original literature studies. This exercise would provide additional context on the uncertainty of the elasticity parameters used in the model.

Dr. Leard noted that the Report misses several relevant citations, while pointing out that the interpretation of one of the citations is incorrect and suggested an alternative take. Regarding the studies that Dr. Leard suggested adding, he pointed out that each of these sources would provide additional context on elasticities.

1.3 Are the Data and Assumptions Appropriate and Objectively Chosen?

The reviewers provided mixed responses to this charge question.

Dr. Langer and Dr. Sallee both found that the data, key model parameters and parameter assumptions were appropriate, reasonable, based on reliable data sources, and consistent with an objective analysis. However, Dr. Sallee posed a question on the baseline scrappage scale factors for which he suggested a minor clarification.

Conversely, Dr. Leard found several citations to be inappropriate. He expressed concerns regarding the use of certain papers to construct a range of elasticities, offered some suggestions on how those papers should be cited in other sections of the Report and shared alternative papers that could be cited instead. The concern around using certain elasticities is consistent with Dr. Sallee’s remark on the need to address the uncertainty of the elasticity parameters used in the model (see Dr. Sallee’s response to charge question 2). If the authors of the Report accept Dr. Leard’s comments on this charge question, the estimates of the market price elasticity of demand for new vehicles might have to be revised.

1.4 Are the Methods and Procedures Employed Technically Appropriate and Reasonable?

Each of the peer reviewers expressed different views regarding methods and procedures employed in the Report.

Dr. Langer had no objections and felt that the authors are clear about the assumptions of the model.

Dr. Leard raised six specific concerns with the method. Some concerns (Concerns 2, 3, 4, 6) appear to be solvable by providing additional clarifications, while others (Concerns 1 and 5) might require a more in-depth analysis of methods and assumptions. A brief summary of Concerns 1 and 5 is provided here. Concern 1 questions the modeling of vehicle ownership costs, and suggests a simplified approach to model used-car demand by considering vehicle prices or vehicle prices plus fuel costs only. This would simplify the model set up and it would be consistent with how supply is defined, which is a function of used vehicle prices. Concern 5 is related to the assumptions of price elasticities for used and new vehicles and points out that there has to be a distinction based on households’ income. Data show that new vehicles are more frequently bought by wealthy households, and that wealthy households tend to be less responsive to price than low-income ones. For this reason, Dr. Leard expects the own-price elasticities to be larger in magnitude for older vehicles.

In addition, Dr. Leard’s Concern 3 suggests that the price of a new vehicle should not be taken as given, and that it would be useful to add more details on why the new vehicle market was assumed to be perfectly competitive.

Dr. Salle provided an extensive answer to this charge question. He recognized that the model is complex and that the Report must make assumptions, including focusing the model on vehicle ownership (this is in contrast with Dr. Leard’s view on including vehicle ownership; see Concern 1 in the response to charge question 4). However, Dr. Salle expressed a concern regarding ‘the assumption about how the demand system was assumed to be structured across used vehicles of different ages’ which appears to be related some of Dr. Leard’s concerns around the treatment of elasticities associated with vehicle age(s).

Finally, Dr. Salle incorporated a qualitative analysis of the model applicability to future scenarios. For example, if the model were to be used to contemplate a transition to electric vehicles or vehicles featuring different levels of automation, attributes affected by the policy could radically change the substitution between new and used vehicles.

1.5 Does the Modeling Analysis Appear to Produce Results Consistent with the Assumptions and Data?

Regarding the results produced by RTI’s modeling analysis, all three reviewers agreed that the modeling results are consistent with the assumptions and data used in model development, and that such results appear logical. As specified by Dr. Salle, the qualitative results that emerge in the baseline model conform with theoretical expectations. Dr. Leard pointed out that it makes sense that the policy elasticities are smaller in magnitude than the new vehicle demand elasticities.

1.6 Are the Results Sensitive to the Data and Assumptions Used in Model Development; Are There Alternative Assumptions?

All three reviewers recognized the overall qualitative and quantitative value of the Report but had several recommendations in response to this charge question. Dr. Langer suggested adding a discussion to clarify the limitations of the model in predicting or representing the effect of policies that do not uniformly affect the new vehicle fleet. Dr. Salle suggested that the authors explore the role of an additional elasticity - the substitutability between vehicles of different ages – in section 7.4, which presents the core sensitivity analysis. He also notes that while it is acceptable that the Report does not feature standard errors, it would be beneficial to add a discussion covering uncertainty about key parameters and overall model uncertainty.

Finally, Dr. Leard encouraged applying a different range for the market price elasticity of demand for new vehicles. ICF noted that this might require a re-run of the simulations. Dr. Leard also recommended adding greater detail to the definition of a vehicle to make the model more suitable to performing simulations of an actual policy.

2. Comments by Reviewer

2.1 Comments by Dr. Ashley Langer

CHARGE QUESTION	COMMENTS
1. Does the presentation describe the data and methods sufficiently to allow the reader to form a general view of the quality and validity of the analysis approach?	Yes. This is the best government report I've ever read. The model is laid out clearly and is in line with the state-of-the-art research in the best economics journals. The authors are extremely clear about where each parameter is coming from and why they made the modeling assumptions that they did. I feel like after reading this report I could, with relatively little additional thought, actually code up the dynamic simulation in the paper and test alternative models. That is a feat in a technical report like this.
2. Does the report miss relevant literature in its review? Are the interpretations of the elasticities in the literature review, and the estimates of elasticities used in the model stemming from the literature review, defensible?	I think that the authors overall have gone well beyond just conducting a literature review. They have calculated elasticities using important estimates from the literature and in some cases have literally gone back to the data and code from the papers and calculated elasticities that are not recoverable from only the published version. They have also used good judgement to choose which elasticities from the literature are more likely to be applicable to their model and give the reader solid reasoning for why they made the choices they did. That said, I do think that there are a couple of papers that should probably be at least discussed. Schiraldi (2011) and Adda and Cooper (2000) both model dynamic adoption of new and used cars. They aren't based in the U.S., but they may be the closest models to the ones the authors are after here. Putting their model in the context of the dynamic car purchasing literature seems important.
3. Are the data and assumptions appropriate for the analysis conducted and objectively chosen? If not, do you know of other data or proposed alternative assumptions that might be used in this analysis?	I think that the data and assumptions of the model are appropriate and reasonable. The authors have provided extensive sensitivity testing and their take-aways from the model appear to be extremely generalizable.

4. Are the methods and procedures employed technically appropriate and reasonable? Please distinguish between cases involving reasonable disagreement in methods as opposed to cases where you conclude that current methods involve specific technical errors.	The methods and procedures are technically appropriate and reasonable. In particular, because the authors are not attempting to defend a specific point estimate, but are more attempting to lay out intuition for how policy affects equilibrium new and used vehicle adoption and pricing, I don't have any real critiques of the approach. The authors are clear about the assumptions of the model and the ways that these assumptions could be relaxed in future work.
5. Does the modeling analysis appear to produce results consistent with the assumptions and data used for model development? Do the results presented by RTI follow from the data and assumptions used in the analysis?	The results are consistent with the assumptions and data used in model development. Beyond following from the data, the results provide intuition for how the results would change with alternative modeling assumptions and the authors are very clear about which assumptions are most critical for changing the results.
6. In what ways are the results sensitive to the data and assumptions used in model development? Are there alternative assumptions and data that the researchers should consider providing improved analysis?	The authors are very careful to keep the analysis to the adoption and pricing of a single representative vehicle of each age. I do worry slightly that others who read the report may want to use these results to draw conclusions about the effect of things like fuel economy policy on the overall fuel economy of the vehicle fleet over time. I think it might make sense for the authors to add some discussion of the complications that within-vintage vehicle heterogeneity is likely to add in the real world. In particular, a policy like a CAFE standard, which penalizes fuel inefficient vehicles while subsidizing fuel efficient vehicles, will have different long-run effects on the age distribution of fuel inefficient and efficient vehicles. I see this not as something that the authors should do to improve this analysis: adding heterogeneity is very complicated as the authors point out. But I do think that it would be helpful for the authors to be fairly clear about the limits of the analysis they have conducted by explicitly saying that policies that do not uniformly affect the new vehicle fleet will have complicated effects on the long-run age distribution of vehicles.

ADDITIONAL OVERALL COMMENTS PROVIDED (NOT CHARGE QUESTION-SPECIFIC):

Overall, this is the best report of this type I have ever read. The analysis provides both intuition and a concrete path forward for future analysis. The model rests on reasonable assumptions and is very clear about how things might change under alternative assumptions. The calibration is reasonable and makes choices about which values from the literature to use that are based on the quality of the studies rather than weighting all previous work equally. The authors conduct extensive sensitivity analysis that allows the reader to fully understand which results are robust to alternative assumptions and which depend on the parameterization. Congratulations on great work!

ADDITIONAL COMMENTS BY SPECIFIC REPORT CHAPTER:

Dr. Langer also commented on specific sections of text throughout the report:

3.1, Bullet 4 ("Production of vehicles..."), which discusses long-run changes in vehicle models: Cite Knittel and coauthors here on long-run model changes? Was that paper ever published?

3.1.1.1, Paragraph 3 ("In the literature estimating..."), which discusses new-vehicle demand systems and the distinction between purchase price and net ownership costs: This paragraph was helpful for explaining what you are trying to get at.

3.1.1.2, Paragraph 2 ("Much of the literature..."), which discuss the distinction between average own-price elasticity among individual models of new vehicles and aggregate own-price elasticity: But it's also important to recognize that the identifying variation behind these estimates may not be well set up to get at these aggregate elasticities.

3.1.1.3, Paragraph 1, in reference to the sentence: "The derivatives of demand here reflect a world where (at least from the consumer's perspective) only the price of the vehicle has increased:" Single vehicle?

3.1.1.3, Paragraph 2 ("More often..."), which discusses derivatives of demand and consumer willingness to pay for vehicle attributes/regulations that change vehicle attributes: It's probably worth being clear here that these derivatives are at least theoretically possible to get from the models, if the models include the right vehicle attributes.

3.1.1.3, Paragraph 3 (starting at "A consumer might..."), which discusses the effects of fuel economy regulations on consumer demand and explains that consumers may perceive fuel economy-related gas savings differently. This doesn't need to be phrased as heterogeneity in beliefs. It could just be that some consumers driver more and others drive less so increases in fuel economy have different effects for different consumers.

3.1.1.3, Paragraph 4, in reference to the sentence: "We will refer to the combined change in perceived cost at the time of purchase as a 'generalized cost:" Shouldn't this be something

like the compensating variation? Is it really "perceived cost"? and it's clearly heterogeneous, so it's likely the mean change in perceived cost?

3.1.2, Paragraph 1, in reference to the sentence: "If assuming perfect competition, supply will in fact be perfectly elastic." But we wouldn't do this in the car industry.

3.1.2, Paragraph 1, in reference to the sentence: "However, the presence of a used market interacts with the equilibrium system and the new-vehicle demand elasticity is no longer sufficient to measure the effect of a policy, even when new vehicle supply is perfectly elastic." Related to the above, I would be stronger here that supply isn't perfectly elastic, even in the long run.

3.1.2, Paragraph 2, in reference to the sentence: "When a particular used model's price rises, more of that model become available (e.g., because scrap dealers, insurance companies, and mechanics decide to repair and sell more of them as vehicles instead of as scrap metal)." Owners don't also respond? Trade in vehicles rather than keeping them as an extra car for a child or relative?

3.1.2, Paragraph 3, which discusses scrappage functions. In response to the phrase "vehicle scrappage depends only on own price of vehicles." It should depend on the price of other vehicles to the extent that parts for repairs are interchangeable and supply curves for those parts slope up.

3.1.2, Paragraph 4, in reference to the sentence: "Empirical estimates will typically be presented as a derivative or elasticity of this function, rather than of the density of underlying shocks that determines the function." Derivative of the density, no?

3.1.3, Paragraph 1, which discusses how the presence of equilibrium effects in observational data may lead to biased estimates." Would this be clearer if it were framed as omitted variables bias? So there are omitted variables that are correlated with both the likelihood of scrappage and the cost of repair?

3.1.3, Paragraph 3, which discusses the effects of new-vehicle prices on scrap rates of used vehicles, in reference to the phrase: "a downward shock to the scrap rate will create more competition for new versions of a vehicle (because there are now more used ones entering the market)." Remaining in the market, not entering the market.

3.1.3, Paragraph 3, which discusses the effects of new-vehicle prices on scrap rates of used vehicles, in reference to the phrase: "Estimating the reduced form successfully would require good quasi-experimental variation in new-vehicle prices, but the likely confounders and long time series required have meant that the literature has been unable to find many suitable settings." And is this even the right elasticity? As the authors say above, this is only for a price change for a single vehicle, and the cross-price elasticities are likely to matter because used car prices are determined in equilibrium.

3.2.1, Paragraph 3, in reference to the question: "How do the effects on the composition of the vehicle inventory relate to the initial policy goals?" It seems like somewhere here there also needs to be recognition that it's not just one "substitution" effect to used cars but that it will depend a lot on which vehicles the new car buyers are substituting toward, and how substitution is happening within the used car market (so 20 year old vehicles may still be

scrapped at high rates, but there may be much less scrappage of 10 year old vehicles and this changes the policy impacts).

3.2.1, Paragraph 3, in reference to the question: "For the group of consumers that substitutes from new to used, how much does this substitution drive up equilibrium used-vehicle prices?" And WHICH used vehicle prices?

3.2.2, Paragraph 6, which starts with "A dynamic model..." in reference to the sentence "Assuming the policy shock is long-lived the effects on used-vehicle prices and scrappage evolve, likely strengthening, over time." Why? Not sure it's not true, but not completely obvious why it is. Couldn't producers evolve/adapt to weaken the effects?

4.1, Paragraph 3, in reference to the sentence "The fact that the elasticities differ in this way is entirely consistent with economic theory; more disaggregated choice sets (in terms of attributes, time of purchase, etc.) mean that the best substitutes for any given good within the choice set will be more similar to it (e.g., more substitution would be expected between sedan models or between makes of sedan than between new and used vehicles or between a personal passenger vehicle and alternative means of transportation)." This is not exactly how I would think about the issue. Substitution happens for many reasons. In particular, since people are heterogeneous, having more products means that there are products that are similar to a vehicle on different dimensions, which means that more heterogeneous people can be on the margin between buying this vehicle and another.

4.2, Table 4-2: List of Papers Included and Parameter Estimates Provided. Busse, Knittel, and Zettlemeyer doesn't have any relevant elasticities? I guess everything that they're looking at is responses to gas prices rather than responses to price changes, but isn't that central to their generalized price measure?

5.1, in reference to this sentence about Table 5-1: "The elasticities in the first panel hold constant most other aspects of the equilibrium system, including substitution possibilities to used vehicles." What does this mean, exactly? Are you just saying that the attributes of used vehicles are held constant? Not sure what a substitution "possibility" means.

5.1.1, which discusses aggregate own-price elasticity of demand for new vehicles with respect to the price of new vehicles: I feel like somewhere in here there needs to be more discussion of the fact that this is not necessarily all substitution to used vehicles but also includes substitution to fewer total vehicles (either not traveling at all, using a used vehicle more, or switching to bike/transit/etc.). But maybe I missed that elsewhere?

5.1.1, Paragraph 1, in reference to the sentence: "In addition, we made a simple assumption regarding average model-level elasticities and average substitution to the outside good." This is a little confusing, but I think is saying that everything should technically be share-weighted averages instead of simple averages, right?

5.1.1, Paragraph 4, which starts with "Barry, Levinson..." noted that it should say "(2004)" after Pakes in the last sentence.

5.1.3, Paragraph 3, in reference to the first sentence: "As expected given the relationship between the new- and used-vehicle markets, the available elasticities of new-vehicle demand when used-vehicle prices can adjust (-0.18, -0.36) indicate less responsiveness." What

timeframe should we think of these elasticities as being over? In BLP, it's very clearly a year. But here are they longer-term? Presumably not super long-term though?

5.2.2, Paragraph 1, in reference to a sentence about estimates of own-price elasticity for used vehicles, "Their estimate of own-price elasticity for used vehicles ranged from -0.75 to -1.93 with a mean of -1.23 ." Aggregate own price elasticity?

5.2.2, Paragraph 2, in reference to the sentence "Bento et al. (2009) also reported model-level price elasticities among used vehicles: the average vehicle-level elasticity among all used vehicles was -0.54 ." I guess I'm a bit confused about what a used vehicle own-price elasticity is given that there are consumers on both the supply and demand sides. Given that there are lots of substitutes I would expect an own-price elasticity of a used vehicle to be well above 1 in magnitude. But if the seller is basically going to sell the car one way or another regardless, then does that pull down the elasticity? So if prices go up, the supply of used vehicles goes up and the demand goes down so the number of vehicles TRANSACTED doesn't change? Is there a way to provide some clarity on this?

5.2.4, but seems to be in reference to section 5.2 as a whole: Does Schiraldi provide an estimate that is useful? Adda and Cooper? They're not on US data, but it still seems like they're informative.

5.3.1, which discusses the elasticity of aggregate scrappage with respect to the average price of used vehicles, in reference to the sentence: "In Hahn's (1995) Table 2, he relates an exogenous "bounty" (subsidy to scrappage) to changes in the absolute number of vehicles scrapped relative to a baseline." Timeframe seems important to mention here. Short-run, I assume?

5.3.1, Paragraph 3, in reference to the sentence: An important caveat of studies looking at a bounty for scrappage lies in the temporary nature of the program: it may be that a short-lived, salient subsidy will quickly harvest a stock of "almost-scrapped" vehicles, overstating the elasticity. Yes, maybe just move this up front in the discussion?

5.4, Table 5-4. "Time Frame of Data Used" is hard to read.

Section 6 (overarching statement about the section): I get that this is sort of an obnoxious comment, but I could have used more of a road-map up front on this section. For instance, it wasn't completely clear up front that the price vector is sort of secondary and just guarantees equilibrium in the system of equations. It's obvious after reading through, but being a little bit clearer about how this section would be laid out would have been helpful.

6.1, in reference to " q_a^D number of vehicles of age a demanded" (as well as the notation for number of vehicles of age a supplied). Demanded in transactions or overall? Ditto with supplied.

6.2, Paragraph 1, in response to the sentence "The elasticities for individual vehicles measured by this approach do not provide clear signals about an elasticity of demand for new vehicles in the aggregate, because the outside good for individual vehicles includes other new vehicle models; thus, demand for an individual model is likely to be much more elastic than demand for a generic new vehicle." I get what you're saying here, but wouldn't it be clearer to just say "the alternative choice for each individual vehicle includes other new vehicle models and the outside good"?

6.2, Paragraph 2, which starts with "We assumed..." and discusses assumptions about the demand system and the electrification of the vehicle market. Good discussion in this whole paragraph.

6.4, Paragraph 5, which starts with "Importantly, supply and demand..." Related to an earlier comment: this paragraph is super helpful for understanding how everything is coming together and more of this earlier in the section would be helpful to the reader.

6.4.1, Paragraph 3, in reference to the sentence "Because scrappage occurs when repair costs exceed the value of the vehicle the form of the scrap function (constant elasticity) determines the form of the repair cost density." Isn't this backwards? The repair cost density is the primitive and the scrap function follows from it? I appreciate that you are putting an assumption on the scrap function rather than the repair cost density though.

6.6, Paragraph 6 about β_a (Share of aggregate spending on each vehicle age), in reference to the sentence "Expenditure shares come from combining the age profile above with data on the cost of vehicles of different ages." I'm fairly confident that you explained this somewhere and I'm just missing it, but why isn't β_a just calculated in the model from the equilibrium prices and quantities? Why does it need to be treated as an exogenous parameter? It seems very odd given the simulation exercise?

6.6, Figure 6-2: Baseline Price Profile by Age. I think that the "baseline" here just means that you use it for the expenditure shares and then update it in the simulation, yes? A clearer title would be helpful.

7.1, Paragraph 2, which discusses Table 7-1: Demonstration of Channels of Adjustment in Quantities and Prices When Generalized Cost of New Vehicles Rises by 1%, in reference to the sentence "The 1% increase in generalized cost measures the strength of the policy, hence the term "policy elasticity." Impact? I'm not really sure what "strength" means here.

7.1, Table 7-1, in reference to the Cross-Price New/Used column: Maybe I missed this earlier, but this is the elasticity to all ages of used vehicles? I get that this might make sense since new buyers switch to newer used vehicles and those buyers switch to older used vehicles and it all works its way through the system, but being explicit about this would probably be helpful.

7.2, Paragraph 5, in reference to the first sentence: "To complete the setting, note that the demand system contains 30 ages, but so far we have only specified three demand elasticities." Ok, this makes sense re: my above comment. A quick sentence about this earlier where you talk about assumptions would be helpful.

7.4, Paragraph 3, which describes Table 7-2: Policy Elasticities Corresponding to Selected Demand and Scrappage Elasticities, in reference to the sentence: "The first panel (rows A through E) holds the scrappage elasticity fixed at a value of -0.7 (Jacobsen and van Benthem [2015] and also close to the median of values in Table 5-3)." Rows A through E: Not labeled.

7.4, Paragraph 3, which describes Table 7-2: Policy Elasticities Corresponding to Selected Demand and Scrappage Elasticities, in reference to the sentence: "The second panel (rows F through I) explores changes in this elasticity, spanning most of the range in the literature with values of -0.2 to -1.2)." No second panel.

7.4, Paragraph 4, in reference to the sentences: "Scenarios A through C explore an increase in substitution to the outside good. As we saw in Table 7-1, this is one of the most important elasticities in determining the effect of policy on new-vehicle sales." It's probably worth being really clear early in the report that this is critical and perhaps the worst-identified parameter in the modern discrete choice literature.

7.4, Paragraph 11, which starts with "Figure 7-1 displays..." in reference to the sentence "When the scrap elasticity is -0.7 on the horizontal axis, the values on the Y axis reflect the policy elasticities as shown in Table 7-2 for Scenario D." Instead of Y, vertical. Or X instead of horizontal [for parallel sentence structure].

8.3, Paragraph 3, referring to the sentence "For instance, there is some empirical evidence of increased buying in advance of regulatory changes going into effect, once those changes have been announced." Do you want cites on these types of statements?

9.1, Paragraph 2, referring to the sentence "This is a small-scale version of changes in real estate prices that can occur even when the announced change is decades away." Exactly what Holland, Mansur, and Yates get for electric vehicle mandates in their recent AEJ.

9.1, Paragraph 2, referring to the word "sales" in the last sentence, "Preannouncement has no effect on the long-run outcome: sales converge to the 0.25% decline in a little over 5 years." New vehicle sales. Just being clear that the changes in the used vehicle market take longer to get worked out (I assume).

2.2 Comments by Dr. Benjamin Leard

CHARGE QUESTION	COMMENTS
1. Does the presentation describe the data and methods sufficiently to allow the reader to form a general view of the quality and validity of the analysis approach?	In general, yes. The document could cite a few citations that have either been updated or published. Citation 1: Dou and Linn (2018), which was a working paper, is now a published paper appearing in the <i>Journal of Environmental Economics and Management</i>. The citation is Dou, X. and J. Linn (2020). How do US passenger vehicle fuel economy standards affect new vehicle purchases? <i>Journal of Environmental Economics and Management</i>. 102: 1-21. Citation 2: Leard (2021) is an updated version of Leard (2020). The current version is available at https://media.rff.org/documents/WP_19-01_rev_2021.pdf. The market price elasticity of demand estimate from the most recent version is -0.37.
2. Does the report miss relevant literature in its review? Are the interpretations of the elasticities in the literature review, and the estimates of elasticities used in the model stemming from the literature review, defensible?	Yes, the report misses several relevant citations. Citation 1: James A. Stock et al. Comment on Proposed SAFE Rule, EPA-HQ-OAR-2018-0283-6220 (October 26, 2018). The Stock et al. comment on the proposed SAFE Rule does not report elasticities, but new vehicle market price elasticities in the range of -0.03 to -0.09 can be calculated from their results. The Stock et al. comment also includes an important correction of a spreadsheet error in the Notice of Proposed Rulemaking, resulting in a revised elasticity estimate for that model of -0.07. It is worth mentioning how this citation is different from other papers such as Leard (2021). My understanding of the paper is that it has a model that accounts for general equilibrium effects. It might be useful to include a new column in Table 5-1 that has a binary yes-no variable “Includes general equilibrium effects” since the point of the current document is to account for these effects. Citation 2: The report. Sean P. McAlinden et al., <i>The Potential Effects of the 2017-2025 EPA/NHTSA GHG/Fuel</i>

	<i>Economy Mandates of the US Economy</i>, Center for Automotive Research, 27 (Sept. 2016), https://www.cargroup.org/wp-content/uploads/2017/02/The-Potential-Effects-of-the-2017-2025-EPANHTSA-GHGFuel-Economy-Mandates-on-the-US-Economy.pdf This is a report that estimates the long-run market-price elasticity of demand for new vehicles to be -0.61. The interpretation of one of the citations is incorrect: - Fischer, Harrington, and Parry (2007): This study finds an implied new vehicle market price elasticity of demand equal to -1 by reporting separate elasticities for cars and light trucks. This ignores substitution between the two vehicle categories. This paper also reports a long-run elasticity for the combined market for new motor vehicles, finding it to be -0.36. This value is reported in the bottom panel of Table 5-1. I am puzzled why the -1 value is reported in the top panel when the combined effect is what is relevant here.
3. Are the data and assumptions appropriate for the analysis conducted and objectively chosen? If not, do you know of other data or proposed alternative assumptions that might be used in this analysis?	Certain papers that report new vehicle market price elasticities are, in my opinion, inappropriate to cite and use to construct a range of elasticities. The following papers cited in Table 5-1 are inappropriate: Berry et al. (2004): This study <i>assumes</i> a new vehicle market price elasticity of demand for new vehicles equal to -0.4 (in addition to model where they assume it is equal to -1). This elasticity is not estimated in this paper. Therefore, it is inappropriate to cite. Fischer, Harrington, and Parry (2007): This study finds an implied new vehicle market price elasticity of demand based on model simulations, not estimated from data. Generally, these types of "calibration" results should not be included with other studies that estimate the elasticity. BLP (1995), Goldberg (1998), Bento et al. (2009), Knittel and Metaxoglou (2014), Dou and Linn (2020): My issue with citing these papers is that none of them focus on properly estimating the market price elasticity of demand for new vehicles. None of them exploit proper statistical variation necessary to identify this parameter. Take, for

	example, BLP (1995). This study uses a panel of aggregate sales and price data for vehicle models sold during 1971-1990. The study is almost exclusively focused on obtaining unbiased estimates of the model-specific own-price elasticity of demand (e.g., Toyota Camry). One small part of the paper discusses the implied market-price elasticity of demand. As discussed in Leard (2021), this elasticity is a function of the own-price elasticity and the propensity of new vehicle buyers to leave the new vehicle market. It is not clear how BLP (1995) identify this propensity (they have no discussion of this). BLP (1995) should instead be cited when discussing the model level own-price elasticity of demand, since this is the focus of the paper. The other papers – Goldberg (1998), Bento et al. (2009), Knittel and Metaxoglou (2014), Dou and Linn (2020) – also have alternative focuses and do not discuss much at all how the market price elasticity of demand is identified from the data. Therefore, these papers should not be cited to construct a plausible range of values for the market price elasticity of demand. A paper like Bento et al. (2009), for example, which focuses on the link between vehicle choice and VMT choice, should be cited when constructing a plausible range for the VMT elasticity.
4. Are the methods and procedures employed technically appropriate and reasonable? Please distinguish between cases involving reasonable disagreement in methods as opposed to cases where you conclude that current methods involve specific technical errors.	I have a few concerns with the methods. Concern 1: The modeling of vehicle ownership costs (r) and prices (p) is unclear to me. Why is it necessary to model vehicle ownership costs in the first place? Why not just model vehicle prices? The discussion in 6.4.1 seems to be motivated from this distinction. But it is not clear why the model cannot be simplified to only have vehicle prices or vehicle prices plus fuel costs. Ownership costs appear in the vehicle demand function equation (6 – 1). The authors motivate using ownership costs with the following: “Modeling ownership cost as the variable of interest to the consumer follows the approach in Bento et al. (2009) and

	is important when allowing choices to be made between new and used vehicles.” Bento et al. (2009) define ownership costs as the sum of fuel costs and a rental rate, where the rental rate is defined as the sum of - Foregone return of the real value of the car, which is proportional to the used vehicle price. - Depreciation, which is typically proportional to the used vehicle price (e.g., a typical used vehicle depreciation rate is often cited to be 15 to 20% of the value of a used vehicle). - Insurance and registration, which are generally tiny relative to the first two components. Given these features, it seems straightforward to model used car demand as a function of vehicle prices and fuel costs only. This would simplify the model set up and it would be consistent with how supply is defined, which is a function of used vehicle prices. Concern 2: It is unclear how a vehicle is defined in the model. Is a vehicle defined by age only? So the model has 31 vehicles: new, 1 year old, 2 years old, ..., 30 years old. This should be more explicitly stated in the first paragraph of Section 6.2. Concern 3: Section 6.3.1 New Vehicles states that the price of a new vehicle is taken as given. Is it equivalent to say that the new vehicle market is perfectly competitive? This is in contrast to most of the vehicle market literature (Bento et al. 2009 for example), which models the new vehicle market is imperfectly competitive. It would be useful to include more details on this assumption, why it was adopted, etc. Concern 4: In Section 6.6, the authors mention that they explore a range of the demand elasticities θ_{aa} and θ_{Na}. It would be useful to provide a table of elasticities assumed. I realize this table is presented in Table 7-2, but it would be constructive to include a table in Section 6.6 with discussion defending the choice of elasticities.
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	Concern 5: The authors assume that the own-price elasticities for used vehicles is identical to the new-vehicle elasticity. This is problematic because of how households sort into different vehicle ages. New vehicles are more frequently bought by wealthy households, and a lot of literature has shown that wealthy households tend to be less responsive to price than average or low income households. For this reason, I would expect the own price elasticities to be larger in magnitude for older vehicles. Concern 6: At the end of Section 7.2, the authors state that "Cross-price demand elasticities between individual ages are set to fall at 8% per year of difference in age." This seems reasonable, but it is not clear where 8% comes from. Is this based on results from a demand system such as Bento et al. (2009)?
5. Does the modeling analysis appear to produce results consistent with the assumptions and data used for model development? Do the results presented by RTI follow from the data and assumptions used in the analysis?	Yes. It is clear that the policy elasticities are smaller in magnitude than the new vehicle demand elasticities, which makes sense given the equilibrium response.
6. In what ways are the results sensitive to the data and assumptions used in model development? Are there alternative assumptions and data that the researchers should consider providing improved analysis?	Given my comments above, I suggest applying a different range for the market price elasticity of demand for new vehicles. I would also consider adding greater detail to the definition of a vehicle. It seems to me that part of the value of this exercise is to simulate the effects of various policies (for example, CAFE standards). The current structure of the model has a highly aggregated vehicle definition that seems lacking for performing simulations of an actual policy. At a minimum, it would be useful to provide a discussion on how the model could practically be expanded, and the potential calibration and computational challenges involved with such a task.
ADDITIONAL OVERALL COMMENTS PROVIDED (NOT CHARGE QUESTION-SPECIFIC):	
ADDITIONAL COMMENTS BY SPECIFIC REPORT CHAPTER:	

2.3 Comments by Dr. James Sallee

CHARGE QUESTION	COMMENTS
1. Does the presentation describe the data and methods sufficiently to allow the reader to form a general view of the quality and validity of the analysis approach?	The report is clear. Data and assumptions are documented well. Assumptions and modeling choices are explained thoroughly. Moreover, the results of the analysis are well explained, and every effort is taken to provide basic economic intuition that gives context to the quantitative model output. As a result, readers should be fully able to evaluate the merits of the model and understand both its contributions and its limitations from the information contained in the report. The report lays out the main analysis in a sequence of steps that ensure clarity by adding complexities and features one at a time (e.g., section 7.1). The report also has many sections that seek to consolidate results and illustrate the underlying intuition of the results (e.g., the summary in section 7.6). Altogether, the report explains a complex economic model in a concise and effective way. This is not easy to do, and the authors of the report should be commended for the quality of the exposition.
2. Does the report miss relevant literature in its review? Are the interpretations of the elasticities in the literature review, and the estimates of elasticities used in the model stemming from the literature review, defensible?	The report does an admirable job of surveying the literature and putting the information available from past research into context. I am not aware of other studies that should have been included in the literature review. As the report notes at several points, there are many studies of the automobile market, but attention to the elasticities pivotal for the current analysis are rarely the focus and are often not even calculated. Thus, the report's finding that few existing papers can be used to calculate the key elasticities conforms with my understanding of the literature. All interpretations of the elasticities offered in the report appear appropriate. The use of elasticities from the literature to inform the model is certainly defensible.

	The report explains clearly why the existing literature offers limited guidance on key values. It therefore emphasizes strongly the need for sensitivity analysis, which is then provided. This is the appropriate stance to take. If anything, I would recommend even more caution in relying on the existing literature (and hence elevate even further the critical role of sensitivity analysis) for two reasons, both of which are noted in the report itself, but I note here for emphasis. First, many of the studies are more than a decade old. Mechanically, vehicles are becoming more durable and lasting longer (this is noted in section 2). At the same time, embedded technology is also changing rapidly, which might make newer and older vehicles less close substitutes. Both factors imply that empirical relationships observed in the automobile more than a decade in the past may be misleading indicators of the vehicle market of the future. Moreover, one might suspect that the elasticity of demand for new vehicles shrinks as consumers get richer. If true, older studies might exaggerate the new vehicle price elasticity. (Some interaction between income and price sensitivity is often estimated in prior studies. Perhaps there is some information in those that would suggest how large might be any trend in the elasticity driven by economic growth. The report does not mention this.) Second, a substantial share of the available studies do not focus on the elasticity parameters that are of interest here. In several cases the authors of the report had to construct an estimate that was not even reported in the original research. This is relevant because researchers have discretion in how to construct and estimate these models. When a parameter (like the substitution to the outside good) is viewed as a byproduct of estimation, the original researchers may not have exerted maximal effort in ensuring that the estimation was reliable. The report does not discuss standard errors in the original studies. Instead it uses the point estimates from prior studies to form a qualitative sense of the range of
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	estimates across papers. This is a defensible approach, especially because the standard errors are likely calculated with different methods across studies and are thus not always comparable. Even so, where available, it might be useful to have standards errors from the original studies included in the review to more fully characterize uncertainty.
3. Are the data and assumptions appropriate for the analysis conducted and objectively chosen? If not, do you know of other data or proposed alternative assumptions that might be used in this analysis?	The data and key model parameters used in the baseline analysis and the various scenarios appear appropriate. The data sources cited are all reliable and would be considered standard, objective sources. I do not know of any alternative data sources that I believe to be superior. Likewise, the parameter assumptions are appropriate and appear consistent with an objective analysis. (In answering this question, I interpret “assumptions” to refer specifically to parameters used in the model, rather than assumptions about the structure of the model or analysis, which I interpret as falling under the definition of “methods and procedures” addressed in the next question. Essentially, this answer is about section 6.6 of the report.) The baseline scrappage scale factors (b_a in the notation of the model) are said to simply be “calculated using baseline vehicle survival probabilities.” The relevant paragraph (p. 6-10) adequately explains the source of data, but I was uncertain of whether there was a degree of modeler discretion here, or if each b_a is simply an algebraic calculation based on equation 6-5, where both s_a and p_a are raw data inputs. This is a minor detail that could be easily clarified.
4. Are the methods and procedures employed technically appropriate and reasonable? Please distinguish between cases involving reasonable disagreement in methods as opposed to cases where you conclude that current methods involve specific technical errors.	The methods and procedures developed and deployed in the report are technically appropriate and reasonable. The model is complex: it deals with thirty vehicle ages while incorporating forward looking expectations. To maintain tractability, it must make simplifications—there is a single composite vehicle model; consumer heterogeneity is not explicated; forward looking beliefs are fully rational; new vehicle supply is perfectly elastic; the model abstracts from the choice of miles traveled; and, there are no income effects.

	Those assumptions are justified and reasonable. They serve the interest of transparency and clarity, and they are all appealing as a benchmark case. The report is transparent and clear about assumptions and offers caveats where appropriate. For instance, the analysis is built on a model of vehicle purchase, rather than vehicle use. As noted in the report, it is difficult to see exactly how incorporating changes in mileage in response to vehicle prices might play out in the full equilibrium. This decision could perhaps be added to the model at some future date, but even then, the key parameters (e.g., how vehicle price affects mileage, presumably through a change in depreciation) are not well estimated in the literature and thus would require additional conjecture. As such, a model focused on ownership seems appropriate. The report offers a generalized interpretation of a cost shock to new vehicles. It suggests that a regulatory change that creates a combination of changes in price and attributes can be interpreted as a tax equal to the “net utility cost.” This is sensible and appealing as a modeling approach. (It is also very important because new vehicle regulations create just this sort of collection of changes simultaneously.) That said, a minor caveat would seem to be that a change in other attributes (as opposed to just a change in price) might change the substitutability between new and used vehicles; that is, a regulation that changes the attributes of new vehicles might change the theta terms in the demand system. This is likely to have a minimal impact in the current setup. But, it could become a more important issue in the future if the model is used to contemplate a transition to electric vehicles or vehicles featuring different levels of automation, in which case attributes affected by the policy could radically change the substitution between new and used vehicles. The report assumes full pass through of cost shocks to consumers: because new vehicle supply is assumed to be perfectly elastic, all regulatory burdens are borne by
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	consumers. The report argues that this is a sensible interpretation for the long run, and it is indeed the most appealing benchmark. That said, I am not certain that we would expect full pass through of costs in a market with imperfect competition. It is perhaps useful then to contemplate how the results would be different if part of a regulatory cost increase is borne by new vehicle sellers, and part is passed on to consumers. Is it possible to simply interpret the cost shock in the model as the portion passed on to consumers? The only method and procedure in the modeling analysis that I had trouble understanding was the assumption about how the demand system was assumed to be structured across used vehicles of different ages. The text describes the assumptions deployed in an intuitive (and appealing) way, but it does not write out algebraic explanations. Furthermore, in some places, the report offers as intuition that older vehicles are less close substitutes for newer vehicles, which is meant to rationalize several patterns. I struggled to follow this reasoning because I could equally imagine a world in which there was only substitution between adjacent ages (e.g., the only cross-price elasticity for a 3-year old car is between 2- and 4-year old cars), but nevertheless a price shock to new vehicles cascades through the entire distribution of ages.
5. Does the modeling analysis appear to produce results consistent with the assumptions and data used for model development? Do the results presented by RTI follow from the data and assumptions used in the analysis?	The modeling analysis produces results that appear to follow logically from the assumptions and data. As a peer reviewer, I did not attempt to replicate any of the findings or inspect the model code that executes the simulations (nor was I given the material to do so). Instead, my judgment that the results reported follow from the data, model, and assumptions is based on the fact that the pattern of results are consistent with the economic theory that the analysis purports to embody. The qualitative results that emerge in the baseline model, and the difference across the nine scenarios described in section 7.4 (as well as the additional versions in the appendix), conform with theoretical expectations. These results, and the economic reasoning that aligns with them, are described clearly in the report. I found that discussion sound and convincing.
6. In what ways are the results sensitive to the data and	The report explores the sensitivity of key results by generating simulations based on a range of parametric

assumptions used in model development? Are there alternative assumptions and data that the researchers should consider providing improved analysis?	assumptions that span the range of elasticities from the literature. While it is always possible to perform more sensitivity analysis (and future users will apparently be able to do so on their own, which is a great feature of the project), the current report does a nice job overall of showing the likely range of key results. Quantitative values of course vary across scenarios, but several key findings, including the policy elasticity being significantly smaller than the new vehicle elasticity and the overall fleet size shrinking in response to a new vehicle price increase, appear to be robust to a range of inputs and assumptions. This is reassuring and seems to suggest that key qualitative insights that can inform policy are very likely to hold. Section 7.4 presents the core sensitivity analysis by running a variety of scenarios. This section emphasizes variation in three parameters: the new vehicle elasticity, overall substitution to the outside good, and the scrap elasticity. But, it seems there is a fourth crucial elasticity, which is the substitutability between vehicles of different ages. The appendix provides a couple of alternative model runs that seem to address this point. The discussion around these scenarios is quite concise, and as a result I am not fully certain I understand how to interpret those results. The appendix results seem to suggest that key model results are not sensitive to big changes in these assumptions. The report does not feature standard errors. Instead of focusing on standard errors, the report focuses on alternative scenarios. This is a sensible approach, but it does open the possibility that some users may underestimate (or overestimate) uncertainty. To help future users, it might be useful to include a discussion about uncertainty in one section, starting with a bullet point of sources of uncertainty in the results, with some guidance as to what the authors believe are the biggest sources. These would include both uncertainty about key parameters, which might be amenable to some statistical
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	characterization, and model uncertainties, which might be harder to quantify. At a few points, the report simply states that the model results are robust to differences in data inputs. This includes reference to vehicle market data used in a paper by Leard and one by Jacobsen and van Benthem, rather than the AEO report that is the main data source. I fully believe this is true, but it might be useful to report those robustness checks somewhere.
ADDITIONAL OVERALL COMMENTS PROVIDED (NOT CHARGE QUESTION-SPECIFIC):	
The analysis described in this report is a significant step forward and represents a valuable tool for improving regulatory analysis of the car market. Attempts to incorporate interactions between new vehicle regulations and the used vehicle market that are based on statistical relationships between new vehicle prices and the used and new vehicle markets studied separately are inadequate grounds for policy analysis and highly likely to be misleading. In essence, this model studies long-run general equilibrium interactions, something that is notoriously difficult to study using observed market data. Thus, an understanding of how interactions between new and used vehicle markets dampen, amplify or propagate the effects of new vehicle regulations requires an integrated model that provides internally consistent effects constrained by economic theory. The analysis in this report does exactly that, which is why it is so valuable. Specifically, this new tool should foster improved analysis of the ways in which used vehicle market interactions may alter the net impact of new vehicle regulations on consumers and the environment. The model described in the report identifies key mechanisms that govern the relationship between new vehicle costs and the used vehicle market. There is substantial uncertainty about key parameters, so care should be taken in interpreting the quantitative output of the model (this is also true, of course, of other models that underly regulatory analysis of automobiles), but the analysis here reveals that certain facts are robust (e.g., the policy elasticity is smaller than the new vehicle demand elasticity). Moreover, it provides a tool that can be used to assess the plausibility of different outcomes (e.g., the total fleet size can increase in response to a new vehicle cost increase only under implausibly limited substitution to the outside good). A central contribution of the report is to highlight the difference between a new vehicle demand elasticity and what the report refers to as the policy elasticity, the latter of which takes into account the feedback of used vehicle prices into the demand for new vehicles following a cost shock to new vehicles. The analysis demonstrates that the policy elasticity is consistently smaller than the demand elasticity, and that the magnitude of the gap between the two is of importance. This is a critical insight, and it conforms with economic theory. The report explains this relationship effectively, and it provides a tool that users can customize to	

perform sensitivity tests and additional analyses.

This tool might become even more useful in the future if subsequent studies provide new information about the key parameters that serve as inputs to the model. Having run many iterations of the model (presumably many more than are presented in the report), the authors likely have insights about the most important parameter choices and modeling decisions. A possible addition to the report would be a prioritized list that emphasizes where additional estimates from future research, or additional sensitivity analysis from future users, would provide the most valuable new information

ADDITIONAL COMMENTS BY SPECIFIC REPORT CHAPTER:

Included here are a series of comments, many of which are quite minor expositional notes, ordered with respect to each section of the report.

Section 3

This section provides a valuable discussion of the key economic forces that link the new and used vehicle markets and the role of scrappage. I suspect that it would be possible to illustrate these relationships further by using a pair of simple supply and demand diagrams to illustrate the steady-state (long-run equilibrium) effects.

Initially, a new vehicle cost increase shifts up the supply curve in the new vehicle market. The magnitude of the impact depends on the elasticity of demand. The change in the new vehicle market impacts used vehicle supply (shifted down because there are fewer new cars to become used) and used vehicle demand (more so as buyers view used and new as close substitutes), resulting in a new price and quantity. The size of this change depends on the scrap elasticity, which determines the slope of used vehicle supply. The resulting change in used vehicle price then shifts the new vehicle demand curve, resulting in a new quantity.

The role that each of the key elasticities places could be labeled in a heuristic diagram of these two markets.

Analytical expressions that show the simple relationship between key elasticities would also be helpful. This is not feasible in the full model with 30 vintages of used vehicles, but a future modeling exercise that reduced the used vehicle market to a single composite might make it possible to characterize the key comparative statics, showing how they depend on the handful of key elasticities.

Section 5

The literature synthesis is effective and clear overall.

The report offers a rich discussion of some of the studies, but in a few cases the results of prior studies are reported without a lot of commentary about their reliability or applicability to the purpose at hand.

For example, my impression is that authors of papers in the tradition of Berry, Levinson and Pakes (1995) are relatively skeptical of the reliability of their own estimates of the elasticity of substitution to the outside good, relative to the other estimates in those systems. I gather that the magnitude of this elasticity can scale with assumptions made by the researcher about what fraction of consumers are shopping for a car in a given period, something which is nebulous and often chosen without hard data. This is not to suggest that there are better

estimates available, but rather to emphasize our relative ignorance about the market level elasticities that are central to the analysis here.

In some cases, the authors of the report had to do additional calculations to extract an elasticity estimate from a prior paper. To the extent possible, it would be useful to have those steps explicated somewhere, for future reference. (For example, I'm not sure that someone else can replicate the author's interpretation of the elasticity in the Hahn study, or replicate the new calculations from Bento et al. 2009.)

Section 6

It might be helpful to include units (for the prices) in Figure 6-2.

The demand system has $(A+2)(A+1)/2$ substitution elasticities, where A is 30 in the present analysis. This is a large number. The calibration feeds in basically three elasticities. All of the rest are filled in via assumption about the structure of cross-price elasticities through the age structure. This assumption (described in sections 7-2 and 7-3, in particular footnote 21) appears reasonable and is an appealing solution to the problem that no estimates of this age substitution pattern exists. But, I was not confident that I could fully reconstruct the age structure of substitution from the description in the text. A small, helpful step would be to add algebraic expressions detailing this procedure and commenting further on how interested users might conduct additional sensitivity analysis (noting that some is provided in the appendix).

Section 7

Table 7-1 (and several related tables) do not label the main results as the "policy elasticity." This term is emphasized in the text, but it is not called out in the tables. Adding it might be helpful.

It might be helpful to also include some measure of price changes (for used cars this may be an index across ages) in Table 7-2. The price changes are not the object of interest, but they are a key channel, so including them might help the reader.

Figure 7-1 should include somewhere (title or note) that the new vehicle elasticity is -0.8 because the main point of the graph is that the policy elasticity is substantially smaller. This is not apparent without referring back to a prior table.

Section 10

Scrappage plays a key role in the analysis, but scrappage can mean either that a vehicle is exported from the United States or that it is fully decommissioned. The paper does not mention this distinction. It is standard practice for regulatory analysis to focus on emissions in the US fleet, and as such it is logical to ignore this distinction in the present analysis. The distinction is, however, relevant to the true greenhouse gas emissions implications of new standards. This might be a subject of future work.

The model built here is free of transaction costs. In reality, transaction costs associated with buying and selling vehicles are substantial. This is unlikely to matter for analysis of long-run effects, but it could have an impact on transition dynamics. Modeling transaction costs is certainly beyond the scope of the current report, but it might be useful to speculate as to whether transaction costs would amplify or dampen the main effects during a transition.

Appendices

It might be helpful to add further labels to Figure D-1, which is showing two completely different model runs with separate assumptions about expectations, rather than two sets of results that come out of the same model run.

Some of the figures in appendix D label time just as “year” instead of the more helpful “years since policy effective date.”

Appendix A: Resumes of Selected Reviewers

1. Ashley Langer

Ashley Langer

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Appointments:

- Assistant Professor, Department of Economics, University of Arizona, 2013-present
- Faculty Research Fellow, National Bureau of Economic Research, 2020-present
- Adjunct Research Scientist, Ford School of Public Policy, University of Michigan, 2013-2016
- Visiting Scholar, Energy Policy Institute of Chicago, University of Chicago, Fall 2013
- Visiting Assistant Professor, Department of Economics, University of Arizona, 2012-2013
Assistant Professor, Ford School of Public Policy and Department of Economics, University of Michigan, 2010- 2013

Degrees:

Ph.D. University of California, Berkeley, Economics, 2010
B.A. Northwestern University, Mathematical Methods in the Social Sciences and Economics, 2002

Publications:

- “Escalation of Scrutiny: The Gains from Dynamic Enforcement of Environmental Regulations” (with Wesley Blundell and Gautam Gowrisankaran) *American Economic Review*, 110(8), August 2020, 2558-85
- “From Gallons to Miles: A Disaggregate Analysis of Automobile Travel and Externality Taxes” (with Vikram Maheshri and Clifford Winston) *Journal of Public Economics*, 152, August 2017
- “The Intergenerational Transmission of Automobile Brand Preferences” (with Soren Anderson, Ryan Kellogg, and James Saltee) *Journal of Industrial Economics*, 63(4), December 2015
- “Automakers’ Short-Run Responses to Changing Gasoline Prices” (with Nathan Miller), *Review of Economics and Statistics* 95(4), October 2013
- “Toward A Comprehensive Assessment of Road Pricing Accounting for Land Use” (with Clifford Winston) *Brookings-Wharton Papers on Urban Affairs*, 2008

“The Effect of Government Highway Spending on Road Users’ Congestion Costs”
(with Clifford Winston) *Journal of Urban Economics* 60(3), 2006

Working Papers:

- “Designing Dynamic Subsidies to Spur Adoption of New Technologies” (with Derek Lemoine)
Revisions requested at JAERE
- “What Were the Odds? Estimating the Market’s Probability of Uncertain Events” (with Derek Lemoine)
- “Fueling Alternatives: Evidence from Naturalistic Driving Data” (with Jackson Dorsey and Shaun McRae)
- “Energy Transitions in Regulated Markets” (with Gautam Gowrisankaran and Mar Reguant)

Teaching:

- Empirical Environmental Economics, University of Arizona (PhD)
- Environmental Economics, University of Arizona (Undergraduate)
- Honors Principles of Economics, University of Arizona (Undergraduate)
- Government Regulation of Industry and the Environment, University of Michigan (Masters in Public Policy)
- Energy and Environmental Policy, University of Michigan (Undergraduate)
- Microeconomics B: Cost-Benefit Analysis in Depth, University of Michigan (Masters in Public Policy)
- Economics of Discrimination Reading Group

Fellowships and Awards:

- | | |
|-----------|---|
| 2019 | Sloan Foundation Grant for “Economics of Innovation in the Energy Sector”
(With David Popp and NBER,
\$399,895) |
| 2018 | Eller Dean’s Research Award for Assistant Professors
NBER Economics of Energy Use in Transportation Grant (With Shaun
McRae, \$15,000)
Kalt Prize for best graduate student placement in the Eller Business School
(Jackson Dorsey, Kelley Business School at Indiana University) |
| 2017 | Center for Management Innovations in Health Care (With Derek Lemoine,
\$3,900)
Eller Small Grant (With Derek Lemoine, \$2,500) |
| 2016 | Institute of the Environment Program Development Grant (With Derek
Lemoine, \$15,000) |
| 2015-2016 | Eller Small Grant (With Derek Lemoine, \$2,500) |
| 2014-2016 | University of Arizona Renewable Energy Network Grant (With Derek
Lemoine, \$15,000 each year) |
| 2013-2014 | University of Arizona Renewable Energy Network Grant (\$15,000) |
| 2011,2012 | Faculty Teaching Honor Roll for Microeconomics B (twice), Energy Policy,
and Government Regulation of Industry and the Environment |
| 2009-2010 | Association of American University Women Dissertation Fellowship |
| 2008 | U.C. Berkeley Dean’s Normative Time Fellowship |

2007-2008 Outstanding Graduate Student Instructor Award

Research Presentations:

- 2021: Allied Social Science Association Conference, University of Georgia, University of Albany (scheduled), ZEW Mannheim Energy Conference (keynote, scheduled)
- 2020: Allied Social Science Association Conference, University of Arizona Quant Law Conference, Booth, Wharton, ETH/ZEW/CMCC/Grenoble Ecole de Management
- 2019: NBER Future of Energy Use in Transportation, Yale University Economics/SOM, University of Chicago (EPIC), NBER EEE Summer Institute, University of California Berkeley, University of Virginia Law School, National University Singapore, Jinan University, Triangle Resource and Environmental Economics Seminar, University of Calgary, University of California, San Diego
- 2018: Arizona State University, NBER Future of Energy Use in Transportation, University of Nevada, Reno, Cowles Foundation Conference on Structural Microeconomics, NBER Industrial Organization Summer Institute, NBER Environmental and Energy Economics Summer Institute, University of Maryland Environmental Tax Workshop, Georgetown University, Santa Barbara Occasional Workshop in Environmental Economics, Yale University School of Forestry and Environmental Studies
- 2017: Stanford Institute for Theoretical Economics Conference, NBER Environmental and Energy Economics Summer Institute, MIT CEEPR Workshop
- 2016: POWER Conference, IIOC Conference, AERE Annual Conference, Heartland Conference, University of Michigan Conference on Transportation, Energy, Economics and the Environment
- 2014: University of Colorado Boulder Environmental and Resource Economics Workshop, University of California, Davis, University of Michigan Conference on Transportation, Energy, Economics and the Environment
- 2013: Northeast Workshop on Energy Policy and Environmental Economics, American Environmental and Resource Economists (AERE) Annual Conference, NBER Summer Institute in Industrial Organization, Quantitative Marketing and Economics Conference, University of Chicago (Harris)
- 2012: American Economic Association Annual Conference, University of Arizona, University of British Columbia, Copenhagen Business School
- 2011: International Industrial Organization Conference, Midwest Bioenergy Conference, University of Chicago (Harris School), UC Berkeley (Energy Institute), Carnegie Mellon University (Heinz School)
- 2010: Arizona State University, Columbia University, Michigan State University, Ohio State University, Stanford University, Tufts University, University of British Columbia, University of Maryland (Agricultural and Resource Economics), University of Michigan (Economics, Erb Institute, and Public Policy), University of Toronto, University of Wisconsin (Economics and Agricultural and Applied Econ.)
- 2009: Collegio Carlo Alberto (MOOD Conference), San Francisco Federal Reserve, University of California, Berkeley (Economics, Business, and Public Policy)

Service:

2021-	Editorial Council, Journal of Environmental Economics and Management
2021	Presentation to Congressional Budget Office (“Recent Research on the Economics of Discrimination”)
2015-2020	Program Committee for the Summer Conference of the Association of Environmental and Resource Economists

Graduate Advising (all UA):

EK Green (chair), Paul Fisher (chair), Wei Zhou, Chase Eck (chair), Wendan Zhang (chair), Timothy Roberson (chair), Yujia Peng, Phuong Ho (co-chair), Arundhati Tillu, Jackson Dorsey (chair), SangUk Nam, Keith Meyers, Wesley Blundell (co-chair), Anatolii Kokoza, Kyle Wilson, Ahmad Mohassel (chair), Cong Liu, Charles He, Alex Hollingsworth, Michael Matheis, Soudeh Mirghasemi, Hoa Nguyen, Thiagarajah Subramaniam, Leila Asgari

2. Benjamin Leard

Contact Information:

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Academic Positions and Professional Affiliations

Assistant Professor, Department of Agricultural and Resource Economics, University of Tennessee, 2020-present

Faculty Fellow, Howard Baker Jr. Center for Public Policy, 2020-present

University Fellow, Resources for the Future, 2020-present

Fellow, Resources for the Future, 2014-2020

Education

Cornell University, Applied Economics and Management, Ph.D. 2014

James Madison University, Economics and Mathematics, B.S. 2008

Refereed Publications

How Much Do Consumers Value Fuel Economy and Performance? Evidence from Technology Adoption (with Joshua Linn and Christy Zhou). forthcoming, *Review of Economics and Statistics*.

Pushing New Technology into the Market: California’s Zero Emissions Vehicle Mandate (with Virginia McConnell). *Review of Environmental Economics and Policy*, 2021, 15(1): 169-179.

What Does an Electric Vehicle Replace? (with Jianwei Xing and Shanjun Li). *Journal of Environmental Economics and Management*, 2021, 107: 1-33.

The Effect of Fuel Price Changes on Fleet Demand for New Vehicle Fuel Economy (with Virginia McConnell and Christy Zhou). *Journal of Industrial Economics*, 2019, 67(1): 127-159.

Explaining the Evolution of Vehicle Miles Traveled in the United States (with Joshua Linn and Clayton Munnings). *Energy Journal*, 2019, 40(1): 25-54.

Voluntary Exposure Benefits and the Costs of Climate Change (with Kevin Roth). *Journal of the Association of Environmental and Resource Economists*, 2019, 6(1): 151-185.

Flawed Analyses of U.S. Auto Fuel Economy Standards (with Antonio M. Bento, Kenneth Gillingham, Mark R. Jacobsen, Christopher R. Knittel, Joshua Linn, Virginia McConnell, David Rapson, James M. Sallee, Arthur A. van Benthem, and Kate S. Whitefoot). *Science*, 2018, 362(6419): 1119-1121.

Consumer Inattention and the Demand for Vehicle Fuel Cost Savings. *Journal of Choice Modeling*, 2018, 29: 1-16.

Fuelling Behaviour Change. *Nature Energy News & Views*, 2018, 3: 541-542.

How Do Low Gas Prices Affect Costs and Benefits of US New Vehicle Fuel Economy Standards? (with Joshua Linn and Virginia McConnell). *Economics of Energy & Environmental Policy*, 2018, 7(2).

New Markets for Credit Trading Under U.S. Automobile Greenhouse Gas and Fuel Economy Standards (with Virginia McConnell). *Review of Environmental Economics and Policy*, 2017, 11(2): 207-226.

Fuel Prices, New Vehicle Fuel Economy, and Implications for Attribute-Based Standards (with Joshua Linn and Virginia McConnell). *Journal of the Association of Environmental and Resource Economists*, 2017, 4(3): 659-700.

Are Consumers Willing to Pay to Let Cars Drive for Them? Analyzing Response to Autonomous Vehicles (with Ricardo Daziano and Mauricio Sarrias). *Transportation Research Part C: Emerging Technologies*, 2017, 78: 150-164.

On the Importance of Baseline Setting in Carbon Offsets Markets (with Antonio Bento and Ravi Kanbur). *Climatic Change*, 2016, 137(3): 625-637.

Designing Efficient Markets for Carbon Offsets with Distributional Constraints (with Antonio Bento and Ravi Kanbur). *Journal of Environmental Economics and Management*, 2015, 70(2): 51-71.

The Welfare Effects of Allowance Banking in Emissions Trading Programs. *Environmental and Resource Economics*, 2013, 55(2): 175-197.

Equivalencies in the Fishery (with Jon Conrad). *Natural Resource Modeling*, 2013, 26(2): 154-163.

Working Papers

Firms and Collective Reputation: a Study of the Volkswagen Emissions Scandal (with Rudiger Bachmann, Gabriel Ehrlich, Ying Fan, and Dimitrije Ruzic)

Estimating Consumer Substitution between New and Used Passenger Vehicles. accepted, *Journal of the Association of Environmental and Resource Economists*

Interpreting Tradable Credit Prices in Overlapping Vehicle Regulations (with Virginia McConnell). 2020 RFF Working Paper 20-07, revisions requested.

Pass-Through and Welfare Effects of Regulations that Affect Product Attributes (with Joshua Linn and Katalin Springel). 2019 RFF Working Paper 19-07.

Have US Fuel Economy and Greenhouse Gas Emissions Standards Improved Social Welfare? (with Joshua Linn and Katalin Springel). 2020 RFF Working Paper 20-06.

Other Publications

Federal Climate Policy 104: The Transportation Sector, 2021 RFF Explainer

The Potential Role and Impact of Electric Vehicles in US Decarbonization Strategies (with Virginia McConnell). 2020 RFF Report 20-16.

Carbon Pricing 202: Pricing Carbon in the Transportation Sector (with Joshua Linn and Kathryn Cleary). 2020 RFF Explainer.

What Does Ridesharing Replace? (with Jianwei Xing). 2020 RFF Working Paper 20-03.

Targeting Subsidies to Get More Electric Vehicles on the Road. RFF Resources Magazine Issue 202, Fall 2019.

What Does an Electric Vehicle Replace? RFF Resources Magazine Issue 201, Summer 2019.

Comments to NHTSA and US EPA on the Safer Affordable Fuel-Efficient (SAFE) Vehicles Rule for Model Years 2021-2026: Passenger Cars and Light Trucks (with Joshua Linn, Alan J. Krupnick, and Virginia McConnell). Public Comments, October 2018.

Fleet Vehicles and Fuel Economy: How Do Fuel Prices Affect Vehicle Purchase Decisions for Big Buyers? (with Virginia McConnell and Christy Zhou). RFF Resources Magazine Issue 197, Spring 2018

The Benefits of Flexible Policy Design: US Energy Conservation Standards for Appliances (with Josh Blonz and Karen Palmer). RFF Resources Magazine Issue 197, Spring 2018.

Comments on the US Department of Energy’s Office of Energy Efficiency and Renewable Energy’s Request for Information on Energy Conservation Standards Program Design (with Josh Blonz and Karen Palmer). RFF Report, February 2018.

The Effect of Standards for New Vehicle Fuel Economy and GHG Emissions on US Consumers (with Joshua Linn and Christy Zhou) RFF Resources Magazine Issue 195, Fall 2017.

The Net Emissions Impact of HFC-23 Offset Projects from the Clean Development Mechanism (with Clayton Munnings and Antonio Bento). 2016 RFF Discussion Paper 16-01.

Fuel Prices, Economic Activity, and the Rebound Effect for Heavy-Duty Trucks (with Joshua Linn, Virginia McConnell and William Raich). 2016 RFF Discussion Paper 15-43 REV.

How Climate Change Affects Traffic Accidents (with Kevin Roth). RFF Resources Magazine Issue 191, Winter 2016.

Comments on Midterm Evaluation Draft Technical Assessment Report for Model Year 2022-2025 Light Duty Vehicle GHG Emissions and CAFE Standards (with Joshua Linn, Virginia McConnell and Kenneth Gillingham). RFF Report, September 2016.

Do Low Oil Prices Undermine US Passenger Vehicle Fuel Economy Standards? (with Joshua Linn and Virginia McConnell). RFF Policy Brief, July 2016, No. 16-08.

Comment on Greenhouse Gas Emissions and Fuel Efficiency Standards for Medium- and Heavy-Duty Engines and Vehicles; Phase 2. NHTSA-2014-0132-0109. The Potential for Improvement in On-road Truck Fuel Economy: Evidence from the VIUS (with Jen He and Virginia McConnell) <https://www.regulations.gov/document?D=NHTSA-2014-0132-0109>.

Research Funding

Oak Ridge National Laboratory, 2021-2022

Sloan Foundation, 2020-2021

Energy Foundation, 2020

Merck Family Fund, 2020

Georgetown Climate Center, 2019

United States Climate Alliance, 2019

Sloan Foundation, 2017-2019

Resources for the Future New Frontiers Competition, 2015-2016

Student Advising

Dissertation committee member for Kevin Ankney, Georgetown University

Dissertation defense external examiner for Cheng Xu, George Washington University, June 2019

Teaching

Policy Analysis for Environmental and Natural Resource Management, University of Tennessee

Economic Perspectives on Natural Resource and Environmental Issues, University of Tennessee

Environmental Economics, Ithaca College

Math Camp for incoming Masters and Ph.D. students, The Charles H. Dyson School of Applied Economics and Management

Presentations

2020: ASSA Annual Meeting, San Diego, CA; Howard H. Baker Jr. Center for Public Policy, University of Tennessee; Ford Motor Company; NBER Economics of Transportation in the 21st Century, online; Exploring Innovative Transportation Policies, Resources for the Future, online

2019: Northeast Workshop on Energy Policy and Environmental Economics, Harvard University; TE³ Conference on Transportation, Economics, Energy, and the Environment, University of Michigan

2010-2018: Howard H. Baker Jr. Center for Public Policy Public Forum on Electric Mass Transit as an Option for Urban Mobility, University of Tennessee; 6th World Congress of Environmental and Resource Economists, Gothenburg, Sweden; 2018 TE³ Conference on Transportation, Economics, Energy, and the Environment, University of Michigan, 2017 TE³ Conference on Transportation, Economics, Energy, and the Environment, University of Michigan; AERE 6th Annual Summer Conference, Pittsburgh, PA; Department of Transportation Workshop, Washington, DC; AERE 5th Annual Summer Conference in Breckenridge, Colorado; Transportation Research Board Annual Meeting, Washington, DC; The Economics of Low-Carbon Markets, University of de Sao Paulo; MIT CEEPR Workshop, Cambridge, MA; Iowa State University, Ames, IA; Wake Forest University, Winston-Salem, NC; California State - Long Beach, Long Beach, CA; Resources for the Future, Washington, DC; AERE 2nd Annual Summer Conference, Banff, Canada; 2nd Northeast Workshop on Energy Policy and Environmental Economics, Cornell University, Ithaca, NY; EAERE Summer Conference, Prague, Czech Republic; AERE 1st Annual Summer Conference, Asheville, NC; EAERE-FEEM-VIU European Summer School, Venice, Italy; Eastern Economics Association Meetings, New York, NY; United Nations Conference of Parties Climate Change (COP16) Conference, Agricultural and Rural Development Day, Ideas Marketplace Presentation, Cancun, Mexico

Referee Experience

American Journal of Agricultural Economics, Climate Policy, Economic Inquiry, Energy Economics, Energy Journal, Energy Policy, Environmental and Resource Economics, International Economic Review, Journal of Choice Modeling, Journal of Environmental

Economics and Management, Journal of the Association of Environmental and Resource Economists, Journal of Natural Resources Policy Research, Journal of Public Economics, Nature Climate Change, Nature Energy, Regional Science and Urban Economics, Research in Transportation Economics, Resource and Energy Economics, Transportation Research Part A: Policy and Practice, Transportation Research Part D: Transport and Environment

3. James Sallee

James M. Sallee

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CURRENT EMPLOYMENT AND AFFILIATIONS

Associate Professor, Department of Agricultural and Resource Economics University of California, Berkeley	2019—present
Previously Assistant Professor	2015—2019
Research Associate, National Bureau of Economic Research Public Economics and Energy and Environmental Economics Programs	2019—present
Previously Faculty Research Fellow	2010—2019
Research Associate, Energy Institute at Haas	2016—present
Faculty Affiliate, E2e Program	2014—present
Faculty Affiliate, Institute for Research on Labor and Employment	2016—present

PAST EMPLOYMENT

Assistant Professor, Harris School of Public Policy Studies University of Chicago	2008–2015
Visiting Researcher, University of California Energy Institute	2010

EDUCATION

University of Michigan, Ph.D. in Economics (2008)
University of Michigan, M.A. in Economics (2005)
Macalester College, B.A. in Economics and Political Science, *Summa Cum Laude*, ΦBK (2001)

SELECTED PROFESSIONAL SERVICE

Member, National Academy of Sciences Committee to Review of Methods for Setting Building and Equipment Performance Standard (2019-2020)

PUBLISHED ARTICLES (peer reviewed unless otherwise noted)

- “Who Benefits When Firms Game Corrective Policies?” (with Mathias Reynaert) *American Economic Journal: Economic Policy*. Forthcoming.
- “The Use of Regression Statistics to Analyze Imperfect Pricing Policies” (with Mark R. Jacobsen, Christopher R. Knittel and Arthur van Benthem) *Journal of Political* (May 2020), pp. 1826-1876.
- “Should Electric Vehicle Drivers Pay a Mileage Tax?” (with Lucas Davis) *Energy Policy and the Economy* v. 1, Editors Matthew Kotchen, James Stock and Catherine Wolfram, NBER: University of Chicago Press (2020), pp. 65-94.
- “Flawed Analyses of U.S. Auto Fuel Economy Standards” (with Antonio Bento, Kenneth Gillingham, Mark Jacobsen, Christopher Knittel, Benjamin Leard, Joshua Linn, Virginia McConnell, David Rapson, Arthur van Benthem, and Kate Whitefoot) *Science* vol. 362, issue 6419 (December 2018), pp. 1119-1121
- “The Economics of Attribute-Based Regulation: Theory and Evidence from Fuel-Economy Standards” (with Koichiro Ito) *Review of Economics and Statistics* 100, May 2018, pp. 319-336.
- “Tax Incidence with Endogenous Quality and Costly Bargaining: Theory and Evidence from Hybrid Vehicle Sales” (with Sumeet Gulati and Carol McAusland) *Journal of Public Economics* 155, November 2017, pp. 93-107.
- “Disparities in Complex Price Negotiations: The Role of Consumer Age and Gender” (with Ambarish Chandra and Sumeet Gulati). *Journal of Industrial Economics* 64(2), June 2017, pp. 235-74.
- “Do Consumers Recognize the Value of Fuel Economy? Evidence from Used Car Prices and Gasoline Price Fluctuations” (with Sarah West and Wei Fan) *Journal of Public Economics* 134, March 2016, pp. 61-73.
- “Designing Policies to Make Cars Greener: A Review of the Literature” (with Soren T. Anderson) *Annual Review of Resource Economics* 8, 2016, 157-80.
- “The Intergenerational Transmission of Automobile Brand Preferences: Empirical Evidence and Implications for Firm Strategy” (with Soren T. Anderson, Ryan Kellogg and Ashley Langer) *Journal of Industrial Economics*, 63(4), December 2015, pp. 763-793.
- “New Evidence on Taxes and the Timing of Birth” (with Sara LaLumia and Nicholas Turner) *American Economic Journal: Economic Policy*, 7(2), May 2015, pp. 258-293.
- “Rational Inattention and Energy Efficiency” *Journal of Law and Economics*, 57(3), August 2014, pp. 781-820.

“What Do Consumers Believe About Future Gasoline Prices? (with Soren T. Anderson and Ryan Kellogg) *Journal of Environmental Economics and Management*, 66(3), November 2013, pp. 383-403.

“The Value of Honesty: Empirical Estimates from the Case of the Missing Children” (with Sara LaLumia) *International Tax and Public Finance*, 20(2), April 2013, pp. 192-224.

“Car Notches: Strategic Automaker Responses to Fuel Economy Policy” (with Joel Slemrod) *Journal of Public Economics*, 96(11-12), December 2012, pp. 981-999.

*Awarded the 2015 Atkinson Award (Best Paper in the *Journal of Public Economics* 2012-4)

“Financial Reporting, Tax, and Real Decisions: Toward a Unifying Framework” (with Douglas A. Shackelford and Joel Slemrod), *International Tax and Public Finance*, 18(4), August 2011, pp. 461-494.

“Using Loopholes to Reveal the Marginal Cost of Regulation: The Case of Fuel-Economy Standards” (with Soren T. Anderson) *American Economic Review* 101(4), June 2011, pp. 1375-1409.

“The Surprising Incidence of Tax Credits for the Toyota Prius” *American Economic Journal: Economic Policy*, 3(2), May 2011, pp. 189-219.

“Forecasting Gasoline Prices Using Consumer Surveys” (with Soren T. Anderson, Ryan Kellogg and Richard M. Curtin) *American Economic Review Papers & Proceedings* 101(3), May 2011, pp. 110-114. (Not Peer Reviewed)

“Fuel Economy Standards: Impacts, Efficiency, and Alternatives” (with Soren Anderson, Carolyn Fischer and Ian Parry), *Review of Environmental Economics and Policy*, 5(1), Winter 2011, pp. 89-108.

“The Taxation of Fuel Economy” *Tax Policy and the Economy* v. 25, Editor Jeffrey R. Brown, NBER: University of Chicago Press, 2011, pp. 1-38. (Not Peer Reviewed)

“A Cautionary Tale About the Use of Administrative Data: Evidence from Age of Marriage Laws” (with Rebecca M. Blank and Kerwin Kofi Charles), *American Economic Journal: Applied Microeconomics*, 1(2), April 2009, pp. 128 - 149.

“On the Optimal Allocation of Students and Resources in a System of Higher Education” (with Alexandra M. Resch and Paul N. Courant) *The B.E. Journal of Economic Analysis & Policy* (Advances Tier), 8(1), Article 11.

WORKING PAPERS

“Pigou Creates Losers: On the Implausibility of Pareto Improvements from Pigouvian Taxation” (May 2019) NBER Working Paper No. 25831

AWARDS AND HONORS

Excellence in Advising Award: Outstanding Faculty Advisor (2018)
Club Six (2017, 2019 recognition for teaching scores above 6 out of possible 7, Haas MBA)
Hellman Family Faculty Fund Award (2017)
UC Regents’ Junior Faculty Fellowship (2016)
Atkinson Award (2015, for best Paper in the *Journal of Public Economics* between 2012-2014)
Best Teacher in a Core Course, The Harris School (2012, 2013)
John V. Krutilla Research Award from Resources for the Future (2009 - 2010)
National Tax Association Dissertation Award (2008)
National Science Foundation Graduate Research Fellowship (awarded 2003)
Population Studies Center Trainee Fellowship, University of Michigan (2003-2008)

GRANTS

Next 10 Foundation (*Why are High Electricity Prices a Problem for Climate Change?* 2019-2021)
Giannini Foundation (*Evaluating Optimal and Second-Best Nitrogen Regulations in California Agriculture with Biogeochemical Simulations* 2020-2021)
Institute for Transportation Research, UC Berkeley (*Can Targeted Rebates Foster Equity in Congestion Pricing Schemes?* 2019-2020)
Giannini Foundation (*An Optimal Tax Approach to Policy Problems in California Agriculture*, 2019-2020)
Sloan Foundation (*Heterogeneity, Equity and Energy Policy* 2017-8)
France-Berkeley Fund Award (2016)
W.E. Upjohn Institute Early Career Research Grant (with Reed Walker) (2012)

TEACHING

University of California, Berkeley
Environmental and Resource Economics, ARE 261 (PhD)

Empirical Energy and Environmental Economics, ARE 264 (PhD)
Environmental Economics, EEP 101/ECON 125 (Undergraduate)
Economic Analysis for Business Decisions (Core micro for MBAs), MBA201A
taught at Haas School of Business

University of Chicago (all for MPP students)

Policy Approaches to Mitigating Climate Change
Topics in U.S. Tax Policy
Empirical Methods in Policy Analysis II
Science, Technology and Policy

REFeree

Editorial Board Member (2021-) *Review of Environmental Economics and Performance*

Editorial Council Member (2014-) *Journal of the Association of Environment and Resource Economists*

American Economic Review, Journal of Political Economy, Quarterly Journal of Economics, Econometrica, American Economic Review: Insights, Journal of Public Economics, American Economic Journal: Economic Policy, American Economic Journal: Applied Economics, Review of Economics and Statistics, RAND Journal of Economics, Journal of Environmental Economics and Management, National Tax Journal, Journal of the Association of Environment and Resource Economists, Journal of Labor Economics, International Economic Review, European Economic Review, International Tax and Public Finance, Journal of Law & Economics, Economic Journal, Energy Journal, Canadian Journal of Economics, Nature, B.E. Journal of Economic Analysis & Policy, Economic Inquiry, Journal of Human Resources, Economic Letters, Energy Economics, Environmental and Resource Economics, Journal of Urban Economics, Journal of Policy Analysis and Management, Transportation Research Part A, Journal of Population Economics, Environmental Policy and Governance, Scottish Journal of Political Economy **Grants:** National Science Foundation, European Science Foundation, Sloan Foundation, Smith Richardson Foundation, Time-Sharing Experiments for the Social Sciences

SELECTED PRESENTATIONS

Invited **2018:** Berkeley (Transportation Research Institute) **2017:** Toulouse School of Economics, UC Santa Cruz (Economics) **2016:** Berkeley (Economics), UC Davis (ARE),

Texas A&M (Economics), FGV Rio de Janeiro (Economics), Resources for the Future, Arizona State **2015**: LSE (Economics), Berkeley (Goldman), UCLA (Luskin), Colorado School of Mines (Economics), Universidad de Chile (Business School), Pontificia Universidad Catolica de Chile (Economics) **2014**: Michigan (Ross), Berkeley (ARE), University of Pennsylvania (Wharton), Berkeley (POWER Conference), Yale (FES), Illinois (Economics), National Tax Association Spring Symposium, Federal Trade Commission, EPA, University of Leuven (Economics), Universidad de Chile (Business School), Pontificia Universidad Catolica de Chile (Economics) **2013**: Georgetown (Economics), Illinois (Economics), Wisconsin (Economics) **2012**: Maryland (Economics), Northwestern (Law), Universidad de Chile (Business School), Oxford (Business School); **2011**: Columbia (Economics), Maryland (AREC), Syracuse (Maxwell), Illinois (Finance), Ohio State (Economics), Illinois (Sustainability Center), NYU (Law conference), University of Illinois at Chicago (Sustainability workshop), Treasury, EPA, Resources for the Future (Conference); **2010**: MIT (Economics), Yale (FES), Berkeley (ARE), Berkeley (UCEI), NBER Tax Policy and the Economy, University of Chile; **2009**: Cornell (Economics), Minnesota (Applied Economics), North Carolina State University (Economics), Berkeley (POWER Conference), University of Illinois at Chicago (Economics), Macalester College (Economics); **2008**: Resources for the Future, University of Chicago (Harris), University of Pennsylvania (Wharton), University of British Columbia (Economics), University of Kentucky (Martin/Economics), University of Indiana (SPEA), University of California, Irvine (Economics), Treasury, Ford Motor Company

Conference **2019**: NBER Energy and Environmental Economics (Spring), Association of Environmental and Resource Economics, NBER conference on Economics of Autonomous and Electric Vehicles **2018**: ASSA (AEA), Heartland Environmental and Resource Economics, National Tax Association **2017**: National Tax Association, ASSA (AEA) **2016**: NBER EEE Summer Institute, Stanford Institute for Theoretical Economics, National Tax Association, Heartland Environmental and Resource Economics **2015**: NBER EEE **2014**: ASSA (AEA and AERE), NBER EEE, NBER Public Economics, Oxford Tax Systems Conference, Michigan Tax Invitational **2012**: NBER Public Economics, National Tax Association, Michigan Tax Invitational **2011**: National Tax Association, ASSA, Association of Environmental and Resource Economics, International Institute of Public Finance, University of California Energy Institute; **2010**: NBER Public Economics, Iowa State Bioenergy Camp; **2009**: ASSA, National Tax Association, Heartland Environmental and Resource Economics; **2008**: APPAM, National Tax Association; **2007**: NBER Summer Institute (EEE), National Tax Association, APPAM

Appendix B. Conflicts of Interest Declarations

1. Ashley Langer



ORGANIZATIONAL CONFLICT OF INTEREST CERTIFICATE

Customer: U.S. Environmental Protection Agency

Contractor: ICF Incorporated, LLC, 9300 Lee Highway, Fairfax, VA 22031

Prime Contract: EP-C-16-020, Work Assignment 4-04

Subject Report: The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage

Subcontract/Peer Reviewer: Ashley Langer

In accordance with EPAAR 1552.209-70 through 1552.209-73, Subcontractor/Consultant certifies to the best of its knowledge and belief, that (check one):

☒ X No actual or potential conflict of interest exists.

☐ An actual or potential conflict of interest exists. See attached full disclosure.

Subcontractor/Consultant certifies that its personnel, who perform work on this contract, have been informed of their obligations to report personal and organizational conflict of interest to Contractor and Subcontractor/Consultant recognizes its continuing obligation to identify and report any actual or potential organizational conflicts of interest arising during performance under referenced contract.

A handwritten signature in black ink, appearing to read "AL", written over a horizontal line.

Subcontractor/Consultant

5/24/2021

Date

2. Benjamin Leard



ORGANIZATIONAL CONFLICT OF INTEREST CERTIFICATE

Customer: U.S. Environmental Protection Agency

Contractor: ICF Incorporated, LLC, 9300 Lee Highway, Fairfax, VA 22031

Prime Contract: EP-C-16-020, Work Assignment 4-04

Subject Report: The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage

Subcontract/Peer Reviewer: Benjamin Leard

In accordance with EPAAR 1552.209-70 through 1552.209-73, Subcontractor/Consultant certifies to the best of its knowledge and belief, that (check one):

☒ X No actual or potential conflict of interest exists.

☐ An actual or potential conflict of interest exists. See attached full disclosure.

Subcontractor/Consultant certifies that its personnel, who perform work on this contract, have been informed of their obligations to report personal and organizational conflict of interest to Contractor and Subcontractor/Consultant recognizes its continuing obligation to identify and report any actual or potential organizational conflicts of interest arising during performance under referenced contract.

Benjamin Leard

Subcontractor/Consultant

5-15-21

Date

3. James Sallee



ORGANIZATIONAL CONFLICT OF INTEREST CERTIFICATE

Customer: U.S. Environmental Protection Agency

Contractor: ICF Incorporated, LLC, 9300 Lee Highway, Fairfax, VA 22031

Prime Contract: EP-C-16-020, Work Assignment 4-04

Subject Report: The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage

Subcontract/Peer Reviewer: James M. Sallee

In accordance with EPAAR 1552.209-70 through 1552.209-73, Subcontractor/Consultant certifies to the best of its knowledge and belief, that (check one):

☒ X No actual or potential conflict of interest exists.

☐ An actual or potential conflict of interest exists. See attached full disclosure.

Subcontractor/Consultant certifies that its personnel, who perform work on this contract, have been informed of their obligations to report personal and organizational conflict of interest to Contractor and Subcontractor/Consultant recognizes its continuing obligation to identify and report any actual or potential organizational conflicts of interest arising during performance under referenced contract.

A handwritten signature in black ink, appearing to read "J. Sallee", is written over a horizontal line.

Subcontractor/Consultant

May 25, 2021

Date

UNIVERSITY OF CALIFORNIA, BERKELEY

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SANTA BARBARA • SANTA CRUZ

JAMES M. SALLEE

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BERKELEY, CALIFORNIA 94720-3310

May 30, 2021

Paola Massoli and Sarah Lettes
ICF

RE: Peer Review

To Whom It May Concern:

I have conducted a peer review of "The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage" (RTI 0215574.004.028).

I do not have any conflicts of interest to report.

However, I wish to disclose that I have collaborated professionally with the lead author of the report, Mark Jacobsen of the University of California, San Diego. I had no involvement in his work on this report, nor have I discussed it with him or the other authors of the report. I have no financial interest connected to the report in anyway.

Sincerely,

A handwritten signature in blue ink, appearing to read "James M. Sallee".

James Sallee
Associate Professor

Appendix C. Peer Reviewer Mid-Review Meeting Notes



SUMMARY NOTES: PEER REVIEW FOR THE EFFECTS OF NEW VEHICLE PRICE CHANGES ON NEW AND USED VEHICLE MARKETS AND SCRAPPAGE MID-REVIEW MEETING, MAY 6, 2021 – 3:00 PM

Attendees:

Elizabeth Miller, EPA
Dana Jackson, EPA
Gloria Helfand, EPA
Paola Massoli, ICF
Sarah Lettes, ICF

Ashley Langer, University of Arizona
Benjamin Leard, University of Tennessee
James Sallee, University of California

Welcome, Introductions, and Roles

Elizabeth Miller opened the meeting and all participants gave brief introductions of themselves, their backgrounds, and their role in this Work Assignment.

Paola Massoli gave an overview of the peer review process as it applied to the report “The Effects of New Vehicle Price Changes on New and Used Vehicle Markets and Scrappage.” She also confirmed with the reviewers that everyone received their technical packages. Each package included a charge letter, conflict of interest form, and the Report for review.

Overview of the Project and Report

Gloria Helfand gave a brief overview of the RTI report, and EPA’s expectations of the peer review process in terms of timeline and outcome. The meeting then turned to open discussion to answer the initial peer reviewers’ questions.

Open Discussion and Questions

James Sallee pointed out that this is the first peer review process where there is a kickoff meeting amongst all reviewers and asked if peer reviewers would be prevented from talking to each other during the review period. Paola Massoli responded that the EPA Handbook doesn’t prevent peer reviewers to interact, as long as the reviews are independently conducted.

EPA agreed that it would be up to the peer reviewers’ good judgement to maintain an independent behavior during the process.

James Sallee also asked clarifications regarding what constitutes a Conflict of Interest (COI) and specified that he might have to disclose an existing professional relationship with one of the authors of the RTI Report, whom he was not aware of when he accepted to perform the peer review. James Sallee specified that he is not working with the RTI team, and there are no monetary COI. Paola Massoli responded that it is fairly common for professionals working in one field to know each other and work together at some stage, and that the matter can be solved by a simple disclosure statement where it is acknowledged that the individuals have professionally interacted in the past.

Ashley Langer asked about the format of the answers to the report. Paola Massoli responded that the peer reviewers should use the tabular format provided in the charge letter so that it is easy to compile and summarize the answers to each question. Gloria Helfand added that reviewers are encouraged to provide additional comments including edits to the draft Report, as EPA wants to ensure that the report is thoroughly reviewed.

Benjamin Leard had no additional questions.

Schedule and Next Steps

Paola Massoli gave an overview of the project’s schedule. She reminded the panel of the **May 24, 2021** deadline for the reviews to be returned to ICF (to Paola Massoli and Sarah Lettes). If additional review time is required, the reviewer should reach out to ICF so that an extension can be coordinated and accommodated.

ICF will then compile all comments and share with EPA. Reviewers should also re-submit a current CV or resume, and a cover letter that includes their name, name and address of their organization. The completed COI form provided in the review package is also required.

During the review period, the reviewers will send any questions to ICF. ICF will forward the questions to EPA as necessary. ICF will then share all questions and responses with the entire review team.