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Re: Joint Comments of Health, Environmental, and Conservation Groups on EPA's Proposed Rule: The Safer Affordable Fuel-Efficient (SAFE) Vehicles Rule for Model Years 2021–2026 Passenger Cars and Light Trucks, 83 Fed. Reg. 42,986 (Aug. 24, 2018)

The Environmental Defense Fund and Union of Concerned Scientists ("Organizations") hereby submit these joint comments in opposition to the Administrator's proposal to roll back existing light-duty vehicle greenhouse gas emissions standards. See 83 Fed. Reg. 42,986 (Aug. 24, 2018) ("Rollback" or "Proposal"). These comments discuss certain of the Organizations' objections to the analysis and application of the rebound effect in the Proposal. Some of the Organizations are also filing separate comment letters to provide more detail and to address additional issues.

As described in detail in our comments (attached as Appendix A), the agencies' analysis and application of the rebound effect is fatally flawed. The agencies fail to acknowledge or defend their changes in position. Specifically, they mischaracterize their own analysis of the appropriate rebound rate in their 2010 and 2012 final rules, and completely fail to mention the analyses in the 2016 Draft TAR and EPA's 2016 Final TSD. The agencies also utilize unweighted, average values of the studies they consider, contrary to prior acknowledgement that such a methodology is unreasonable and inadequate.

Moreover, the agencies do not acknowledge that they previously considered 13 of the 16 studies listed in PRIA Table 8-8 and concluded that those studies supported the agencies' previously adopted value for the rebound effect of 10%. The remaining three studies are all based on international data, which the agencies have previously acknowledged is of questionable relevance to the U.S. context.

To arrive at their conclusion that the rebound effect should be revised, the agencies contort findings of key studies to suggest that the rebound effect is not declining over time, and rely on studies utilizing international data, data from the National Household Travel Survey, and data relating to gasoline demand elasticities rather than Vehicle-Miles-Travelled (VMT) elasticities – all of which the agencies have previously acknowledged are of limited reliability or wholly inapplicable to the rulemaking context. The agencies also omit discussion of a broad range of recent studies, all of which demonstrate that the agencies' proposal to adopt a 20% value is unreasonable, and which demonstrate that the agencies' prior estimate of 10% must be retained or revised downwards.

Additionally, the agencies ignore the various reasons that the estimates returned by the rebound effect literature are likely too high. They ignore the broad findings suggesting that the fuel economy rebound effect is smaller than the fuel price rebound effect. They ignore that recent studies confirm that the rebound effect has decreased, and will continue to decrease. They ignore that this decrease is due to both increases in income and decreases in driving costs – the latter of which would be caused by existing greenhouse gas emissions and augural fuel economy standards themselves. They ignore that the rebound effect is asymmetrical, and the consumer response is smaller for declines in the cost of driving (as is caused by increases in fuel economy) than it is for increases. And they ignore their own projections of increased congestion due to the augural standards, which the literature finds causes the rebound effect to be smaller.

The agencies' rebound analysis is also inconsistent with other portions of the agencies' proposed rule. The agencies fail to acknowledge the inconsistency between their assumption in the rebound analysis that an increase in the cost of driving will always cause consumers to drive less, and their assumption in the Dynamic Fleet Share Model that when consumers shift from cars (which cost less to drive) to trucks (which cost more to drive) their driving will *increase*. They also erroneously calculate the rebound VMT off of the resulting increase, thereby artificially magnifying the rebound effect in the augural/existing standards scenario.

Finally, although the agencies correctly acknowledge that any safety impacts from the rebound effect are the result of consumers' choice to drive more, and thus cannot be attributed to the rule, the agencies nevertheless invoke rebound fatalities as a central justification for their proposal to rollback the augural/existing standards. Safety impacts attributable to rebound are not properly considered as part of the agencies' analysis, the agencies cannot rely on them as a rationale for rolling back the standards.

Sincerely,

Peter Zalzal
Chet France – Consultant to EDF
Environmental Defense Fund

Dave Cooke
Senior Vehicles Analyst
Union of Concerned Scientists

APPENDIX A
ANALYSIS OF THE VALUE AND APPLICATION OF THE REBOUND EFFECT

I. Use of a rebound effect greater than 10% is arbitrary and capricious

a. The agencies fail to adequately acknowledge or defend their change in position

The agencies do not adequately acknowledge or describe their prior positions on the value of the rebound effect, much less provide a reasoned or justified explanation for why they have now chosen to depart from their historical stances.

The agencies acknowledge that they estimated the rebound effect to be 10% in their 2010 Final Rule for CAFE and GHG standards for MYs 2012-2016,[1] and that the agencies also adopted the 10% value their analysis for MYs 2017-2025.[2] However, the agencies' description of how the 10% value was adopted in 2010 is erroneous. First, NHTSA portrays the decision to use 10% in the MY 2012-2016 rulemaking as though NHTSA had fallen off a precipice from 20% to 10% based on a single study (Small and Van Dender (2007)[3]).[4] However, in fact, even NHTSA had revised its estimate of the rebound effect downward *before* the joint MY2012-2016 rulemaking. In 2008 NHTSA, used a value of 15% in its Notice of Proposed Rulemaking for MYs 2011-2015 CAFE standards[5] and finalized that value in its final MY 2011 CAFE standards[6] (a fact the agencies do not even mention in this analysis).[7] And in those two analyses, far from relying on a single study, NHTSA considered 22 studies conducted from 1983 through 2005, containing 66 separate estimates of the rebound effect.[8] Among the agency's observations were that: (1) the average of the estimates derived from analysis of U.S. annual time-series data were in line with the proposed value; (2) according greater importance to the updated estimates from studies allowing the rebound effect to vary (including but not limited to Small and Van Dender (2005)[9] – a different, earlier paper than the one the agencies state was "singled out" in the 2010

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[1] *See* 83 Fed. Reg. 42,986, 43,100 (Aug. 24, 2018); *see also Light-Duty Vehicle Greenhouse Gas Emission Standards and Corporate Average Fuel Economy Standards; Final Rule*, 75 Fed. Reg. 25324, 25490 (May 7, 2010) (finalizing 10% value).
[2] 2018 PRIA, July 2018 – (Updated August 23, 2018) at 982.
[3] Kenneth A. Small and Kurt Van Dender (2007), "Fuel Efficiency and Motor Vehicle Travel: The Declining Rebound Effect," Energy Journal, Vol. 28, No. 1, pp. 25-51. Full citations to all studies cited herein are also available in the References section at the end of this comment.
[4] *See* 83 Fed. Reg. at 43,100; *see also* 2018 PRIA at 993 (suggesting that "the previous 10% estimate was based almost exclusively on the finding of the 2007 study by Small and Van Dender").
[5] *Average Fuel Economy Standards, Passenger Cars and Light Trucks; Model Years 2011-2015, Notice of Proposed Rulemaking*, 73 Fed. Reg. 24352, 24408 (May 2, 2008).
[6] *Average Fuel Economy Standards Passenger Cars and Light Trucks Model Year 2011, Final Rule*, 74 Fed. Reg. 14196, 14327 (March 30, 2009).
[7] *See id*.
[8] 73 Fed. Reg. at 24408; 74 Fed. Reg. at 14323.
[9] Kenneth Small and Kurt Van Dender (2005), The Effect of Improved Fuel Economy on Vehicle Miles Traveled: Estimating the Rebound Effect Using U.S. State Data, 1966-2001, University of California Energy Institute's (UCEI).

rulemaking) supported the proposed value; (3) and "recalculating the 29 original estimates of variable rebound effects to reflect current (2006) values for retail fuel prices, average fuel economy, personal income, and household vehicle ownership reduces their median estimate" returned values in line with the proposed value of 15%.[10] Indeed, contrary to NHTSA's suggestion that the adoption of a value below 20% relied entirely on the work of Small and Van Dender,[11] in the 2009 Final Rule, the agency considered comments noting that the 2005 Small and Van Dender paper had found certain rebound values of less than 5% and had projected that the value would continue to decline into the future,[12] yet, noting that Small and Van Dender's research was still new at that time, NHTSA affirmatively declined to place outsize weight on that one paper.[13] Instead, NHTSA considered the full literature to date, and concluded that 15% was the most appropriate value. The agencies cannot now plausibly argue that the only thing supporting revision of the historically-used 20% figure in the 2009 and 2010 rulemakings was the 2007 study by Small and Van Dender.

Relatedly, the NPRM's description of the 2010 Final Rule initially adopting the 10% value is erroneous. Again, as described above, the NPRM asserts that the 2010 decision was based entirely on Small and Van Dender (2007).[14] However, the 2010 final rule expressly refused to fully adopt the findings of Small and Van Dender, which the agency acknowledged could warrant a rebound effect "in the range of 5% or lower."[15] Instead, the agency gave consideration to "the larger body of historical studies" and selected a value of 10%.[16] Moreover, the agencies expressly described that "the 10 percent value was not derived from a single point estimate from a particular study, but instead represents a reasonable compromise between the historical estimates and the projected future estimates."[17] Among the studies considered and discussed was Greene (2007) which confirmed the central findings of Small and Van Dender (2007) (namely, that the rebound effect has declined over time), and which separately found that fuel economy changes did not "have a statistically significant impact on VMT."[18] Also among the studies were "several new estimates of [the rebound effect's] magnitude" developed by NHTSA itself.[19] NHTSA described that "[t]hese estimates were developed by estimating and testing several econometric models of the relationship between vehicle miles traveled and factors that influence it, including household income, fuel prices, vehicle fuel efficiency, road supply,

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[10] 73 FR at 24,408.
[11] *See* 83 Fed. Reg. at 43,100 (suggesting that in the agencies' 2010 analysis, "a then recently published analysis by Small & Van Dender (2007), which reported that the rebound effect appeared to be declining over time in response to increasing income of drivers, was singled out.").
[12] 74 Fed. Reg. at 14,326.
[13] *Id*. at 14,327.
[14] 83 Fed. Reg. at 43,100; *see also* 2018 PRIA, July 2018 – (Updated August 23, 2018) at 981-82.
[15] 75 Fed. Reg. at 25,517.
[16] *Id*.
[17] 2010 Final Joint TSD, EPA-420-R-10-901 (April 2010) at 4-22.
[18] *Id*. at 4-19.
[19] *Id*. at 4-20.

the number of vehicles in use, vehicle prices, and other factors."[20] And NHTSA's work returned estimates of the rebound effect in the 3 to 16% range.[21] Yet in the NPRM the agencies fail to even *mention* the Greene or NHTSA studies.[22] And, although the PRIA does contain a single sentence mentioning Greene's study, it acknowledges only his finding confirming the existence of an income effect, and fails entirely to note that Greene *also* found that there was no statistically significant impact on VMT from fuel economy changes.[23]

Even more significantly, the agencies entirely fail to acknowledge or discuss their analysis of the appropriate estimate of the rebound effect presented in the 2016 Draft TAR, wherein they reaffirmed a 10% value.[24] As discussed in more detail where relevant below, that discussion: (a) considered fully thirteen (13) of the sixteen (16) papers that the agencies now describe as "new" and purport to rely on in the NPRM (as shown in Table 1 below); (b) considered *all* of the studies discussed in the NPRM that use U.S. data; (c) correctly described the findings of the papers it considered, contrary to the demonstrably erroneous portrayal of those papers in the NPRM; (d) discussed a number of papers that the agencies inexplicably do not even mention in the NPRM; and (e) qualitatively analyzed the applicability of those papers to the CAFE and GHG rulemaking contexts (noting, for example, that studies based on European data are likely of limited relevance; and distinguishing among studies based on the source and quality of data used by those studies).[25] Similarly, the agencies do not acknowledge that EPA finalized and further expanded its TAR rebound analysis in its 2016 Final TSD supporting its 2017 Final Determination, in part by adding consideration of one additional study that was not considered in the TAR (and which study is again omitted in the NPRM).[26]

Moreover, even after the agencies re-analyzed and re-confirmed the 10% value in the TAR (based, in part, on consideration of the majority of the studies which the agencies again discuss in the NPRM), the agencies again finalized the use of a 10% light-duty rebound effect in adopting standards for heavy-duty pickups and vans in 2016.[27] Therefore, although the NPRM

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[20] *Id*.
[21] *Id.* at 4-21, 4-22.
[22] *See* 83 Fed. Reg. at 43,100.
[23] *See* 2018 PRIA at 981-82.
[24] The failure to acknowledge the analysis in the TAR is all the more egregious given that the agencies appear to have copied-and-pasted discussion of numerous studies from the TAR to the NPRM. As discussed below, the discussions of 5 of the studies in the NPRM are identical to the discussions in the TAR. The others appear to have had varying degrees of editing.
[25] *See* Draft TAR at 10-9 to 10-20.
[26] *See* 2016 Final TSD, EPA-420-R-16-021 (November 2016) at 3-8 to 3-20.
[27] *Greenhouse Gas Emissions and Fuel Efficiency Standards for Medium- and Heavy-Duty Engines and Vehicles — Phase 2*, *Proposed Rule*, 80 Fed. Reg. 40,138, 40,453 (July 13, 2015) ("Since [HD pickups and trucks] are . . . more similar in use to large light-duty vehicles, we have chosen the light-duty rebound effect of 10 percent . . ."); *Greenhouse Gas Emissions and Fuel Efficiency Standards for Medium- and Heavy-Duty Engines and Vehicles — Phase 2*, *Final Rule*, 80 Fed. Reg. at 73,872, 73,478 (Oct. 25, 2016) (finalizing use of 10%);

and PRIA are nominally correct in describing that the studies they discuss comprise "estimates of the rebound effect reported in research that has become available since the agencies original survey" supporting the selection of 10% in 2010, the agencies portrayal disingenuously insinuates that the agencies have not subsequently considered the impact of those studies. That is, the agencies omit the fact that the vast majority of the studies it now purports are "new" (and, as described below, *all* of the studies that are relevant) were in fact rigorously considered in previous analyses in which the agencies found those studies support the continued to use of 10%.

We also note that in EPA's 2018 Revised Final Determination, EPA did not substantively acknowledge or discuss, much less update, the Draft TAR's or 2016 TSD's rebound analyses. Instead, EPA simply asserted that "[e]conomic inputs such as . . . the rebound effect . . . should . . . be updated to be consistent with the literature and empirical evidence,"[28] and that "EPA believes it is important to fully consider the effects of a rebound effect to project an accurate assessment of the projected fuel savings, and EPA intends to do so in its new rulemaking."[29]

But, as shown below, EPA has not updated its analysis in the NPRM to be "consistent with the literature and empirical evidence." Instead, the agencies have omitted key literature and empirical evidence that they previously considered, misconstrued both the pre- and post-2008 literature that they have opted to discuss in the NPRM, and removed any semblance of an attempt to differentiate between the relevance and quality of various studies (*e.g.*, by refusing to give appropriately lower weight to international studies, by acknowledging that most studies address fuel price rebound and not fuel economy rebound, by distinguishing studies based on the source and quality of data they analyzed, etc.). As shown below, a thorough and reasoned review of the literature cannot support the agencies' decision to abandon its prior analyses indicating a value of 10%. Such a thorough and reasoned review is, of course, a necessary part of the agencies' legal obligation to accurately and comprehensively acknowledge and grapple with the existing factual record in order to justify a change of position, as well as showing a rational connection between record facts and decisions. *Fox*, 556 U.S. at 515; *State Farm*, 463 U.S. at 43. The agencies' analysis has no basis in the available research nor in the real world, and the agencies must continue to use the 10% value as was rigorously supported in the 2010 Rule, the 2012 Rule, the Draft TAR, and the 2016 TSD.

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[28] 83 Fed. Reg. at 16078 (Apr. 13, 2018).
[29] *Id*. at 16085.

Table 1. "New" studies from 2018 PRIA Table 8-8 which were considered in the draft TAR and 2016 TSD, and the agencies' list of associated long-run rebound estimates		
Study	Estimate of Long-Run Effect	Considered in Draft TAR and 2016 TSD?[30]
Barla et al. (2009)	20%	Yes
Bento (2009)	21-38%	Yes
Wadud (2009)	1-25%	Yes
West & Pickrell (2011)	9-34%	Yes
Ajanovic & Haas (2012)	44%	
Su (2012)	11-19%	Yes
Greene (2012)[31]	8-12%	Yes
Linn (2013)	20-40%	Yes[32]
Frondel & Vance (2013)	46-70%	Yes
Liu (2014)	39-40%	Yes
Gillingham (2014)	22-23%	Yes
Weber & Farsi (2014)	19-81%	
Hymel & Small (2015)	18%	Yes
West et al. (2015)	0%	Yes
DeBorger (2016)	8-10%	Yes
Stapleton (2016, 2017)	14-30%	
TOTAL considered in TAR		**13**
TOTAL new		**3**
TOTAL new based on US data[33]		**0**

[30] Note that although the agencies state that full citations to the studies considered are available in the PRIA, 83 Fed. Reg. at 43099 n. 288, the PRIA does not, in fact, contain the full citations. This deficiency increases the burden of confirming which studies the agencies are actually referring to, and renders it more difficult for the public to understand the justification for the proposal and to comment on it. The agencies should release a list of the full citations to each study.

[31] Although Greene (2012) is listed in the 2018 PRIA Table 8-8, it is not otherwise discussed in the NPRM or the PRIA. Green (2009), however, is afforded a single sentence. *See* 2018 PRIA at 981-82.

[32] The TAR considered a working paper by Linn, which was Linn, J., 2013, "The Rebound Effect of Passenger Vehicles," RFF Discussion Paper, No. 13-19 [EPA-HQ-OAR-2010-0799-0761]. The NPRM continues to cite Linn (2013), and appears to have copied and pasted much of its discussion from the TAR. However, that working paper has since been revised, updated, and published. Therefore, it appears the agencies considered the outdated version of the paper. The updated, published version that should be considered is Linn, J. (2016) The Rebound Effect for Passenger Vehicles, *The Energy Journal*, 37(2): 257-288.

[33] Ajanovic & Haas (2012) used data from the E.U. from 1970-2007; Weber and Farsi (2014) used data from Switzerland in 2010; and Stapleton (2016) and (2017) used data from Great Britain from 1970-2012.

In addition to the studies shown in the table above, the PRIA discusses Hymel et al. (2010), but omits that study from Table 8-8.[34] Hymel et al. (2010) was also discussed in the Draft TAR.

b. The agencies' presentation of unweighted, average values from pre-2008 studies is misleading, inadequate, and contrary to historical agency practice

In the NPRM and PRIA, the agencies suggest that pre-2008 studies of the rebound effect "displayed a strong central tendency" insofar as "the average values of all estimates, those that were published, and authors' preferred estimates from published studies were 22-23%, and the median estimates in each category were close to these values, indicated nearly symmetric distributions."[35] The agencies then suggest that, in 2010, they "singled out" Small and Van Dender (2007)'s finding that the rebound effect appeared to be declining over time, and chose a 10% value on that basis alone.[36]

As described above, the agencies' description of the support underlying the 2010 decision is erroneous. Additionally, however, the 2018 PRIA wholly omits any effort to engage with the substantive findings of the pre-2008 studies, or to analyze how those findings interact with or influence the weight to be afforded to the post-2008 studies, instead purporting simply that an unweighted average of those studies supports their proposed revision to the value of the rebound effect. But even in the interagency review process, EPA observed that utilizing unweighted averages is unreasonable and misleading. The agency described, "[g]iven the broad range of values, EPA believes it is important to critically evaluate which studies are most likely to be reflective of the rebound effect of future GHG/fuel economy standards. In other words, we can't just take the "average" rebound estimates from literature."[37] EPA itself has thus admitted that the NPRM's approach is unreasonable.

Moreover, the deficiency in the agencies' analysis is made apparent by comparison to the Draft TAR, where the agencies' analysis belies any notion that simply citing an unweighted average value of historical rebound rates is adequate. In the TAR, the agencies noted that the "studies that include more recent information (e.g., data within the last decade) may provide more reliable estimates" of the rebound effect,[38] and that affording equal weight to all "historical estimates of the rebound effect may overstate the effect of a gradual decrease in the cost of

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[34] PRIA at 989-90 (discussing Hymel et. al. 2010); id. at 983 (omitting Hymel e. al. from Table 8-8).
[35] 2018 PRIA at 981.
[36] *Id*.
[37] Docket Entry: E.O. 12866 Review Materials for The Safer Affordable Fuel-Efficient (SAFE) Vehicles Rule for Model Years 2021-2026 Passenger Cars and Light Trucks NPRM, Docket ID EPA-HQ-OAR-2018-0283-0453 (hereinafter "EO12866 Review Materials") at PDF page 120.
[38] Draft TAR at 10-10.

driving due to the standards."[39] But the agencies nevertheless presented results from individual historical studies and discussed their relative weights, rather than presenting merely average and median values.[40] And the agencies grouped these individual studies into categories according to whether they utilized Aggregate Time-Series Data on vehicle travel throughout the U.S., U.S. State Level Data, or U.S. Survey Data,[41] and noted that the "values based on overall aggregate rebound effects" are "more applicable to quantifying the impact of" CAFE and GHG standards.[42] The agencies then provided an in-depth discussion of the individual studies, noting that some studies were less relevant than others. The agencies observed, for example, that some studies "actually quantify the price elasticity of gasoline demand . . . or the elasticity of VMT with respect to the price of gasoline . . . , rather than the elasticity of VMT with respect to fuel efficiency or the fuel cost per mile of driving."[43] And the agencies observed the nuances of the various findings, observing (for example) that one study could not identify a consistent effect for SUVs,[44] one study found that fuel economy did not have a statistically significant impact and therefore "rejected the hypothesis of equal elasticities for gasoline prices and fuel efficiency,"[45] and that some studies found asymmetric responses – that is, that consumers are more responsive to an increase in prices than to a decrease in prices.[46]

Finally, rather than focusing solely on the average or median value of the historical studies (as the agencies do in the NPRM and PRIA), in the TAR the agencies noted that the range of estimates "for the historical, aggregate rebound effect in most research" was between 10 to 30 percent,[47] and weighed this range against the projections of future estimates shown in more recent literature to conclude that a value at the low end of the range was appropriate.[48]

As to the historical, pre-2008 studies, the agencies cannot now simply waive their hands, render their previous, nuanced and rigorous analysis inapplicable without explanation, and choose instead to appeal only to uncritical, non-discriminating historical averages to suggest departing from their previous conclusion is justified.

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[39] *Id*. at 10-20. The term "historical estimates" is used by the agencies, and in this comment, to refer to any estimate obtained through use of real-world data from years past, rather estimates projected for future years.
[40] *Id*. at 10-10 to 10-15.
[41] *Id*. at 10-10 to 10-11.
[42] *Id*. at 10-20; EPA reiterated its position on this point in the interagency review process, stating that "[w]ithin the existing literature, aggregate, time series studies of the U.S. provide the most reliable estimates of the rebound effect for use in LDV rulemakings." EO12866 Review Materials, File: Email_5_-_Email_from_William_Charmley _to_Chandana_Achanta_ -_June_18, =_2018 at PDF page 122.
[43] *Id*. at 10-11.
[44] *Id*. at 10-13.
[45] *Id*. at 10-14.
[46] *Id*. at 10-14 to 10-15.
[47] *Id*. at 10-20.
[48] *Id*. at 10-20.

c. The agencies' discussions of the purportedly "new" studies cited in the NPRM and PRIA are erroneous and cannot support revising the rebound rate to 20%

The agencies' discussion of new, post-2008 studies is likewise deficient and misleading. As shown above, in the Draft TAR the agencies considered 13 of the 16 studies shown in Table 8-8 of the PRIA. However, although in the Draft TAR the agencies correctly concluded that these studies supported a rebound effect value of 10%, now the agencies (without acknowledgement or justification) change their reading of those studies, and misconstrue, contort, or simply omit key findings (as well as entire studies) to arrive at their conclusion that a 20% value is compelled. The agencies are wrong and their treatment of the underlying research is unreasonable.

First, we note that the agencies' discussions of 5 specific studies are *identical* to their discussion of those studies in the Draft TAR.[49] That is, the agencies appear to have simply copied and pasted the discussion of those studies from the Draft TAR into the PRIA. The discussions of the remaining overlapping studies have been revised to varying degrees.[50] And, as described above, the agencies have simply omitted discussion of other studies that were included in the Draft TAR and 2016 TSD.

i. The agencies depart from their own prior (accurate) discussions of studies demonstrating that the rebound effect is declining, and instead misread and distort the findings of those studies

Perhaps most problematically, the agencies now suggest in the PRIA that the finding of Small and Van Dender (2007) that the rebound effect is declining over time has been undermined by later findings, including those of Hymel, Small and Van Dender (2010) and Hymel and Small (2015). This suggestion is directly contrary to the studies themselves, and to the agencies' previous (accurate) assessment of them.

In both the Draft TAR and the 2012 Final Rule, the agencies observed that Hymel, Small and Van Dender (2010) confirmed "that the rebound effect was declining over time," finding that the long-run rebound effect from 1966 to 2004 was 24 percent, while for 2004 it was 13

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[49] These studies are: Bento et al. (2009); Wadud et al. (2009); West & Pickrell (2011); Su (2012); and Liu et al. (2014).

[50] These studies are: Barla (2009); Hymel, Small, and Van Dender (2010); Hymel and Small (2015); Linn (2013); Greene (2012); Frondel et al. (2012); Gillingham (2014); West et. al. (2015); De Borger et. al (2016).

percent.[51] The Draft TAR also observed that in Hymel and Small (2015) found that, "[c]onsistent with previous results, the VMT rebound effect declines with increasing income and urbanization, and it increases with increasing fuel cost."[52] And the agencies noted that the most significant factor impacting variation in the rebound effect "is income, whose effect is large enough to greatly reduce the projected rebound effect for time periods of interest to current policy decisions."[53] Nevertheless, the agencies noted that Hymel and Small (2015) also found that, considering average income, fuel cost, and urbanization in the U.S. during 2000-2009, the rebound effect for those years had increased somewhat, up to 17.8%.[54] But both agencies correctly noted that: (1) Hymel and Small (2015) hypothesized that this increase was due to media coverage of fuel prices, price volatility, and asymmetric responses to price changes;[55] (2) while the first two of these factors "are important to understand the rebound effect based on fuel prices, they may not be as relevant to the rebound effect due to fuel efficiency;"[56] (3) these two factors (media coverage and volatility) together accounted for more than half of the increase in long-run rebound, rendering that impact less relevant in the CAFE and GHG standards context;[57] and (4) the overall impact of the factors causing the increase "are small enough in magnitude that they do not fully offset the downward trend in VMT response elasticities due to higher incomes and other factors."[58] Thus, the agencies concluded, "even assuming that the variables retain their 2003–2009 values into the indefinite future, they would not prevent a further diminishing of the magnitude of the rebound effect if incomes continue to grow at anything like historic rates."[59]

In the NPRM and PRIA, the agencies abandon their previous assessments of both Hymel, Small and Van Dender (2010) and Hymel and Small (2015), omitting any discussion of key findings and ignoring their prior nuanced analysis, and skewing the paper without support or justification to suggest that it defeats, rather than supports, the notion that the rebound effect is declining over time.

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[51] Draft TAR at 10-14; 77 Fed. Reg. at 62,924, 62,995; *accord* Hymel, Small and Van Dender (2010) at 23, 35.
[52] *Id*. at 10-17.
[53] *Id*.
[54] *Id*.
[55] *Id*. Indeed, Hymel and Small (2010) observed that the data ("suggests that the rise in the magnitude of the elasticity of VMT during the 2000s was due more to volatility than to the higher level of fuel price"). Thus, even if using the 18% value returned by Hymel and Small (2010) were not fundamentally flawed as described below, weighting that value equally in the agencies' analysis would nevertheless be inconsistent with the agencies' assertions elsewhere that fuel prices in the future will not be volatile. *See* 83 Fed. Reg. at 43,214 & n.444 (suggesting that, while oil prices may increase in the future, they will not subject to "sudden and large" shifts).
[56] *Id*.
[57] *Id*.
[58] *Id*.
[59] *Id*.; *See also* Hymel and Small (2015) at 103.

Although the PRIA does contain one sentence acknowledging that the two papers demonstrate that "the fuel economy rebound effect declined over time in response to increasing personal income and urbanization,"[60] the agencies' only discussion of that finding is an attempt to minimize it by asserting without support that the finding the studies were not "able to detect whether [the] apparent decline in response to rising income levels over time truly reflects its changing effect on drivers' response [sic] improving fuel economy – the rebound effect itself – or simply capture the effect of rising income on their sensitivity to fuel *prices*."[61] This assertion demonstrates the absurdity of the agencies' analysis generally, as virtually *all* of the studies discussed in the PIRA estimate only consumers' responses to fuel prices. Thus, to the extent the agencies propose to minimize the importance of these two papers' findings as relevant only to fuel price changes and not to fuel economy changes, that proposal undermines the agencies' consideration of nearly every other study they discuss. And, as discussed in more detail below, those studies that do consider fuel economy directly almost universally found that the fuel economy effect is lower than the fuel price effect, and Hymel and Small (2015) in particular found that the fuel economy effect was "statistically indistinguishable from zero."[62] Correctly understood, Hymel et al. (2010) and Hymel and Small (2015) show that consumers' response to fuel prices is declining over time, and that their response to fuel economy is even lower than that their response to fuel prices - and might in fact be zero.

Regardless, notwithstanding their (erroneous) assertion that the research is not relevant because it regards fuel price rebound and not fuel economy rebound, the agencies focus the remainder of their discussion of Hymel, Small and Van Dender (2010) and Hymel and Small (2015) on those studies' express findings regarding fuel price (and fuel cost) rebound. In particular, the agencies note that the studies found that the rebound effect "rose during periods when fuel price increased."[63] And even this description of the studies' finding is misleading, insofar as it connotes mere correlation between historical fuel costs and rebound rates. In contrast, the Draft TAR described causation, observing that studies found that the rebound rate "increases with increasing fuel cost," which finding was "consistent with previous results."[64] That the relationship is causal suggests that the rebound rate also *decreases* with *decreasing* fuel costs, as would be caused by more stringent fuel economy standards. *See* Draft TAR at 10-12 (acknowledging that fuel costs "depend in part on each vehicle's fuel economy"). This implies that as fuel economy standards become more stringent, the rebound effect will become less pronounced. But the agencies do not discuss this implication at all.

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[60] 2018 PRIA at 989.
[61] *Id*. (emphasis in original).
[62] *S*ee Comment filed by Kenneth A. Small at 2.
[63] *Id*.
[64] Draft TAR at 10-17.

Further, the PRIA's focus on the fuel price findings stands in stark contrast to the Draft TAR's correct observation that Hymel and Small (2015) found that price volatility and media coverage thereof caused the rebound effect to increase, and that those two factors were of limited relevance in the fuel economy rulemaking context.[65] The PRIA omits any similar analysis. Instead, the PRIA retains *only* the observation from the Draft TAR that "half of the apparent increase in the rebound effect for recent years could be attributed to greater volatility in fuel prices and more media coverage of sudden price changes," and omits the Draft TAR's discussion of the importance or relevance of this fact.[66] That is, the PRIA fails entirely to acknowledge or discuss the Draft TAR's conclusions that (a) while these two factors may be relevant "to understand the rebound effect based upon fuel *prices*, they may not be as relevant to the rebound effect due to fuel *economy*," and the agencies are only concerned with the latter in the CAFE and GHG standards context; and (b) even if these factors were relevant, the overall impact of the factors driving the increase in the rebound effect through 2009 were not significant enough to "fully offset the downward trend in VMT response elasticities due to higher incomes and other factors" and therefore even if those factors were to continue into the future, they "would not prevent a further diminishing of the magnitude of the rebound effect."[67]

Having deleted all analysis necessary to understand the relevance, applicability, and importance of the results of Hymel, Small and Van Dender (2010) and Hymel and Small (2015), the agencies then rely solely on the raw magnitude of the increased estimates returned by those studies. But the agencies cannot simply ignore their own previous analysis demonstrating that these figures, in context, do not undermine the agencies' prior determination that the appropriate estimate of the rebound effect is 10%.

Further, the agencies depict those estimates incorrectly. The agencies proclaim that the two studies "each revised Small & Van Dender's original estimate of any 11% rebound effect for 1997-2011 upward when they included more recent experience – to 13% for the period 2001-2004, and subsequently to 18% for 2000-2009."[68] But even the studies' author repudiates the agencies' characterization of the latter estimate. As Kenneth Small describes, this latter estimate is the "'base model', which is the starting point for the models which are the main object of the paper."[69] The "two more realistic models . . . yield estimates of 4.0% and 4.2%, respectively."[70]

And even these 4.0% and 4.2% values may be high. Hymel and Small (2015) noted that the data could support the conclusion that fuel economy rebound was lower than fuel price rebound, but the authors sought to ensure that they were "conservative both in the sense of

[65] *Id.*
[66] 2018 PRIA at 990.
[67] *Compare id. with* Draft TAR at 10-17.
[68] 2018 PRIA at 989-90; 993.
[69] Comment of Kenneth Small, NHTSA-2018-0067-7789, at 1.
[70] *Id.*; see also Hymel and Small (2015) at 103 (Table 8).

adhering to standard theory and of ensuring that [they] do not underestimate the rebound effect on this account," and so they "maintain[ed] the hypothesis of equality" between fuel price rebound and fuel economy rebound" in calculating and presenting their results.[71]

Adding insult to injury, the agencies take one more misleading step and include only their erroneous 18% figure from Hymel and Small (2015) in their summary table, omitting the 4% and 4.2% estimates from what Small describes as the "more realistic models" as well as the 13% estimate from Hymel, Small and Van Dender (2010) entirely.[72] The agencies cannot support the choice of 20% by merely hiding those estimates which do not support that figure.

Finally, the agencies make one more revision which hides the fact that these two studies actually undermine their decision to adopt a 20% rebound effect. In the Draft TAR, the agencies observed that Hymel and Small (2015) showed "strong evidence of asymmetry in responsiveness to price increases and decreases" and "suggest that a rebound adjustment to fuel price rises takes place quickly; the rebound response elasticity is large in the year of, and the first year following, a price rise, then diminishes to a smaller value. The rebound response to price decreases occurs more slowly."[73] In the PRIA, the agencies revise their prior discussion to instead state that the studies "suggest that households curtail their vehicle use within the first year following an increase in fuel prices and driving costs, while the increase in driving that occurs in response to declining fuel prices – and by implication, to improvements in fuel economy – occurs more slowly."[74] This revision is again significant. In the PRIA, the agencies have erased their prior acknowledgement that the response to fuel prices "diminishes to a smaller value" over time, which further suggests that the consumer response measured in 2009 was a high point of the rebound rate rather than the start of an upward trend.[75] In other words, the agencies removed yet another correct observation which, if included, counters the agencies' attempt to minimize the studies' central finding that the rebound rate is declining over time.

In addition to misconstruing and misrepresenting the Hymel, Small and Van Dender (2010), and Hymel and Small (2015) studies, the agencies eliminate almost entirely their prior discussions of other studies confirming that the rebound effect is declining over time. In particular, the Draft TAR contained extensive discussion of Greene (2012).[76] In that discussion, the agencies observed that Greene's findings support the conclusions that (1) the rebound effect "is by now on the order of 10 percent;" (2) the rebound effect could decline over time; (3) although fuel prices "had a statistically significant impact on VMT, . . . fuel efficiency did not,"

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[71] Hymel and Small (2015) at 97.
[72] *Id*. at 983; see *also* 83 Fed. Reg at 43101 (Table II-44).
[73] Draft TAR at 10-17.
[74] 2018 PRIA at 990.
[75] *See also* Comment of Kenneth Small, NHTSA-2018-0067-7789, at 1 (describing the finding that "the response to fuel-price rises is greater in magnitude than the response to fuel-price declines").
[76] *See* Draft TAR at 10-14.

suggesting the relevant rebound effect for policymaking purposes "could be zero;" and (4) the rebound effect for changes in price per mile were "approximately 12 percent in 2008, . . . drops to 10 percent in 2020 and to nine percent in 2030."[77] In contrast, in the NPRM the agencies do not reference Greene (2012) at all - other than a single unexplained reference to the study in Table 8-8, which lists the study as finding a rebound rate of 8-12%.[78] And even that presentation is contrary to EPA's prior observations (both in the TAR and in the interagency review process for this rulemaking) that the study in fact found that the rebound effect "is by now on the order of 10%."[79]

It should be noted that, although neither the NPRM nor the PRIA discusses Greene (2012), it affords one sentence for Greene (2009), describing that in 2010, "[t]he agencies also took particular note of recent EPA-funded research by Greene (2009), which replicated the finding that the rebound effect appeared to be declining over time as U.S. income levels increased using time-series data for the U.S., and projected that it could decline to 10% by the year 2020 with continued income growth."[80] But the agencies do not further analyze or apply this finding, which contradicts their analysis and conclusions. Again, the agencies have eliminated virtually their entire analysis of the relevance, applicability, and importance of the results of these two studies that contradict their own proposed outcome - they simply delete the discussion almost entirely, and present the results of the 2012 study in a manner inconsistent with their prior analyses.

ii. The agencies erroneously rely on studies based on international data, which the agencies have previously acknowledged are of limited relevance to the U.S. rulemaking context

In previous analyses, including the Draft TAR and 2016 TSD, the agencies have correctly acknowledged that "[i]t is not clear whether studies of LDV VMT rebound estimates for countries different from the U.S. would provide estimates that are appropriate to the U.S. context."[81] As the agencies observed, "[f]or example, European countries have higher fuel prices and more transit options, both factors which would possibly produce a VMT rebound effect that is higher than in the U.S."[82] EPA even acknowledged as much in the interagency review

[77] *Id.*
[78] 2018 PRIA at 987.
[79] *Id.*; Docket Entry: E.O. 12866 Review Materials, File: EPA_comments_on_the_NPRM_sent _to_OMB,_June_ 29,_2018 at 172; Greene (2012) at 27.
[80] 2018 PRIA at 981-82. We note, again, that the agencies have not provided full citations to these studies. In the PRIA, the agencies cite to Greene (2009), while in the Draft TAR the agencies analyzed Greene, David, 2012. "Rebound 2007: Analysis of U.S. light-duty vehicle travel statistics," Energy Policy, vol. 41, pp. 14-28. The agencies must clarify which study the PRIA and NPRM refer to.
[81] *See* Draft TAR at 10-19; 2016 TSD at 3-19.
[82] Draft TAR at 10-19; 2016 TSD at 3-19.

process, describing that "[e]ven well executed international studies do not provide reliable estimates of the U.S. rebound effect, as the U.S. has different travel patterns from other countries due to a variety of factors."[83]

As a result, the agencies have historically centered their analysis on the plethora of studies available using U.S. data, and afforded little or no weight to international studies. In particular, in the Draft TAR the agencies expressly noted that De Borger (2016) (which studied Denmark), Barla (2009) (which studied Canada), and Frondel and Vance (2012) (which studied Germany) were of limited value in the U.S. context.[84,85]

However, in the PRIA the agencies do not qualify their consideration of international studies at all. Instead, they expand their consideration of De Borger (2016) without qualification, and use that study as a centerpiece of their unjustified effort to discredit key findings that the rebound effect declines over time, suggesting that those findings are limited to fuel price effects rather than fuel economy effects, as described above.[86] But, just as with the agencies' attempt to discount Small and Van Dender (2010) and Hymel and Small (2015) described above, the agencies again simply highlight their own inconsistencies - nearly all of the papers they discuss study the fuel price effect, and not the fuel economy effect, yet the agencies only highlight this fact as a reason to discount a finding if that finding weighs against their desired conclusion. Moreover, even De Borger describes its use of Danish data as an "obvious limitation" on its applicability, describing that Denmark "has relatively high car taxes and (partly as a consequence) a low share of car-owners, and it is not obvious that the results can be transposed to other countries."[87] And finally, contrary to the agencies' portrayal, De Borger in fact found that "fuel price sensitivity of the demand for kilometers is declining with household income."[88] Thus, the agencies not only ignore the authors' own assertion that this study is inapplicable, the agencies get the central findings wrong.

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[83] EO12866 Review Materials, File: Email_5_-_Email_from_William_Charmley_to_Chandana_ Achanta_ -_June_18, =_2018 at PDF page 122.
[84] Draft TAR at 10-16, 10-19, 10-20.
[85] The 2018 PRIA references Frondel and Vance (2012), while the Draft TAR discussed Frondel, M., and Vance, C., 2013. Re-Identifying the Rebound: What about Asymmetry? Energy Journal 34 (4):43-54. Again, without full citations in the PRIA, it is difficult to determine whether the agencies are in fact referring to the same study in both documents.
[86] *See* 2018 PRIA at 993 (arguing that "DeBorger et al. (2016) separated the effects of variation in household income on the sensitivity of vehicle use to fuel prices and fuel economy, and found that the decline in the rebound effect with rising income reported in Small & Van Dender (2007) and subsequent research resulted entirely from a reduction in drivers' sensitivity to fuel prices as their incomes rose").
[87] De Borger (2016) at 15.
[88] *Id*.

The agencies also dramatically expand their discussion of Barla (2009) from a single sentence[89] to two full paragraphs.[90] And the agencies revise the discussion of Frondel and Vance (2012) from a brief mention with a significant caveat (e.g, that the utility of European studies is limited)[91] to two full paragraphs.[92]

Moreover, only three studies in the NPRM and PRIA were not included in the TAR and 2016 TSD, and *all three* are international studies – at least two of which pre-date the TAR. Anajovic and Haas (2012) studied six European nations,[93] Weber and Farsi (2014) studied Switzerland,[94] and Stapleton (2016, 2017) studied Great Britain.[95]

The agencies must remove these studies from consideration – including them infects the analysis with undue upward bias for the very reason the agencies gave them limited or no weight in prior analyses: the studies reflect higher fuel prices and a wider range of transportation alternatives and so are not directly applicable to domestic conditions. Indeed, the three highest outlier estimates of the rebound effect are derived from Weber & Farsi (2014) (estimating 19-81%); Frondel & Vance (2013) (46-70%); and Ajanovic & Haas (2012) (44%) and demonstrates the undue bias they insert into the analysis. The agencies cannot bias their sample upward by considering studies that the agencies themselves have observed have little or no relevance in the U.S. context.[96]

Moreover, removing these studies reveals that the *only* relevant studies considered in the NPRM and the PRIA were *also* considered in the Draft TAR and 2016 TSD, wherein the agencies concluded that they supported a rebound effect value of 10%. The agencies' current analysis is not a reconsideration based on new information, it is a wholesale revision of its prior analysis based not on reason but on whim.

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[89] Draft TAR at 10-19.
[90] 2018 PRIA at 984-85.
[91] Draft TAR at 10-16.
[92] 2018 PRIA at 987.
[93] *Id*. at 986.
[94] *Id*. at 988.
[95] *Id*. at 991-92.
[96] Further highlighting the agencies' error – there are numerous other studies available using international data which the agencies have not mentioned or considered. *See, e.g.*, Chitinis, et. al, (2014) "Who rebounds most? Estimating direct and indirect rebound effects for different UK socioeconomic groups." Ecological Economics 106 (2014). 12–32. Thus, not only have the agencies failed to justify their change in position regarding the relevance of international studies, they have failed to justify their decision to consider only *some* international studies to the exclusion of others. The agencies must re-adopt their previous stance that international studies have limited or no relevance for projecting U.S. policy outcomes.

iii. The agencies erroneously rely on a study of gasoline demand elasticity, which the agencies have previously acknowledge is "not appropriate for measuring the VMT rebound effect"

The agencies also cite and discuss Wadud (2009). That study estimated the elasticity of gasoline demand, not the elasticity of VMT. *See* Wadud (2009) at 2740. The agencies have previously acknowledged that "[e]stimates of the elasticity of demand for gasoline, while a useful point of comparison, are not appropriate for measuring the VMT rebound effect because they reflect consumer selection of vehicle fuel efficiency in addition to VMT." 77 Fed. Reg. at 62,924. The agencies do not so much as acknowledge their prior position, much less explain why they have departed from it. Moreover, Wadud (2009)'s data was from 1984 to 2003. Wadud (2009) at 2743. As established elsewhere in this comment, the agencies cannot plausibly rely on data from 17 to 36 years prior to first model year subject to this rulemaking, and data from that period cannot render incorrect the agencies' 2012 analysis regarding the projected future value of the rebound effect. Thus, the agencies cannot rely on Wadud to support revising the rebound effect above 10%.

iv. The agencies erroneously portray as "new" studies that were considered in the 2012 rulemaking, and studies that provide only outdated historical estimates of the rebound effect

In addition to the fact that all of the relevant studies now considered by the agencies were also considered in the Draft TAR, several of the studies were also considered in the original 2012 final rule. There, the agencies observed that Bento et. al. (2009) "estimated that the rebound effect averaged 34 percent for all households, but varied widely among those owning different types and ages of automobiles, and among households with varying demographic characteristics."[97] The agencies' discussions in the Draft TAR and the PRIA are identical, and similarly observe that Bento returned a "composite estimate of 34%," but with results varying by household composition.[98] The agencies also discussed West and Pickrell (2011) and Su (2012) in the 2012 rule.[99]

More important than the agencies' substantive discussion of these studies is the agencies' portrayal of them as "new" since the agencies' original analysis.[100] As described above, none of these studies are "new" insofar as the agencies considered them in the 2012 final rule (and, as with all of the other studies in the NPRM, in the Draft TAR).

[97] *See* 77 Fed. Reg. at 62995.
[98] 2018 PRIA at 984; Draft TAR at 10-12.
[99] *See* 77 Fed. Reg. at 62995.
[100] 2018 PRIA at 994 (referring to table 8-8, which includes Bento, as containing research conducted since the agencies' original analysis).

Further, although the agencies purport that their proposed revision of the rebound effect value is due in part to consideration of studies "conducted since the agencies' original 2008 review of evidence,"[101] the agencies' prior analyses centered on findings that the rebound effect was declining, and thus would be smaller in the future than at the time of the rulemaking. *See* 2010 Joint TSD at 4-22 ("The 10 percent estimate meets this condition, since it lies below the 15-30 percent range of estimates for the historical rebound effect reported in most previous research, and at the upper end of the 5-10 percent range of estimates for the future rebound effect."); *id*. at 4-20 (observing that the rebound effect "is declining in magnitude," and NHTSA's "forecast values of the rebound effect . . . suggest that this decline is likely to continue through 2030.").[102]

Given that the agencies' prior analyses centered on projections of the rebound effect *after* the rules were adopted means the relevant consideration for the current NPRM is not whether individual studies were published after those rulemakings, but whether those new studies utilized data newer than that utilized in the studies already considered, and thus could confirm or deny the validity of the agencies' previous projections regarding future rebound rates.

Yet some of the studies the agencies now suggest are "new" in fact provide estimates only for the historical rebound effect. For example, Bento (2009)'s dataset was from 2001 – fully 11 years before the 2012 rule, and 20 years before model year 2021 – the first year that is relevant to the NPRM's analysis.[103] Given the various studies that have confirmed that the rebound effect is declining over time, affording equal weight to Bento (2009)'s outlier value in the analysis regarding whether the agency's 2012 projection of future rebound was justified is unreasonable.

Simply, the agencies cannot plausibly suggest that Bento (2009) represents "new" information. In 2012, the agencies considered Bento's analysis alongside the other studies based on data from similar time periods, and concluded that, when weighing those historical estimates alongside projections of future evidence, the data supported a rebound effect of 10% moving forward.[104] That Bento was included within that prior analysis belies the agencies' suggestion that Bento militates against the conclusions of that analysis.

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[101] 2018 PRIA at 993.
[102] *See* also Draft TAR at 10-20 ("[H]istorical estimates of the rebound effect may overstate the effect of a gradual decrease in the cost of driving due to the standards. As a consequence, a value on the low end of the historical estimates is likely to provide a more reliable estimate of its magnitude during the period spanned by the analysis of the impacts of the MYs 2022–2025 standards.")
[103] *See* 77 Fed. Reg. at 62995.
[104] *Id.*

Moreover, that the PRIA discusses Bento (2009)'s outlier value separately, but buries the remaining studies published prior to the 2012 rule in a table presenting only (misleading) average values (as discussed above), is inappropriate. The agencies cannot plausibly afford Bento equal weight to other, more recent studies in their analysis, and the study's 34% estimate should thus be removed from Table 8-8. And the same is true of the other studies considered in the 2012 analysis.

v. The agencies erroneously afford equal weight to studies based on 2009 NHTS survey data, which the agencies have previously acknowledged may not provide reliable estimates

As in the Draft TAR, the agencies again include discussion of six studies based on data from the 2009 National Household Travel Survey (NHTS). These include West and Pickrell (2011), Su (2012), Linn (2013) (as discussed above), Liu et. al (2014), Gillingham (2014), and West et. al. (2015). As the EPA observed in the 2016 TAR, these studies, "each using NHTS 2009 data, find rebound effects that vary from 11-40 percent based upon household survey data."[105] And the EPA observed that "[t]hese widely different results based on survey data from the same year suggest that these studies may not necessarily provide reliable estimates of the VMT rebound effect."[106] EPA even acknowledged again that this data is not reliable in the interagency review process. EPA stated, "[e]ven well executed U.S. studies using single year data, particularly from the NHTS 2009 time period with the onset of the Great Recession, have difficulties in providing reliable estimates of the U.S. rebound effect."[107] EPA has offered no justification for now affording equal weight to studies based on 2009 NHTS data, when just two years ago it concluded that studies based on that data should be viewed with skepticism.

Moreover, rebound experts appear to agree that 2009 NHTS data in particular is of questionable quality. Ken Gillingham observes that the most appealing attribute of the NHTS data to researchers is that it is easily accessible.[108] However, he notes that the data has limitations. In particular:

> The VMT estimates in this data source are derived from self-reported travel diaries, which require a fairly substantial amount of effort by the survey-takers. This raises some questions about the validity of the survey data, as households that are willing to spend their time taking down their driving may also be households that pay more attention to the cost of driving and make driving decisions accordingly. This is an

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[105] 2016 TSD at 10-20.
[106] *Id*.
[107] EO12866 Review Materials, File: Email_5__Email_from_William_Charmley_to_Chandana_Achanta_-_June_18,_2018 at PDF page 122.
[108] Comment submitted to the docket by Ken Gillingham (observing that "the availability of the National Household Transportation Survey (NHTS) has led to many papers that use this data source").

inherent challenge in using such survey data. Survey data can certainly still provide useful insights, but one must be cautious in interpreting it, especially when there is other evidence available.[109]

Joshua Linn has similarly observed that the estimates of VMT in the 2009 NHTS survey "may be noisy when compared to VMT calculated from multiple odometer readings," and that "[s]tudies that use VMT based on multiple odometer readings therefore should have lower measurement error, and yield preferable estimates from a statistical point of view."[110] And Cinzia Cirillo (co-author of Liu et. al. (2014)) notes that the NHTS data may cause estimates based thereon to skew higher, because that data was collected during a period of unusually high gas prices, and that it is "well known in economics" that "[w]hen the fuel price is high, a higher percentage of the income is used to pay for fuel, and this causes a higher elasticity to fuel cost."[111] Indeed, even Liu et. al. (2014) noted that "dataset that we used was collected in 2009 where fuel prices were particularly high and that the conditions of the US economy at that time were not particularly good."[112] This latter observation is an understatement – the data was collected from March 2008 to May 2009: the start of the great recession. The limited applicability of this data to normal conditions should be intuitive. Indeed, the agencies themselves have acknowledged the limitations of utilizing data derived from the period of the great recession.[113]

Simply, as EPA has previously acknowledged, estimates of the rebound effect derived from 2009 NHTS data suffers significant limitations, and should be afforded little weight by the agencies, if not ignored altogether.

vi. The agencies omit a broad range of recent studies

In addition to ignoring the agencies' own previous analyses of the studies that they now contort and purport to rely on, the agencies ignore recent literature on the rebound effect – virtually all of which supports a rebound value lower than 10%.

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[109] *Id*.
[110] Comment of Joshua Linn, NHTSA-2018-0067-7188, at 2.
[111] Comment of Cinzia Cirillo, NHTSA-2018-0067-7819, at 2.
[112] Liu et al. (2014); *See also* Comment of Cinzia Cirillo, NHTSA-2018-0067-7819, at 2; (author of Liu et. al. (2014) observing that the data derives from a period in which gas prices were largely greater than $4 per gallon.) Given the findings in the literature that rebound decreases as fuel costs decrease, relying on data from a period of $4 gas prices to estimate the rebound effect for the period of the standards is inconsistent with the agencies' assumption that "average gasoline prices would not exceed $4/gallon (in real dollars)" between 2018 and 2050. 83 Fed. Reg. at 43,214-215.
[113] *See*, *e.g.*, 77 Fed. Reg. at 62655 (describing that the agencies "do not believe" market forecasts that were "particularly influenced by the [great] recession . . . are reasonably reflective of future trends"); *See also* EO12866 Review Materials, File: Email_5_-_Email_from_William_Charmley_to_ Chandana_ Achanta_ -_June_18, =_2018 at PDF page 120 (describing the anomalous conditions during the time in which NHTS data was collected).

In particular:

- Wang and Chen (2014)[114] (which was considered in the 2016 TSD, but is omitted in the NPRM) used data from the 2009 National Household Travel Survey. As the EPA previously described, they found that the rebound effect for fuel efficiency changes is only significant for the lowest income households (up to $25,000)."[115] The agencies further observed that "Wang and Chen hypothesize that travel demand for these households are far from saturation, therefore getting more fuel efficient cars provides the opportunity to fulfil so called "latent demand."[116] Wang and Chen's finding is consistent with the broader set of findings suggesting rebound declines as income increases.
- Leung (2015) used 2009 NHTS survey data, and estimated that the rebound effect is 10%.[117]
- Gillingham (2011) used data from odometer readings in California from 2001 to 2009, and estimated that the rebound effect from fuel economy is 1%.[118]
- Gillingham et. al. (2015) used data from odometer readings from Pennsylvania from 2000 to 2010, and estimated that the rebound effect is 10%.[119] However, even that figure is likely too high for the agencies' purposes, as the study also found that "a high percentage of the vehicles are almost entirely inelastic in response to gasoline price changes" and that "the lowest fuel economy vehicles in the fleet drive the responsiveness, with higher fuel economy vehicles highly inelastic with respect to gasoline price changes."[120]
- Langer et. al. (2017) used data from odometer readings in Ohio from 2009 to 2013 and estimated that the rebound effect is 11.7%.[121]

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[114] Wang, T. and Chen, C., 2014. "Impact of fuel price on vehicle miles traveled (VMT): do the poor respond in the same way as the rich?" Transportation 41(1): 91-105.
[115] 2016 TSD at 3-16; *see also* Wang and Chen (2014) at 102.
[116] *Id*. Because Wang and Chen did not find statistically significant effects for higher income levels, it is difficult to extract a single reliable estimate of the overall rebound effect from their study, and thus no such figure is included in Table 3, below.
[117] Leung, W. (2015) Three Essays in Energy Economics, Dissertation, at 46, available at: https://escholarship.org/content/qt3h51364m/qt3h51364m.pdf
[118] Gillingham, K. (2011) The Consumer Response to Gasoline Prices: Empirical Evidence and Policy Implications, Dissertation, at 100 (Table 3.3) (value for log(fuel economy)), available at: https://stacks.stanford.edu/file/druid:wz808zn3318/Gillingham_Dissertation-augmented.pdf
[119] Gillingham, K., A. Jenn, and I. Azevedo (2015) Heterogeneity in the Response to Gasoline Prices: Evidence from Pennsylvania and Implications for the Rebound Effect, Energy Economics, 52(S1): S41-S52, at S41.
[120] *Id*.
[121] Langer, A., V. Maheshri, and C. Winston (2017) From Gallons to Miles: A Disaggregate Analysis of Automobile Travel and Externality Taxes, *Journal of Public Economics*, 152: 34-46, at 38.

- Knittel and Sandler (2015) used odometer data from California from 1998 to 2010, and estimated that the rebound effect is 13%.[122]
- Wenzel and Fujita (2018) used data from odometer readings from over 30 million vehicles in four urban areas of Texas, over a six-year period from 2005 to 2010. They concluded that the average fuel price rebound effect is 9% and the average cost-per-mile rebound effect is 16%. However, Wenzel and Fujita also found that the rebound effect declines as a vehicle's fuel economy increases.[123] For vehicles with "high" fuel economy (which are the most relevant category for purposes of the 2021-2026 standards),[124] they estimate the fuel economy rebound effect is 5.2%.[125]
- Ficano and Thompson (2014) used 2009 NHTS survey data to "estimate vehicle miles traveled rebound from fuel price variation and from hybrid vehicle ownership."[126] Although they estimated that the average rebound effect is between 56% and 78%,[127] they also estimated that for vehicles rated at greater than 20 mpg, the rebound effect was only 14.2%, while for vehicles rated at less than 20 mpg the rebound effect was 82.1%.[128] Again, for purposes of the 2021-2026 standards, the high-mpg category is the most relevant.

As shown above, *all* of these studies estimate the relevant rebound effect to be substantially lower than the agencies' proposed estimate of 20%, and all of them stand contrary to the agencies' assertion that the 20% rebound effect value "more accurately represents the findings from . . . more recent analyses."[129] To the contrary, as shown below, these studies demonstrate that the average value of the most relevant and reliable studies undertaken since 2012 directly

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[122] Knittel, C. and R. Sandler (2015) The Welfare Impact of Second-Best Uniform Pigouvian Taxation: Evidence from Transportation, at 2, available at: https://pdfs.semanticscholar.org/f424/ef079e4c60b787c48a95ac04b6310e1573e8.pdf. Publication of this paper appears to be forthcoming in 2018 in the *American Economic Journal: Economic Policy. See* https://www.aeaweb.org/articles?id=10.1257/pol.20160508&&from=f

[123] This is consistent with the above-described findings that the rebound effect declines as the cost-per-mile of driving declines. It is also consistent with intuition – as the cost per mile decreases, the dollar-value of any given percentage change in that cost-per-mile likewise diminishes. And consumers will naturally change their behavior more for a larger dollar-figure shift in costs. In other words, consumes will change their behavior more to save $10 than they will to save $0.10.

[124] Wenzel and Fujita defined their "high MPG" threshold as 23 mpg for cars, 16 mpgs for small pickups/SUVs, 13 mpg for large pickups, 20 mpg for CUVs, 18 mpg for minivans, and 14 mpg for full vans. Wenzel and Fujita (2018) at 34. All of these values are below those the agencies project to will be achieved by 2020 even under the rollback scenario. *See* 83 Fed. Reg. at 43,394 (projecting light trucks will achieve 31.6 mpg in MY 2020); *id.* at 43,398 (projecting passenger cars will achieve 43.9 mpg in MY 2020).

[125] Wenzel & Fujita (2018) at 45.

[126] Ficano and Thompson, American Economist, Vol. 59, No. 2 (Fall 2014), pp. 167-175, 149.

[127] *Id*. at 167.

[128] *Id*. at 173.

[129] 2018 PRIA at 994.

contradict the agencies proposed 20% value, and support a value of 10% or lower. The agencies cannot plausibly suggest that recent literature supports the 20% value, while excluding virtually all of the relevant literature contradicting their position.

vii. Correcting the deficiencies in the agencies' analysis demonstrates that the agencies' proposed 20% value is unsupportable, and validates the prior 10% value

For the reasons described above (and identified again in the table below), the agencies cannot rely on the following studies as relevant estimates of the rebound effect, and they should be removed from the agencies' analysis.

Table 2. Studies that should not be considered "new" and/or should be excluded

Study	Estimate of Long-Run Effect	Reason
Barla et al. (2009)	20%	International Data (Canada)
Bento (2009)	21-38%	Considered in 2012 Final Rule; Outdated 2001 NHTS Data
Wadud (2009)	1-25%	Not an estimate of the rebound effect; Old Data
West & Pickrell (2011)	9-34%	Considered in 2012 Final Rule; 2009 NHTS Data
Su (2012)	11-19%	Considered in 2012 Final Rule; 2009 NHTS Data
Ajanovic & Haas (2012)	44%	International Data (EU)
DeBorger (2016)	8-10%	International Data (Denmark)
Stapleton (2016, 2017)	14-30%	International Data (Great Britain)
Frondel & Vance (2013)	46-70%	International Data (Germany)
Weber & Farsi (2014)	19-81%	International Data (Switzerland)

Further, and again as described above, the agencies must ignore or afford less weight to the following studies.

Table 3. Recent studies that should be afforded less weight or excluded		
Study	Estimate of Long-Run Effect	Reason
Linn (2013)	20-40%	2009 NHTS Data
West et al. (2015)	0%	2009 NHTS Data
Liu (2014)	39-40%	2009 NHTS Data
Gillingham (2014)	22-23%	2009 NHTS Data
Leung (2015)	10%	2009 NHTS Data

After making the above adjustments, the remaining relevant studies that should inform the agencies' estimate of the relevant rebound effect (together with the actual rebound estimates returned by those studies) are below.

Table 4. Recent studies that should be given more weight	
Study	Estimate of Long-Run Effect
Greene (2012)[130]	10%
Hymel, Small, and Van Dender (2010)	13.1%
Hymel & Small (2015)	4.2%
Gillingham (2011)	1%
Ficano and Thompson (2014)	14.2%
Gillingham et. al (2015)	10%
Langer et. al. (2017)	11.7%
Knittel and Sandler (2015)	13%
Wenzel and Fujita (2018)	5.2%
Un-weighted Average	**9.16%**

[130] Hymel, Small, and Van Dender (2010) and Greene (2012) were considered in the agencies' analysis for the 2012 Rule, and thus arguably should be omitted from this table insofar as they cannot be relied upon to support a revision of the agencies' prior estimate, because these studies informed that prior estimate. Nevertheless, we have included them in this table because they are among the studies that demonstrate that the rebound effect is declining over time. Moreover, these studies are among the highest values in this updated data set, and removing them would in fact decrease the average rebound estimate of the studies in this table down to 8.47%, even further validating the agencies' 2012 assessment that the rebound effect would decline into the future.

As Table 4 demonstrates, the agencies thus cannot justify departing from their previous use of a rebound effect value of 10%. Correcting the deficiencies in the agencies' analysis confirms that the 10% value is both appropriate and is the highest defensible value. Moreover, as described below, this value is – if anything – too high, as various additional factors suggest that the value relevant to the CAFE and GHG standards context is likely lower than the values returned by the studies described above.

d. Even the 10% value previously selected by the agencies is likely too high, as various additional factors suggest that the value relevant to the CAFE and GHG standards context is lower than the values estimated by the studies described above

i. Contrary to the agencies' previous analyses, the agencies now omit evidence that fuel price or fuel cost rebound effects are, at most, the upper bound for the fuel economy rebound effect

The agencies have previously acknowledged that the fuel economy rebound effect is likely smaller than the fuel price rebound effect. For example, in the Draft TAR the agencies acknowledged that "[m]ost of the studies reviewed use changes in fuel prices or fuel cost/mile to derive estimates of the VMT rebound effect instead of using the actual variable of interest, changes in fuel economy, and its impact on VMT. It is not clear how reliable the use of changes in fuel prices/fuel costs are in attempting to estimate the impacts of changes in fuel economy on VMT."[131] And they clarified that "studies may overstate the potential impact of the rebound effect resulting from this rule, if people are more responsive to changes in fuel price than the variable directly of interest, fuel economy."[132] The agencies also acknowledged specific findings in this regard, observing (for example) that some studies returned findings that "fuel prices had a statistically significant impact on VMT, while fuel efficiency did not."[133]

However, the agencies have now edited these key findings out of their analysis. As they did in the Draft TAR,[134] the agencies discuss Linn (2013)'s contrary finding suggesting that the fuel economy rebound effect is greater than the fuel price rebound effect.[135] But in the NPRM, the agencies go on to acknowledge that Linn's findings are "at variance with the Hymel et. al. and Greene results described above."[136] However, the NPRM does not, in fact, otherwise mention, much less discuss, Hymel et. al.'s nor Greene's results regarding the relative magnitude

[131] Draft TAR at 10-20.
[132] *Id*. at 10-14.
[133] *Id*.
[134] *Id*. at 10-15 to 10-16.
[135] 2018 PRIA at 987; 83 Fed. Reg. at 43,102.
[136] 83 Fed. Reg. at 43,102.

of fuel economy rebound and fuel price rebound.[137] The PRIA is, at least, more accurate insofar as in that version of the discussion the agencies have deleted the reference to the Hymel and Greene findings from the discussion of Linn (2013).[138] But, like the NPRM, the PRIA similarly does not discuss the Hymel and Greene findings at all.

The deletion of the reference to Hymel and Greene from the PRIA's discussion of Linn (2013) is the *only* substantive change the agencies made in their discussion of Linn (2013) between the Draft TAR and the 2018 PRIA.[139] And the Draft TAR *did* discuss the Hymel and Green findings. It appears that the agencies deleted discussion of the Hymel and Greene findings – which weigh against their proposed outcome (namely, that the fuel economy effect is smaller than the fuel price effect) – and retained discussion of Linn's finding that the agencies purport supports their proposed outcome (that the fuel economy effect is larger than the fuel price effect). The agencies' deliberate omission of data and studies weighing against their proposal is arbitrary and unlawful.

Moreover, Linn's is the *only* study we are aware of in the entire body of literature finding that the fuel economy effect might be greater than the fuel price effect. To the contrary, at least the following studies of U.S. data have found the opposite: Greene (2012),[140] Gillingham (2012),[141] Small and Van Dender (2007),[142] Hymel and Small (2015),[143] West et. al. (2015),[144] and Wang and Chen (2014).[145] And DeBorger (2016)[146] and Stapleton, et. al. (2016, 2017)[147] have arrived at similar conclusions based on international data. Kenneth A. Small has stated as much, describing that his studies found that the rebound effect of "fuel economy is statistically indistinguishable from zero;" that "[t]his is also true of the vast majority of other studies that

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[137] *See* 83 Fed. Reg. at 43,099-105 (failing to mention Greene *at all* and failing to discuss Hymel's fuel economy-specific findings).
[138] 2018 PRIA at 987.
[139] *Compare id*. *with* Draft TAR at 10-15 to 10-16.
[140] *See* Draft TAR at 10-14 (describing that "Greene found that fuel prices had a statistically significant impact on VMT, while fuel efficiency did not").
[141] *See* 77 Fed. Reg. at 62995 (describing Gillingham's estimate that the fuel economy effect was 6% while the fuel price effect was 17%).
[142] *See* 2010 Joint TSD at 4-19 ("While Small and Van Dender did not find a statistically significant coefficient for fuel efficiency, they did find a statistically significant coefficient for the price of fuel."); *see also* Comment of Kenneth Small, NHTSA-2018-0067-7789.
[143] *See* Draft TAR at 10-17 (describing finding that factors contributing to rise in overall rebound effect do not impact fuel economy rebound effect); *see also* Comment of Kenneth Small, NHTSA-2018-0067-7789.
[144] *See id*. at 10-18 (describing that West et. al. "conclude there is no evidence of a rebound effect in response to improved fuel economy")
[145] *See* Wang and Chen (2014) at 102.
[146] De Borger (2016) at 10 (describing that "the coefficient of fuel efficiency is systematically smaller than the fuel price effect," and expressly noting that their findings contradict those of Linn (2013)).
[147] *See* 2018 PRIA at 991 (noting, and attempting to minimize, that "the authors conclude that there is little evidence of a fuel efficiency rebound effect").

have tried to measure separately these two responses;" that "the most defensible result empirically is that people do respond to fuel price as expected, but that they do not respond to fuel economy at all;" and that "Small and Van Dender (2007) make this point explicitly, and point out that we are therefore assuming a positive [fuel economy] rebound effect when actually we cannot prove that it's greater than zero."[148]

Against this backdrop of contrary research, even Linn (2016) itself acknowledges its own limitations. Although one of the primary focuses of the paper was to test whether fuel price effects and fuel economy effects are similar, he describes that his own sensitivity run "[a]ssuming that the effect of gasoline prices on VMT is equal in magnitude to the effect of fuel economy ha[d] ambiguous effects on the results."[149] And he notes that "in many cases the point estimates [of VMT rebound were] statistically indistinguishable when comparing the estimates that [were] obtained with and without imposing [the] assumption" that "gasoline prices and fuel economy have equal and opposite effects on VMT."[150] In other words, although Linn (2016) returned separate coefficients for each of fuel price and fuel economy changes, it also described that the difference between those two estimates as not statistically significant. Therefore, it appears that relying on Linn (2016) to support the notion that fuel economy rebound is greater than fuel price rebound is unsupportable.

And Linn himself has questioned the utility of the data underlying his findings, describing that Linn (2016) "rel[ies] on self-reported estimates of VMT" which "may be noisy when compared to VMT calculated from multiple odometer readings."[151] He thus observes that "[s]tudies that use VMT based on multiple odometer readings therefore should have lower measurement error, and yield preferable estimates from a statistical point of view." [152] As described in more detail above, *all* of the post-2012 studies using odometer readings yield significantly lower estimates of the rebound effect.

Simply, Linn (2016) cannot be used to justify a finding that the fuel economy rebound effect is greater than the fuel price rebound effect. To the contrary, as described above, Linn's finding rests on questionable data, and stands contrary to the numerous other studies finding that fuel economy rebound is smaller than fuel price rebound – and may be zero.

Additionally, that fuel economy rebound is smaller than fuel price rebound is consistent with the fact that the cost of driving changes differently in response to fuel price shifts than it changes in response to fuel economy shifts. As the agencies acknowledge, the rebound effect

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[148] Comment of Kenneth Small, NHTSA-2018-0067-7789, at 2.
[149] Linn (2016) at 277.
[150] *Id*.
[151] Comment of Joshua Linn, NHTSA-2018-0067-7188, at 2.
[152] *Id*.

"refers to the well-documented tendency of vehicles' use to increase when their fuel economy is improved and the cost of driving each mile."[153] And the cost of driving each mile includes "the increase in their per-mile fuel *and total driving costs*."[154] Total driving costs, the agencies again acknowledge, include depreciation and maintenance and repair outlays.[155] Both depreciation and maintenance and repair outlays may change along with fuel economy shifts, whereas they do not change in response to fuel price shifts. In other words, fuel price changes are free – when gas prices decline, consumers do not pay any up-front cost to earn the ability to pay less per gallon of fuel, and fuel prices do not affect per-mile maintenance costs. But the agencies argue that fuel economy changes *are* attended by up-front technology costs and additional maintenance costs.[156] Therefore, even if a given fuel price shift and a given fuel economy shift both decrease the *fuel* cost-per-mile equally, the fuel economy shift decreases the *total* cost-per-mile less than the fuel price shift. Therefore, the rebound effect of the fuel economy change should be lower. Again, this natural conclusion is consistent with the broad literature suggesting the fuel economy rebound effect is lower than the fuel price rebound effect.

However, while the agencies acknowledge the various components of costs-per-mile, which will shift along with more stringent fuel economy standards, the agencies refuse to consider those additional costs in their rebound calculations – and instead focus only on the price effect. The agencies purport to support this decision by asserting that "[e]ven if new vehicles' per-mile depreciation costs decline by enough to offset the increase in their fuel costs and thus cause a decline in the total cost of driving each mile, the reduction in their fuel economy that occurs in response to reducing future [fuel economy] standards would *by itself* cause an increase in their per-mile driving cost and a decline in their annual use."[157] In other words, the agencies appear to suggest (correctly) that the fuel economy change and the additional depreciation of technology costs are two independent factors with opposing effects on cost-per-mile. But the agencies' logic then falls apart. They suggest that empirical estimates of the rebound effect . . . cannot . . . be applied to the change in vehicles' [total] per-mile driving cost (including fuel, depreciation, and its other components) to estimate the resulting change in their use" because those empirical estimates measure only the response to the change in the cost of driving from fuel cost-per-mile shifts, and not from the other component costs-per-mile.[158] Thus, they conclude, "incorporating depreciation costs would not change the estimates of the reduction in

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[153] 83 Fed. Reg. at 43099.
[154] *Id*. (emphasis added)
[155] *See* 2018 PRIA at 974 (describing that "vehicles' per-mile operating costs include the cost of fuel they consume, the expected cost associated with potential crashes, maintenance and repair outlays, operating costs other than fuel (oil, tire wear, etc.), depreciation associated with vehicle use, and the value of their drivers' and other occupants' travel time.")
[156] *See id*. at 43229; *see also* Hymel and Small (2015) at 97 n. 22 (describing that one explanation for the difference between fuel price and fuel economy rebound is that "changes in fuel efficiency . . . also raise vehicle purchase costs.").
[157] 2018 PRIA at 975-76 (emphasis in original).
[158] *Id*. at 976.

vehicle use stemming from lower fuel economy levels permitted by less stringent CAFE standards or its associated economic costs."[159]

The agencies' reasoning reveals the flaws in their rebound analysis. First, although the agencies in the above-described discussion refer to "empirical estimates of the *fuel economy* rebound effect,"[160] in fact (as described above) the agencies have failed entirely to distinguish between estimates of fuel economy rebound and estimates of fuel price or fuel cost rebound in their discussion of empirical estimates. To the contrary, the agencies observe that most analyses "measure the rebound effect using the elasticity of vehicle use with respect to fuel cost per mile – equal to fuel price per gallon divided by fuel economy . . . under the *assumption* that drivers respond identically to changes in fuel cost per mile that resulting [sic] from either varying fuel prices or changes in fuel economy."[161] In other words, the agencies admit that the studies they consider *do not estimate fuel economy rebound*, but only *fuel cost per mile rebound*.[162] Said differently, the agencies observe that these studies don't consider the impacts of fuel economy on the total cost of driving, then observe that because the studies don't consider the impacts of changes in the total cost of driving, including the total cost of driving won't change the outcome, and therefore conclude that it's unnecessary to include the total cost of driving. The agencies' logic is thus circular, and amounts to an admission that they're using studies about apples to project growth rates for oranges.

More importantly, the agencies' discussion misses the point, which is not that depreciation costs should be included in their model, but that consumers' driving behavior should change in response to *total* cost, not just to *fuel* costs. And in the agencies' compliance cost analysis they project that increases in fuel economy will decrease total costs less than decreases in fuel price would. This fact supports the intuitive conclusion and economic theory that the fuel economy rebound effect should be less than the fuel price rebound effect. And the agencies must consider that fact (and the literature finding as much) when weighing estimates of the fuel cost rebound effect to estimate the value of the fuel economy rebound effect.

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159 *Id*.
160 *Id*.
161 *Id*. at 979 (emphasis added).
162 In fact, as discussed above, the agencies have omitted discussion of studies and findings regarding the fuel economy rebound effect that were discussed in prior agency analyses.

ii. As the agencies admit, studies have confirmed that the rebound effect has decreased and will continue to decrease as income increases and as fuel costs decrease (as will be caused by increases in fuel efficiency)

Real-world evidence shows that the rebound effect is dynamic and declines over time in response to (most relevantly) increases in income and decreases in driving costs. The agencies have admitted as much in their previous analyses, describing "evidence that the magnitude of the rebound effect is likely to be declining over time."[163] Even in the PRIA the agencies observe that recent studies confirm this downward trend, observing that both Hymel, Small and Van Dender (2010) and Hymel and Small (2015) confirmed that "the fuel economy rebound effect declined over time."[164] Nevertheless, the agencies attempt to obfuscate and minimize the importance of these findings. As described above, the agencies do so by suggesting that the findings might be relevant only to fuel price rebound, and by pointing to more recent point-estimates of the rebound effect. As discussed above, both of these undermine the agencies' position, and in particular the most recent research supports the notion that the rebound effect is declining.

The agencies further suggest new studies undermine the previous findings that the rebound effect declines with income. In particular, they suggest that "some studies" find that "the fuel economy rebound effect increases with the number of vehicles [households] own."[165] The agencies then assert that "[b]ecause vehicle ownership is strongly associated with household income, this common finding suggests that the rebound effect is unlikely to decline with rising incomes as the agencies had previously assumed." But the agencies do not describe which studies they are referring to, rendering it difficult to assess and comment on the specific findings underlying the assertion.

Regardless, the assertion does not withstand scrutiny. The agencies themselves describe that data from a "U.S. households owning multiple vehicles" has "enabled analysts to examine households' *substitution* among them."[166] This sentence contains a crucial observation – findings that individual vehicles in a household's fleet have high rebound effects do *not* measure the households' total driving, but rather measure driving for specific vehicles. Therefore, the estimates returned do not show that the households drive more in total, but (at least in part) that the household substitutes driving the old car with driving the newer, more efficient car.[167] This

[163] Draft TAR at 10-20.
[164] 2018 PRIA at 989.
[165] *Id*. at 993.
[166] *Id*. at 982 (emphasis added).
[167] *See* Linn (2016) at 270-71 (describing that "[t]he coefficient on the vehicle's own fuel economy implies that VMT increases when that vehicle's fuel economy increases, but the increase in the fuel economy of the household's other vehicles causes the vehicle's own VMT to decrease (i.e., the vehicles

type of rebound is not additional driving, but *replacement* driving, which provides a social benefit and serves the purposes of both EPCA and the CAA by reducing fuel consumption and carbon emissions. Thus, the agencies cannot plausibly rely on individual-vehicle data from multi-vehicle households to suggest that increasing income does not cause the rebound effect to decline.[168]

Additionally, the agencies attempt to minimize the fact that rebound is declining by suggesting that the evidence demonstrated that the rebound effect declines solely due to increasing incomes, and asserting that the income effects anticipated in 2012 have not and will not materialize.[169] Specifically, the agencies state that "[i]n contrast to the 2-3% annual growth assumed by the agencies when developing earlier forecasts of the future rebound effect, the income measure (real personal income per capita) used in these analyses has grown approximately 1% annually over the past two decades, and is projected to grow at approximately 1.5% for the next 30 years."[170] The agencies' argument suffers several flaws, each of them fatal.

First, the agencies' assertions regarding expected and realized income rates is contrary to fact. Although the agencies assert that the agencies previously assumed future income growth rates in their prior analysis, the agencies do not cite to any prior discussion relying on projections of future income growth, nor there is no record of any specific growth rates having been mentioned or otherwise playing any role whatsoever in the 2010 Final Rule, the 2012 Final Rule, the Draft TAR, or the 2016 TSD.[171] The most recent analyses – the Draft TAR and the 2016 TSD – simply observed that AEO reports projected that income would increase. And even the agencies acknowledge that income has, in fact, increased since that time.[172] Nothing in the agencies' prior analyses suggests that their estimate was dependent on specific growth rates

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are substitutes for one another)."); *id*. at n.8 (observing that "Knittel and Sandler (2013) also find some evidence of within-household substitution for vehicles in California" and that "[w]ithin-household substitution appears to be more substantial for the NHTS sample").

[168] Further, even if these studies could be read to suggest that high-income, multiple-vehicle households have higher rebound rates, the agencies have nevertheless failed to consider other studies which militate against that conclusion. In the 2016 TSD, EPA discussed other studies, and concluded that "the evidence of how the rebound effect varies between households across different income classes is mixed and inconclusive." 2016 TSD at 3-20. The agencies do not now offer any reason to support departing from that previous position by focusing in on one category of studies that (they assert) supports their preferred outcome, to the exclusion of others.

[169] *Id*. at 993.

[170] *Id*.

[171] 2010 Joint TSD at 4-22 (not discussing income projections at all); 2012 Joint TSD at 4-25 to 4-26 (not discussing income projections at all); Draft TAR at 10-20 (describing only that "[s]ince the AEO 2015 projects that household incomes will be rising throughout the analysis period, the agencies believe that it is appropriate to factor in studies that account for income on the rebound effect."); 2016 TSD at 3-20 (same, but referencing AEO 2016).

[172] 2018 PRIA at 989.

being realized, and the agencies do not now provide even any post-hoc rationale for why specific growth rates would have been required.

Moreover, even if the agencies had previously relied on specific estimates of income growth, the analysis in the PRIA and NPRM nevertheless appear to be without basis in fact. The agencies assert (without citation or support) that income has grown only "1% *over the last two decades*."[173] The proposition that the average growth rate since 1998 – which period included both the tech bubble and the great recession – is somehow relevant to the validity of the agencies' forward-looking rebound projections in 2010, 2012, and 2016 is unsupportable. Nearly 75% of the "last two decades" of data occurred before the EPA even finalized the 2022-2025 GHG standards. If anything, the only relevant data to re-visiting those standards would be income growth rates since 2012 at the earliest. And average income has grown by 14% since 2012, or at an average of 2.8% annually.[174]

Similarly, the agencies do not cite any support for their assertion future income is projected to grow at only 1.5% annually.[175] This projection appears to be similarly without support. In fact, AEO 2018 projects income to rise at an average of 2.2% through 2050.[176] And the agencies failed to identify any authority to the contrary. The agencies' 1.5% figure appears to be without basis.

Second, even if the agencies' assertions regarding projected and realized income were correct, the agencies' suggestion that the declining rebound rate is solely attributable to income gains is erroneous. The agencies previously acknowledged the declining rebound rate as follows: "the responsiveness to the fuel cost of driving will be larger when it is a larger proportion of the total cost of driving."[177] In other words, the rebound effect declines when "fuel costs as a share of total monetary travel costs" decrease.[178]

One way in which fuel costs become a smaller proportion of the total cost of driving is for incomes to rise, as the agencies acknowledge. This is because rising incomes cause the time-cost of driving to become a larger component of the total cost.[179] But another way fuel costs become a smaller proportion of total costs is for those fuel costs to decline. Thus, evidence has

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[173] *Id*. (emphasis added).
[174] *See* United States Census, Historical Income Tables, Table P-1, available at https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-income-people.html (accessed October 15, 2018).
[175] *See* 2018 PRIA at 993.
[176] *See* AEO 2018, Table 20 (Macroeconomic Indicators), available at https://www.eia.gov/outlooks/aeo/data/browser/#/?id=18-AEO2018&cases=ref2018&sourcekey=0 (accessed October 15, 2018).
[177] *See* Draft TAR at 10-13.
[178] *Id*.
[179] *See id*.

shown that "the rebound effect declines with increasing income and urbanization, and it increases with increasing fuel cost."[180] And because "fuel costs . . . reflect both fuel prices and fuel efficiency,"[181] "an increase in fuel efficiency has the same impact . . . as that of a fuel price cut."[182] As Hymel and Small describe, "[t]his makes sense from a theoretical standpoint because most of the changes in fuel efficiency we are interested in are improvements, *i.e.*, they lower the fuel cost per mile just like price cuts."[183]

By 2020 fuel economy will have increased dramatically relative to the era from which the historical studies have pulled their data. For example, fuel economy will have increased by more than 50% since 2005, when light-duty vehicles' average fuel economy was 24.8 mpg (unadjusted).[184] In 2020, the agencies project that achieved average fuel economy will be at least 37.2 mpg.[185] This amounts to a 33% decrease in per-mile fuel costs.[186] Because evidence demonstrates that consumer demand will become less elastic as prices decline, applying a point-estimate of rebound derived from circa 2004-2005 (such as Hymel et al. (2010)'s estimate that the rebound rate in 2004 was 13%) to a point on the curve that is 33% lower on that curve is unreasonable. The agencies must account for the declining cost of driving driven by both the recent dramatic increases in fuel economy and the increases that would be driven by the augural/existing standards.

Finally, notwithstanding evidence that the rebound effect is dynamic and declining, in the PRIA, the agencies describe that their analysis "assum[es] the demand curve for vehicle use is linear over the relevant range."[187] This, too, is a departure from previous rules. In those rules, as discussed at length above, the agencies acknowledged that the rebound effect (and thus the demand curve) is *changing* over time, and thus *not* linear. They then acknowledged that, because their modeling "requires a single point estimate for the rebound effect as an input to its analysis,"[188] "a value on the low end of the historical estimates . . . is likely to provide a more reliable estimate of its magnitude during the future period spanned by the agencies' analyses of the impacts of this rulemaking."[189] In other words, in the past the agencies expressly adopted a

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[180] Hymel and Small (2015) at 96.
[181] Draft TAR at 10-13 n. G.
[182] Hymel and Small (2015) at 98.
[183] *Id*. at 98-99; *See also* Knittel and Sandler (2015) at 12 ("Obviously vehicle owners with more fuel efficient vehicles will respond less to changes in the per-gallon gasoline price").
[184] EPA, Light-Duty Automotive Technology, Carbon Dioxide Emissions, and Fuel Economy Trends: 1975 Through 2017 (January 2018) [EPA-420-R-18-001] at 122, available at: https://nepis.epa.gov/Exe/ZyPDF.cgi?Dockey=P100TGDW.pdf
[185] 83 Fed. Reg. at 43,390.
[186] Assuming constant fuel prices.
[187] 2018 PRIA at 975; *see also* Equation 67 p.92 of Draft CAFE Model Documentation, July 2018 (linear equation used to calculate impacts of rebound effect).
[188] 2012 TSD at 4-25.
[189] *Id*.

linear approximation of an admittedly non-linear relationship. Now, the agencies simply assume without support (and contrary to their own acknowledgements) that the real-world relationship is linear.

The agencies must acknowledge that the rebound effect is dynamic, and must (at a minimum) follow the same procedure for reflecting that dynamic nature as they have in the past – namely, to adopt a value on the low-end of historical estimates.[190] Specifically, in 2012, the agencies described that the low end of the values produced by historical estimates was 10%.[191] Today, as shown in Table 4 above, the low end of the most relevant, historically-measured values is closer to 5%. This new data supports the agencies' prior projections of a declining rebound effect, and therefore suggests that – consistent with the agencies' prior analyses – the agencies should, at a minimum, again adopt a value at the low end of the historical estimates.

However, it should be noted that even the method the agencies have historically used (adopting a value on the low-end of historical estimates) carries limitations as well. Specifically, applying a first-order linear approximation of rebound to decades-long time periods when the evidence shows a dynamic, declining rebound that will be much smaller later in the analysis will yield erroneous results – and will specifically overestimate the impact of the rebound effects in the later years of the analysis. The better approach would be to approximate the dynamic relationship, and apply an even lower rebound rate in those later years.

Regardless, the agencies cannot support the proposal to increase their estimate of the rebound effect to 20%. If anything, the data supports decreasing their estimate of the rebound effect to below 10%.

iii. The agencies fail to acknowledge the asymmetrical characteristics of the rebound effect, which suggest that the impacts of more stringent standards should be at the low end of the available estimates

The agencies have previously acknowledged evidence showing that the rebound effect is asymmetrical – that is, that VMT changes more in response to price increases than it does to

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[190] This historical approach also comes with limitations. The agencies apply a first-order linear approximation to decades long-periods and large changes in fuel economy values. First order approximations have inherent limitations, evidenced by the fact that, due to compounding, applying the approximation to each incremental (year-to-year) change in CPM from 2020 to 2026 would result in a larger estimate of total rebound VMT as compared to applying it to the entire period all at once. Similarly, estimates of VMT for any given calendar year will vary depending on the baseline cost-per-mile used to calculate that calendar year's VMT. Calculating MY 2025 rebound off of a MY 2012 cost-per-mile baseline will yield a different result than calculating it off of a MY2016 cost-per-mile baseline.
[191] *Id*.

price decreases, but they have now softened and removed such acknowledgement. For example, in the Draft TAR the agencies acknowledged that Hymel and Small (2015)'s results "show strong evidence of asymmetry in responsiveness to price increases and decreases."[192] And they observed literature showing that fuel-demand is asymmetric – and that the response to an "increase in oil prices can be on the order of five times larger than the response to a price decrease."[193] Therefore, the agencies observed, "[s]ince these standards would decrease the cost of driving gradually over time, it is possible that the rebound effect would be much smaller than some of the historical estimates included in the literature."[194]

In the PRIA, however, rather than observing the "strong evidence of asymmetry" in Hymel and Small (2015), they now observe only that "households curtail their vehicle use within the first year following an increase in fuel prices and driving costs, while the increase in driving that occurs in response to declining fuel prices . . . occurs more slowly."[195] And the agencies have deleted entirely the discussion of evidence showing asymmetrical fuel demand responses.[196] Instead, they highlight only that one international study using European data found no evidence of asymmetry.[197]

The agencies' revisions are misleading and unreasonable. First, the agencies do not offer any support for their apparent change in position regarding the potential that the response to increasing fuel efficiency may be "much smaller" than historical evidence might suggest due to asymmetry. The agencies cannot simply delete their discussion of asymmetries, replace it with one inapplicable European study, and hope nobody notices.

And second, even the agencies' portrayal of the asymmetry findings within their discussion of Hymel and Small (2015) are misleading. Although, as the agencies observe, that study did find that asymmetries manifest in the speed with which VMT shifts occur in response to price increases versus price decreases, studies have also found that the *magnitude* of the shift differs. In another report Kenneth Small prepared for EPA, he showed that modeling of asymmetric effects leads to projections of a long-run rebound effect of 1.0% by 2025 – far lower than the projected effect without considering asymmetric responses.[198] The PRIA unreasonably fails to even discuss the magnitudes of these asymmetric effects,[199] consideration of which would further demonstrate that historical estimates of the rebound effect are, at most, the upper bound for the value the agencies could plausibly use in their analysis. And because, as shown in Table

[192] Draft TAR at 10-17.
[193] *Id*. at 10-15.
[194] *Id*.
[195] 2018 PRIA at 990.
[196] *See Id*. at 979-92.
[197] *Id*. at 986 (discussing Ajanovic and Haas (2012)).
[198] Comment of Kenneth Small, NHTSA-2018-0067-7789, at 1.
[199] *See* 2018 PRIA at 990.

4, all of the most relevant historical estimates estimate the rebound effect at lower than 20%, adopting a value of 20% is unsupportable.

iv. The agencies fail to acknowledge or analyze the effects of increased congestion on rebound

As described above, the magnitude of the rebound effect declines when fuel costs decline as a share of total travel costs. And, because total travel costs include time-cost of driving, and the time-cost of driving increases as the total time required to drive increases, factors that impact the total time required thus theoretically impact the rebound effect. One of these factors is congestion. Indeed, at least one study has directly addressed the impact of congestion on the magnitude of the rebound effect, and has observed that fuel efficiency improvements may "tend to worsen congestion, which will itself tend to deter travel."[200] In other words, "[t]his increased congestion modifies the overall rebound effect by curtailing some of the incentive for travel."[201]

Although Hymel, Small and Van Dender (2010) found that the "congestion effect" was relatively small, it is nevertheless a relevant effect. Moreover, the agencies' failure to consider it at all stands in contrast to the fact that elsewhere in the cost-benefit analysis the agencies expressly monetize the social cost imposed by projected increases in congestion attributable to additional driving under the augural scenario. Indeed, these FHWA values are expressly intended to monetize the time-value of additional driving, and specifically to "measure the increased costs resulting from added congestion and the delays it causes." The agencies should analyze whether the congestion impacts they have projected to result from the existing standards are similar to the congestion impacts studied by Hymel, Small and Van Dender (2010) (and any other studies that have considered the congestion effect), and the degree to which that projected impact will "curtail some of the incentive for travel" and reduce the rebound VMT projected to result under the agencies' proposed alternatives. At a minimum, the "congestion effect" demonstrates that historical estimates of the rebound effect are at most the upper bound for the appropriate rebound effect value to adopt in the CAFE and GHG context.

II. The agencies' rebound analysis is inconsistent with other portions of the proposed rule

As demonstrated above, the agencies proposal to adopt a 20% value for the rebound effect is without real-world support. Additionally, the agencies' application of a fixed, linear rebound effect across all cars in the fleet and across all years of the analysis is in inherent tension with other aspects of the proposal.

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[200] Hymel, Small and Van Dender (2010) at 1220.
[201] *Id*. at 1221.

a. The Dynamic Fleet Share Model and "New Vehicle" VMT assumptions conflict with the rebound analysis

In particular, with the Dynamic Fleet Share Model ("DFS") the agencies have hard-wired into their model an assumption that consumers will shift from cars to trucks, such that the percentage of the fleet comprised of trucks grows at a pre-defined rate as fuel economy standards become more stringent.[202] Additionally, the agencies' model contains a hard-wired assumption that the average truck will accrue more annual VMT than the average car.[203] However, the average truck is also projected to be less fuel efficient than the average car, and thus is projected to have a higher cost-per-mile (CPM).[204] In other words, the agencies assume that as fuel economy standards become more stringent consumers will choose to purchase more expensive vehicles with higher driving costs – and will drive those vehicles more than they would have driven the vehicles with lower driving costs.

The assumption that consumers will drive more notwithstanding higher driving costs associated with trucks appears to imply a negative rebound rate for those consumers. This assumption is in inherent tension with the agencies' adoption and implementation of a positive rebound effect value, which inherently assumes that drivers will always drive less for a given increase in driving costs. The agencies have not acknowledged this apparent contradiction, much less have they supported it with any analysis or real-world evidence.

Further, the fact that the agencies project a higher share of trucks and a higher VMT for those trucks not only conflicts with the analysis of the rebound effect, it magnifies the impact of the rebound effect under more-stringent alternatives. Even the agencies admit as much.[205] Also, the agencies do not explain the reason for the magnification. It appears to result because the hard-wired shift to a greater share of trucks projected by the DFS model in the augural scenario also causes a spontaneous increase in baseline VMT (because trucks are assumed to have higher VMT than cars), which when compounded by the rebound effect (because new trucks are more efficient than old trucks), results in an even greater VMT increment above VMT in the rollback scenario. From a theoretical perspective, this amounts to a suggestion that consumers will ignore the increase in CPM associated with replacing cars with new trucks, but after that point will begin to consider CPM – not as compared to the old cars, but as compared to older, less efficient trucks – in determining their new-truck driving habits. Assuming consumers will only respond in accordance with the rebound effect after they have accrued a significant jump in CPM is

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[202] *See* comment submitted to these dockets: Rick Rykowski, Supporting Report for Environmental Defense Fund Comment, *Review of the Agencies' Technical Analysis Supporting the SAFE Vehicle NPRM* (October 2018).
[203] 83 Fed. Reg. at 43,187.
[204] *Id*. ("light trucks are less efficient than passenger cars on average").
[205] *Id*. (describing that the shift to trucks can "can magnify the influence of the rebound effect on vehicles that go through the compliance simulation (MY 2016–2032)").

illogical and unreasonable, and works to unrealistically exacerbate the projected impact of the rebound effect in the agencies' analysis.

Similarly, the agencies assume that new cars will be driven more than old cars, irrespective of relative fuel efficiency or cost-per-mile.[206] Thus, the agencies suggest that CPM will not influence a consumer's decisions regarding which car to drive, nor will it influence the consumer's decision of how much to drive that car initially. But, the agencies then suggest that after incurring the initial (CPM-agnostic) sudden increase in VMT, consumers will *then* start to consider changes in CPM and will adjust their driving habits according to the rebound effect. In other words, when a consumer buys a new more-efficient car, that consumer will drive that car more *because it is new* and irrespective of CPM, and then will *further* increase his or her driving off of the new, bigger baseline VMT due to the decline in CPM. As with the DFS, the agencies again admit that this will "magnify" the rebound effect.[207] And again, the agencies' suggestion that consumers will be CPM-agnostic in determining how much to initially drive a new car, but will then be CPM-aware thereafter, conflicts with their discussion of the rebound effect, which assumes that that drivers will always respond in the same way to a change in driving costs. It also conflicts with the literature that studied the difference in VMT between new fuel-efficient cars, and older, less-fuel efficient cars.[208] The rebound effect returned by that literature inherently includes any increase in VMT that would be attributable to the fact that a car is new. Therefore, applying both a "new car" effect and a rebound effect to the same car is likely double-counting at least some portion of the increased VMT. Once again, the agencies analysis is unreasonable.

b. The rebound effect for existing vehicles is not "identical for all regulatory alternatives," contrary to the agencies' assertions

The agencies' statements regarding the impact of the rebound effect on the existing U.S. fleet are erroneous. The agencies suggest that "[s]ince the fuel economy of [existing] vehicles is already fixed, only the fuel price influences their travel demand relative to the mileage accumulation schedule and so is identical for all regulatory alternatives."[209]

The agencies' suggestion is wrong. Although they are correct that the average fuel economy is already fixed, the agencies fail to mention that their model changes the total size of the U.S. fleet over time – and as it removes existing cars from the fleet, it also removes their associated VMT from the road. Thus, under the rollback scenario, total "baseline" existing-fleet VMT is projected to be lower than in the augural scenario, and the rebound effect will therefore

[206] *Id*.
[207] *Id*.
[208] See, e.g., West et al. (2015)
[209] *Id*.

cause a different impact on existing-car VMT in the rollback scenario than it will in the augural scenario. In other words, existing-fleet rebound impacts will differ because it will be calculated off of a bigger base in the augural scenario.[210]

The agencies must acknowledge and explain the impact this variation in existing-fleet rebound has on the agencies' cost-benefit analysis.

III. The agencies deceivingly and inconsistently discuss the safety impacts of the rebound effect in the NPRM, PRIA, and publicly, and the agencies cannot justify the rollback based on rebound safety impacts

The agencies correctly acknowledge that fatalities and other crash costs from rebound are not attributable to the standards. Specifically, the agencies describe that "[i]ncreased driving associated with rebound is a consumer choice. Improved CAFE will reduce driving costs, but nothing in the higher CAFE standards compels consumers to drive additional miles."[211] And "[i]f consumers choose to do so, they are making a decision that the utility of more driving exceeds the marginal operating costs as well as the added crash risk it entails."[212] Therefore, the agencies acknowledge, crash costs attributable to rebound are not "actually imposed on consumers by CAFE standards."[213]

Nevertheless, in their analysis the agencies quantify projected fatalities and non-fatal crash costs attributable to the rebound effect.[214] But, because those fatalities are not "directly attributable to the CAFE standards," the agencies "valu[e] extra rebound miles at the full value of their added driving cost plus the added safety risk consumers experience, which completely offsets the societal impact of any added fatalities from this voluntary consumer choice."[215] In other words, the agencies simply insert a counter-balance in their calculations by assuming that a

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[210] Moreover, as described above, the evidence shows the rebound effect declines over time along with the cost of driving. And in applying their rebound calculations, the agencies use an average cost-per-mile value for the U.S. fleet. This cost-per-mile average will be lower under the augural scenario, due to the increased fuel economy of new vehicles. Thus, faithful application of the rebound literature requires application of a lower rebound rate to the fleet-average CPM under the augural standards than under the rollback standards – which would thereby cause less existing-fleet rebound under the augural scenario than under the rollback.

[211] 83 Fed. Reg. at 43,107.

[212] *Id*.

[213] *Id*.; *see also id.* at 43,148 ("although a safety impact from the rebound effect is calculated, these impacts are considered to be freely chosen rather than imposed"); *id.* at 43158 (the agency does not add rebound effects to the other CAFE-related impacts because rebound-related fatalities and injuries result from risk that is freely chosen and offset by societal valuations that at a minimum exceed the aggregate value of safety consequences plus added vehicle operating and maintenance costs.").
by CAFE

[214] *Id*. at 43,107.

[215] *Id*.

benefit exactly offsets the cost of additional rebound driving, thereby zeroing out the safety impact of additional rebound miles on the agencies' analysis.[216] And they do the same for non-fatal crash costs.[217] Moreover, the agencies admit that even the assumed equal-and-opposite benefit does not reflect reality, in which "rebound-related fatalities and injuries [are] . . . offset by societal valuations that *at a minimum exceed* the aggregate value of safety consequences plus added vehicle operating and maintenance costs."[218]

The agencies' analysis prompts the question why the agencies have quantified or monetized fatalities attributable to rebound *at all.* Indeed, doing so is contrary to historical practice. For example, the agencies did not quantify them in the 2012 final rule. Instead, NHTSA's cost-benefit tables for the 2012 Final Rule quantified only the reduced fatalities attributable to mass reduction, and omitted any quantification or monetization of fatalities attributable to rebound.[219] And the agencies noted that the appropriate safety consideration in their analysis was fatality *rates* per-VMT, not *total* fatalities.[220]

Contrary to the approach taken in 2012, quantifying rebound fatalities in the analysis – even if the monetized costs are ultimately zeroed out – is misleading and unreasonable. That is, the agencies' analysis should consider those impacts that are fairly attributable to the rule. And the agencies themselves admit that rebound fatalities are *not* fairly attributable to the rule. Thus, the agencies should not quantify those impacts.

That rebound fatalities should not be monetized or attributed to the rule is demonstrated by the fact that, notwithstanding the agencies' admonitions that rebound fatalities shouldn't impact their analysis, the agencies rely on rebound safety impacts to justify their proposed rollback throughout the NPRM and in public. For example, in the EPA's analysis, the agency describes that "increased fatalities and accidents due to the rebound effect" are "an important consideration in determining the appropriate standards under section 202."[221] And in that discussion, the agency provides a single, non-segmented number of total fatalities attributable to the rule to suggest that that the NPRM's "analysis projects adverse impacts on safety that are significantly different from the analysis included and considered in the 2012 rule which established the MY 2021–25 GHG standards and the 2016 Draft Technical Assessment Report." But that statement is misleading, because the fatalities number contains significant fatalities attributable to the rebound effect, which, as described above, the agencies have acknowledged

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[216] *See id*. at 43,255 (describing the "Benefits Offsetting Rebound Fatality Costs" line item in the cost-benefit analysis).
[217] *Id*. (describing the "Benefits Offsetting Rebound Non-Fatal Crash Costs" line item).
[218] *Id*. at 43,158.
[219] *See, e.g.,* 77 Fed. Reg. at 63,080.
[220] *See* 77 Fed. Reg. at 62,740 n.313 (describing that "[i]n this rulemaking document, ''vehicle safety'' is defined as societal fatality rates per vehicle miles traveled (VMT)").
[221] 83 Fed. Reg. at 43,231.

are *not* attributable to the rule.[222] Moreover the agencies insinuate that the number of fatalities projected has grown relative to their 2012 analysis. But in the 2012 analysis, correctly recognizing that rebound fatalities are not attributable to the rule, the agencies did not quantify rebound fatalities. Thus, including quantified rebound fatalities to suggest the safety impacts of the rule are now projected to be worse than first thought is wholly unreasonable.

The agencies rely on the rebound fatality figures elsewhere in the NPRM as well. In NHTSA's "maximum feasible standards" analysis under EPCA, the agency describes that it "consider[ed] the safety effect associated with the additional vehicle miles traveled due to the rebound effect."[223] Specifically, the agency described that the fact that rebound has an "adverse safety consequence" because rebound "results in consumers driving more miles, which results in more crashes and increased highway fatalities" impacts the conclusion regarding the appropriate level of the standards.[224]

Indeed, NHTSA's analysis even more explicitly centers on rebound safety impacts a few pages later. The agency states that "most of the estimated overall improvement in highway safety from this proposal is attributable to reduced travel demand (attributable to the rebound effect) and accelerated turnover to safer vehicles."[225] And based on these two safety impacts, NHTSA asserts that "[t]he trend in these results is clear, with the less stringent alternatives producing the greatest estimated improvement in highway safety and the proposed standards producing the most favorable outcomes from a highway safety perspective."[226] The agency thus concludes, "[t]hese considerations bolster our determination that the proposed standards are maximum feasible based upon current and projected technology for the model years in question."[227] Indeed, in this section NHTSA abandons all pretenses that it has actually adhered to its acknowledgement that rebound safety effects are not attributable to the rule. Moreover, the agency does not here acknowledge that any safety benefits from reducing rebound are attended by corresponding - and *even greater* - real-world consumer dis-benefits. Instead, the agency touts the avoided rebound accidents as a central driver of its decision to roll back its standards.

Justifying the rollback based on rebound safety impacts is unreasonable and arbitrary. As the agencies acknowledge, any such safety impacts result from consumers' freely-made decisions to drive more. And assigning "benefits" to constraining those consumers' ability to drive by forcing up the cost of driving is not only contrary to the agencies' own acknowledgement that those benefits are outweighed by attendant costs, but also contrary to logic and outside the scope of the agencies' mandates. NHTSA's mission is to make driving

[222] *Id*.
[223] *Id*. at 43,212.
[224] *Id*.
[225] *Id*. at 43,266.
[226] *Id*.
[227] *Id*.

safer and to promote fuel conservation for given levels of driving, not to make people stop driving. And EPA's mission is to make driving cleaner, not to make people stop driving. Yet the touted rebound-related safety benefits stem only from constraining consumers' mobility. If NHTSA and EPA want to force people to drive less, they could close highways, erect barricades, promote potholes, and prohibit visits to Grandma's house for Thanksgiving. All of those would have the same type of safety "benefits" the agencies now tout as a key justification for their rollback. And all of them demonstrate the absurdity of the agencies' position.

Moreover, the agencies fail to discuss any technologies that might reduce safety impacts from additional driving in the future. For example, in the 2012 Rule, the agencies observed that "[m]anufacturers are . . . increasingly investigating a variety of crash avoidance technologies — ABS, electronic stability control (ESC), lane departure warnings, vehicle-to-vehicle (V2V) communications — that, as they become more prevalent in the fleet, are expected to reduce the number of overall crashes, and thus crash fatalities."[228] The agencies do not make any such observation about the potential for future technologies to mitigate crashes or crash impacts in the NPRM. And the agencies' failure to acknowledge or discuss even technologies that NHTSA itself has proposed to require on new vehicles, such as V2V communications, is telling.[229] If NHTSA is genuinely concerned with consumers' safety, it should finalize policies that will make every mile of driving safer, rather than proposing to make every mile of driving so expensive that people cannot drive.

Simply, safety impacts attributable to rebound are not properly considered as part of the agencies' analysis, the agencies cannot rely on them as a rationale for rolling back the standards, and the agencies should remove the quantified and monetized fatalities and non-fatal crash costs from their analysis entirely.

[228] 77 Fed. Reg. at 62,759.
[229] *See Federal Motor Vehicle Safety Standards; V2V Communications,* 82 Fed. Reg. 3854 (Jan. 12, 2017).

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