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**The Rebound Effect of Fuel Economy Standards:
Comment on the Safer Affordable Fuel-Efficient (SAFE) Vehicles
Proposed Rule for Model Years 2021-2026 Passenger Cars and Light
Trucks**

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Short biographical statement:

Kenneth Gillingham is an Associate Professor of Economics at Yale University, with appointments in the School of Forestry & Environmental Studies, Department of Economics, and School of Management. He is also a faculty research fellow at the National Bureau of Economic Research. In 2015-2016 he served as the Senior Economist for Energy & the Environment at the White House Council of Economic Advisers and in 2005 he served as a Fellow for Energy & the Environment at the White House Council of Economic Advisers. He is an energy and environmental economist, with research in transportation, energy efficiency, and the adoption of new technologies.

He has published over 40 articles, including in top journals in economics, science, and business. Many of these publications focus on the economics of fuel economy standards and related issues, including the rebound effect. He has presented this work at top universities both in the United States and internationally. In 2007, he was a Fulbright Fellow in New Zealand and he has held visiting positions at the University of Chicago, Stanford University, Indiana University, and University of California-Berkeley. He holds a PhD from Stanford University in Management Science & Engineering and Economics, an MS in Statistics and an MS in Management Science & Engineering from Stanford, and an AB in Economics and Environmental Studies from Dartmouth College.

This comment is based on his expertise in econometrically modeling the rebound effect and reviewing the literature on the rebound effect. This includes papers on the rebound effect that were cited by the Agencies as well as two review articles on the rebound effect. This comment was also informed by conversations with colleagues who also work on fuel economy standards, including Arthur van Benthem of the University of Pennsylvania, Mark Jacobsen of the University of California-San Diego, Josh Linn of the University of Maryland, David Rapson of the University of California-Davis, and Antonio Bento at the University of Southern California.

Executive Summary

This comment focuses on the choice of a 20% rebound effect in the proposed rulemaking “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). This commenter strongly believes that the justification for the 20% rebound effect provided in the Notice of Proposed Rulemaking (NPRM) does not follow the best evidence available. In summary, the current justification is based on old evidence, evidence from Europe, a selective review of the literature that is missing several key papers, and an interpretation of several papers that is at odds with the authors’ own interpretation (e.g., see comments in the docket from K. Small, J. Linn, A. Bento, and C. Cirillo).¹

A more appropriate review of the literature focuses on recent work, excludes evidence from Europe (where fuel prices are higher and there is a more viable substitute to driving in public transportation), includes a comprehensive look at the latest literature, and follows the authors’ own interpretation of their estimates. In the following table, I provide a review of the recent literature on the rebound effect in the United States in the past decade. It is recognized in the academic community that the more reliable work is based on multiple odometer readings, rather than a single survey, and thus the table identifies studies that use odometer readings. The Agencies also argue that this is the most reliable data to use when they are discussing the relationship between annual vehicle-miles-traveled (VMT) and vehicle age. Odometer readings are preferred because a single survey captures a smaller snapshot of time and because survey data are self-reported, rather than measured, so may not be as representative.

Summarizing Table of the Best Evidence Available for a Central Estimate of the Rebound Effect (Studies in Boldface are <i>not</i> included in the NPRM)		
<i>Study</i>	<i>Data</i>	<i>Rebound Estimate</i>
Bento et al. (2009)	2001 survey	34%
Hymel et al. (2010)	State-level 1966-2004	9%
Gillingham (2011)	Odometer; CA 2001-2009	1%
Greene (2012)	Aggregate 1966-2007	0%
Su (2012)	2009 survey	11-19%
Liu et al. (2014)	2009 survey; MD/DC/VA	40%*
Gillingham et al. (2015)	Odometer; PA 2000-2010	10%
Hymel & Small (2015)	State-level 1966-2004	4-18%
Leung (2015)	2009 survey	10%
Linn (2016)	2009 survey	20-40%*

¹ These comments have the following docket numbers: K. Small (EPA-HQ-OAR-2018-0283-2698), J. Linn (EPA-HQ-OAR-2018-0283-1642), C. Cirillo (NHTSA-2018-0067-7819), A. Bento (NHTSA-2018-0067-5679).

Langer et al. (2017)	Odometer; OH 2009-2013	11%
West et al. (2017)	Odometer; TX 2010-2011	0%
Knittel & Sandler (2018)	Odometer; CA 1998-2010	14.7%
Wenzel & Fujita (2018)	Odometer; TX 2005-2010	7.5-15.9%
Average over all studies above		14.1%
Average over all studies using odometer readings		8.1%
<p><i>Notes: * refers to studies that the authors themselves suggest we interpret with caution. The studies in this table estimate the elasticity of vehicle-miles-traveled with respect to fuel economy, fuel prices, or the cost per mile of driving. For studies with a range, the average is taken over the range. The NPRM references a 1-25% range from Wadud et al. (2009), but this study is excluded because it estimates the elasticity of gasoline consumption with respect to fuel prices and thus is not directly comparable to the above studies. The NPRM also referenced a 9-34% range from West and Pickrell (2011), but this does not appear to be a working paper or publication that is publicly accessible. The NPRM references Gillingham (2014), but this study is focused on a gasoline price shock and thus in the author's own view is inappropriate to use for the rebound effect. A better reference is Gillingham (2011) that attempts to descriptively look at the effect of fuel economy, although without quasi-experimental variation. All studies from Europe referenced in the NPRM are excluded from this table. The NPRM incorrectly references Linn (2016) as Linn (2013). Bento et al. (2009) give the average VMT elasticity with respect to the price of gasoline as -0.34 on p.685 (implying a 34% rebound); the NPRM reports a range of 21-38%, but this range does not appear in the paper, and it is unclear where this range comes from. The 9% estimate from Hymel et al. (2009) was taken from the authors' preferred estimate in the conclusion (p.1235) with the calculation of variables at 2004 values, but a variety of other estimates were reported. The 4-18% estimates from Hymel and Small (2015) is from the authors' preferred estimates in Table 8; the NPRM chooses only the high estimate. The 7.5%-15.9% range for Wenzel & Fujita (2018) is based a conversation between the commenter and the authors; the authors suggest considering both the estimate based on fuel prices and the estimate based on the cost per mile to be consistent with the rest of the literature, which use both.</i></p>		

This review of the literature clearly reveals that the central case estimate is in the range of 8.1%-14.1% and 8.1% would be preferred when the focus is on the most reliable evidence, which is based on multiple odometer readings. This is the best evidence available and it does not support 20% as a central case estimate for the rebound effect of fuel economy standards. A notable aspect of the table is that many recent papers were omitted from the review in the NPRM and in general, these omitted papers tend to have lower estimates of the rebound effect (note the papers in boldface in the table).

Economists who have carefully considered the rebound effect may note that the change in fuel use or emissions from consumer rebound in response to a fuel economy standard is more complicated than what the studies above capture. The studies above focus on the direct response in driving to a change in the cost of driving, which is a very useful starting

point for understanding the rebound effect. However, there are several further factors that may lead the rebound effect in response to a change in fuel economy to be higher or lower than the simple average taken in the table above. These factors are listed as follows (in no particular order):

- 1) **Consumers may respond differently to changes in fuel economy than to changes in fuel prices.** Evidence suggests that this is likely the case. There are several papers in the literature suggesting that the response to fuel economy may be less than the response to fuel prices, implying that the evidence above *overestimates* the rebound effect (West et al. 2017, De Borger et al. 2016, Greene 2012, Gillingham 2011). The logic is that gasoline prices are more visible and thus more salient to consumers. There is one paper providing evidence suggesting that because the response to fuel economy is a more permanent effect than changes in gasoline prices, it may be higher, suggesting an *underestimate* of the rebound effect (Linn 2016), although this paper did not use odometer reading data. It is possible the sign depends on the exact circumstances.
- 2) **There is likely to be a larger response in the long-run than the short-run.** Many of the estimates listed above are short-run or medium-run estimates, which means they are appropriate for the first few years of the policy but would be expected to *underestimate* the rebound effect in the long-run. In the longer-run, households may make larger decisions, such as where to live and work, based in part on how expensive driving is. So, while the studies above would be expected to capture most of the response, one would expect a larger response in the long-run. This of course could be countered by consumers becoming habituated to the higher prices, which would reduce the long-run effects. Unfortunately, it is extremely difficult to directly identify long-run effects, so we have limited evidence on the true long-run effects and how they compare to the short-run effects.
- 3) **As households get wealthier and roads become more congested, the rebound effect is likely to be smaller.** There is solid theory and several papers suggesting that as households become wealthier, the time value of driving becomes more important than the cost of fuel (Hymel and Small 2015, Hymel et al. 2010, Small and Van Dender 2007). Similarly, as roads become more congested, consumers will care less about fuel and more about the time spent in traffic. These factors both suggest that the above studies may provide useful guidance for today but are *overestimating* the rebound effect in the future.
- 4) **Fuel economy will change along with a bundle of attributes, and some of these changes may make driving less appealing.** There is quasi-experimental evidence indicating that if other valued attributes are reduced when fuel economy is improved, consumers will not drive more upon moving into higher fuel economy vehicles (West et al. 2017). Of course, this may not happen all the time, as some technologies may improve both vehicle performance and fuel economy at the same

time. But if other attributes are reduced, this would imply that the above estimates are *overestimates* of the rebound effect one would actually observe from fuel economy standards.

- 5) **More costly vehicles will also reduce the budget available for driving, reducing driving.** This can come about from higher monthly car/truck payments or greater depreciation of the value of the vehicle. This direct effect of reduced income is also discussed in Borenstein (2015). This effect would imply that the studies above *overestimate* the rebound effect. In comparing this factor to the previous one, note that the effect of the money saved from a reduced cost of driving on driving behavior is already included in the estimates of the rebound effect.
- 6) **The money saved by fuel economy standards at the gasoline pump may be diverted to other uses that may lead to additional fuel use, but more costly vehicles will imply less is available for other uses.** This is commonly known as an 'indirect rebound effect' and it depends on how much money is saved on net from the fuel economy standards, and if so, the energy intensity of what that money would be used for. The sign of this effect is ambiguous (Borenstein 2015) and the magnitude is challenging to identify (Gillingham et al. 2016). If consumers save money on net from fuel economy standards then this effect would imply that the above studies *underestimate* the rebound effect and if they lose money on net from fuel economy standards then this effect would imply that the above studies *overestimate* the rebound effect. Importantly, this indirect effect could influence total societal emissions, but would not influence driving, and thus would not lead to additional vehicle crash fatalities. Moreover, there is no evidence this commenter is aware of on indirect rebound effects from fuel economy improvements from standards.
- 7) **Fuel economy standards would also reduce the global demand for oil, lowering the global oil price, and leading to more consumption globally in equilibrium (and possibly influencing the direction of innovation).** This is known as a 'macroeconomic rebound' and is not mentioned in the NPRM discussion of the rebound effect. On net, these effects may be positive or negative but are usually expected to increase the rebound effect (Gillingham et al. 2016). Note that most of the effect of this macroeconomic rebound will be seen elsewhere in the world, and thus, will not affect driving, fatalities, or emissions in the United States. The effect will influence emissions elsewhere in the world, and a small portion would influence driving in the United States just as any fall in gasoline prices would. Thus, it would imply that the above studies may modestly *underestimate* the rebound effect. Reliably quantifying these effects is very difficult.

The NPRM does not weigh these additional factors as part of the justification for the choice of the rebound effect. These factors increase the uncertainty bounds around the central case estimate, as all of these factors are areas that warrant future research. This implies

that sensitivity analysis of different values of the rebound is essential (recognizing that some of the factors are likely to only minimally influence driving in the United States). Importantly, we can note that **these factors do not point in a single direction**—a roughly equal number imply that the studies mentioned above are an underestimate as imply that they are an overestimate and we do not currently have a reason to believe that there is an upward or downward bias on net. It would be difficult to defend a higher or lower central case rebound effect based on these factors, and this commenter notes that the NPRM does not attempt to use these factors to justify a higher or lower estimate. These are areas that this commenter encourages the Agencies to track going forward as the literature continues to advance to a point where these factors can be incorporated into future analyses.

Finally, this comment also points out that the rebound effect is being applied in an unconventional way that mixes a forecast of the aggregate VMT with the response to the change in fuel economy. Under a wide range of parameters similar to those in the NPRM, this implies that the modeled change in VMT is an overestimate of what would be implied by a standard application of the rebound effect. This commenter encourages the Agencies to choose a baseline VMT projection (perhaps based on the Annual Energy Outlook) and apply the rebound effect to this VMT projection.

Introduction

This comment is on the proposed federal “The Safer Affordable Fuel-Efficient (SAFE) Vehicles Rule for Model Years 2021-2026 Passenger Cars and Light Trucks” by the U.S. Environmental Protection Agency and the U.S. Department of Transportation (hereafter the “Agencies”). It is cited as 83 Fed. Reg. 42,986 (Aug. 24, 2018). The proposed rule presents several alternatives to relax greenhouse gas emissions and corporate average fuel economy (CAFE) standards for model years 2020-2026, with a leading proposal to roll back the levels set for 2020. This rollback to 2020 levels and its comparison to the so-called “augural” standards for model years 2022-2026, which are the existing standards. The remainder of the document will use the term “fuel economy standards” to refer generally to “fuel economy standards or greenhouse gas standards” for ease of readability.

This comment focuses narrowly on the Agencies’ justification for their choice of the rebound effect estimate. In the Notice of Proposed Rulemaking (NPRM), the Agencies have clearly undertaken a substantial effort in their review of the literature, especially in reviewing recent work, including some of my own. The goal of this comment is to bring up key points that seem to have been missed in the discussion of the rebound literature in the NPRM, including several studies that were excluded from the literature review in the NPRM. A key finding of this literature review is that the best evidence currently available does not support a central case estimate of the rebound effect of 20%.

This document will first cover the definition of the rebound effect that may occur from a policy like fuel economy standards, which provides the intellectual underpinnings for a subsequent review of the literature. It will also provide some suggestions for further aspects to be considered in the final rule and future rule-makings, including a discussion of how to correctly implement a rebound effect and the consequences of implementing it incorrectly.

Defining the Rebound Effect from Fuel Economy Standards

The rebound effect from energy efficiency standards refers to behavioral and market responses to the policy of fuel economy standards that influence the fuel savings and emissions reductions realized from the policy. To be more concrete, consider the most straightforward behavioral response to a fuel economy standard: when the fuel economy of the vehicle is improved, the cost per mile of driving is reduced, making it cheaper to drive and thus leading to more driving. The additional driving uses fuel, implying that the fuel savings from the improved fuel economy are reduced. In other words, there is a behavioral “rebound” in fuel use after the initial fuel savings.

The rebound described above—whereby a lower cost per mile of driving leads to more driving—is often described in the literature as the “direct rebound effect,” and the Agencies

often refer to it as the “VMT rebound effect,” where VMT refers to vehicle-miles-traveled. It is typically referring to the percentage of fuel savings (or sometimes emissions reductions) that are offset by the rebound effect. So, for instance, if fuel economy standards lead to the cost per mile of driving decreasing by 10%, a direct rebound effect of 20% implies that driving will increase by 2% (the same as applying a cost per mile price elasticity of driving of -0.2), reducing the fuel savings.² The direct rebound effect is the effect that the Agencies have focused on in rulemakings both in the past and in the current NPRM.

In addition to the direct rebound, there may also be indirect rebound effects. The classic indirect rebound effect refers to households using the money saved from purchasing less fuel to buy other desired goods and services, some of which may have fuel use and emissions associated with them. For example, if a household has a more efficient vehicle and saves money on gasoline at the pump, they may decide to take an additional flight to a vacation destination, thus leading to additional fuel use and emissions. Of course, if the vehicle is more expensive due to fuel economy standards, for most buyers the vehicle payments will be higher, and these higher vehicle payments could partly or even entirely offset the savings at the pump. In fact, this indirect rebound effect could even be negative (Borenstein 2015, Gillingham et al. 2016). The Agencies have tended to assume this component of the rebound effect is zero for rulemakings on fuel economy standards, and the latest NPRM does not appear to be any different. This comment supports the Agencies in this decision, as this effect is likely to be very modest relative to the direct effect on driving from fuel economy standards. This comment does suggest that the Agencies continue to monitor the literature for estimates that may be relevant for future fuel economy or GHG standards.

There may also be broader indirect effects that are often called ‘macroeconomic rebound effects.’ For example, if all households in the United States observe fuel savings due to fuel economy standards, then the global demand for oil will be reduced, putting downward pressure on the global oil price, and in equilibrium, leading people to drive more around the world. This effect could be important from a global perspective, but it is important to consider what the ramifications would be. Most of the effect would be felt around the world, outside of the United States. Only a small portion of this effect would be felt in the United States, from the small decline in fuel price. Thus, driving – and emissions, congestion, fatalities, etc. – in the United States will only be very modestly affected. Emissions worldwide would be affected, and there would also be benefits outside of the United States from the lower oil price.

² For a concrete example, suppose a vehicle gets 25 miles per gallon and the fuel price is \$3 per gallon. Then the cost per mile of driving is \$0.12 per mile. Suppose this cost per mile declines by 10%. A rebound effect of 20% indicates that the vehicle will be driven 2% more per year. So if the vehicle would have been driven 10,000 miles per year, after the rebound it will be driven 10,200 miles per year. The fuel savings would have been \$120 per year without a rebound effect (a 10% savings), but with the rebound effect, the fuel savings would be \$98 per year (about an 8% savings).

Quantifying the macroeconomic rebound effect is incredibly challenging, and any estimate is highly speculative. This may be why the Agencies have not discussed the macroeconomic rebound effect in the NPRM or any other recent rulemakings. Even determining the sign of the macroeconomic rebound effect is difficult. If reduced demand for oil due to fuel economy standards leads to lower oil prices, this effect would provide another channel by which fuel economy standards affect fuel use and emissions, separate from the direct effect calculated in most studies of the rebound effect and described conceptually above. However, there are also further forces that may reduce or increase the magnitude of the macroeconomic rebound effect, including terms-of-trade effects along with sectoral reallocation effects (Koesler et al. 2016, Turner 2013), effects of reduced income from the cost of the policy (Fullerton and Ta 2018), and possible effects on the long-run path of innovation (Gillingham et al. 2016).

One key point is that with different assumptions, one can find nearly any answer—ranging from a negative rebound to backfire (where there are no fuel savings at all). Furthermore, when there is a macroeconomic rebound effect, for the most part it is primarily influencing driving elsewhere in the world, not in the United States (there would be some effect from lower gasoline prices in the United States as well, but this would be dwarfed by the effect globally). Thus, given that these effects are primarily global, and the weak evidence base on these effects in general equilibrium, it is perhaps not surprising that the Agencies choose not to attempt to quantify the effects of the macroeconomic rebound in the NPRM or any other recent rulemakings. This comment supports the Agencies in this decision on the macroeconomic rebound effects, as their nature makes it inappropriate to use them in a rulemaking at this time. This comment further supports monitoring the academic literature for further developments in this area that may provide a solid basis for use in regulatory analysis.

One extremely important note to make about the rebound effect is that from a social welfare perspective, *it can bring in benefits as well as costs*. For example, in the context of the direct rebound effect, motorists who drive more due to the lower cost per mile of driving do so because it is in their best interest to do so—it allows for visits to family members, vacations, and other valued travel. This valued travel has a positive welfare effect, which must be weighed against the negative consequences of that travel, including the additional fuel used. This comment supports the Agencies in the current NPRM for attempting to quantify this effect and for being very careful to exclude costs due to the rebound from the benefit-cost analysis when the benefits cannot be appropriately quantified. Further work to quantify both the benefits and costs is encouraged.

Estimating of the Direct Rebound Effect of Fuel Economy Standards

Even when restricting the focus to the direct rebound effect, it is not trivial to estimate the magnitude of the effect that comes about from a policy. In an ideal world for analysis, one would have two identical settings: one with fuel economy standards and one without. The difference in driving and fuel consumption could then be used to calculate a causal estimate of the rebound effect. Unfortunately, such a world does not exist, leaving analysts to use other approaches to get an estimate of the rebound effect.

The most common approach used to try to develop an estimate of the rebound effect is to use data on fuel prices and VMT to empirically discern the relationship between these two variables through an econometric analysis. The analysis nearly always is used to estimate the *elasticity of VMT with respect to the price of driving* (i.e., the percent change in driving that would be expected to come about when the price of driving changes divided by the percentage change in the price of driving). Often this elasticity is represented as the elasticity with respect to the fuel price—in dollars per gallon—and sometimes it is represented as the elasticity with respect to the price per mile of driving—in dollars per mile.³

There are several caveats that are important to consider when using this basic approach to try to develop an estimate of the rebound effect that would stem from a fuel economy policy. Such estimates are derived using variation (i.e., changes) in fuel prices and, when carefully done, can tell us something about how motorists respond to fuel prices. Under a fuel economy standard, we are interested in how motorists respond when placed in a higher fuel economy vehicle. In order to use estimates based on changes in fuel prices as a proxy for the rebound effect of a fuel economy standard, one must assume that consumers respond to a change in fuel economy due to the standards in the same way they respond to changes in fuel prices. As a starting point, this may not be a terrible assumption, for both fuel prices and fuel economy influence the cost per mile of driving, which is fundamentally what underpins the direct rebound effect.

However, there are several important reasons why there may be a difference between how consumers respond to fuel economy standards and how consumers respond to fuel price changes:

1. Fuel economy standards generally imply that the vehicle will cost more. With less money, consumers may not drive quite as much (an income effect), thus reducing the additional miles driven with improved fuel economy (Borenstein 2015).
2. Fuel economy standards may also influence other attributes of vehicles. To the extent that these are attributes that are valued by consumers, such as horsepower and acceleration, the change in these other attributes could to make driving

³ Analysts often convert the elasticity into a percentage and remove the negative sign. So, a VMT elasticity of -0.2, which refers to a 2% change in VMT for a 10% change in price, would be described as a 20% rebound effect.

somewhat less appealing (although do not necessarily do so), again reducing the additional miles driven with improved fuel economy (Gillingham et al. 2016).

3. Behavioral economics provides strong evidence that consumers respond differently depending on the framing of the situation. Thus, motorists may respond differently to a change in fuel prices that leads to unexpected “pain at the pump” (especially during times of high gasoline prices) than they would to a long-term expected change such as improved fuel economy of their vehicle. This effect could enhance or reduce the rebound effect and the empirical evidence is mixed. More papers show that consumers respond more to changes in fuel prices than fuel economy (e.g., West et al. 2017, De Borger et al. 2016, Greene 2012, Gillingham 2011), implying that the rebound effect estimated using fuel prices is overstated relative to the rebound effect estimated using fuel economy, but more research is needed in this area. Similarly, economists have long known that consumers tend to adjust their driving more in response to increases in fuel prices than decreases in fuel prices (Gately and Huntington 2002, Hymel and Small 2015), and thus the exact time frame being considered greatly matters for estimates of the rebound.

All three of these reasons mean that using an estimate of the elasticity based on fuel price changes is an overestimate of the rebound effect from improvements in fuel economy. This turns out to be important in understanding and properly interpreting the literature. This comment encourages the Agencies to consider these factors carefully in their taking of findings from the literature and use for regulatory analysis.

There are also several other important considerations that are critical for interpreting the literature:

1. There is no one single rebound effect for any given policy. There may be short-run effects and longer-run effects. In the short-run, consumers may respond with small changes—an occasional extra trip. In the longer-run, they may change the next vehicle they buy or even where they live or work. In general, one expects the rebound effect to be smaller in the short run than the long run. However, it is extremely challenging to directly identify a long-run rebound effect, as so many other things also change over time. Attempts to estimate a long-run effect tend to be based on structural assumptions or use cross-sectional data. Most papers in this literature, including nearly all of the ones in the tables in this comment are based on short and medium-run responses because these responses are much easier to identify. Further, when there are long-run effects being estimated, it is often difficult to ascertain how many years into the future long-run is referring to. Despite this, understanding longer-run responses is another area worthy of further research.
2. For any given time-frame (e.g., two years), both theory and evidence suggest that the rebound effect may change over time. For instance, the rebound effect in 2018 can be expected to be different than the rebound effect in 2025. Theory suggests

that when expenditures on fuel become a smaller percentage of consumer income, motorists will be less responsive, and the rebound effect will be closer to zero. Evidence supports this as well. There is broad evidence from multiple papers that lower-income motorists (for which fuel costs make up a larger fraction of their budget) tend to be more responsive to fuel price changes. There is also evidence from empirical studies indicating that the rebound effect will be closer to zero in the near future as incomes continue to rise (Small and Van Dender 2007, Hymel et al. 2010, Hymel and Small 2015). Similarly, theory and evidence support a rebound effect closer to zero with greater congestion. When the roads are more congested, the cost of fuel becomes a smaller fraction of the cost of travel, and thus it follows that it would be expected to be less impactful. This effect has been seen in empirical studies as well (Hymel et al. 2010, Hymel and Small 2015). This comment will discuss these empirical studies at greater length below, explaining why these studies provide the best evidence available for consideration in regulatory analysis.

3. The sample of vehicles used for the estimation of the elasticity is important for understanding the rebound effect of fuel economy standards. For example, we should be very cautious in using estimates of the rebound effect from outside of the United States. Theory and evidence indicate that the consumer response to a lower cost per mile of driving differs by setting. For example, areas with greater public transportation tend to see a larger effect (Gillingham 2014, Gillingham and Munk-Nielsen 2018). So, estimates of the elasticity from Europe, where public transportation is much better than in the United States, would be ill-suited to use as a proxy for the rebound effect in the United States. Similarly, one should be cautious about using an estimated elasticity on a small subsample of the population, which may be more or less responsive than others. This could be particularly important due to within-household switching between vehicles. If a new vehicle has higher fuel economy, this would be expected to draw miles from older vehicles also owned by the household (Leung 2015, Archsmith et al. 2018). From a social perspective, we care about all miles driven by all vehicles in the fleet, so an estimate that only uses new vehicles would be an over-estimate of the average response across the entire fleet. This provides essential context for understanding several of the studies in the literature.

Given all of this, the ideal estimate of the rebound effect would be time-varying, changing with conditions, starting with a short-run effect that grows to a long-run effect. It would only be applied to new vehicles at first and would be based on the difference in fuel economy between the new vehicle and the traded-in vehicle (this can be done in aggregate to make it feasible). As new vehicles become used vehicles and are sold to others, there would be a ripple effect on used vehicles as well, but this would not occur for several years. This is important for properly modeling the rebound effect in an analysis.

Applying the Rebound Effect to Model VMT

The rebound effect is also most appropriately applied to examine how a baseline scenario of VMT and a counterfactual or policy scenario of VMT. In other words, the most appropriate way to apply estimates of the rebound effect is to begin with an assumed amount of driving in each future year – for example, based on an Annual Energy Outlook projection – and then apply the rebound effect based on the assumed change in the cost per mile in each year due to the fuel economy standards to create a counterfactual scenario of the amount of driving in each year. Ideally, a short-run rebound effect would be used first, followed by a longer-run rebound effect for each cohort of new vehicles.

This ideal approach to apply the rebound effect is different than the approach the NPRM appears to use. In the NPRM, the rebound effect is based on the difference in the cost per mile between new vehicles and the 2016 model year vehicles for any year, and it is applied to both the augural standards and the NPRM proposed standards. The 20% rebound effect is also applied immediately. From this, the NPRM calculates the change in VMT as the difference in driving between the augural standards and NPRM proposed standards.

This approach in the NPRM is problematic for two reasons. First, a short-run rebound effect should be used for the first year that a cohort of new vehicles is in the fleet, followed by a longer-run rebound effect in later years. By applying a 20% longer-run rebound effect immediately, the NPRM mechanically overestimates the rebound effect for short-run responses. This may only have a small impact, for the overestimation is only for a handful of years, but this would clearly imply an upward bias in the NPRM analysis in VMT, and accordingly, fatalities.

Second, by calculating the rebound effect using the difference in the cost per mile between the 2016 model year and the forecasted model year, the analysis is confounding a baseline projection of VMT with the rebound effect.⁴ In other words, the NPRM analysis appears to be *using the rebound effect to forecast driving*. This is a strange approach, as it boils down the broader influences of driving to only the cost per mile. In reality, the cost per mile is only one of many factors that influence driving. For example, there was a decline in aggregate VMT during much of the late 2000s and early 2010s. For some of this time the cost per mile of driving was actually declining as more efficient vehicles were coming on the road and fuel prices were lower (e.g., consider 2011). Of course, the change in VMT during this period was affected by other factors, such as economic conditions and a movement of households back to living in cities (since 2013 VMT has been slightly rising

⁴ Technically, the NPRM uses the equation $VMT_t = VMT_{2016} \left(1 - R \left(\frac{CPM_t - CPM_{2016}}{CPM_{2016}}\right)\right)$ where R is the assumed rebound effect (e.g., 0.2 is the NPRM assumption), CPM refers to the cost per mile, and this equation is used for each style of vehicle and age of vehicle. t refers to the year of interest.

again).⁵ Thus, from a modeling perspective, using the rebound effect to forecast driving is a poor approach.

But the approach can also have consequences for the results. Consider a simple example. Suppose in calendar year 2021, the fuel price is the EIA's forecast of \$3.13. Under the augural standards, the average fuel economy of cars is set to be 46.4 miles per gallon, while under the NPRM proposed standards, the fuel economy would be 43.6 miles per gallon. Then the augural standards would have a cost per mile of \$0.067, while for the proposed standards it would be \$0.072. Assume the fuel price remains the same (as is the assumption by the Agencies). Further assume that the average VMT in 2016 is 12,000 miles per year, the gasoline price is \$2.14 per gallon (from EIA data), and the average fuel economy for cars is 37.8 miles per gallon.

Under these assumptions, the NPRM approach with a 20% rebound would give a 2021 annual per vehicle VMT of 11,540 for the augural standards and 11,357 for the proposed standards. Note that the VMT under the NPRM approach is lower in 2021 than 2016, despite the higher fuel economy. This is because fuel prices increased between 2016 and 2021 and the rebound effect is applied to the cost per mile of driving. This highlights that the rebound is being used in the analysis to create the forecast of future driving. It turns out this can really matter too. Using the NPRM approach, the difference between the 2021 augural standards and proposed standards under these example assumptions is 184 miles per year.

Under the standard way of applying the rebound effect, one would take a forecast of VMT for each year as the starting point. For comparability, start with the projection of VMT in the augural standards and set it equal to 11,540 miles per year, as above. The augural standards differ from the proposed standards in fuel economy, so there is a difference in the cost per mile. Applying this percentage change in the cost per mile between the two policy scenarios to the 20% rebound effect yields a calculation of 11,392 miles under the proposed standards. Using the standard approach, the difference between the 2021 augural standards and the proposed standards is 148 miles per year.⁶

Thus, the difference in VMT between the two standards using the standard approach is only 80% of the difference using the NPRM modeling approach. This means that the NPRM approach is overstating the fatalities and emissions from the augural standards relative to the proposed NPRM standards. The fundamental reason for the difference is that the NPRM approach is mixing the rebound effect from fuel economy changes with the projection of

⁵ See EIA's database for a graph of aggregate VMT over time: <https://www.eia.gov/opensdata/qb.php?category=1039999&sdid=STEO.MVVMPUS.A>.

⁶ The standard approach uses the formula $VMT_P = VMT_A(1 - R \left(\frac{CPM_P - CPM_A}{CPM_A} \right))$, where P subscripts refer to the proposed standards and A subscripts refer to the augural standards. R is again the assumed rebound effect (e.g., 0.2) and CPM is again the cost per mile of driving.

future fuel prices. In the reasonable example calculations, I found that the NPRM approach biases VMT upwards. It is theoretically possible for the bias to work in the other direction too. It depends on the exact assumptions about fuel prices and fuel economy improvements.⁷ However, under other reasonable values that tend to match with the increase in fuel prices and VMT in the NPRM modeling, I nearly always find that the NPRM approach to incorporating the rebound is overstating the change in VMT relative to the standard approach of applying the rebound. In other words, the VMT change from the rebound is being overestimated in the NPRM.

Thus, this comment strongly encourages the Agencies to find to a reasonable projection of VMT, such as from the Annual Energy Outlook, and apply the rebound effect using the standard approach to determine the driving in the counterfactual policy scenario relative to the baseline projection. Further, this commenter also encourages the Agencies to use a smaller rebound effect for the first year or two of each cohort of new vehicles before having the rebound effect converge to a longer-run value. The bottom line is that it is clear that under a wide range of assumptions similar to those in the NPRM, the current NPRM modeling approach overestimates the change in VMT from the rebound effect, regardless of the estimate of the rebound effect from the literature that is being used.

Review of Estimates in the Literature Relevant to the Direct Rebound Effect

There is a voluminous literature relating to the motorist response to changes in fuel prices. Indeed, this can probably be considered one of the key questions in energy economics. There is unfortunately a scant literature that aims to see how motorists respond to changes in fuel economy.

Literature Based on Changes in Fuel Economy

There are three most relevant recent papers based on changes in fuel economy. These papers use very different data and take quite different empirical strategies. Recall that ideally one would want a strategy that comes as close as possible to mimicking the setting of CAFE standards: that is, is based on variation from a policy that induces households to buy more efficient vehicles.

The only paper that is based on such a setting is West et al. (2017), which is published in the *Journal of Public Economics*. This paper examines the context of cash-for-clunkers and

⁷ If the fuel price and fuel economy are identical in 2016 and 2021, then the two approaches give exactly the same change in VMT between the augural and proposed standards. Holding fuel economy fixed between the two years, if fuel prices rise between 2016 and 2021, then the standard approach will always give a smaller difference in VMT than the NPRM approach. Holding fuel prices fixed between the two years, if fuel economy rises between 2016 and 2021, then the standard approach will give a larger difference in VMT than the NPRM approach. When both change at the same time, then it depends on the relative magnitudes of the changes.

uses detailed odometer reading data from vehicle inspection programs. West et al. examine new vehicle buyers in Texas who were induced to buy more efficient vehicles because of the Cash-for-Clunkers program in 2009. They compared those new vehicle buyers who traded in a “clunker” that was just eligible for the program with those new vehicle buyers who were just ineligible, in a natural experiment or quasi-experiment. The two groups of buyers are similar in all ways to each other, and thus the ineligible households can serve as a useful control group for the eligible households. This clever strategy yields a striking result: the new vehicle buyers induced into more efficient vehicles do not appear to drive more at all. Effectively, this implies a rebound effect of zero.

This result is particularly useful for understanding the effect of fuel economy standards for several reasons. First, it is a rare example where some households are exogenously induced into a higher fuel economy vehicle while others are not, allowing for a clean empirical design for understanding the effect of fuel economy. Second, the study captures an important detail that studies using fuel price variation do not: the fact that when people buy a new vehicle, they buy a bundle of attributes. So, the new higher fuel economy vehicles may have different attributes than the vehicles that would have been purchased otherwise (e.g., less horsepower or acceleration). This may also affect how much is driven. No study based on fuel price variation can capture this but a change in the bundle of attributes is what would happen under fuel economy standards. Third, the evidence is recent and thus is more likely to be relevant for today.

The study is of course not the final word on the subject, in that it is based on new vehicle buyers who are trading in a clunker in Texas in 2009 and then are driving in 2010. This sample reflects only part of the fleet. It is also possible that the changes in the bundle of attributes from fuel economy standards may be somewhat different than the changes observed in the West et al. data. However, it provides very important evidence that should be considered carefully in considering the rebound effect from fuel economy standards. The Agencies appear to dismiss the paper in the NPRM, and this comment strongly urges the Agencies to consider it in the body of evidence.

There are two notable other papers directly relevant for understanding the response to changes in fuel economy. Both of these papers use a strategy that compares the fuel economy and driving across different households (i.e., cross-sectional variation). A challenge when using this approach is that vehicle buyers often choose the fuel economy of the vehicle based on their driving needs. If a vehicle buyer knows that she will be driving a lot, she is likely to change the vehicle she purchases. For example, she may buy a more efficient vehicle to reduce the cost of driving. Thus, simply looking across households is likely to be problematic unless one can find an “instrumental variable,” (IV) which is a variable that leads some households to purchase higher fuel economy vehicles but should not otherwise influence driving. Finding such an instrument can be very challenging and the three recent papers take different approaches to this.

The first paper, De Borger et al. (2016), focuses on households that replace one vehicle with another and use the difference in the fuel economy of the old vehicle of the household and the average fuel economy for new registered vehicles in the year in which the vehicle was purchased. The idea is that if the fuel economy of the old vehicle is much larger than the new vehicle fleet average, households will be influenced more to buy a higher fuel economy new vehicle, and vice versa. And at the same time, after already controlling for time-invariant household preferences (through a first-differencing approach at the household level), it is somewhat difficult to see how the difference between the old vehicle fuel economy and the fleet-wide new vehicle fuel economy directly influences driving decisions. This implies that De Borger et al. (2016) found a plausibly valid instrument. Their result for the rebound effect is an effect of 7.5% to 10%. Notably, they also find that the response to fuel economy is much less than the response to fuel prices. However, De Borger et al. are using a (extremely rich) dataset of odometer readings from Denmark covering the period 2001-2011 and focus only on households in Denmark that only have one vehicle. Thus, as previously mentioned, one should be very careful in applying this estimate to the United States. Furthermore, all other vehicle characteristics are held constant, rather than being allowed to change, as one would want in the ideal case. That said, it provides evidence from a reasonably compelling empirical design that the fuel economy elasticity in at least one other context is closer to zero than the fuel price elasticity⁸ and also provides another point estimate of the fuel economy elasticity.

The second paper, Linn (2016), uses the gasoline price at the time of the purchase of the vehicle as the instrument. The argument for this instrument goes as follows: the gasoline price at the time of the purchase of the vehicle influences the fuel economy of the new vehicle purchased, but because it is typically in the long-past, the gasoline price at the time of purchase should not affect driving today. This argument makes a great deal of sense for older vehicles, but is less likely to make sense for newer vehicles for the gasoline price a short time ago may still influence driving decisions today by influencing consumer expectations. Thus, the instrument is likely invalid for newer vehicles, but valid for older vehicles. This instrument is further interacted with household characteristics, presumably because it is otherwise underpowered. Linn (2016) uses data from a single survey, the 2009 National Household Travel Survey (NHTS), and estimates a rebound effect around 20% without the instrument and a rebound effect on the order of 40% with the instrumental variables approach. More notably, this is the only published paper I am aware of in the literature for which the response to fuel economy is *greater* than the response to fuel prices. There are several possible interpretations for this result. One plausible

⁸ In a doctoral dissertation, Gillingham (2011), finds a similar lower response to fuel economy than gasoline prices using data from California. In a published paper, Greene (2012) uses aggregate data from all of the United States 1966-2007 to come to a similar conclusion. In other published work both the much older paper Greene et al. (1999) and the more recent Frondel et al. (2012) find no statistically significant difference, using survey data from the United States and Germany respectively.

interpretation is that the results are valid but that 2009 was an unusual year in that it was in the depths of the Great Recession, when changes to the cost of driving would likely have been particularly impactful. Another interpretation is that the instrument is not valid for newer vehicles, biasing the coefficients. A third interpretation is that the NHTS survey data, based on self-reported miles driven (and sometimes adjusted based on a lifetime odometer reading), may face a sample-selection bias if households that are willing to self-report may be more attentive and respond more to changes in fuel economy or gasoline prices.⁹ Thus, Linn (2016) serves as another useful estimate that is certainly worth including in our evidence base, but it would be indefensible to weight this paper the same as estimates based on larger and more precise datasets (see comment in docket from J. Linn making the same point; docket number EPA-HQ-OAR-2018-0283-1642).

In addition to these two papers, other recent studies that run a regression of VMT on fuel economy include Gillingham (2011), West & Pickrell (2011; unpublished), Greene (2012), Frondel et al. (2012). None of these papers instrument for fuel economy, and thus their findings should be taken as quite suggestive, rather than as causal estimates of the rebound effect.¹⁰ However, with this caveat in mind they do provide further evidence. A further caveat is that Frondel et al. (2012) is using data from Germany, rather than the United States.

Table 1 provides a brief summary of the small section of recent papers that truly focus on the response to fuel economy using data from the United States (thus, De Borger et al. (2016) and Frondel et al. (2012) are omitted). These are ordered in the same order as the discussion above. All of the estimates are short-run or medium-run estimates. One striking feature of the table is that the papers with much larger estimates of the rebound use self-reported estimated miles driven, rather than odometer readings. But the majority of the estimates are at or below 10%.

Table 1. Evidence on the Response to Fuel Economy from the United States					
Study	Research Design	Setting	Data	Rebound Estimate	Further Caveats

⁹ Using earlier waves of the NHTS that also included odometer readings taken about 3 months apart, Li et al. (2014) show that the self-reported miles driven has a similar mean by smaller variance than the miles driven estimate using odometer readings. This provides some comfort in the use of NHTS data and is why evidence from this data source should not be entirely disregarded. It does not tell us whether the response in driving to gasoline prices or fuel economy from those who self-report is similar or different, so generally analysts prefer odometer readings.

¹⁰ West and Pickrell (2011) is an unpublished study that has a variety of interesting specifications and an attempt at a Heckman selection model. From the data available, it is difficult to know which specifications are the primary specifications and it appears that the primary specifications did not instrument for fuel economy or the gasoline price.

West et al. (2017)	Quasi-experiment	Texas before and after 2009	Odometer readings	0%	Selected sample
Linn (2016)	Instruments	US in 2009	Self-reported	20-40%	Data; IV valid?
Greene (2012)	Controls	US 1966-2007	Aggregate data	0%	No IV
Gillingham (2011)	Controls	California 2001-2009	Odometer readings	1%	No IV
West & Pickrell (2011; unpublished and unavailable)	Controls	US 2009	Self-reported	9-27%	No IV
<i>Notes: IV refers to an instrumental variable. West & Pickrell (2011) is cited in the NPRM as 9-34%, but the only presentation available for this study shows a range of 9-27%.</i>					

Literature Based on Changes in Fuel Prices

The literature on the response to fuel prices is vast and goes back decades. For example, nearly 40 years ago, Sweeney (1979) used aggregate national data from the Federal Highway Administration 1957-1974 to estimate a VMT elasticity with respect to the price of gasoline of -0.12 to -0.23, which has been interpreted in past work to imply a rebound effect of 12-23%. Since this seminal work, the field has come a long way, with better data, improved research designs, and more emphasis placed on how the response to fuel prices might change over time. Thus, this review will focus solely on recent work rather than the much earlier literature. In the 2008 Regulatory Impact Assessment of CAFE standards, there was a quite thorough review of literature of the response to fuel prices dating back to the 1970s, which was mentioned again as a justification for the rebound effect in the current NPRM. This comment strongly encourages the Agencies to be very cautious in using the much older literature and instead focus on what we have learned over the past decade, which is a large literature by itself.

Similarly, the literature is even larger when studies from outside of the United States are included. Given that there is strong evidence that consumers respond differently in different settings, as described above, and that there are many studies from the United States to draw from, this comment strongly encourages the Agencies to focus only on literature from the United States. The two studies from Denmark and Germany were mentioned above solely because they are some of the only papers that actually examine the effect of fuel economy on driving. The remainder of this review will only briefly mention additional studies from outside the United States at the end.

The estimates reviewed in this section all are based on the observed changes in driving that consumers make in response to changes in fuel prices. In other words, they are based on variation in fuel prices. Some of these studies estimate the effect of fuel prices directly and others estimate the effect of the cost per mile of driving (i.e., the fuel price divided by fuel economy), but those that estimate the effect of the cost per mile of driving also use vehicle fixed effects, which allow the econometrician to remove all time invariant differences between vehicles that may be correlated with preferences. For example, vehicle fixed effects would in general remove the influence of fuel economy, which is useful to the extent that different types of people who plan to drive different amounts purchase vehicles with different fuel economy. Thus, all of these studies can be thought of as exploiting changes in fuel prices. There is further a bit of a consistency question in the NPRM, which early in the document argues that we no longer have to worry much about fuel price shocks due to the increase in domestic oil production over the last decade. If one really believes that fuel prices will not be volatile going forward (this commenter does not), then for consistency, it would be inappropriate to use any evidence from changes in fuel prices; all evidence from the rebound effect should be from changes in fuel economy.

Before diving in further into the literature, it is important to re-emphasize that all of these estimates identified based on changes in fuel prices should in general be treated with caution as an estimate of the rebound effect from a fuel economy standard. They rely on the assumption that people respond to fuel economy changes in the same way that they respond to fuel price changes, and the studies that derive the estimates also nearly always hold vehicle characteristics fixed. As discussed above, fuel economy standards would be expected to alter manufacturer decisions and lead to changes in attributes in future vehicles. Thus, the previous evidence given above that is based on analyses of fuel economy changes should be given more weight than this evidence derived from fuel price changes.

In examining the literature estimating a response to fuel price changes, it is useful to make a distinction between papers that estimate the VMT elasticity with respect to fuel prices and papers that estimate the VMT elasticity with respect to the cost per mile of driving (the fuel price divided by fuel economy). An argument sometimes used for using gasoline prices rather than the cost per mile is that it is a cleaner measure of the response because it inherently avoids any concerns about selection that may confound cost per mile estimations that include variation in fuel economy. It is important to note that both approaches rely on variation in fuel prices, but the cost per mile approach may or may not also rely on variation in fuel economy across motorists. For example, several recent studies regress VMT on the cost per mile of driving, other covariates, and include fixed effects at either vehicle model or individual vehicle (i.e., based on the vehicle identification number) level. When vehicle model or VIN fixed effects are included, the estimation is then relying on variation in fuel prices (as every vehicle model or VIN has the same fuel economy). In general, the estimates in the literature tend to be fairly similar regardless of which approach is used. Thus, I will follow the Agencies in discussing them together.

Recent Evidence from the United States that Uses Odometer Readings

The best recent evidence from the United States consists of a handful of papers that tend to use odometer readings and research strategies that account for potential issues in estimating a causal relationship between fuel prices (or the cost per mile) and driving. These papers again use changes in fuel prices as the primary source of variation for identifying how consumers change the amount they drive.

One of the best recent papers estimating the relationship between the cost per mile and VMT is Knittel and Sandler (2018). This paper is published in the *American Economic Journal: Economic Policy*, a top economics journal. Knittel and Sandler use odometer reading data from vehicle inspections in California from 1998 to 2010 (120 million observations). This rich dataset allows for the inclusion of VIN fixed effects so that the estimation relies on time series variation in fuel prices. Knittel and Sandler (2018) find a preferred estimate of the medium-run (two-year) estimate of the elasticity of VMT with respect to the cost per mile of driving of -0.147, which could be described as a 14.7% rebound effect under the assumptions mentioned above. While this is one of the best papers in the literature, no paper is perfect and there are two possible concerns about this estimate. One is that the cost per mile of driving is assumed to be exogenous after including the VIN fixed effects, which effectively means that the gasoline price is assumed to be exogenous and that there is no selection issue from VINs changing households. The study provides a set of robustness checks that help alleviate these worries. Another possible concern is that vehicles in California are not required to have an emissions inspection until the sixth year, so the newest vehicles in the fleet are not included in the dataset. This may bias the rebound effect upwards, as the newest vehicles in the fleet tend to be driven by wealthier households (e.g., see Gillingham 2015). The new vehicle buyers are also the households most directly affected by the rebound effect from fuel economy standards, although over time, all vehicles in the fleet will be affected. This second point suggests that the true effect over the entire California fleet may be a bit smaller (a rebound closer to zero). A third point, and one that was mentioned before and applies to most of the studies mentioned here, is that the response to fuel economy may be different than the response to gasoline prices, which could imply that the effect would be an overestimate of the effect of fuel economy.

Another recent published paper, Gillingham et al. (2015), uses odometer readings from annual vehicle inspections in Pennsylvania over the period 2000 to 2010 to examine the response to gasoline price changes. All registered vehicles in the fleet are included in this study. This paper again uses a similar empirical strategy as Knittel and Sandler (2018) with VIN fixed effects, but it also instruments for the gasoline price using major gasoline refining disruptions in the Gulf Coast, which influence the gasoline price in Pennsylvania, but shouldn't affect driving decisions in Pennsylvania otherwise given the distance between

Pennsylvania and the Gulf Coast. The paper estimates a short-run VMT elasticity with respect to the price of gasoline of -0.1, which would imply a rebound effect of 10%. While this estimate is only valid for one state during the 2000-2010 period, it provides further evidence on the magnitude of the VMT elasticity in response to fuel price changes.

A third recent paper, Wenzel and Fujita (2018), is a recently-released report again using inspection odometer readings. This time the readings are from Texas, much as in West et al. (2017). The instrument used in this paper is the US crude oil spot price (West Texas Intermediate), and this instrument is used for either a gasoline price or a cost per mile variable, rather than a separate fuel economy variable. The logic behind this instrument is that the gasoline price or cost per mile is influenced by the price of the input, oil. For this instrument to be valid, one must also argue that the oil price does not affect driving except through the direct effect on the cost of driving. This rules out the possibility of oil prices affecting the Texas economy and influencing driving indirectly. The results using the fuel price suggest a 7.5% rebound effect, while the results for the cost per mile suggest a 15.9% rebound effect, but there are values close to zero (not statistically significant) in some of the specifications.¹¹ The larger values across different specifications tend to be estimates using an instrumental variables strategy with the oil price as the instrument, and this approach would be invalid if oil prices indirectly affect driving in Texas. However, this work does provide further evidence worth considering.

A fourth recently published paper, Gillingham (2014), uses odometer readings from only new vehicles in California to examine the impact that the gasoline price shock in 2008 had on consumer decisions about how much to drive. The primary specification does not include vehicle fixed effects or instruments, but a robustness check instruments for the gasoline price with the Brent crude oil price. This instrument is valid assuming that California is a small market relative to the global oil market. It uses new vehicles registered in 2001-2003 and subsequently given an inspection in 2005-2009. The resulting elasticity of VMT with respect to the gasoline price—based on the 2008 shock to gasoline prices—is estimated to be -0.22 and the result is nearly identical with the instrumental variables approach (implying a rebound effect of 22% if applied directly). As the study was designed to focus on a period when gasoline prices were both high and very salient to consumers (gasoline prices were in the news all the time in 2008), this estimate is useful for providing guidance on how consumers respond to gasoline price shocks, but is inappropriate to use as an estimate of the rebound effect of fuel economy standards because consumer would be

¹¹ In a personal communication, one of the authors stated: “I think you should report the estimate based on price of gasoline (either 7.5% using model fixed effects without supply instrument, or 8.7% using model fixed effects with supply instrument), rather than the estimates based on cost of driving (15.2% using model FE without supply instrument, or 15.9% using model FE with supply instrument), since most if not all of the other estimates are based on price of gasoline and not cost of driving.”

expected to respond much more to a spike or shock in gasoline prices than to a long-run steady change in the cost per mile of driving.¹²

Finally, Langer et al. (2017) use data from a single insurance company in Ohio that allowed households to opt-in to a program that tracks their odometer readings. These odometer readings were used to examine the effects of VMT taxes versus gasoline taxes. In the process of undertaking that analysis, Langer et al. (2017) also aim to carefully estimate the VMT price elasticity (using the cost per mile). They find a short-run VMT price elasticity of -0.11, which would map to a rebound effect of 11%. This analysis is well-done and its weakness is the selected sample.

While these four papers all use extremely rich data on odometer readings and all aim to use careful research designs, they each provide evidence that must be taken in the context of their time period and sample. For example, it may be entirely consistent that new vehicle buyers in Texas who are induced by Cash-for-Clunkers to buy a higher fuel economy vehicle may be entirely nonresponsive to the higher fuel economy (West et al. 2017), while all other motorists in Texas do respond to fuel price changes to the tune of 7-14% (Wenzel and Fujita 2018). The former evidence provides a clever quasi-experiment for understanding the response most relevant to fuel economy standards, while the latter tells us more about the entire Texas fleet and the response to fuel prices. One reasonable take is that the true value of the rebound effect from fuel economy standards for all of Texas (not just those who are induced by Cash-for-Clunkers) is somewhere in between.

Table 2 provides a summary of the five studies mentioned here. Again, it is important to emphasize that these studies are not directly estimating the rebound effect of a fuel economy standard, but rather are estimating the consumer response to gasoline price changes. Note that West et al. (2017) and Gillingham (2011) are repeated from Table 1.

Table 2. Evidence that Uses Odometer Readings from the United States (Preferred Evidence)					
Study	Research Design	Setting	Data	Estimate	Further Caveats
Knittel and Sandler (2018)	VIN FE	California 1998-2010	Odometer readings	14.7%	Selected sample
Wenzel & Fujita (2018)	VIN FE + Instruments	Texas 2005-2010	Odometer readings	7.5%-15.9%	IV valid?
Langer et al. (2017)	Household FE	Ohio 2009-2013	Odometer Readings	11%	Selected sample

¹² Lin and Prince (2013) estimate the effect of volatility on the elasticity of gasoline consumption with respect to the gasoline price, which is closely related to the VMT elasticity. They estimate an elasticity in the range of -0.03 to -0.29 and they show that volatility in gasoline prices can substantially influence the elasticity.

West et al. (2017)	Quasi-experiment	Texas before and after 2009	Odometer readings	0%	Selected sample
Gillingham et al. (2015)	VIN FE + Instruments	Pennsylvania 2000-2010	Odometer readings	10%	
Gillingham (2014)	VIN FE + Instruments	California 2001-2003/2009	Odometer readings	22%	Price shock
Gillingham (2011)	Controls	California 2001-2009	Odometer readings	1%	No IV
<i>Notes: IV refers to an instrumental variable. The 7.5%-15.9% range for Wenzel & Fujita (2018) is based a conversation with the authors, who suggest considering both the estimate based on fuel prices and the estimate based on the cost per mile to be consistent with the rest of the literature, which use both.</i>					

Recent Evidence from the United States that Uses Aggregate Data

One downside of using detailed odometer reading data to understand the consumer response to fuel prices is that odometer reading data are typically only available at an individual state level, while for fuel economy standards we are interested in the response to fuel economy at the national level. While large diverse states like California, Texas, and Pennsylvania likely include a composition of motorists that at least somewhat match the composition nationwide, it is still useful to consider the evidence provided by nationwide studies. This section will focus on studies that use aggregate data, while the next section will focus on studies that use survey micro-data.

The most prominent nationwide studies are a series of studies by Kenneth Small and colleagues that use state-level aggregate data. A major advantage of these studies is that they can include long time series, which can provide evidence on how the consumer response may be changing over time. A disadvantage of these studies is that the data quality is not as high as odometer reading data and it is more difficult to develop empirical strategies for estimating the causal effect of fuel prices or the cost per mile of driving.

The series of studies are all based on a foundation of a set of simultaneous equations that model vehicle holdings, VMT, and the choice of fuel economy. The number of adults per road mile and the fraction of the population served by rail transit are also modeled. The first paper in this series of papers is Small and Van Dender (2007), which estimates a system of three simultaneous equations using state-level data from 1966-2001. Implicitly this estimation approach includes exclusion restrictions, which act as instruments, much in the same way as several of the previous studies instrument. Hymel et al. (2010) extend the framework to add a further equation that accounts for the relationship between VMT and

congestion, allowing VMT to decline as congestion increases and makes driving less appealing. Hymel et al. (2010) also extend the data to 2004. A key finding emerging from these studies is that the effect of the cost per mile on driving appears to become smaller over time along with higher incomes and more congestion.¹³ These studies also find that the effect increases with increasing fuel cost. One legitimate question is whether the decline over time is truly a changing response to improving fuel economy or a changing response to fuel prices. This vein of research is using time series variation in both fuel prices and fuel economy, so the correct answer is that the decline can be attributed to both. This comment encourages the Agencies to recognize and discuss how estimates of the rebound effect may be affected by fuel prices and how relatively long-run average estimates are in most cases more appropriate than using estimates from methodologies that rely on fuel price changes from a small number of years.

Hymel and Small (2015) is the most recent in the series. In this paper, Hymel and Small use the same basic framework of three simultaneous equations, only they include updated data through 2009. The simultaneous equation framework is designed to capture the same effects as in Small and Van Dender (2007). Hymel and Small (2015) confirm the earlier findings that the response to the cost per mile declines with income, but also have a new finding indicating that the response to a change in the cost per mile was greater in magnitude between 2003 and 2009, which was a time period of increasing gasoline prices until a spike in prices in 2008. This latter finding is indicative of the fuel price variation underpinning the estimates of the study and perhaps can be explained by motorists responding more to the higher and more volatile gasoline prices than to changes in the lower and less volatile gasoline prices during the time frame of previous studies.

Just as in the previous papers, Hymel and Small (2015) present a variety of estimates. The approach provides an estimate of a short-run rebound effect that varies slightly but is around 5%. The methodology also uses a lagged dependent variable as an approach to calculate a “dynamic long-run rebound.” This approach is based on the idea that there is a long-run equilibrium for the rebound effect, a path to that equilibrium, and that by using the coefficient on the lagged dependent variable one can get a sense of the speed of adjustment to this equilibrium, and thus a sense of where the equilibrium is. In a sense, this approach adds structural assumptions to make more headway. One of these structural assumptions is that the response by drivers in the economy eventually converges to a steady state and that this steady state can be recovered through the lagged dependent variable. This assumption of course is impossible to verify, but was a common assumption in price elasticity estimations prior to the past decade. Thus, it is important to take the exact value from this long-run calculation with a grain of salt. Hymel and Small find a preferred dynamic long-run rebound effect that ranges from 4% to 18% (depending on the exact specification and exact time period), when evaluated at the average values of income,

¹³ This result is consistent with the result for the price elasticity of gasoline consumption in Hughes et al. (2008).

fuel cost, and urbanization. If fuel costs are lower than they were during much of this period, then the estimate would be closer to zero. Similarly, if incomes are higher and there is more urbanization, then the estimate would be closer to zero. So a correct interpretation of this 4-18% range would take into account these factors and map out the effect over time, which would bring the mean estimate over time closer to zero. This work provides useful evidence, again underscoring that there appears to be a downward shift in the rebound effect over time, but it also highlights that the context matters and that even this estimate relies heavily on the response to fuel price variation, rather than variation in fuel economy—which is what we really want for a regulatory analysis of fuel economy standards. Ken Small’s comment in the docket (docket number EPA-HQ-OAR-2018-0283-2698) provides further evidence on how this work should be considered, as it is only reasonable to provide the author some deference in how his or her own results are interpreted.

Greene (2012) uses national time series data from 1966-2007 to try to replicate some of the findings of Small and Van Dender (2007). Generally, Greene (2012) finds similar estimates of the consumer response as Small and Van Dender, and notably Greene also examines how a measure of the stringency of CAFE standards affects VMT. This measure comes out to be statistically insignificant and was very close to zero in magnitude. Greene also finds that the elasticity of VMT with respect to the cost per mile of driving is approximately -0.08 to -0.12. The data used for this study are so highly aggregated that it is unclear whether the effects seen are causal effect, but this again provides another piece of evidence.

Table 3 summarizes these studies.

Table 3. Evidence on the VMT Response to Fuel Prices Using Aggregate Data for the United States					
Study	Research Design	Setting	Data	Estimate	Further Caveats
Small & Van Dender (2007)	Simultaneous equations	National 1966-2001	Aggregate state-level	5-22%	Data limitations
Hymel et al. (2010)	Simultaneous equations	National 1966-2004	Aggregate state-level	9% (preferred)	Data limitations
Hymel & Small (2015)	Simultaneous equations	National 1966-2009	Aggregate state-level	4-18%	Data limitations
Greene (2012)	Aggregate time series	National 1966-2007	Aggregate US-level	8-12%	Data limitations
<i>Notes: Data limitations refer to data at the aggregate level which makes determining individual-level behavior more difficult.</i>					

Recent Evidence from the United States that Use Survey Data

While there are only a few studies that use odometer reading data or aggregate data, the availability of the National Household Transportation Survey (NHTS) has led to many papers that use this data source to estimate the relationship between the cost per mile of driving and VMT. Many of the papers referenced by the Agencies use this data source, and this is also the data source used in Linn (2016) mentioned above. 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 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.

Bento et al. (2009) is a study published in the *American Economic Review* that used the 2001 NHTS survey data (a single cross-section) to estimate the distributional and efficiency impacts of increased US gasoline taxes. The study used a structural model of vehicle choice, usage, and scrappage. In the process of estimating the model, one of the parameters is the elasticity of VMT with respect to the operating cost of driving. But it is clear that the paper is not focused on estimating the rebound effect—it is just one of many parameters estimated and the paper was aiming to model the holdings of the entire vehicle stock. The estimation approach also does not attempt to instrument for the cost per mile, which is problematic for an estimation of the rebound effect because it means that the coefficient may be biased, but does not necessarily impinge upon the validity of their other results. The study is based on the NHTS survey data, and the use of a single cross section makes the use of VIN fixed effects impossible. The estimated elasticity of VMT with respect to the price of gasoline is -0.34, which would, if taken at face value, imply a rebound effect of 34% for fuel price changes. However, one must be careful in using this for regulatory analysis for future fuel economy standards in 2020-2026, due to the age of the data, the fact that it is focusing on fuel price changes rather than fuel economy changes, the fact that the data are survey data, and the fact that the data include only a single cross-section. Bento et al. (2009) makes an important contribution to the economic literature on other questions but using the -0.34 estimate for the rebound effect of fuel economy standards going forward is inappropriate for all of the reasons given above. See the comment letter in the docket from A. Bento for more details on the appropriate interpretation of this work (docket number NHTSA-2018-0067-5679).

In the unpublished study previously mentioned above, West and Pickrell (2011), use the 2009 NHTS dataset (the same dataset used by Linn 2016) and explore a variety of specifications including those that examine the relationship between VMT and the gasoline price/fuel economy, as well as VMT and the cost per mile of driving. The estimations are

run separately for one-vehicle, two-vehicle, and three-vehicle households. While there appears to be an attempt at instrumenting, the primary specifications do not appear to instrument for the cost per mile of driving. The elasticity of VMT with respect to the cost per mile of driving ranges from -0.006 for one-vehicle households (not statistically significant) to -0.34 for three-vehicle households (if applied directly, this would imply a rebound effect of 1-34%). Unfortunately, there only appears to be a presentation presenting the results of this study, so it is difficult to fully assess it. While there may indeed be some quite interesting results from the West and Pickrell (2011) study, this comment urges the Agencies not to rely on incompletely-documented, unpublished, and unavailable work.

Su (2012) also uses the 2009 NHTS survey data. This study uses a quantile regression approach to examine the relationship between VMT and the fuel cost per mile. No instruments or VIN fixed effects are used in this study either. The results indicate an elasticity of VMT with respect to the fuel cost per mile ranging from -0.11 to -0.19, suggestive of a rebound effect on the order of 11-19%. While an interesting quantile regression study, given the data being used are travel diary data, that fixed effects are not used (implying that key confounders are not controlled for), and that there are no instruments (so the estimates may be biased), this study should be given very little weight in regulatory analysis.

Liu et al. (2014) use the 2009 NHTS—restricted to the Washington, DC metro area—to develop a structural model of vehicle ownership, vehicle choice, and usage decisions. Much like Bento et al. (2009), this paper is attempting to estimate many parameters that influence the fleet. It is an ambitious undertaking that helps us understand how different policies might impact the vehicle fleet. It is not a study intended to estimate a causal effect of the cost per mile of driving on VMT. No instruments or fixed effects are used to help identify the relevant coefficient for the VMT price elasticity. The VMT elasticity with respect to fuel cost is -0.4, which would translate into a 40% rebound effect for fuel price changes. The authors recognize that this estimate is high and aim to explain it by pointing to the fact that the 2009 NHTS was a survey taken during a time when fuel prices were high and volatile, a point that is very important for interpreting this result (see letter from C. Cirillo, in the docket with comment number NHTSA-2018-0067-7819). This comment urges the Agencies to again place little weight on this study for use in the regulatory analysis of fuel economy standards.

Leung (2015) is a dissertation from the UC San Diego Department of Economics, and it includes a chapter on gasoline prices and household fleet utilization. It again uses the 2009 NHTS, but examines the effect of fuel prices on VMT. A main focus of the chapter is on the VMT allocation across a household's vehicle fleet. No instruments are used, but a much more extensive set of controls are used than in nearly all of the other studies using the NHTS. Further, by relying entirely on variation in fuel prices, the concern about selection

confounding the estimate on fuel economy/cost per mile is avoided. Leung (2015) estimates a short-run VMT elasticity with respect to the fuel price of -0.1, suggestive of a rebound effect of 10%. Due to the fact that the study uses fuel price rather than cost per mile, and faces some similar data weaknesses to the previously mentioned studies, this comment suggests not given this study the same weight as the studies that use odometer readings and/or quasi-experimental variation.

One take-away from the discussion of the many papers that use the NHTS self-reported data is that there are remarkably different results even when exactly the same dataset is being used. For example, Linn finds a rebound effect in the range of 20%-40%, while Leung finds a rebound effect of 10%, and both studies use the 2009 NHTS. The differences stem largely from differences in the methodology used in the analyses. This underscores the importance of a comprehensive review of studies' relevance and reliability.

One related study that the Agencies include in their literature review is Wadud et al. (2009). This paper does not estimate a VMT elasticity, but rather estimates a fuel consumption elasticity. It uses used aggregated data at the income quintile level from the Consumer Expenditure Survey (CEX) from 1984 to 2003 in system of simultaneous equations. While there are many numbers reported, the range in gasoline price elasticities is varies from -0.01 to -0.25. As there are additional margins of adjustment in fuel consumption than just driving, this would inherently be an overestimate of the rebound effect and thus is inappropriate for a regulatory analysis of fuel economy standards. Furthermore, it does not provide much actionable guidance.

Table 4 presents the results from these papers that are based on survey data (aside from the unpublished and unavailable West and Pickrell (2011) study).

Table 4. Evidence from Survey Data for the United States					
Study	Research Design	Setting	Data	Estimate	Further Caveats
Leung (2015)	Controls		Self-reported	10%	No IVs or fixed effects
Liu et al. (2014)	Structural model	MD/DC/VA 2009	Self-reported	40%	No IVs or fixed effects
Su (2012)	Controls	National 2009	Self-reported	11-19%	No IVs or fixed effects
Bento et al. (2009)	Structural model	National 2001	Self-reported	34%	No IVs or fixed effects

Notes: West & Pickrell (2011) is already mentioned above. Wadud et al. (2009) estimates a fuel price elasticity and thus is not appropriate for inclusion in this table.

Recent Evidence from Elsewhere in the World

The Agencies have often referenced work from elsewhere in the world. This section briefly discusses some of this work that has been mentioned by the Agencies. However, it is important to again state the caveat that applying estimates from elsewhere is usually an inappropriate exercise. While we may learn something from reviewing well-done studies from elsewhere, differences in public transportation access, income, and even consumer preferences can lead to very different elasticities. For example, Gillingham and Munk-Nielsen (2018) find a medium-run (two-year) elasticity of VMT with respect to the gasoline price for all vehicles Denmark from 1998 to 2011 of -0.3. However, this elasticity was much closer to zero and more in-line with US estimates after excluding a group of highly responsive motorists who live far from work, but have excellent access to public transportation. This group of motorists simply does not exist in the United States. But it likely exists in other European countries, and perhaps in many other countries as well.

Examples of recent studies from Europe include Ajanovic and Haas (2012), Frondel and Vance (2013), Weber and Farsi (2014), De Borger et al. (2016), and Stapleton et al. (2016, 2017). With the exception of De Borger et al. (2016), all of these studies show estimates of the VMT elasticity with respect to either the cost per mile of driving or the fuel price of -0.14 or greater (in absolute value). Some of the estimates are even much higher, such as one of the specifications in Frondel and Vance providing an estimate as elastic as -0.7, which if taken literally would imply a rebound effect as high as 70%. This comment strongly encourages the Agencies exclude such estimates from Europe as part of the regulatory analysis of fuel economy standards in the United States, when there is sufficient evidence from studies in the United States given the differences in public transportation access and other relevant variables between Europe and the United States.

While using European estimates seems misguided, there may be more of an argument for using estimates for Canada because Canada has similar urbanization patterns and vehicle stock as the United States. Barla et al. (2009) estimate a very similar simultaneous equations model to that of Small and Van Dender (2007), only using province-level data for Canada from 1990-2004. Thus, the methodology has all of the same advantages and disadvantages of the Small and Van Dender approach. The short-run estimate for the vehicle-kilometers-traveled (VKT) elasticity with respect to the cost per kilometer is -0.08, suggesting a short-run rebound effect of 8%. Using the same “long-run equilibrium” approach as in Small and Van Dender (2007), the long-run VKT elasticity with respect to the cost per kilometer is -0.2, suggesting a long-run rebound effect of 20%. However, this is subject to the same caveats as discussed above for the Small and Van Dender (2007)

approach. The authors see suggestive evidence of a declining rebound effect over time with greater income, but cannot place much confidence in this finding given the short time frame of the analysis and small dataset. In general, these results can be viewed as further confirming the evidence provided by Small and Van Dender (2007) and the subsequent papers by Ken Small and co-authors.

Final Considerations

As this review has highlighted, there are a wide range of values in the literature that have some relevance for the rebound effect of fuel economy standards. Which leads to the following question: What central estimate should be used for regulatory analysis?

The characteristics of the ideal estimate to use in regulatory analysis is one focused on the effect of fuel economy, based in the United States, using highly accurate data (e.g., odometer readings, rather than self-reported data), using an empirical design that accounts for the change in characteristics as well as selection and other concerns, and covers a sufficiently long and recent time period to be useful for extrapolating into the near future. None of the studies described above are ideal, but there is now a body of evidence that we can draw from.

The following table provides a summary of the evidence from the United States over the past decade (repeated from the executive summary). It brings in several studies that were not included in the NPRM and very briefly summarizes potential concerns about the different studies.

Table 5. Summary of Recent Evidence from the United States				
Study	Data	Rebound Estimate	Concerns	In NRPM?
Bento et al. (2009)	2001 survey	34%	Data limitations	Yes
Hymel et al. (2010)	State-level 1966-2004	9%	Data limitations	Yes
Gillingham (2011)	Odometer; CA 2001-2009	1%	No IV	
Greene (2012)	Aggregate 1966-2007	0%	Data; No IV	
Su (2012)	2009 survey	11-19%	Data; No IV	Yes
Liu et al. (2014)	2009 survey; MD/DC/VA	40%*	Data; No IV	Yes
Gillingham et al. (2015)	Odometer; PA 2000-2010	10%		
Hymel & Small (2015)	State-level 1966-2004	4-18%	Data limitations	Yes

Leung (2015)	2009 survey	10%	Data limitations	
Linn (2016)	2009 survey	20-40%*	Data limitations	Yes
Langer et al. (2017)	Odometer; OH 2009-2013	11%	Selected sample	
West et al. (2017)	Odometer; TX 2010-2011	0%		Yes
Knittel & Sandler (2018)	Odometer; CA 1998-2010	14.7%		
Wenzel & Fujita (2018)	Odometer; TX 2005-2010	7.5-15.9%		
Average over all studies above		14.1%		
Average over studies using odometer readings		8.1%		

*Notes: * refers to studies that the authors themselves suggest we interpret with caution. For studies with a range, the average is taken over the range. The NPRM references a 1-25% range from Wadud (2009), but this study is excluded because it estimates the elasticity of gasoline consumption with respect to fuel prices and thus is not directly comparable to the above studies. The NPRM also referenced a 9-34% range from West and Pickrell (2011), but this does not appear to be a working paper or publication that is publicly accessible. The NPRM references Gillingham (2014), but this study is focused on a gasoline price shock and thus in the author’s own view is inappropriate to use for the rebound effect. A better reference is Gillingham (2011) that attempts to descriptively look at the effect of fuel economy, although without quasi-experimental variation. All studies from Europe referenced in the NPRM are excluded from this table. The NPRM incorrectly references Linn (2016) as Linn (2013). Bento et al. (2009) give the average VMT elasticity with respect to the price of gasoline as -0.34 on p.685; the NPRM reports a range of 21-38%, but it is unclear where this range comes from. The 9% estimate from Hymel et al. (2009) was taken from the authors’ preferred estimate in the conclusion (p.1235) with the calculation of variables at 2004 values, but a variety of other estimates were reported. The 4-18% estimate range from Hymel and Small (2015) is from the authors’ preferred estimates in Table 8; the NPRM chooses only the high estimate. The 7.5%-15.9% range for Wenzel & Fujita (2018) is based a conversation with the authors, who suggest considering both the estimate based on fuel prices and the estimate based on the cost per mile to be consistent with the rest of the literature, which use both.*

The key take-away from this review is that the current base of evidence makes it very difficult to justify a central case estimate of 20% that remains constant over time. To the contrary, the studies using the most robust data available—odometer reading data—suggest that the rebound effect is on the order of 10% and may be even lower. This central estimate is consistent with the Agencies’ previously assumed value of 10% in the 2012 rulemaking and the 2016 Draft Technical Assessment Report.

Figure 1 below shows the same studies in a graphical form and clearly shows that the recent evidence using odometer readings is much closer to 10% than 20%.

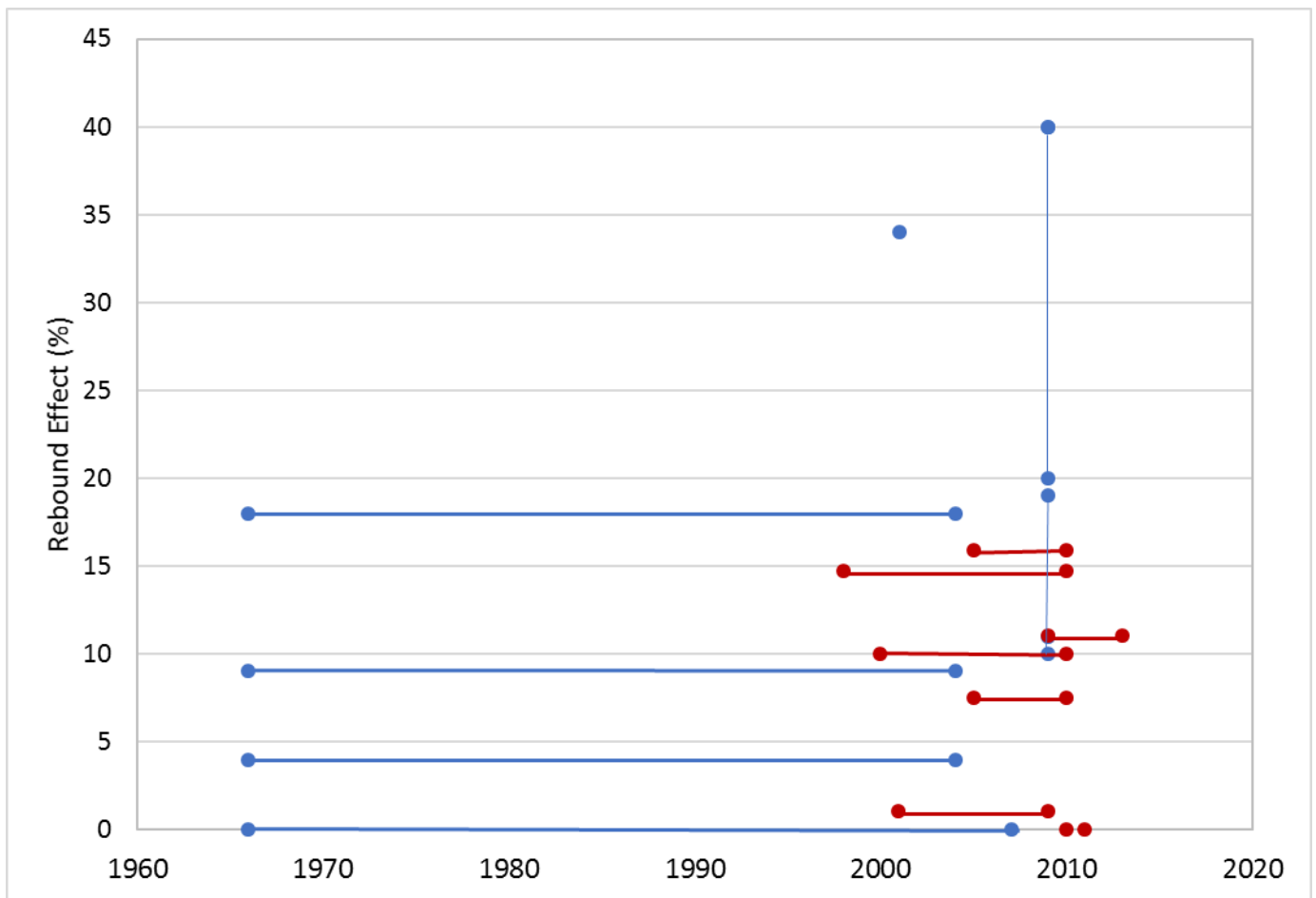


Figure 1. A graphical illustration of rebound effect estimates, with the x-axis indicating the years that the study data covered. Each study is a dot or, when a study covers a range, is a dot and a line. For example, Su (2012) uses the 2009 NHTS data to estimate a rebound range of 11-19%, while Linn (2016) uses the same data to estimate a rebound ranging from 20-40%. The red dots and lines indicate studies using odometer reading data.

This review also brought out some further clear findings:

1. The Agencies are correct to focus on the existing literature on the VMT response for the use in regulatory analysis. While there are other factors that influence the rebound effect, some of these factors imply that the effect is overestimated, while some imply that it is underestimated. These factors are important areas for future work, but the current literature is currently insufficient to provide guidance for regulatory analysis at this time.
2. The most recent studies using odometer readings tend to provide an estimate of the rebound effect that falls in the 0-15% range, with an average of 8.1%, and if studies

using cross-sectional survey data are included, this range widens to 0-20% (with a few notable outliers).

3. There are further factors that also influence the rebound effect. For example, there is evidence from multiple studies suggesting that the rebound effect declines over time with income and congestion. The long-run rebound effect is also greater than the short-run rebound effect. As well, there is evidence that when other attributes of future vehicles change at the same time as fuel economy, the rebound effect may be small. There are also indirect and macroeconomic rebound effects. The net result of all of these factors is difficult to pin down and the evidence base does not currently unambiguously point to whether the net of these factors increases or decreases the rebound effect.

An important finding of this literature review is that the review in the NRPM could be further improved. Table 6 replicates the NPRM Table 11-44 (page 251), providing notes for proper interpretation.

Table 6. Replication of NPRM Table 11-44 With Notes on Proper Interpretation			
Study	Data	Rebound Estimate in NPRM	Notes for Proper Interpretation
Barla et al. (2009)	Canada 1990-2004	8-20%	Non-U.S. Study
Bento (2009)	US 2001 survey	21-38%	Incorrect read of paper; study not intended for rebound; not instrumented
Wadud (2009)	U.S. 1984-2003	1-25%	Not a rebound estimate
West & Pickrell (2011)	U.S. 2009	9-34%	Study unavailable; estimate range appears to be 9-27%
Ajanovic & Haas (2012)	E.U. 1970-2007	44%	Non-U.S. Study
Su (2012)	U.S. 2009	11-19%	
Linn (2013)	U.S. 2009	20-40%	Should be Linn (2016); data concerns
Fronzel & Vance (2013)	Germany 1997-2009	46-70%	Non-U.S. Study
Liu (2014)	U.S. 2009	39-40%	Should be Liu et al. (2014); Should be 40%; not instrumented; data concerns
Gillingham (2014)	U.S. 2009	22-23%	Not appropriate for average rebound effect estimate

Weber & Farsi (2014)	Switzerland 2010	19-81%	Non-U.S. Study
Hymel & Small (2015)	U.S. 2003-2009	18%	Should be 4-18%; see letter from Ken Small
West et al. (2017)	U.S. 2009	0%	Should be West et al. (2017); particularly relevant study
DeBorger (2016)	Denmark 2001-2011	8-10%	Non-U.S. Study; Should be DeBorger et al. (2016)
Stapleton (2016, 2017)	Great Britain 1970-2012	14-30%	Non-U.S. Studies

The differences between Table 5 and Table 6 are striking. Table 6 relies heavily on non-U.S. studies, misses most of the U.S.-based current literature, and appears to improperly reference several studies and describe the findings incorrectly or incompletely. This may be due to a rushed literature review, which is understandable, but should be addressed before the final rule. This comment strongly encourages the agency to update their analysis with both accurate findings from the above studies, and the latest literature from the U.S. to be closer to Table 5.

To summarize, the evidence suggests that there is a wide potential range for the rebound effect, but that most of the recent evidence—and the strongest evidence—lies closer to 10% and in some cases even below 10%.

In a previous comment in the docket from 2016, co-authored with Josh Linn and his colleagues at Resources for the Future, I emphasized the wide range in the literature and the need for a systematic justification for the choice of the rebound effect.¹⁴ I would like to re-emphasize that comment and strongly encourage the Agencies to develop a clearer justification for their choice of a central estimate of the rebound effect that appropriately weights the relevant literature and excludes inappropriate literature, such as work from Europe or work based on a single gasoline price shock. I also strongly support the Agencies performing sensitivity cases that appropriately reflect the range we see in the most relevant recent studies. For example, the above tables suggest using an upper bound case of 20% and a lower bound case somewhere closer to zero (e.g., matching West et al. 2017). But as mentioned already, the bulk of the relevant evidence points to a central case estimate somewhere around 10%, although a reasonable case could be made for slightly above or slightly below 10%.

Finally, there are two further issues. There is a great need for additional research to pin down several of the factors mentioned above that could either increase or reduce the magnitude of the rebound effect. In addition, as discussed above, the NPRM modeling of the

¹⁴ See <http://www.rff.org/files/document/file/RFF-Rpt-MidtermCAFEComments.pdf> for the full comment.

rebound effect, using 2016 as a base, is nonstandard and almost certainly is upwardly biasing the change in VMT between the augural and proposed standards due to the rebound effect. I encourage the Agencies to correct this modeling bias, which should not be very difficult and would allow for a more accurate regulatory impact analysis.

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