Peer Review of December 2013 LDV Rebound Report by Small and Hymel

January 31, 2014

Prepared for
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Acronyms and Abbreviations

Acronym / Abbreviation	Stands For
3SLS	Three-Stage Least Squares
AEO	Annual Energy Outlook
CAFE	Corporate Average Fuel Economy
EPA	U.S. Environmental Protection Agency
FHWA	Federal Highway Administration
GHG	Greenhouse Gas
ICF	ICF International
NHTSA	National Highway Traffic Safety Administration
OTAQ	Office of Transportation and Air Quality
S&H	Small & Hymel
UKERC	United Kingdom Energy Research Center
VMT	Vehicle Miles Traveled
WAM	Work Assignment Manager

1. Introduction

The Office of Transportation and Air Quality (OTAQ) of the U.S. Environmental Protection Agency (EPA) is responsible for developing regulations to reduce the emissions of greenhouse gases (GHG) from light-duty vehicles in the U.S. The regulatory option of encouraging the adoption of advanced technologies for improving vehicle efficiency can result in significant fuel savings and GHG emission benefits. At the same time, it is possible that some of these benefits might be offset by additional driving that is encouraged by the reduced costs of operating more efficient vehicles. This so called “rebound effect”, the increased driving that results from an improvement in the energy efficiency of a vehicle, must be determined in order to reliably estimate the overall benefits of GHG regulations for light-duty vehicles.

Dr. Ken Small, an Economist at the Department of Economics, University of California at Irvine, with contributions by Dr. Kent Hymel, Department of Economics, California State University at Northridge, have developed a methodology to estimate the rebound effect for light-duty vehicles in the U.S. Specifically, rebound is estimated as the change in vehicle miles traveled (VMT) with respect to the change in per mile fuel costs that can occur, for example, when vehicle operating efficiency is improved. The model analyzes aggregate personal motor-vehicle travel within a simultaneous model of aggregate VMT, fleet size, fuel efficiency, and congestion formation. The model uses three-stage least squares (3SLS) in order to account for the endogeneity of explanatory variables. The results contain both short-run and long-run estimates based upon lagged effects within annual data. For VMT, the behavioral responses underlying short run effects could include changes in travel mode, discretionary trips, destinations, or the combining of several trips into a single chain. Long-run responses might include changes in the vehicle stock, job or residential relocations, and changes in land use.

The model is estimated using a cross-sectional, time series data set with each variable measured for 50 U.S. states, plus District of Columbia, annually for years 1966-2009. Variables are constructed from public sources, mainly the U.S. Federal Highway Administration, U.S. Census Bureau, and U.S. Energy Information Administration.

Since the effectiveness of regulatory efforts to reduce GHG emissions is strongly influenced not only by the technical attributes of vehicles, but also by vehicle usage levels, it is important to assure that the methodologies considered by the U.S. EPA for estimating VMT rebound have been thoroughly examined. Comprehensive, objective peer reviews like the one described here are an important part of that examination process.

This report details the peer review of the subject report, *The Rebound Effect from Fuel Efficiency Standards: Measurement and Projection to 2035 (December 24, 2013)*. A number of independent subject matter experts were identified and the process managed to provide reviews and comments on the methodology of the report. This peer review process was carried out under EPA's peer review guidelines[1].

This report is organized as follows:

- Chapter 2 details the selection of the peer reviewers
- Chapter 3 details the peer review process
- Chapter 4 summarizes the reviews
- Appendix A provides resumes and conflict of interest statements for the three selected reviewers
- Appendix B provides the charge letter sent to the selected reviewers
- Appendix C, D and E provide the actual reviews submitted by the three selected reviewers

[1] U.S. Environmental Protection Agency, Peer Review Handbook, 3rd Edition with appendices. Prepared for the U.S. EPA by Members of the Peer Review Advisory Group, for EPA's Science Policy Council, EPA/100/B-06/002. Available at http://www.epa.gov/peerreview

2. Selection of Peer Reviewers

The EPA and ICF WAM compiled a list of 14 reviewers who would be capable of reviewing the subject report. They are listed in Table 2-1.

Table 2-1. Potential Reviewers

Potential Reviewer	Available	Affiliation	Degree
David Greene	Yes	Senior Fellow in the Howard H. Baker, Jr. Center for Public Policy and a Research Professor of Civil and Environmental Engineering, the University of Tennessee	Ph.D., Geography and Environmental Engineering
Lucas Davis	No – too busy	Associate Professor University of California, Berkeley	Ph.D., Economics
Joshua Linn	Yes	Fellow (indefinite appointment), Resources for the Future	Ph.D., Economics
Jonathan Rubin	Yes	Professor, Margaret Chase Smith Policy Center and School of Economics, University of Maine	Ph.D., Agricultural Economics
Sarah West	Yes	Professor, Macalester College, Economics	Ph. D., Economics
James Sallee	Yes	Assistant Professor, Harris School of Public Policy Studies University of Chicago	Ph.D., Economics
Kenneth Gillingham	Yes	Assistant Professor of Economics, School of Forestry & Environmental Studies, Yale	Ph.D., Management Science & Engineering and Economics
Chris Knittel	No response	William Barton Rogers Professor of Energy Economics Massachusetts Institute of Technology Sloan School of Management	Ph.D., University of California, Berkeley
Mark Jacobson	Yes	Associate Professor, University of California	Ph.D., Economics
David Rapson	Yes	Assistant Professor of Economics, UC Davis	Ph.D., Economics
Soren T. Anderson	No – too busy	Assistant Professor Michigan State University Department of Economics	Ph.D., Economics
Hunt Allcott	Yes	Assistant Professor of Economics, New York University	Ph.D., Public Policy

Potential Reviewer	Available	Affiliation	Degree
Steve Sorrell	Yes	Senior Lecturer (SPRU - Science and Technology Policy Research, The Sussex Energy Group	Ph.D. by publication - Analyzing controversies in energy policy: the evidence for rebound effects and global oil depletion,
Todd Litman	Yes	Executive director of the Victoria Transport Policy Institute	Masters of Environmental Studies

The three selected reviewers are listed in Table 2-2. Each had the necessary expertise, were available to review the report in a timely manner and had no conflict of interest. All were agreed upon by the EPA WAM.

Table 2-2. Final Reviewers

Reviewer	Contact Information	Necessary Expertise	Conflict of Interest
Kenneth Gillingham	Yale University School of Forestry & Environmental Studies P: 203-436-5465 kenneth.gillingham@yale.edu	Yes	No
David Greene	University of Tennessee Howard H. Baker, Jr. Center for Public Policy P: (865) 974-3839 dgreen32@utk.edu	Yes	No
James Sallee	University of Chicago The Harris School of Public Policy Studies P: 773-316-3480 sallee@uchicago.edu	Yes	No

Resumes and conflict of interest statements for the three reviewers can be found in Appendix A.

3. Peer Review Process

Once the three reviewers had been decided upon and approved by the EPA WAM, a charge letter and the subject report were sent to each reviewer via secure email. Shortly after distributing the charge letter (see Appendix B) and supporting materials for the peer review, a teleconference was held between the selected peer reviewers, the EPA WAM, EPA-identified relevant project-related staff and ICF staff to clarify any questions the peer reviewers may have regarding the report/written materials. At the conference call, EPA provided technical and/or background information on the particular report under review.

During the review process, no reviewers had questions. Each reviewer provided a written peer review in a timely manner. These were sent to ICF who forwarded them directly to the EPA WAM.

ICF managed the peer review process to ensure that each peer reviewer had sufficient time to complete their review of the data analysis by the deliverable date specified (January 17, 2014). ICF adhered to the provisions of EPA's Peer Review Handbook guidelines to ensure that all segments of the peer review conformed to EPA peer review policy.

4. Summary of Review Comments

In this section, review comments from the three reviewers are summarized. Full comments (including those in addition to the charge questions) can be found in Appendix C for Kenneth Gillingham, Appendix D for David Greene and Appendix E for James Sallee. Responses are summarized below relative to the charge questions.

4.1. Responses to Charge Questions

What are the merits and limitations of the authors' approach for estimating the vehicle miles traveled (VMT) rebound effect for light-duty vehicles? Are key assumptions underpinning the methodology reasonable? The VMT rebound effect is defined here as the change in VMT resulting from an improvement in the light-duty efficiency.

The reviewers highlighted a number of merits to the authors' approach. All three reviewers generally agree that authors' selection of FHWA data to be appropriate for this study. Sallee mentioned that the aggregate data used in the report suffer from measurement problems, but due to data gaps in other sources, the data used for this report may be the best available at this time. Other highlighted merits include the authors' accurate understanding of the direct rebound effect and an understanding of estimation issues, resulting in a robust and accurate estimate of the VMT rebound effect.

All three reviewers believed that the assumptions underpinning the methodology were generally reasonable and consistent with the best methods employed in current research in this area. The reviewers did discuss other factors that could be considered or evaluated in more depth. For example Greene noted that the analysis omits part of the effect of increased vehicle prices on the long-run cost per-mile of travel. An increase in the capital cost of a vehicle also affects the long-run cost of vehicle travel via usage-induced capital depreciation. Sallee noted that the data used provides no way to model the relationship between vehicle age and VMT.

Is the implementation of the authors' methodology appropriate for producing estimates of the VMT rebound effect? Specifically, are the input data and the methodology used to prepare the data appropriate? Are sound econometric procedures used? Does the model appropriately reflect underlying uncertainties associated with the assumptions invoked and the parameters derived in the model?

All three reviewers generally thought the authors' approach was appropriate and was representative of best practices. They noted that the research did suffer from some data limitations that the author and the literature more broadly were aware of. A number of tests for robustness and points for additional clarification were suggested.

Sallee noted that most of the independent variables were not independently measured, but imputed using methodologies that may differ across states and over time. On-road fuel economy may vary over time, even for the same vehicle, due to changes in driving conditions, such as congestion or degree of urbanization. While the existing time series data is the best available, there are significant changes that have occurred over time that affect the interpretation of the results. The authors' have documented most of these issues.

Greene notes that "the estimates presented by S&H are based on the maintained hypothesis of economically rational behavior, in the sense that consumers are assumed to respond to changes in fuel cost per mile in the same way whether caused by changes in fuel price or changes in fuel economy", but that the research also demonstrates that the consumer response to fuel economy is less than the response to changes in fuel price, which are more salient to the consumer.

Gillingham notes that standard time series econometric approaches were not used. The paper does account for first order autocorrelation, but second order autocorrelation was not considered, which could introduce some bias into the standard errors.

The methodology used in this report attempts to account for asymmetric responses to increases vs. decreased in per mile fuel costs (and fuel prices). Does the report's finding of an asymmetric response seem reasonable given the methodology that the author's employed? In particular, do the authors' preferred model specifications (3.21 b and 4.21 b) seem appropriate for capturing driver response to an increase in fuel efficiency?

All three reviewers found the authors' finding of an asymmetric response to be reasonable, and that models 3.21b and 4.21b were well chosen as the preferred models. Gillingham raises the following question: If asymmetries come about because of the differing salience of increases and decreases in gasoline prices, should we expect the same effects to apply for changes in vehicle fuel efficiency?

The report describes a methodology for projecting the VMT rebound effect for light-duty vehicles forward in time. The concept of dynamic rebound is introduced to quantify the rebound effect over the period of a vehicle lifetime, during which time the variables that influence the rebound effect are changing. Is this methodology reasonable and appropriate, given the inherent uncertainty in making projections about how future drivers will respond to a change in the fuel efficiency of their vehicles?

All three reviewers agree that the dynamic rebound effect should be used to quantify the rebound effect over the period of a vehicle lifetime. Gillingham suggests that a nonlinear extrapolation (that is asymptotic with 0) may be more appropriate when extrapolating out as far as 2030. Greene and Sallee agree with Gillingham that the rebound effect should not go to 0 and suggest truncating at a value above 0. Sallee notes that it would be instructive to have the authors compare the dynamic rebound forecast to a forecast that assumes a constant rebound over time.

Refer to Appendix C, D, and E for further details on the all the reviewers' comments.

Appendix A. Resumes and Conflict of Interest Statements

Kenneth Gillingham

Contact Information

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School of Forestry & Environmental Studies
195 Prospect Street
New Haven, CT 06511, USA
phone: (203) 436-5465 *fax:* (203) 436-9135
E-mail: kenneth.gillingham@yale.edu
WWW: www.yale.edu/gillingham

Research Interests

Environmental & Energy Economics, Industrial Organization, Public Economics, Econometrics, Technological Change, Transportation Economics, Energy & Climate Policy Modeling.

Current Position

Yale University, New Haven, CT USA
Assistant Professor of Economics, School of Forestry & Environmental Studies July 2011-present
Secondary appointment, Department of Economics May 2012-present
Secondary appointment, School of Management June 2013-present

Education

Stanford University, Stanford, CA USA

Ph.D., Management Science & Engineering and Economics, 2011

Dissertation: "The Consumer Response to Gasoline Price Changes: Empirical Evidence and Policy Implications"
Committee: Jim Sweeney, Larry Goulder, Matt Harding, John Weyant, Jon Levin (orals chair)
Fields: Public & Environmental Economics, Industrial Organization, Econometrics

M.S., Statistics, 2010

M.S., Management Science & Engineering (Economics & Finance), 2006

Dartmouth College, Hanover, NH USA

A.B., Economics and Environmental Studies (minor in Earth Sciences), 2002

Previous Employment

California Air Resources Board, Sacramento, CA USA
Economist (Graduate Student Assistant) 2011

Stanford University, Stanford, CA USA
Research Assistant for Prof. Matt Harding, Stanford Economics Department 2008-2010
Research Assistant for Prof. John Weyant, Stanford Energy Modeling Forum 2008
Research Assistant for Prof. Jim Sweeney, Precourt Energy Efficiency Center 2004-2006

Fulbright New Zealand, University of Auckland, Auckland, New Zealand
Fulbright Fellow 2007

White House Council of Economic Advisers, Washington, DC USA
Fellow for Energy and Environment 2005

Resources for the Future, Washington, DC USA
Research Assistant 2002-2004

Dartmouth College, Hanover, NH USA
Research Assistant for Prof. Karen Fisher-Vanden 1998-2002

Working Papers

Gillingham, K. Selection on Anticipated Driving and the Consumer Response to Changing Gasoline Prices (previously titled: How Do Consumers Respond to Gasoline Price Shocks? Heterogeneity in Vehicle Choice and Driving Behavior)

Gillingham, K., M. Kotchen, D. Rapson, G. Wagner, The Rebound Effect and Energy Efficiency Policy, In preparation for *Review of Environmental Economics & Policy*

Gillingham, K. The Economics of Fuel Economy Standards versus Feebates

Work-in-Progress

Learning-by-Doing in the Solar Photovoltaic Industry (with Bryan Bollinger)

Automaker Responses to Fuel Economy Standards (with Antonio Bento, Kevin Roth, Yiwei Wang)

The Economic Efficiency of Renewable Portfolio Standards in the Presence of Cap-and-Trade (with Arthur van Benthem)

Consumer Welfare and Environmental Effects of Registration Fees and Driving Fees in Denmark (with Bertel Schjerning, Fedor Iskhakov, John Rust, and Anders Munk-Nielsen)

HOV Stickers and the Consumer Adoption of Hybrids: Evidence from California (with Calanit Kamala)

A Dynamic Model of Household Vehicle Choice and Usage (with David Rapson)

Salience and Upstream versus Downstream Cap-and-Trade

The Geographic and Demographic Distributional Effects of Gasoline Taxes

Uncertainty in Integrated Assessment Models of Climate Change Policy (with Bill Nordhaus)

Publications

Gillingham, K. and K. Palmer (2014) Bridging the Energy Efficiency Gap: Policy Insights from Economic Theory and Empirical Analysis. *Review of Environmental Economics & Policy*, forthcoming.

Gillingham, K. (2013) Identifying the Elasticity of Driving: Evidence from a Gasoline Price Shock in California, *Regional Science & Urban Economics*, forthcoming.

Yeh, S., G. Mishra, G. Morrison, J. Teter, R. Quiceno, and K. Gillingham (2013) Long-Term Shifts in Lifecycle Energy Efficiency and Carbon Intensity. *Environmental Science & Technology*, 47(6): 2494-2501.

Gillingham, K., M. Kotchen, D. Rapson, G. Wagner (2013) The Rebound Effect is Over-played. *Nature*, 493: 475-476.

Bollinger, B. and K. Gillingham (2012) Peer Effects in the Diffusion of Solar Photovoltaic Panels. *Marketing Science*, 31(6): 900-912.

Gillingham, K., M. Harding, and D. Rapson (2012) Split Incentives in Household Energy Consumption. *Energy Journal*, 33(2): 37-62.

Gillingham, K. and J. Sweeney (2012) Barriers to Implementing Low Carbon Technologies. *Climate Change Economics*, 3(4), 1-25.

Gillingham, K. and J. Sweeney (2010) Market Failure and the Structure of Externalities. In: Harnessing Renewable Energy, Boaz Moselle, Jorge Padilla, Richard Schmalensee (eds). RFF Press.

Leaver, J. and K. Gillingham (2010) Economic Impact of the Integration of Alternative Vehicle Technologies into the New Zealand Vehicle Fleet. *Journal of Cleaner Production*, 18: 908-916.

Gillingham, K., R. Newell, and K. Palmer (2009) Energy Efficiency Economics and Policy. *Annual*

Review of Resource Economics, 1: 597-619. Reprinted in Italian in Energia (2010).

Gillingham, K. (2009) Economic Efficiency of Solar Hot Water Policy in New Zealand. *Energy Policy*, 37(9): 3336-3347.

Leaver, J., L. Leaver, and K. Gillingham (2009) Assessment of Primary Impacts of a Hydrogen Economy in New Zealand using UNISYD. *International Journal of Hydrogen Energy*, 34(7): 2855-2865.

Gillingham, K., R. Newell, and W. Pizer (2008) Modeling Endogenous Technological Change for Climate Policy Analysis. *Energy Economics*, 30(6): 2734-2753.

van Benthem, A., K. Gillingham, and J. Sweeney (2008) Learning-by-Doing and the Optimal Solar Policy in California. *Energy Journal*, 29(3): 131-151.

Gillingham, K., S. Smith, and R. Sands (2008) Impact of Bioenergy Crops in a Carbon Constrained World: An Application of the MiniCAM Linked Energy-Agriculture and Land Use Model. *Mitigation and Adaptation Strategies for Global Change*, 13(7): 675-701.

Safirova, E., K. Gillingham, and S. Houde (2007) Measuring Marginal Congestion Costs of Urban Transportation: Do Networks Matter? *Transportation Research A*, 41(8): 734-749.

Gillingham, K., R. Newell, and K. Palmer (2006) Energy Efficiency Policies: A Retrospective Examination. *Annual Review of Environment and Resources*, 31: 193-237.

Shih, J-S, W. Harrington, W. Pizer, and K. Gillingham (2006) Economies of Scale in Community Water Systems. *Journal of American Water Works Association*, 98(9): 100-108.

Safirova, E., P. Nelson, W. Harrington, K. Gillingham, and A. Lipman (2005) Choosing Congestion Pricing Policy: Cordon Tolls vs. Link-Based Tolls. *Transportation Research Record*, 1932: 169-177.

Safirova, E., I. Parry, P. Nelson, W. Harrington, K. Gillingham, D. Mason (2004) Welfare and Distributional Effects of HOT Lanes and Other Road Pricing Policies in Metropolitan Washington, DC. *Research in Transportation Economics*, 9(1): 179-206.

Reports & Other Publications

Bollinger, B. and K. Gillingham (2012) Do Peer Effects Matter? Assessing the Impact of Causal Social Influence on Solar PV Adoption, *Photovoltaics International*, 17: 160-165.

Friedland, A. and K. Gillingham (2010) Carbon Accounting is a Tricky Business. Letter to the Editor, *Science*, 327(5964): 411-412.

Sweeney, J., J. Weyant, K. Gillingham, et al. (2008) Analysis of Measures to Meet the Requirements of California's Assembly Bill 32. Precourt Institute for Energy Efficiency Working Paper.

Gillingham, K. (2007) Hydrogen Internal Combustion Engine Vehicles: A Prudent Intermediate Step or a Step in the Wrong Direction? Stanford Global Climate and Energy Project Working Paper.

Gillingham, K., R. Newell, and K. Palmer (2006) The Effectiveness and Cost of Energy Efficiency Programs. In: The RFF Reader in Environmental and Resource Policy, Wallace Oates (ed). RFF Press. 193-201.

Safirova, E., W. Harrington, P. Nelson, and K. Gillingham (2004) Are HOT Lanes a Hot Deal? Analyzing the Potential of HOV to HOT Lanes Conversion in Northern Virginia. RFF Issue Brief 03-03.

Nelson, P., E. Safirova, and K. Gillingham (2003) Revving up the Tax Engine: Gas Taxes and the DC Metro Area's Transportation Dilemma. RFF Issue Brief 03-05.

Grants

"The Influence of Novel Behavioral Strategies in Promoting the Diffusion of Solar Energy," US Department of Energy, PI, 2013-2015 ($1,899,978)

"Density, Walkability, and VMT," Yale Center for Business and the Environment Sobotka Research Fund, PI, 2013-2014 ($10,100)

"Deep Dive Solar Cost Analysis," Lawrence Berkeley National Laboratory/US Department of Energy, PI, 2013-2015 (Yale budget: $74,924)

"Modeling Household and Transportation Vehicle Choice and Usage," California Air Resources Board, co-PI with Dave Rapson, Chris Knittel, and Pat Mokhtarian, 2012-2014 ($300,000)

"Sunrise New England," US Department of Energy, co-PI with Stuart DeCew, 2012-2014 (Yale budget: $215,000)

"The Consumer Response to Gasoline Price Changes," Stanford Institute for Economic Policy Research (SIEPR) Grant, 2010 ($10,000)

"The Consumer Response to Gasoline Price Changes," Shultz Graduate Student Fellowship in Economic Policy, SIEPR, 2010 ($4,000)

"Economics of New Zealand Solar Distributed Generation," Fulbright Fellowship, 2007

"The Effect of Income and Congestion on the Rebound Effect of CAFE Standards," US Environmental Protection Agency STAR Fellowship, 2006-2009 ($111,000)

Heitz Graduate Fellowship, Stanford University, 2006

Battelle Memorial Institute Fellowship, 2001 ($6,000)

Honors and Awards

Full Member, Sigma Xi	2011
Dennis O'Brian Best Student Paper Award, US Association for Energy Economics	2010
Thesis and Research Essay Publication Scholarship, University of Auckland	2008
Outstanding Teaching Assistant Award, Stanford Economics	2006
National Science Foundation Graduate Fellowship, Honorable Mention	2006
American Water Works Association Best Paper Award	2006
Departmental Honors, Dartmouth Economics	2002
Departmental High Honors, Dartmouth Environmental Studies	2002
Associate Member, Sigma Xi	2002
First Prize, Dartmouth Sigma Xi Senior Thesis Competition	2002

Teaching

Yale University

2012-2013: Ph.D. Environmental and Energy Economics, Energy Economics and Policy Analysis (Masters), Yale Environmental Economics Seminar.

2011-2012: Economics of the Environment (Masters), Yale Environmental Economics Seminar.

Stanford University, Teaching Assistant

2009-2010: Ph.D. Microeconomics, Introductory Econometrics, Natural Resource Economics (Graduate).

2008-2009: Transportation Policy (Graduate), Energy Policy Analysis (Graduate), Natural Resource Economics (Graduate).

2007-2008: Energy & Environmental Policy Analysis (Graduate), Climate Policy Analysis (Graduate), Natural Resource Economics (Graduate).

2005-2006: Principles of Economics

Advising

Ph.D. Primary Advisor

Hao Deng (FES 2nd year), Jesse Burkhardt (FES 3rd year; co-advised with Matthew Kotchen)

Ph.D. Committee Member

Laura Bakkensen (FES 5th year), Peter Christensen (FES 5th year), Nathan Chan (FES 5th year), Rich Langford (Yale econ 5th year), Alan Jenn (Carnegie-Mellon 4rd year), Anders Munk-Nielsen (Copenhagen 3rd year), Nikki Springer (FES 3rd year)

M.E.Sc Advisor

Hilary Staver (2nd year), Paige Weber (2nd year)

Masters Independent Research

2012-2013: Vijeta Jangra (MEM '13)
2011-2012: Howard Chang (MEM/MBA '12), Dustin Schinn (MEM '13), Peter Baum (MEM '13)

Undergraduate Senior Thesis Advising

Ana Grajales (economics '13), Daniel Cheng (math-econ '13)

Presentations

2013 (scheduled): AEA Meetings (discussant); Modeling Uncertainty Project Meeting (New Haven, CT); FES/SOM Yale Environmental Economics Seminar; Carnegie-Mellon University; Villanova Law; Arizona State University Economics of Water Conference (Keynote); International Industrial Organization Conference (Cambridge, MA); AERE Summer Conference (Banff, AB); Empirical Methods in Energy Economics Workshop (Carlton University, Ottawa); DOE Sunshot Kick-off Meeting; EMF Workshop on Climate Change Impacts and Integrated Assessment (Smowmass, CO); Stanford Institute for Theoretical Economics (SITE) Advances in Environmental Economics Workshop; Columbia University SIPA; Indiana University Kelley School of Business; Tsinghua University Institute of Energy, Environment, and Economy; Fudan University Economics; Indiana University SPEA; Behavior, Energy, and Climate Change (BECC) Conference.

2012: AEA Meetings; Yale FES Seminar Series; Lawrence Berkeley National Laboratory/DOE Sunshot Initiative Workshop; Triangle Resource & Environmental Economics seminar (Duke, NCSU, UNC); Indiana University Kelley School of Business/Economics/SPEA; University of Massachusetts Amherst Resource Economics; Rice University Economics; Texas A&M Economics; UC Santa Cruz Economics; Naval Postgraduate School Economics; UC Santa Barbara Economics/Bren School; ETH Zurich Economics; University of Lugano Economics; AERE Summer Conference (Asheville, NC); EMF Workshop on Climate Change Impacts and Integrated Assessment (Smowmass, CO); Connecticut Clean Energy Finance & Investment Authority; UC Berkeley/Lincoln Institute of Land Policy Conference; University of Colorado Boulder Economics; University of Wyoming Economics; US Association for Energy Economics (Austin, TX); University of Connecticut ARE; University of Copenhagen Economics.

2011: University of Maryland AREc; Indiana University SPEA; UC Davis Economics; University of Arizona Economics; Arizona State University Economics; University of Illinois Urbana-Champaign Finance; University of Notre Dame Economics; Yale University FES; Informing Green Markets Conference (University of Michigan); Cowles Foundation Structural Microeconometrics Conference (Yale University); Empirical Methods in Energy Economics Workshop (Southern Methodist University); Harvard Seminar on Environmental Economics and Policy; Re-examining the Rebound Effect in Energy Efficiency Workshop (Environmental Defense Fund); RFF-Stanford Workshop on the Next Round of Climate Economics and Policy Research (Washington, DC); US Association for Energy Economics (Washington, DC); Workshop on Environmental and Transportation Policies to Mitigate Climate Change (New York University); Religare Capital Markets (Singapore); Behavior, Economics, and Energy Panel (National University of Singapore Energy Studies Institute); University of Copenhagen Economics.

2010: UC Berkeley Energy Institute; NBER EEE Summer Institute; World Congress Env & Resource Economists (Montreal); US Association for Energy Economics (Calgary); Behavior, Energy & Climate Change (BECC) Conference; 12th Occasional California Workshop on Environmental and Resource Economics; UC Davis ARE; Resources for the Future.

2009: UC Berkeley ARE; Stanford IO Workshop; DOE EIA Advisory Council; US Association for Energy Economics (San Francisco); UC Energy Institute CSEM Conference.

2008: UC Davis ITS; Victoria University, New Zealand.

2007: University of Auckland Energy Centre, New Zealand; Massey University, New Zealand; International Association for Energy Economics (Wellington, New Zealand).

2006: Dartmouth College Workshop on Technological Change & Environment.

2004: Transportation Research Board Annual Meeting.

REFEREE SERVICE

Economics Journals: *American Economic Journal–Applied, American Economic Review, American Journal of Agricultural Economics, B.E. Journal of Economic Analysis & Policy, Climate Change Economics, Economics of Energy & Environmental Policy, Ecological Economics, Energy Economics, Energy Journal, Journal of the Association of Environmental and Resource Economists, Journal of Economic Surveys, Journal of Environmental Economics & Management, Journal of Institutional & Theoretical Economics, Journal of Public Economics, Management Science, Oxford Economic Papers, Quarterly Journal of Economics, RAND Journal of Economics, Regional Science & Urban Economics, Resource & Energy Economics, Review of Environmental Economics & Policy, Scandinavian Journal of Economics, Southern Economic Journal, The Manchester School.*

Environment/Engineering/Policy/Science Journals: *Building Research, Cityscape, Climatic Change, Energies, Energy, Energy & Fuels, Energy Efficiency, Energy Policy, Environment Development and Sustainability, Environmental Modeling & Assessment, Environmental Research Letters, Environmental Science & Technology, Global Environmental Change, International Journal of Sustainable Transportation, Journal of Cleaner Production, Journal of Environment & Development, Journal of Industrial Ecology, Journal of Policy Analysis & Management, Journal of Sustainable Forestry, Mitigation & Adaptation Strategies for Global Change, Science, Transportation Research A, Utilities Policy.*

REVIEW SERVICE

Alfred P. Sloan Foundation, Alliance for Research on Corporate Sustainability, KU Leuven, MIT Press, National Academy of Sciences Transportation Research Board, National Science Foundation, Swiss National Science Foundation.

PROFESSIONAL SERVICE

US Department of Agriculture NIFA Bioenergy Policy expert review panel (Apr 19-20, 2012), Yale Climate & Energy Institute (YCEI) Steering Committee (2013-present), co-organizer of Modeling Uncertainty in Climate Policy Workshop (Feb 4, 2013 at Yale), US Department of Energy Review Panel (April 26, 2013), co-organizer of Northeast Workshop on Energy Policy & Environmental Economics (May 10-11, 2013 at Cornell), US Department of Agriculture NIFA Climate Change Adaptation expert review panel (Aug 1-2, 2013)

PROFESSIONAL AFFILIATIONS

American Economic Association (AEA), Association of Environmental and Resource Economists (AERE), United States Association for Energy Economics (USAEE), Econometric Society, Industrial Organization Society.

Research Cited in the Media

Wall Street Journal *The Numbers Guy* Blog: "The Rebound Effect," May 26, 2009.
Grist: "Making Buildings More Efficient: Looking Beyond Price," Oct 23, 2009.
Grist: "Solar Power is Contagious," Apr 5, 2011.
Energy Matters: "Australia's Home Solar Power Revolution and the Viral Effect," Apr 6, 2012.
Wired: "Solar Panels are Contagious," Apr 12, 2011.
The David Sirota Show AM 760: "Solar Power is Contagious," Apr 25, 2011.
Connecticut Public Radio (WNPR): "Where we Live: Future of Natural Gas" Aug 8, 2011.
Yale Daily News: "City Wins Transportation Grant," Oct 20, 2011.
The Straits Times (Singapore): "To Save the Earth, Know Human Nature," Nov 20, 2011.
Business Times: "Cutting Green Path Via Behavioural Economics," Nov 21, 2011.
Washington Post: "Solar Power is Contagious – But Not Quite Virulent," Dec 5, 2011.
Forbes: "Keeping Up With the Greens: Neighborhood Solar is Contagious," Dec 9, 2011.
Yale Daily News: "Nuclear's Back with New Clarity," Feb 10, 2012.
CO_2 Scorecard: "Non-Conundrum of the Prius Fallacy," Mar 26, 2012.
Climate Progress Blog by Joe Romm: "Debunking the Fallacy of the Prius Rebound Effect," Mar 26, 2012.
CleanTechnica Blog: "Prius Rebound Effect Wrong," Mar 28, 2012.
Wall Street Journal *SmartMoney*: "For Appliances, Does Energy Efficiency Sell?" Oct 16, 2012.
CleanTechnica.com: "If Your Neighbor Has Solar Panels, You're More Likely to Go Solar," Oct 18, 2012.
Wired UK: "Enthusiasm for Solar Panels is Contagious," Oct 19, 2012.
Albuquerque Express: "Use of Solar Panels Popularized by Example," Oct 19, 2012.
Alternative Energy Blog: "Solar Power Tends to Go Viral, New Report Suggests," Oct 19, 2012.
India Talkies: "Use of Solar Panels Popularised by Example," Oct 19, 2012.
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Solar Industry Magazine: "New Study Shows Solar Installations Are Contagious in Neighborhoods," Oct 19, 2012.
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Yale Daily News: "Sandy Link to Climate Change Questioned," Nov 9, 2012.
AOL Energy: "The Psychology of Small-Scale Solar," Nov 19, 2012.
New York Times: "Solar Industry Borrows a Page, and a Party, from Tupperware," Dec 1, 2012.
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Huffington Post: "Nature: The Rebound Effect is Overplayed," Jan 24, 2013.
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Washington Square News: "Stern, Yale Professors Team Up To Research Solar Energy," October 1, 2013.

ICF INTERNATIONAL

ORGANIZATIONAL CONFLICT OF INTEREST CERTIFICATE

Customer: U.S. Environmental Protection Agency

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

Prime Contract: EP-C-12-011

Subcontract/Peer Reviewer: Kenneth Gillingham

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

_X_ No actual or potential conflict of interest exists.

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

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

Kenneth Gillingham

Subcontractor/Consultant

1/17/2014

Date

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PERSONAL

Born: November 18, 1949, New York, New York
Married, two children

EDUCATION

THE JOHNS HOPKINS UNIVERSITY
Ph.D., Geography and Environmental Engineering, 1973–78

UNIVERSITY OF OREGON
M.A., 1972–73

COLUMBIA UNIVERSITY
B.A., 1967–71

EMPLOYMENT

UNIVERSITY OF TENNESSEE, KNOXVILLE — 2010–PRESENT

1/2010–Present	*Senior Fellow, Howard H. Baker, Jr. Center for Public Policy*
10/2013-Present	*Research Professor, Department of Civil and Environmental Engineering*
1/2010-10/2013	*Research Professor, Department of Economics*

INSTITUTE FOR TRANSPORTATION STUDIES, UNIVERSITY OF CALIFORNIA, DAVIS — 2008–2009

9/2008–6/2009	*Visiting Research Faculty*

OAK RIDGE NATIONAL LABORATORY (ORNL) — 1977–PRESENT

1999–Present	*Corporate Fellow, Oak Ridge National Laboratory*
1989–1999	*Senior Research Staff Member II and Manager of Energy Policy Research Programs, Center for Transportation Analysis*
1988–1989	*Senior Research Analyst, Office of Policy Integration, U.S. Department of Energy (On assignment from ORNL)*
1987–1988	*Head, Transportation Research Section*
1984–1987	*Senior Research Staff Member I*
1982–1984	*Research Staff Member*
1980–1982	*Leader, Transportation Energy Group*
1977–1980	*Research Associate*

AWARDS AND HONORS

Distinguished Career Service Award, U.S. Department of Energy, Energy Efficiency and Renewable Energy, 2013
2012 Roy W. Crum Award for Distinguished Achievement, Transportation Research Board of the National Research Council
2011 DOE Vehicle Technologies Program R&D Award, U.S. Department of Energy
2011 Edward L. Ullman Award, Association of American Geographers
2009 Alliance to Save Energy, Energy Efficiency Hall of Fame
2008 Science Communicator Award, UT-Battelle

Recognition by the Intergovernmental Panel on Climate Change for Contributions to the Award of the 2008 Nobel Peace Prize to the IPCC
2007 Department of Energy Hydrogen Program R&D Award (with P.N. Leiby)
Barry D. McNutt Award for Excellence in Automotive Policy Analysis, Society of Automotive Engineers, 2007
Member Emeritus, Transportation Research Board Committee on Alternative Fuels, 2006
Barry D. McNutt Award for best paper of 2004, Energy Committee, Transportation Research Board
Lifetime National Associate of the National Academies, 2002
UT-Battelle Award for Excellence in Science and Technology, 2001
Oak Ridge National Laboratory Significant Event Award, 2001
Corporate Fellow of Oak Ridge National Laboratory, 1999
Outstanding Paper of 1999, *The Energy Journal*, International Association for Energy Economics
Lockheed-Martin Significant Event Award, 1999
Member Emeritus, Transportation Research Board Committee on Transportation Energy, 1998
Lockheed-Martin Significant Event Award, 1996
Distinguished Service Certificate, Transportation Research Board, 1993
ORNL Special Achievement Award, 1991
Distinguished Service Certificate, Transportation Research Board, 1989
Energy Specialty Group Paper Award, Association of American Geographers, 1986
ORNL Special Recognition Award, Oak Ridge National Laboratory, 1986
Technical Achievement Award, Martin Marietta Energy Systems, 1985
Pyke Johnson Award, Transportation Research Board, 1984

PROFESSIONAL ACTIVITIES

- Board of Directors, American Council for an Energy Efficiency Economy, 2010-2013
- Board of Advisors, Institute for Transportation Studies, University of California, Davis
- Editorial Advisory Board, *Transportation Research Part D*, 1996–present
- Editorial Board Member, *Energy Policy*, 2001–present
- Editorial Board Member, *Journal of Transportation and Statistics*, 2001–2006, 2011-present
- Editorial Board Member, *Transportation Quarterly*, 1999–2005
- Editor-in-Chief, *Journal of Transportation and Statistics*, 1997–2000
- Editorial Board Member, Macmillan Encyclopedia of Energy, 1998–2001
- Editorial Advisory Board, *Transportation Research A*, 1986–1997
- National Research Council

    Transportation Research Board Standing Committees:

    Committee on Transportation and Sustainability, Member, 2006–present
    Committee on Energy, A1F01, Chairman 1983–1986, 1986–1990; Member, 1993–1998; Member Emeritus, 1999–present
    Subcommittee on Forecasting Transportation Energy Demand, A1F01(2), Chairman, 1982–1983
    Section F, Energy and Environmental Concerns, Chairman, 1990–1992
    Committee on Alternative Fuels, A1F05, Member, 1993–2006, Member Emeritus, 2006–present
    Task Force on Freight Transportation Data, A1B51, Secretary, 1989–1996
    Committee on Transportation Information Systems and Data Requirements, Member, 1983–1986, 1986–1989

    Ad Hoc Committees:

    Committee on Assessment of Technologies for Improving Fuel Economy of Light-Duty Vehicles – Phase 2, 2012-2015
    Committee for Research Perspectives on Sustainable Energy and Transportation: A Conference, 2012-2013
    Special Task Force on Climate Change and Energy, 4/15/2012-4/14/2015
    Committee on Transitions to Alternative Vehicles and Fuels, 2011-2012
    Special Task Force on Energy and Climate Change, 2008–2009
    Committee on the Assessment of Fuel Economy Technologies for Light-Duty Vehicles, 2007–2010
    Planning Group for Workshop on Issues Related to Peaking of Global Oil Production, 2005
    Committee on State Practices in Setting Mobile Source Emissions Standards, 2004–2006

Chair, Committee for the Symposium on Introducing Sustainability into Surface Transportation Planning, 2003–2004
Panel on Combating Global Warming through Sustainable Surface Transportation Policy, TCRP Project Panel H-21A, 2002–2005
Committee on Effectiveness and Impacts of Corporate Average Fuel Economy (CAFE) Standards, 2001
Committee for the Study of the Impacts of Highway Capacity Improvements on Air Quality and Energy Consumption, 1993–1994
Committee on Fuel Economy of Automobiles and Light Trucks, Energy Engineering Board, Commission on Engineering and Technical Systems, 1991–1992
Committee for the Study of High-Speed Surface Transportation in the United States, 1990
Planning Group on Strategic Issues in Domestic Freight Transportation, 1990
Steering Committee for Conference on Transportation, Urban Form, and the Environment, 1990
National Cooperative Highway Research Program, Panel on "Evaluating Alternative Methods of Highway Finance," 1991–1992

- Intergovernmental Panel on Climate Change
  - Lead Author, Working Group III, Fourth Assessment Report, 2007
  - Lead Author, Working Group III, Third Assessment, 2001
  - Lead Author, Working Group III, Aviation and the Global Atmosphere, 1999
  - Principal Lead Author, Working Group II, Second Assessment Report, 1995
- Association of American Geographers
  - Board of Directors, Transportation Specialty Group, 1989–1991
  - Secretary-Treasurer, Transportation Geography Specialty Group, 1980–1982
  - Editor, *Transportation Geography Newsletter*, 1980–1982
- Society of Automotive Engineers, member, 1985–present
- International Association for Energy Economics, member
- Consulting
  - MacroSys for U.S. Bureau of Transportation Statistics, 2013
  - Rand Corporation, 2012-2013
  - International Council for Clean Transportation, 2011-present
  - International Transport Forum, 2007
  - Addx Corporation, 2007
  - United Nations Framework Convention on Climate Change, 2007
  - Securing America's Future Energy, 2007
  - Center for Clean Air Policy, 2007
  - Pollution Probe Canada, 2006-2007
  - The Energy Foundation China Project, 2005-2011
  - The Pew Center on Global Climate Change, 2004-2012
  - Eno Transportation Foundation, 1991–1996
  - Transportation Research Board, 1996–1997

BOOKS

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ARTICLES IN PROFESSIONAL JOURNALS

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G. Upreti, D.L. Greene, K.G. Duleep and R. Sawhney, “Fuel cells for non-automotive uses: Status and prospects”, *International Journal of Hydrogen Energy*, volume 37, issue 8, pp. 6339-6348, 2012.

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Carmen Difiglio, K.G. Duleep and D.L. Greene, "Cost Effectiveness of Future Fuel Economy Improvements," *The Energy Journal*, vol. 11, no. 1, 1990.

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"Fuel Choice for Dual-Fuel Vehicles: An Analysis of the Canadian Natural Gas Vehicles Survey," *SAE Technical Paper Series 892067*, Society of Automotive Engineers, Warrendale, Pennsylvania, 1989.

J.J. Erickson, D.L. Greene and A.J. Sabadell, "An Analysis of Transportation Energy Conservation Projects in Developing Countries," *Transportation*, vol. 15, no. 3, pp. 163–189, 1988.

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"Advances in Automobile Technology and the Market for Fuel Efficiency, 1978–1985," *Transportation Research Record 1155*, pp. 18–27, Transportation Research Board, National Research Council, Washington, DC, 1987.

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C. Liu, D.L. Greene and D.S. Bunch, "Fuel Economy and CO_2 Emissions Standards, Manufacturer Pricing Strategies and Feebates", *The Energy Journal*, accepted for publication.

“Energy and Environment”, Ch. 8 in *Transportation Statistics Annual Report 2013*, Bureau of Transportation Statistics, U.S. Department of Transportation, Washington, D.C.

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ORGANIZATIONAL CONFLICT OF INTEREST CERTIFICATE

Customer: U.S. Environmental Protection Agency

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

Prime Contract: EP-C-12-011

Subcontract/Peer Reviewer: David Greene

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

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David L. Greene

Subcontractor/Consultant

1/17/2014

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Public Economics and Energy and Environmental Economics Programs (April 2010 – present)
Visiting Researcher, University of California Energy Institute (August 2010 – December 2010)

EDUCATION

University of Michigan, Ph.D. in Economics (2008)
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Committee: Joel Slemrod (Chair), Rebecca Blank, James Hines, Jeffrey Smith
University of Michigan, M.A. in Economics (2005)
Macalester College, B.A. in Economics and Political Science, *Summa Cum Laude*, ΦBK (2001)

PUBLISHED JOURNAL ARTICLES

"What Do Consumers Believe About Future Gasoline Prices? (with Soren T. Anderson and Ryan Kellogg) *Journal of Environmental Economics and Management* Forthcoming.

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

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

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

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

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

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

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

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

NON-REFEREED PUBLICATIONS

“The Energy Paradox and the Future of Fuel Economy Regulation”, working paper, Institute for Policy Integrity at New York University School of Law, December 2011

“Forecasting Gasoline Prices Using Consumer Surveys” (with Soren T. Anderson, Ryan Kellogg and Richard M. Curtin) *American Economic Review Papers & Proceedings* 101(3), May 2011, pp. 110-114.

“The Taxation of Fuel Economy” *Tax Policy and the Economy* v. 25, Editor Jeffrey R. Brown, NBER: University of Chicago Press, 2011, pp. 1-38.

“Consumer Valuation of Fuel Economy: A Microdata Approach” (with Sarah E. West and Wei Fan) *Proceedings of the National Tax Association’s 102nd Annual Conference on Taxation*, 2009, pp. 254-259.

WORKING PAPERS

“The Economics of Attribute-Based Regulation: Theory and Evidence from Fuel-Economy Standards” (with Koichiro Ito)

“New Evidence on Taxes and the Timing of Birth” (with Sara LaLumia and Nicholas Turner) *Submitted*

“Rational Inattention and Energy Efficiency” *Submitted*

“The Intergenerational Transmission of Automobile Brand Preferences: Empirical Evidence and Implications for Firm Strategy” (with Soren T. Anderson, Ryan Kellogg and Ashley Langer) *Submitted*

SELECTED WORK IN PROGRESS

“Do Consumers Recognize the Value of Fuel Economy? Evidence from Used Car Prices and Gasoline Price Fluctuations” (with Sarah West and Wei Fan)

AWARDS AND HONORS

Best Teacher in a Core Course, The Harris School (2012, 2013)
W.E. Upjohn Institute Early Career Research Grant (with Reed Walker) (2012)
Certificate of Excellence in Reviewing, *Journal of Public Economics* (2012)
John V. Krutilla Research Award from Resources for the Future (2009 - 2010)
National Tax Association Dissertation Award (2008)
National Science Foundation Graduate Research Fellowship (awarded 2003)
Population Studies Center Trainee Fellowship, University of Michigan (2003-2008)

TEACHING

All for Master of Public Policy Students at the Harris School

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

REFEREE

American Economic Review, Journal of Political Economy, Quarterly Journal of Economics, Journal of Public Economics, American Economic Journal: Economic Policy, American Economic Journal: Applied Economics, RAND Journal of Economics, Journal of Environmental Economics and Management, National Tax Journal, Journal of Labor Economics, International Tax and Public Finance, Journal of Law & Economics, Canadian Journal of Economics, Nature, B.E. Journal of Economic Analysis & Policy, Economic Inquiry, Journal of Human Resources, Economic Letters, Environmental and Resource Economics, Journal of Policy Analysis and Management; ***Grants:*** *National Science Foundation, European Science Foundation, Sloan Foundation, Time-Sharing Experiments for the Social Sciences*

SELECTED PRESENTATIONS

Invited **2013:** University of Pennsylvania (Wharton), Georgetown (Economics), Illinois (Economics), Wisconsin (Economics) **2012:** Maryland (Economics), Northwestern (Law), Universidad de Chile (Business School), Oxford (Business School); **2011:** Columbia (Economics), Maryland (AREC), Syracuse (Maxwell), Illinois (Finance), Ohio State (Economics), Illinois (Sustainability Center), NYU (Law conference), University of Illinois at Chicago (Sustainability workshop), Treasury, EPA, Resources for the Future (Conference); **2010:** MIT (Economics), Yale (Forestry), Berkeley (ARE), Berkeley (UCEI), NBER Tax Policy and the Economy, University of Chile; **2009:** Cornell (Economics), Minnesota (Applied Economics), North Carolina State University (Economics), Berkeley (POWER Conference), University of Illinois at Chicago (Economics), Macalester College (Economics); **2008:** Resources for the Future, University of Chicago (Harris), University of Pennsylvania (Wharton), University of British Columbia (Economics), University of Kentucky (Martin/Economics), University of Indiana (SPEA), University of California, Irvine (Economics), Treasury, Ford Motor Company

Conference **2012:** NBER Public Economics, National Tax Association, Michigan Tax Invitational **2011:** National Tax Association, ASSA, Association of Environmental and Resource Economics, International Institute of Public Finance, University of California Energy Institute; **2010:** NBER Public Economics, Iowa State Bioenergy Camp; **2009:** ASSA, National Tax Association, Heartland Environmental and Resource Economics; **2008:** APPAM, National Tax Association; **2007:** NBER Summer Institute (EEE), National Tax Association, APPAM

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

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

____
Subcontractor/Consultant

1/17/14
Date

Appendix B. **Charge Letter**

December 18, 2013

Dr. David L. Greene
Senior Fellow, Howard H. Baker, Jr. Center for Public Policy
1640 Cumberland Avenue
Knoxville, TN 37996-3340

Subject: Peer Review of Light-Duty Vehicle Rebound Effect Research

Dear Dr. Greene,

ICF International has been contracted by EPA to facilitate a peer review. In late November we corresponded by email and you indicated your availability to participate as a paid reviewer to review Ken Small and Kent Hymel's report "The Rebound Effect from Fuel Efficiency Standards: Measurements and Projection to 2035". You have been selected to participate on this panel. ICF will compensate you $3,000 for your services. This charge letter provides you with a list of directed questions for your review, the review schedule, and the materials we would like you to send to us at the conclusion of the review. In addition, attached to this letter is a copy of the report that we would like you to review.

Charge Questions
Listed below are the four directed questions we would like you to pay special attention to when conducting your review:

Element 1:
What are the merits and limitations of the authors' approach for estimating the vehicle miles traveled (VMT) rebound effect for light-duty vehicles? Are key assumptions underpinning the methodology reasonable? The VMT rebound effect is defined here as the change in VMT resulting from an improvement in light-duty vehicle efficiency.

Element 2:
Is the implementation of the authors' methodology appropriate for producing estimates of the VMT rebound effect? Specifically, are the input data and the methodology used to prepare the data appropriate? Are sound econometric procedures used? Does the model appropriately reflect underlying uncertainties associated with the assumptions invoked and the parameters derived in the model?

Element3:
The methodology used in this report attempts to account for asymmetric responses to increases vs. decreases in per mile fuel costs (and fuel prices). Does the report's finding of an asymmetric response seem reasonable given the methodology that the authors employed? In particular, do the authors' preferred model specifications (3 .21 b and 4.21 b) seem appropriate for capturing driver response to an increase in fuel efficiency?

Element 4:
The report describes a methodology for projecting the VMT rebound effect for light-duty vehicles forward in time. The concept of dynamic rebound is introduced to quantify the rebound effect over the period of a vehicle lifetime, during which time the variables that influence the rebound effect are changing. Is this methodology reasonable and appropriate, given the inherent uncertainty in making projections about how future drivers will respond to a change in the fuel efficiency of their vehicles?

Schedule
The schedule for this peer review is as follows:

December 18, 2013: Charge letter distributed to reviewers
Early January, 2014: Kick-off conference call with reviewers
January 17, 2014: Comment/review due via email to Larry.orourke@ICFI.com

The kick-off conference call will be an opportunity for you to speak with the other reviewers, ICF and EPA staff to provide you with any clarification you may require.

Materials

Upon completion of your review, you should submit your report under a cover letter that states 1) your name, 2) the name and address of your organization, and 3) a statement of any real or perceived conflict(s) of interest.

Should you have any questions or concerns, feel free to contact me via phone at 617-250-4226 or by email at Larry.orourke@icfi.com. In addition, the EPA project manager for this effort is Jeff Cherry and he may be reached at 734-214-4371. We will send you a meeting request for the kick-off conference call shortly. Thanks for your participation!

Sincerely,

Larry O'Rourke
Manager, ICF International

Attachment: The Rebound Effect from Fuel Efficiency Standards: Measurements and Projection to 2035

Appendix C. **Kenneth Gillingham Review Comments**

Review of Small and Hymel (2013)

The Rebound Effect from Fuel Economy Standards: Measurement and Projection to 2035

By: Kenneth Gillingham, Yale University
January 2014

Overview

This review of the final report by Ken Small and Kurt Hymel "The Rebound Effect from Fuel Efficiency Standards: Measurement and Projections to 2035" first provides a brief overview and then quickly turns to the four charge questions.

The report follows the methodology of Small and Van Dender (2007) and Hymel et al. (2010), with updated data and some minor additions. This is a thoughtful and careful effort aiming to address a difficult question: the change in VMT resulting from an increase in light-duty vehicle efficiency across the entire United States.

The primary methodology is to bring together aggregate state-level data on driving per adult M, fuel prices, vehicle stocks , fuel intensity, urbanization, and congestion. The authors then estimate a system of simultaneous equations to address endogeneity in key regressors, such as the cost per mile of driving. The system of equations is clearly summarized in Hymel et al. (2010) as follows:

$$vma_t = \alpha^m vma_{i,t-1} + \alpha^{mv} veh_t + \alpha^{mc} cong_t + \beta_1^m pm_t + \beta_{K1}^m cap1_t + \beta_3^m X_t^m + u_t^m \quad (1)$$

$$veh_t = \alpha^v veh_{t-1} + \alpha^{vm} vma_t + \beta_1^v pv_t + \beta_2^v pm_t + \beta_3^v X_t^v + u_t^v \quad (2)$$

$$fint_t = \alpha^f fint_{t-1} + \alpha^{fm} vma_t + \beta_1^f pf_t + \beta_2^f cafe_t + \beta_3^f X_t^f + u_t^f \quad (3)$$

$$cong_t = \alpha^{cm} vma_t + cap2_t + \beta_3^c X_t^c + \epsilon_t^c. \quad (4)$$

Here vma_t is natural log of the vehicle miles travelled per adult M, veh_t is the natural log of the number of vehicles per adult, $fint_t$ is the natural log of the fuel intensity (i.e., 1/fuel economy), and $cong_t$ is the log of the hours of travel delay per adult. In addition, pm_t is the log price per mile of driving, $cap1_t$ is the log total length of roads divided by state land area, pv_t is the log of an index of the price of a new vehicle, $cafe_t$ is a pre-estimated measure of stringency of CAFE standards, $cap2_t$ is the log of urban lane miles per adult, and the X's are additional variables such as the square of price, interactions between pm and the other variables, time trends, and state fixed effects. All variables are normalized for ease of interpretation.

The approach assumes first order autocorrelation in the error term for equations (1), (2), and (3). Identification of the key parameter of interest (the price elasticity of VMT demand β_1^m) relies primarily on within-state time series variation in M and the price of gasoline (conditional on the other covariates). The fuel cost per mile coefficient β_1^m is potentially endogenous because fuel economy itself is endogenous. This endogeneity is addressed by including another equation for the fuel intensity (3). Equation (2) addresses a potential endogeneity in veh_t and also allows for an interpretation of the effect of a change in fuel economy on the size of the vehicle stock.

If I understand correctly, the model is estimated in the same way as Small and Van Dender (2007), using a modified Cochrane-Orcutt transformation and nonlinear least squares (to address autocorrelation in the context of a lagged dependent variable).

The results are presented with equation (4) (from Hymel et al. (2010)) both included and not included. The results are largely in line with the results in the previous two papers and other previous papers in the literature. With the updated dataset covering 1966-2009, there is a short-run rebound effect on the order of 5%, a long-run effect on the order of 28-30%, evidence of the rebound effect declining with income, and evidence of a greater response when gasoline prices are increasing than decreasing. There is also some evidence of a structural break in 2003, with slightly larger rebound effects after this year. The rebound effect is then projected forward linearly using forecasts of key variables. When this leads to a negative rebound effect, it is replaced by zero.

Now I turn to each of the four charge questions. Since questions 1 and 2 are so closely linked, I will address them together.

Elements 1 and 2

Element 1: *What are the merits and limitations of the authors' approach for estimating the vehicle miles traveled (VMT) rebound effect for light-duty vehicles? Are key assumptions underpinning the methodology reasonable? The VMT rebound effect is defined here as the change in VMT resulting from an improvement in light-duty vehicle efficiency.*

Element 2: *Is the implementation of the authors' methodology appropriate for producing estimates of the VMT rebound effect? Specifically, are the input data and the methodology used to prepare the data appropriate? Are sound econometric procedures used? Does the model appropriately reflect underlying uncertainties associated with the assumptions invoked and the parameters derived in the model?*

There are many merits to the authors' approach for estimating the VMT rebound effect. It tackles a difficult question using what is likely the best data publicly available across all of the United States. It carefully considers many estimation issues and provides estimates that appear to be reasonable. It provides a valiant (and reasonable) attempt at forecasting the VMT rebound effect forward. There is no question that it was a major effort and a thoughtful one at that. It would be difficult to do much better given the task at hand.

As in any study, there are also limitations, most of which the authors recognize. All of these limitations relate to the difficulty of the question being asked. I will address these limitations next, emphasizing unavoidable challenges of estimation and providing a few suggestions.

1. To begin, the definition of the VMT rebound effect is vague. This is not the authors fault, for they are clear about the question they intend to answer. But, the definition, "the change in VMT resulting from an improvement in light-duty vehicle efficiency," provides much room for different interpretations. It provides no guidance on whether the improvement is costly, leading to higher vehicle prices or costless, leading to lower vehicle prices. Similarly, it does not specify whether other attributes of vehicles change along with vehicle efficiency. On one (unlikely) extreme, one could imagine expensive improvements in light-duty vehicle efficiency that also involve a trade-off leading to less desirable characteristics of the vehicles. At this extreme, the number of vehicles in the fleet would decline (vehicles are more expensive and less exciting) and at the same time driving is less exciting, so people drive less. This would suggest a very small rebound effect. Consider another (also unlikely) extreme, where improvements in light-duty vehicle efficiency are free and lead to no change in the attributes of the fleet. This would suggest a larger rebound effect. This extreme is the assumption made in the report. If we are discussing a tightened greenhouse gas (GHG) standard for light-duty vehicles, the truth could be expected to be somewhere in the middle. Put in terms of the notation in the report, the methodology estimates $\varepsilon_{\hat{M},pm}$, where $\hat{M}$ includes both the driving response and the "fleet size" response. In the report, the fleet size response is positive, for vehicles are more efficient and no more expensive. This is entirely consistent with what the authors state they intend to do, but not likely to be the case in the real world. If the vehicle fleet shrinks (or stays constant), we would expect fewer additional miles driven than in the results. Thus, for this reason the results are likely a slight *over-estimate* of the rebound effect from a GHG standard.

2. A second limitation, heterogeneity, is entirely a data limitation. The authors clearly recognize this. The only way data can be assembled on all states in the U.S. over time is to use aggregate data at the state level. Despite improvements in data availability in some states, this is the best we can do for all states. Using aggregate data masks known heterogeneity in the rebound effect, which may be important for projecting the rebound effect forward. This is recognized clearly by the authors on page 3: "In particular, the model assumes that changes in fleet average fuel economy will have the same impact on behavior whether those changes are caused entirely by new vehicles entering the fleet, or partly by new vehicles and partly by the retirement of older ones. It should be adequate insofar as the pattern of mileage driven by vehicle age is reasonably stable; if it is not, a more fine-tuned analysis tracking elasticities by vehicle age would reveal additional effects not captured here." I believe this is an important caveat, given that elasticities do vary by vehicle

age (I can see this in my own work). However, there is not much that can be done about this using aggregate data. Is this a major bias? It's hard to say. It is not even clear what the direction of the bias would be, since it could go either way. I see this as an assumption worth noting, as the authors clearly do, and an area worth researching further in the future. But I don't see any way around this given the current U.S.-wide question being asked.

3. Another limitation is the reliance on within-state time series variation in the study. Relying on time series variation is not necessarily a problem, but using a time series over many years typically lends itself to using time series approaches. For example, testing for the order of autocorrelation and for unit roots are common time series approaches. To its credit, the methodology does account for first-order autocorrelation. But what if the data are second-order autocorrelated? In this case, the coefficients could still be consistently estimated, but the standard errors would be incorrect. This raises a possible issue of incorrect standard errors. It is not clear what the direction of the bias in standard errors would be.

4. Similarly, since the time series econometric approaches are not used, one might have expected the standard panel data approach that includes time fixed effects to be employed. The dataset would make this possible. In this case, the identifying variation would be gasoline price shocks off the mean. I am sure the authors have considered and run such a specification before. I suspect one of two things happened: either there was not enough variation and the estimates were all statistically insignificant, or the results were crazy because the variation identifying the coefficients was not reliable variation. So instead, the paper includes linear time trends in each equation. These are helpful and much better than nothing. They do not control for other changes as flexibly as fixed effects, but they do retain more variation. Another possibility could be decade fixed effects or a quadratic or higher order polynomial in time. Would inclusion of these further time controls make a major difference? Perhaps not, but it could be worth discussing and exploring as further robustness checks. The direction of the bias would again be unclear. One way in which it might not make a difference is if the time-varying unobservables was only correlated with fuel intensity, which is effectively instrumented for in the third equation.

5. Another limitation is the difficulty in finding great instruments for the fuel cost per mile and fuel intensity. The system of equations can be thought of in an instrumental variables context. So the system of equations must have exclusion restrictions (i.e., variables that are not in the first equation, but are in the third equation) in order to address the possible endogeniety of the pm_t variable. In my read of the report and previous papers, it looks to me like the only exclusion restrictions are the CAFE stringency variable $cafe_t$ and lagged fuel intensity $fint_{t-1}$ (although it is a little odd to me that vma_t is in the third equation; usually one would expect to

see vma_{t-1} so that the lagged variable is an instrument for itself). So one way of looking at the results is that we are instrumenting for pm_t with the CAFE variable and lagged fuel intensity. Are these good instruments? Perhaps one could argue so, although they are not obviously so. The identification of the rebound effect does in part rest of this assumption. There is a similar assumption for the vehicle stock variable veh_t, where the price of vehicles and the lagged vehicle stock are the exclusion restrictions that help identify the vehicle stock variable veh_t in equation (1). I am not going to say that these exclusion restrictions are flat-out wrong, for I imagine you could argue for them and I personally would have a very tough time finding much better ones in this context. The bottom line is that β_1^m is a difficult coefficient to reliably identify with aggregate data, so there is reason to be at least somewhat cautious.

6. As the CAFE stringency variable $cafe_t$ is a key exclusion restriction, it is important to understand how it was derived. It was cleverly constructed, as a predicted variable using vehicle efficiency data prior to the implementation of CAFE standards in 1977. In this sense, I like the variable and think it is useful. However, given that it is a predicted variable, we know that using a predicted variable in an estimation means that we really have a two-step estimation approach, which requires adjusting the standard errors for the standard error in the first stage. One could easily get around this (and address any possible autocorrelation without the modified Cochrane-Orcutt approach) using bootstrapped standard errors. This is what I would suggest as another robustness check. Typically, bootstrapped standard errors lead to larger standard errors, but given how statistically significant the coefficients are in the current estimation, I would still expect statistical significance for the key coefficients of interest. Note that the coefficients themselves would not change.

7. A final limitation relates to the assumption of no measurement error in the variables, which may be important given the sources of the data (which to my knowledge are the best available for data of this ilk). Hymel et al. (2010) provide a very clear caveat on this point on page 1227: "Perhaps the greatest danger is that persistent measurement error in a given state (across years) could cause an overestimate of the coefficient in a given equation on the lagged value of the dependent variable. This coefficient is crucial in estimating the relationship between short-run and long-run elasticities. Thus the rather large difference we find between these elasticities (roughly a factor of five in the VMT equation) might be partly caused by measurement error." I think this is a fair caveat that applies equally to this report. If we have classical measurement error in the regressors, we would expect attenuation bias of the coefficients, so β_1^m could be biased downwards; thus it would be an *under-estimate* of the true value. The two things that can be done for this are to use instruments (which is done for some of the variables) and be very careful with the data collection process, which I believe they have been.

8. An assumption (not necessarily limitation) worth highlighting is the choice of a partial-adjustment model with a lagged dependent variable. There is a long history in energy economics using partial-adjustment models. They rely on a few assumptions. First, for consistency, there cannot be autocorrelation in the errors, otherwise there is an endogeneity issue. I believe that the methodology in the report addresses this concern. Second, for the interpretation of long-run elasticities, one must believe that we are in a dynamic system converging to an equilibrium response and that the structure we have put on this dynamic system is correct. Many, if not most applied econometricians today harbor some doubts about this approach, but we cannot rule it out. It relies on variation in the previous year's dependent variable to provide guidance on how quickly we are moving to a hypothetical equilibrium. Is this variation free of confounds? Hard to say. In any event, it is a major assumption that may be reasonable, even if many economists feel more comfortable with research designs where the identification is cleaner and there is no lagged dependent variable. The robustness check that many economists would want to see is the coefficient on pm_t when the first equation is estimated separately and without the lagged dependent variable. From Small and Van Dender (2007), we can see that estimating the first equation separately does not change the coefficient on pm_t much (an increase to -0.085). It would be nice to know what the result would be without the lagged dependent variable as well. At the end of the day though, these assumptions may be defensible.

To summarize, while there are many merits to this study, there are also some limitations. Some are data limitations and some should best be thought of as *possible* concerns that perhaps warrant further robustness checks and thought. I should emphasize that all applied econometric work has possible concerns and it is impossible to address them all. My overall take is that given the state of the literature, the coefficient estimates in this report provide a reasonable sense of what the VMT rebound effect is in the U.S. on average over the period 1966-2009.

Element 3

***Element 3**: The methodology used in this report attempts to account for asymmetric responses to increases vs. decreases in per mile fuel costs (and fuel prices). Does the report's finding of an asymmetric response seem reasonable given the methodology that the authors employed? In particular, do the authors' preferred model specifications (3.21 b and 4.21 b) seem appropriate for capturing driver response to an increase in fuel efficiency?*

This report uses a well-established approach to account for asymmetric responses to increases and decreases in per mile fuel costs based on variation in fuel prices. There are many energy economics papers that indicate a greater response to price increases than price decreases, and the authors find results that corroborate this literature. I believe the sign and relative magnitudes of these results, with the caveats above applying of course.

That said, I agree with the authors in questioning whether the driver response to an increase in fuel efficiency would be different than the response to gasoline prices. The asymmetries could come about for two primary reasons. First, gasoline price increases could be more salient than price decreases. Second, investments could be made when gasoline prices are high, limiting a short-run downward response when gasoline prices drop. Both factors probably play a role, and Figures 4.2 and 4.3 may be consistent with both.

But if asymmetries come about because of the differing salience of increases and decreases in gasoline prices, should we expect the same effects to apply for changes in vehicle fuel efficiency? My first inclination is that the answer is "not necessarily." Perhaps the downward price movement would be the better indicator of what the response would be to an increase in fuel efficiency, which is effectively what the asymmetric response results do. But given that saliency of the gasoline price may be different than saliency of the fuel price per mile, I see this as a relatively strong assumption.

The authors clearly recognize this, but must use the variation in the data that they have. Given the strong assumption, I would be more more comfortable using the results assuming the symmetric response. This seems to me to be a more neutral assumption, for it is effectively the mean effect. Fortunately, it does not make a huge difference.

Element 4

Element 4*: The report describes a methodology for projecting the VMT rebound effect for light-duty vehicles forward in time. The concept of dynamic rebound is introduced to quantify the rebound effect over the period of a vehicle lifetime, during which time the variables that influence the rebound effect are changing. Is this methodology reasonable and appropriate, given the inherent uncertainty in making projections about how future drivers will respond to a change in the fuel efficiency of their vehicles?*

Truly projecting the VMT rebound effect for light-duty vehicles forward in time requires a detailed model of the vehicle stock, along with elasticity estimates for each part of the age profile of the vehicle stock. It would involve allowing new vehicles to enter into the stock, which would lead to several dynamics. These new vehicles are more efficient, so they are driven more. Households also switch a bit to these vehicles from others, likely less-efficient vehicles, reducing emissions, but perhaps leading to a slightly more miles driven. Similarly, older vehicles are driven a bit less. As well, different types of people may switch to the new vehicles (e.g., people who have long commutes).

The authors face real data limitations that prevent this ideal modeling of the fleet. Instead they cleverly develop a "dynamic" rebound effect. The dynamic rebound effect attempts to take into account a variety of factors: the transition from the short-run to long-run rebound effect, the change in income, urbanization/congestion over time, and the decrease in driving from vehicles along the vehicle age profile. From my perspective, given the caveat that a true vehicle stock model is unavailable, this approach is sound for estimating the VMT rebound effect going forward in the next several years.

I am less comfortable linearly extrapolating as far out as 2030. It is very likely that the relationship between the rebound effect and income is relatively linear within the observed range of the variables, but moving forward, I believe it is less likely that the relationship would continue. The issue is quite clear in the need to truncate the rebound effect for any given state and year at zero. It seems more likely that there would be a smooth decline in the rebound effect that asymptotes to a level above zero. Congestion would reach saturation. Consumers would be wealthier so perhaps would be driving so much more that the utility of driving on the margin is very low (which could imply a larger rebound effect). These are just two possibilities. Perhaps with some exploration the authors could estimate a non-linear specification a nonlinear effect that asymptotes. If we must extrapolate out to 2030, I would feel more comfortable with this approach than allowing the rebound effect for some states to approach zero and then be zeroed out.

Would such an approach change the results much? I suspect not, but it is worth considering.

Appendix D. David Greene Review Comments

Review of "The Rebound Effect from Fuel Efficiency Standards: Measurement and Projection to 2035" by Kenneth A. Small and Kent Hymel, December 24, 2013.

David L. Greene

January 16, 2014

I have carefully read the paper, "The Rebound Effect from Fuel Efficiency Standards: Measurement and Projection to 2035" by Small and Hymel. This review is based on the December 24, 2013 corrected version.

Element 1:

What are the merits and limitations of Small's approach for estimating the vehicle miles traveled (VMT) rebound effect for light-duty vehicles? Are key assumptions underpinning the methodology reasonable? The VMT rebound effect is defined here as the change in VMT resulting from an improvement in light-duty vehicle efficiency.

Response

The Small & Hymel (S&H) approach for estimating the direct rebound effect is theoretically and methodologically rigorous and has been executed by the researchers without errors, to the best of this reviewer's knowledge. It has both merits and limitations, as do all existing studies of this phenomenon.

Merits

The authors demonstrate an accurate understanding of the direct rebound effect as distinguished from other definitions of the rebound effect. The model they have formulated and the data they use are appropriate for measuring the direct rebound effect.

The system of equations used to estimate the rebound effect allows for fuel intensity (the inverse of miles per gallon)[1] to affect vehicle travel via, 1) the effect of a change in fuel cost per mile on miles traveled per adult person, 2) the effects of fuel cost per mile on automobile ownership and 3) the effect of increased travel on traffic congestion (4-equation model). This formulation allows for quantification of the importance of these potential pathways by which fuel intensity might affect vehicle travel. The general similarity in results between S&H's 4-equation system and their 3-equation system (omitting congestion) adds to the evidence that the estimates are robust.

The lagged adjustment formulation used in the S&H model allows for estimation of both short-run and long-run rebound effects. The authors have used appropriate econometric methods for estimating this

[1] The terms "fuel economy" and "fuel intensity" are used throughout this paper. Fuel economy is defined as miles per gallon of motor fuel. Fuel intensity is the inverse of fuel economy.

type of model in a system of equations, taking into account the possibility that error terms within each equation may be correlated over time, a potentially serious issue for such lagged adjustment models.

The approach makes use of a large volume of data covering the fifty states and the District of Columbia over a period of 44 years. The source of data for vehicle travel is the U.S. Department of Transportation, Federal Highway Administration (FHWA), which collects the data from the individual states. These data have been scrutinized by the FHWA and checked against other data, such as permanent and periodic traffic counts. The data are certainly not ideal (there is no ideal source for VMT data) but are very unlikely to misrepresent year-to-year changes in vehicle travel due to the very large number of permanent and temporary traffic counters in use across the United States. In their estimation methods, the authors have used appropriate statistical procedures to account for any persistent state-specific errors. Aggregate vehicle travel data, such as used in this study, are appropriate for estimating the direct rebound effect since it is the effect of changes in fuel intensity on total vehicle travel that is of greatest relevance to the Environmental Protection Agency's (EPA) and National Highway Traffic Safety Administration's (NHTSA) rulemakings. Other sources of data, such as household travel surveys, cover a large fraction of total vehicle travel but omit vehicle travel by businesses and governments and also by heavier vehicles. Furthermore, models estimated on survey data generally do not insure that the estimated individual household changes integrate to the total national change. Total national vehicle travel as reported in the FHWA's table VM-1 is also a useful data source for estimating the rebound effect but the quantity of data available is smaller by a factor of 50.

There have been many studies of the rebound effect and S&H include the most important research papers in their review. In general, the studies based either on a national vehicle travel data time series, time series cross-sectional state vehicle travel data or panel survey data (covering several years and including significant fuel price changes) are very consistent with the empirical findings of S&H. S&H demonstrate that when their estimation is restricted to the time periods covered by previous studies, the rebound effects estimated by their method are very close to the central tendency of the studies. Higher estimates of the direct rebound effect have come from studies in other countries and from U.S. studies using only a single year of survey data. Statistical analysis based on a single year of survey data is prone to spurious correlations. In general, models attempting to explain variations in vehicle travel based on a single year of survey data have low explanatory power (in the statistical sense, i.e., low R^2). This makes controlling for factors that may influence both fuel economy and vehicle travel critical for obtaining coefficient estimates that are not biased by correlations with omitted variables. More robust estimates are likely to be obtained using time-series, cross-sectional data sources, such as used by S&H.

S&H have carefully investigated the possibility that the rebound effect is not constant over time. They test this possibility first by estimating different rebound effects for different periods of time without consideration of what might be causing any changes. They also test for a varying rebound effect by means of a formulation the authors have used in previously published papers that estimates correlations between the rebound effect and income and fuel price. The limitations of the latter method for forecasting purposes are discussed below. However, the authors have shown significant correlations and have proposed a plausible theoretical explanation for the results.

S&H have also investigated the possibility that fuel price and fuel intensity may affect vehicle travel differently, and that fuel price (or fuel cost per mile) rises and reductions may not have equal effects. This is important because the rebound effect, strictly speaking, pertains only to fuel intensity and not to fuel price yet many studies rely on estimation methods that constrain the elasticities of fuel price and fuel intensity to be equal but opposite in sign. For the purposes of the EPA, it is the effects of fuel intensity reductions that are of interest rather than the effects of fuel intensity increases. In theory, the effects could be symmetrical but, as S&H note, there is a substantial literature that indicates that market responses to fuel price rises and fuel price reductions are not symmetrical. The rigorous investigation of this issue is a valuable contribution about which more will be said below. The results confirm that responses to fuel price or fuel cost per mile reductions are smaller than the responses to increases. They also find that it is not possible to estimate a statistically significant effect of fuel intensity alone using their data and methods. This latter result is consistent with the small number of other studies that have reported on this issue.

The inability to estimate the separate effects of fuel price and fuel efficiency on VMT is worthy of further investigation. The authors' decision to proceed using fuel cost per mile is consistent with the interpretation that this outcome is caused by a poor sample design for the fuel efficiency variable. That is, the fuel efficiency of the on-road vehicle fleet changes very gradually and thus tends to follow a smooth trend, making it difficult to distinguish the effects of fuel intensity from other smoothly trending variables. In addition, state-level fuel economy is not directly measured but estimated by the states by various methods (e.g., by dividing fuel use by vehicle travel). Fuel prices on the other hand, have changed relatively quickly and by relatively large amounts. Fuel prices are also based to a large extent on direct measurements. This makes it easier to accurately estimate at least the short-run price effect. The authors' decision is therefore a prudent one given the information available. It is also appropriate for them to note that, if anything, it is more likely to result in an overestimate of the rebound effect.

Given the above, the authors recommend using the rebound effect estimated using cost per mile, which constrains the price and fuel intensity elasticities to be equal in magnitude and opposite in sign. This is the most important assumption of their study, since without it the estimated rebound effect would not be statistically significant from zero. They also note that this assumption, in all likelihood, leads to an overstatement of the rebound effect. Their decision seems prudent although it is a subjective one and, strictly speaking, not supported by the empirical data. The alternative would be to assign a value of zero to the rebound effect. This, however, would imply that drivers do not behave rationally from an economic perspective, since they would treat changes in cost caused by changes in the price of fuel differently from changes in cost due to changes in fuel intensity. Economic theory suggests that such a conclusion should itself be supported by more evidence than the lack of statistical significance of the fuel intensity coefficient. S&H present their reasoning on this issue transparently, as they should.

Limitations

In their review of the literature, S&H should have included the important review of studies of the rebound effect by Sorrell (2007) and companion reports by the UK Energy Research Center (Sorrell and Dimitropoulos, 2007; Dimitropoulos and Sorrell, 2006). Since the UKERC study is a review of the literature, by itself it does not add much new material to the S&H review but it does cover more of the literature and reaches conclusions that support S&H's interpretation of the literature.

The definition of the rebound effect on p. 6 is the definition appropriate when fuel economy improvements come about due to pure technological change. That is, the improvement in fuel economy does not involve trading off purchase cost or other vehicle attributes (e.g., size, acceleration) for fuel economy. The rebound effects of fuel efficiency due to pure technological change versus fuel economy standards are almost certainly different. Technological change shifts the trade-off between fuel economy and cost (or other attributes) while standards generally cause manufacturers to move to a different location within the same trade-off function. Of course, technological change is always occurring and there is the likelihood that standards induce technological change but the basic point remains valid since standards, in general, will induce a trade-off of fuel economy for other vehicle attributes, especially manufacturing cost. For the purposes of evaluating the EPA/NHTSA rule makings, trade-offs with vehicle cost are highly relevant.

Although the study does a good job of recognizing and describing a wide range of pathways for the rebound effect, it omits part of the effect of increased vehicle prices on the long-run cost per mile of travel. According to all studies of which I am aware, including the rule making itself, the 2025 fuel economy and greenhouse gas (GHG) standards are expected to result in an increase in the long-run cost of manufacturing vehicles. The increased cost will cause an increase in vehicle transaction prices, assuming only that vehicles' selling prices increase with increasing long-run average cost. The S&H model allows the increase in vehicle price to affect VMT through the effect of new vehicle prices on the vehicle stock and the effect of vehicle stock on vehicle travel. But an increase in the capital cost of a vehicle also affects the long-run cost of vehicle travel via usage-induced capital depreciation. This mechanism is not included in either the 3-equation or 4-equation versions of the model and could be important because capital costs are a large fraction of total vehicle ownership costs.

The potential for feedback effects to be generated via institutional processes is appropriately acknowledged but a potentially important one is missing. That is the effect of major fuel economy improvements on highway user fees. In the past, fuel economy improvements have been second only to inflation as a threat to Highway Trust Fund revenues (e.g., Greene, 2011). Historically, motor fuel taxes have been raised by federal and state governments in order to maintain adequate funding for highway construction and maintenance. Whether this will continue to be the case in the future and what type of tax may be used (possibly one that does not fall on motor fuel) are open questions but certainly relevant ones. Raising motor fuel taxes would, *ceteris paribus*, increase the retail price of motor fuel, thereby increasing the fuel cost per mile of travel and partially offsetting the rebound effect of fuel intensity. A careful review and analysis of this subject would likely lead to the conclusion that raising fuel taxes in order to maintain highway user fee revenues should be included in regulatory analyses of the rebound effect. This is not something that S&H need to include in their econometric analysis but it should be mentioned in the discussion of possible institutional effects.

Summary for Element 1

In brief, S&H's study is a technically proficient assessment of the rebound effect of fuel economy on vehicle travel using appropriate state-level vehicle travel and associated data. The conclusions drawn are well supported by the empirical analyses in this paper and, in general, by the previous literature. The authors have made several important contributions:

1. Re-estimating the rebound effect using more recent state-level data and demonstrating the consistency of their historical estimates with the central tendency of the existing literature.
2. Estimating the effects of income and fuel price on the size of the rebound effect over time and showing the ability of these factors to statistically explain a large portion of the apparent changes.
3. Testing the potential asymmetry of response to increases and decreases in fuel cost per mile. The analysis also shows that the asymmetric response to fuel price changes implies a smaller rebound effect than that found assuming a symmetric response to fuel cost per mile.
4. The projections of future rebound effects are useful but may understate the rebound effect in cases where many states' rebound effects approach zero. This is likely a consequence of the linear functional form and truncation rule and could be an artifact of those assumptions.

Overall, this paper makes an important contribution to the literature and, like the authors' previous work, represents the current state of knowledge about the rebound effect of motor vehicle fuel economy on vehicle travel.

Element 2:

Is the implementation of the Small methodology appropriate for producing estimates of the VMT rebound effect? Specifically, are the input data and the methodology used to prepare the data appropriate? Are sound econometric procedures used? Does the model appropriately reflect underlying uncertainties associated with the assumptions invoked and the parameters derived in the model?

Response

The S&H method represents best practice and is appropriate for producing estimates of the rebound effect. As discussed above, the data used are well suited to the problem. The econometric methods are also appropriate and consistent with the state of practice. Incorporating uncertainty, on the other hand, poses a difficult challenge that has not yet been given much attention in the literature on the rebound effect. There are uncertainties due to data shortcomings, issues with the experimental design available in the historical record, uncertainties due to model formulation, uncertainties inherent in econometric estimation and uncertainties about the future state of the world. S&H have addressed many of these issues by constructing alternative projections based on different assumptions. These are useful. However, adequately addressing uncertainty and incorporating it into a projection methodology requires an identification of the nature of the uncertainties to be included, which should follow from the purpose for representing uncertainty. It is not clear to this reviewer what the goal of including uncertainty is, and therefore it is not possible to give a definitive response concerning the S&H method's handling of uncertainty.

Data Definitions

It would be helpful to the reader for S&H to spend a little more time explaining the nature of the state level data. According to this reviewer's understanding, state level data include VMT and fuel use by all vehicle types, not only the light-duty vehicles affected by past fuel economy regulations. This introduces substantial heterogeneity in the vehicle populations across states, from motorcycles to diesel-powered 18-wheelers, although light-duty vehicles still predominate. Fuel intensity is believed by this reviewer to be total state highway use of motor fuel (not only gasoline) divided by total state highway vehicle travel. It would be helpful to clarify these definitions in the report to alert the reader to the meaning of the data and possibly help interpret the results. It is likely that state-specific constants will account for much of the differences across states in the composition of traffic. Remaining effects of heterogeneity are not likely to cause important problems for estimating the rebound effect.

Cost per Mile versus Fuel Intensity Rebound

The question of whether the data actually support the existence of a rebound effect for fuel economy has been addressed above and is mentioned again here to emphasize its importance and the uncertainty it creates. The estimates presented by S&H are based on the maintained hypothesis of economically rational behavior, in the sense that consumers are assumed to respond to changes in fuel cost per mile in the same way whether caused by a change in fuel price or a change in fuel economy. However, the new research presented by S&H concerning the asymmetry of responses sheds new light on this subject, as explained in greater detail in Element 3. The consequence of the analysis of asymmetry is that there is now strong evidence that the market response to reductions in fuel intensity (a goal of the fuel economy and GHG standards) is less than the response of the market to increases in the price of fuel, and that it is also smaller than estimates of the rebound effect based on the assumption of a symmetrical response to changes in fuel cost per mile. This finding of S&H is potentially of major significance. It implies that the best estimate of the rebound effect for the purpose of estimating the effects of fuel economy and GHG standards is the asymmetric elasticity of reductions in fuel cost per mile. Since it is a relatively novel result with respect to the rebound effect, further research is warranted, yet the results presented by S&H are strong and should now represent the current state of knowledge.

Statistical Insignificance of Endogenous Variables in Some Equations

S&H do not provide an adequate discussion of the fact that some of the endogenous variables in either the 3- or 4-equation models are not statistically significant. For example, in the 3-equation models, *vma* does not appear to be statistically significant at the 0.05 level in any of the equations for vehicle stock, and *pf+vma* is not statistically significant in the equation for *fint* in models 3.3, 3.18, 3.21b, or 3.29 (Table B1). In the 4-equation models, *pf+vma* is not statistically significant in the equation for *fint* in models 4.3, 4.13, 4.21, and possibly 4.23. This calls into question the necessity for the simultaneous equation framework, at least as formulated, and requires explanation. The secondary, simultaneous equation effects are small relative to the direct effect of pm in the *vma* equation and so the empirical significance of these pathways is not great but it would be interesting to see if the hypothesis of simultaneity is rejected by the data or not.

In particular, the equation for *fint* in table 4.2 raises questions. Why it is preferable to interact fuel price and VMT rather than test also for the main effects of the two variables? As explained on p. 30, the interacted fuel cost variable turns out not to be statistically significant. This result increases the need for an explanation of the choice of this formulation. Is this a parsimonious way of getting both variables into the *fint* equation? Would they be less statistically significant individually? And if neither vehicle travel nor the price of fuel is statistically significant in the equation for fuel intensity, doesn't this undermine the rationale for including this equation in a system of equations? If this is the best formulation and yet the log of fuel price times VMT is not statistically significant in the equation for *fint* then it would seem that the data do not support including *fint* in a simultaneous equation formulation. Again, this does not appear to be of great practical importance since the simultaneous equation effects are relatively small.

A Caveat on Long-run and Short-run Effects and Lagged Adjustment Models

The lagged adjustment model used by S&H is a useful formulation and widely adopted for modeling phenomena such as aggregate VMT. However, it implies two important maintained hypotheses. The first is that the correlation between the dependent variable and its lagged value measures only the adjustment process. If there are other causes of a strong positive correlation, the long run elasticities will be overestimated. By using econometric methods that allow for error correlation in the lagged adjustment equation S&H have taken a prudent step to deal with possible correlation between the current and lagged values of the dependent variable from that source. Second, it implies the same adjustment rate for all variables, which would seem to be a special case. These observations do not diminish the value of this analysis or others using the lagged adjustment formulation but are something to be borne in mind when interpreting results.

The Effect of Vehicle Cost on Vehicle Use

The S&H model allows changes in vehicle price to affect vehicle travel via its effect on the size of the vehicle stock. However, this may not be adequate since increased vehicle cost also affects the cost per mile of travel to the extent that use of a vehicle depreciates its value. There is no question that capital depreciation is a component of the long-run cost per mile of travel. The question is how important it is as a determinant of long-run travel demand.

Estimates of the elasticity of total vehicle travel with respect to car purchase cost were found in at least one literature review to have a central tendency of -0.19 in the short run and -0.42 in the long run (Goodwin et al., 2004, table 7). While the plurality of studies reviewed come from the United States, the majority do not. In addition, it is not clear from the study cited how many studies combine the effect of purchase cost via the size of the vehicle stock with the effect of purchase cost via long-run cost per mile. Nonetheless, for illustrative purposes only, I will use the -0.4 elasticity. If a doubling of fuel economy caused a 10-20% increase in VMT at a cost of $2,000 per vehicle for vehicles with a prior average cost of

$25,000, the 8% increase in vehicle cost would reduce VMT by about 3%, offsetting 15-30% of the estimated rebound effect. This reviewer is not arguing here that these numbers correctly represent the magnitude of this possible effect for the United States but rather to illustrate the possibility that there may be an important issue here that is worthy of formal investigation.

Element 3:

The methodology used in this report attempts to account for asymmetric responses to increases vs. decreases in per mile fuel costs (and fuel prices). Does the report's finding of an asymmetric response seem reasonable given the methodology that Small employed? In particular, do the authors' preferred model specifications (3 .21 b and 4.21 b) seem appropriate for capturing driver response to an increase in fuel efficiency?

On this subject, S&H have produced potentially important results. Their analysis supports the inference that rebound estimates based on a symmetric response to fuel cost per mile overstate the rebound effect of fuel intensity. The price asymmetry model has been found in other studies of the response of gasoline demand to gasoline price and petroleum demand to petroleum price. Thus, it is very likely that the difference between rises in fuel cost per mile and decreases in fuel cost per mile is attributable to asymmetric market responses to rises in the price of fuel and not to asymmetric responses to changes in fuel intensity. This would mean that the symmetric model, by estimating an average effect of rises and reductions in fuel cost per mile, would overestimate the effect of reductions in fuel cost per mile. S&H's results confirm this. This result is important because it implies that for purposes of estimating the rebound effect of fuel economy regulations, the asymmetric elasticity of a reduction in fuel cost per mile should be a more accurate estimate of the rebound effect than the fuel cost per mile elasticity estimated assuming a symmetric relationship between fuel cost per mile and vehicle travel.

The analysis of the possibly asymmetric effects of fuel price rises and cuts appears to be separating price effects (which are asymmetric) from fuel intensity effects (which are not asymmetric). As the authors explain, in the asymmetric model the rebound effect is mathematically the sum of the asymmetric effects. The partial effect of fuel efficiency (holding other variables constant) does not depend on whether prices are rising or falling. Rather, it is the effect of the price of gasoline that depends on whether prices rise or fall. Thus, this reviewer concurs with the authors' decision to adopt this result in their preferred models (3.21 b and 4.21 b), especially since these empirical results are also consistent with their earlier inference that by using fuel cost per mile alone (based on a symmetric model) one would almost certainly overestimate the rebound effect.

Generally, two possible explanations are put forward for the asymmetrical response to fuel price rises and cuts. The first is that consumers are more likely to extrapolate fuel price rises than cuts and thus respond more strongly to fuel price rises when purchasing durable goods. The second explains the persistence of asymmetry in the long run as a consequence of technological change or public policy (i.e., efficiency standards) induced by fuel price rises. In either case, the asymmetry method used in this section of the paper should be able to separate these irrelevant effects from the rebound effect. Empirically, the effects of fuel price and fuel intensity changes are not the same (see above). The asymmetric model offers a logical explanation of the conundrum.

Because the media and price volatility effects almost certainly apply to the effects of price but not fuel efficiency, the authors are correct in abandoning the models including these variables. The anomalous results in certain formulations also support this decision. As the authors note, the erratic behavior of the Asymmetry model 3.23 suggests that it is not a plausible model. Because the asymmetric models are also not able to separately estimate the fuel price and fuel efficiency effects, as the authors note, their preference for the models of section 4.4.1 is well reasoned.

In this and previous work, S&H have found that the rebound effect varies with income. In this study, they also found that it varies with the price of fuel. It would be interesting to test whether changes in the distribution of income as well as average income have affected the rebound effect. There is some evidence that the distribution of income has affected the growth rate of aggregate VMT. The result that the rebound effect varies with income and fuel price is both important and useful for analyzing the future costs and benefits of fuel economy and GHG regulations.

Element 4:

The report describes a methodology for projecting the VMT rebound effect for light-duty vehicles forward in time. The concept of dynamic rebound is introduced to quantify the rebound effect over the period of a vehicle lifetime, during which time the variables that influence the rebound effect are changing. Is this methodology reasonable and appropriate, given the inherent uncertainty in making projections about how future drivers will respond to a change in the fuel efficiency of their vehicles?

The dynamic rebound model provides a reasonable method of accounting for the fact that as fuel economy improvements penetrate the vehicle stock, new vehicles have higher fuel economy than older vehicles. What is not clear is how much of an improvement this method makes over basing the rebound effect on the vehicle miles weighted average fuel intensity of the vehicle stock. If distributional effects were important (if it were important to know how much the usage of different vehicles changed), detailed modeling of changes in vehicle use in the vehicle stock by vintage would be necessary. It is not clear that this is necessary for EPA's analysis of the costs and benefits of fuel economy regulations. That said, there is no compelling reason not to use the dynamic method proposed by S&H.

In this and previous papers, the authors have presented strong evidence that the rebound effect has changed over time and that the changes are correlated with changes income and fuel price. The income result was also confirmed in a recent study using national time series data (Greene, 2012). There is also theoretical justification for including these effects, since income affects the value of travelers' time and fuel prices affect the fuel cost share of the long-run cost per mile of travel. Thus, it is appropriate to include these effects in the forecasting model. While there is uncertainty about future incomes and fuel prices, basing the estimated rebound effect on price and income assumptions used elsewhere in the estimation of costs and benefits of the standards will result in a more consistent assessment. That said, the linear extrapolation of the income and price effects is problematic. Whatever the correct functional form may be, it is not linear over the full range of possible future incomes and fuel prices. This leads to the problem of rebound effects with theoretically implausible signs, which the authors have addressed by truncation at zero. Truncation at zero is better than not truncating at zero. A better functional form should be sought that approaches zero as income goes to infinity and fuel price goes to zero.

Final Comments

The S&H analysis is very well done, uses appropriate models, data and econometric methods and makes several important contributions to knowledge of the rebound effect. The results are consistent with both the central tendency of other estimates in the literature and with the best studies contained in the peer-reviewed literature. The range of issues investigated and statistical tests performed is a particular strength of the analysis. The projected rebound effects are useful and plausible. The results are useful to EPA as they now stand. The issues raised in this review and those noted below suggest avenues of additional research and model development that may or may not lead to improvements in the model as currently recommended by S&H.

The chief limitations of the study are the possibly inadequate representation of the effect of vehicle purchase costs on the long-run cost per mile of vehicle travel, the need for an interpretation of the lack of statistical significance of key endogenous variables in many of model equations, and the truncation of the rebound effect in the projecting model when the estimated rebound effect becomes negative.

It is appropriate to adopt the models that include the asymmetric response to reductions in fuel intensity (models 3.21b and 4.21b) as the current best estimates of the rebound effect. The finding of asymmetry in the elasticity of cost per mile should be incorporated in the projection methodology. It is statistically significant and consistent with the peer-reviewed published literature. It also addresses the inability to estimate a significant elasticity for fuel intensity alone and the conclusion that the rebound effect is thereby overestimated. Use of the “price cut” elasticity of fuel cost per mile from the asymmetric model has the advantage of at least removing the fuel price rise asymmetry from the estimated rebound effect.

S&H’s investigation of how the rebound effect may vary systematically with other factors is an important contribution to the understanding of the rebound effect. Incorporating rebound effects that vary with income (value of time) and fuel price (fuel cost share of operating costs) in forecasting the rebound effect is supported both theoretically and empirically. The fact that the rebound effect varies with both income (interpreted as representing the value of time) and fuel price (perhaps representing the fuel cost share of the long-run costs of vehicle travel) suggests that an alternative model formulation explicitly including all the important long-run costs of vehicle travel (and the elasticities of substitution among them) might produce an improved forecasting model. Such an approach might also permit inclusion of use-related depreciation as a component of the cost per mile of travel. Fuel cost is not the only component of the long-run cost of vehicle travel. The short-run cost of travel includes the traveler's time and the long-run cost includes many factors, notably the capital cost of the vehicle.

The assumption of constant elasticity (as a function of income and fuel price) should be considered only one possible functional form. In particular, it is recommended that forecasts of the rebound effect be based on a more explicit representation of the total cost of vehicle travel, including fuel, maintenance, capital and travelers' time costs. Because in the end S&H are left with only a partial explanation for the apparent increase in the rebound effect after 2003, understanding the correct functional form of the rebound effect should be given a higher priority.

It would also be appropriate to update the projected rebound effect estimates using the most recent Annual Energy Outlook (e.g., 2014 Early Release). Undoubtedly this was not available at the time the study was carried out.

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Appendix E. James Sallee Review Comments

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January 20, 2014

Larry O'Rourke
ICF International
9300 Lee Highway, Fairfax, VA 22031-1207

Dear Dr. O'Rourke:

Attached to this letter please find my peer review of "The Rebound Effect from Fuel Efficiency Standards: Measurements and Projections to 2035" written by Kenneth Small, with contributions by Kent Hymel.

I have no conflicts of interest relevant to the report or the contents of this review.

Sincerely,

James M. Sallee
Assistant Professor
University of Chicago

Summary statement:

"The Rebound Effect from Fuel Efficiency Standards: Measurement and Projection to 2035", written by Kenneth Small (with contributions from Kent Hymel), uses an appropriate methodology and defensible assumptions. It uses the best available data (given significant constraints on what is available), and emphasizes modeling choices and specifications that are sensible and consistent with both theory and data. As a reviewer, I agree with most of the assumptions and emphases in the paper. Where I do disagree (detailed below), I believe that the preference of one method or specification over the other involves an element of subjective judgment about how to weigh the costs and benefits of different approaches. I did not identify any issues that I believe are objectively incorrect. Thus, while I might have made some different choices myself, I believe that the choices made in the report are *defensible.*

My detailed comments are included below in a numbered list, categorized according to the four charge questions that were given to me by ICF International. I did not restrict myself to comments on how the immediate report ought to be changed given realistic constraints on time and effort; many of my comments are intended to point to areas where future reports could, in my opinion, make the biggest improvements. My comments should be read in that light.

Before proceeding to those comments, two issues are worth highlighting. First is a big picture question regarding methodology and data. This report uses data aggregated to the state-by-year level over five decades. Recent research (e.g., work by Kenneth Gillingham and joint work by Chris Knittel and Ryan Sandler) has made use of microdata from vehicle odometers, which is available for some cars in some recent years in some states. The aggregate data used in the Small report analyzed here suffer from measurement problems (detailed below, see item 7) and limit the available econometric identification strategies (see items 1-3). The odometer microdata suffer from limited coverage, both across states and over time, and existing estimates are focused on a short-run elasticity that is inconsistent with some of the measures emphasized in the report. In the end, which data and methodology should be preferred likely depends on exactly what specification one wishes to use. I think that a case can certainly be made for sticking with the aggregate data used in the Small report, but I suspect that, in the near future when researchers have gained access to data from a somewhat more representative set of states and have a few more years worth of data, that the case for the microdata will become stronger. In any case, it would be very valuable to know how projections based on the microdata estimates compare to those used here, were it possible to construct such projections.

The second issue worth highlighting is how the report models the relationship between income and the rebound effect for use in projections. In brief, the literature seems consistent in finding evidence that the rebound effect varies over time and that, on a decadal time scale, the effect is smaller in more recent years than in prior decades. The paper posits that this may be due to rising income. This is theoretically sensible in that the total cost of driving involves a cost of time as well as a cost of fuel, and as income rises, so does the wage and hence the time cost of driving, which eventually comes to dominate the price per mile. In the report's projections, with income projected to rise, the rebound effect is quickly driven to zero in many states, which greatly affects the final estimates. But, given the nature of the identification, which relies on time series correlations between income and the rebound effect (see items 1-3), it is difficult to have confidence that income is the driving factor. Even if income is the driving force in the historical data, it is not certain that it will continue to have the same relationship in the future. One must make a stand on the relationship between income and this elasticity, and the one that the paper makes is consistent with economic theory and with the data.

Thus, as with many modeling decisions, I think the paper's choice on how to handle this is defensible, though alternative choices might be defensible as well (see item 11). I highlight this issue in particular because it appears to be pivotal to the results. Below, I include a few thoughts on how the projections might be refined (item 13) and how this issue might affect which results are most useful to report (item 14). Here, I want to make the point that any additional analysis that could corroborate the relationship between income and the VMT elasticity would be very valuable.

I would find it reassuring if the cross-sectional relationship between income and the rebound effect was similar to the estimated aggregate time-series relationship. According to the research cited in the report, the available microdata evidence suggests otherwise; it finds that the rebound effect is U-shaped in income. A rationale for this is that wealthier people have more travel options, which makes them more responsive. This factor competes against the time cost factor, and at different levels of income different factors dominate, resulting in a U-shape. The projections *might* change significantly if the relationship between income and the elasticity is U-shaped in the time series. This depends on whether or not future aggregate income is high enough to reach the upward sloping portion of the U.

Rather than using a cross-section of microdata, one could look at a cross-section of states states (or countries) to see how estimated elasticities are correlated with income. For example, one could estimate the VMT-elasticity separately for each state for some span of years (say, a decade) not controlling for income and then see how that correlates with state income. Are wealthier states less responsive? One might reasonably argue that the cross-sectional relationship between income and the VMT elasticity is a fundamentally different parameter than the over-time relationship, but they seem to me to be based on the same theoretical arguments. As a result, I would like to see some sort of corroborating evidence—either in this report or in a completely separate study—though I recognize that the suggestions made here are themselves far from perfect.

Element 1: *What are the merits and limitations of the authors' approach for estimating the vehicle miles traveled (VMT) rebound effect for light-duty vehicles? Are key assumptions underpinning the methodology reasonable? The VMT rebound effect is defined here as the change in VMT resulting from an improvement in light-duty vehicle efficiency.*

1. The paper uses a panel regression, but it is best understood as deriving results form time-series variation because the panel regressions do not include time period fixed effects and the lion's share of variation in the key measures come from the time-series. In most cases, the extra credibility that is often attributed to panel data models comes from their ability to include both time and entity level fixed effects. The report does *not* use time fixed effects, and generally has very sparse controls for time. The most important variable in the analysis is the price of gasoline. This is measured at the state-year level, but once state fixed effects are controlled for, a vast majority of the variation in the data will be attributable to fluctuations in the global oil price (or national gasoline price).

   I do not necessarily advocate that the paper add time period fixed effects; if year fixed effects were added, the remaining variation in gasoline prices that would identify the coefficients would be state-specific fluctuations in gasoline prices in each time period, which often represent short run imbalances in local supply and demand that should not be expected to persist (and therefore may have a limited impact on behavior). In that sense, the report uses the best available variation, but this implies that the paper's results are largely driven by the national time series in gasoline prices and VMT, which has implications discussed in the next two points.

2. The nature of the panel identification means that, in my judgment, the additional benefit of having 51 states as opposed to 1 national time series may be somewhat overstated. I do not see mention in the paper of any attempt to control for correlation across states in error terms. The standard way of handling this is to cluster standard errors on some larger level of observation, the rule of thumb being "at the level of variation in the key independent variable". Given my argument above that identification is driven primarily by the national price of gasoline, one might interpret this as implying that standard errors should be clustered at the time period level (year), though technically most of the variables vary at the state-by-year level. I suspect that if the standard errors were clustered on time period that much of the added precision that results from moving from a national time series to a panel regression would be lost. To be clear, none of this implies bias in any coefficients, but the confidence one might have in distinguishing between certain specifications might be reduced by attention to the standard errors. As with other issues, I believe there is ambiguity here, and one could perhaps defend more vigorously the decision not to cluster.

3. The nature of the panel identification also opens the possibility for standard omitted variable bias problems. With sparse time controls and trending variables, anything

that is correlated with gasoline prices as well as with VMT per adult could induce bias. Some factors that might be relevant are the fraction of driving that is personal as opposed to work-related,[1] the quality of automobiles,[2] commuting norms, changes in the fraction of families with two wage-earners, the expansion of urban sprawl, etc. This is especially important for an analysis that spans so great a time frame. The report attempts to control for measures of the most important variables, but it is *a priori* difficult to be confident that all such secular trends have been accounted for by a limited number of demographic variables. What is usually done in response is to (a) show precisely how sensitive the coefficients of interest are to the inclusion of the available set of controls and (b) show the robustness of the coefficient to many additional tweaks.

Along these lines, an appealing permutation would be to add state-specific time trends, and to add differential time trends for different periods of time where we have reason to believe that there might be structural breaks. (The appendix to the 2007 working paper indicates that three distinct time trends are used, but this includes a single trend for all years after 1980, which may be inadequate. Moreover, I did not see the set of time controls used spelled out clearly in the current report.) I suspect that the author has tried these permutations, and I recognize that the tests for structural breaks in the data do not yield conclusive results upon which to base these decisions. But, I would hope to see greater evidence of robustness of the results to richer controls for time, and perhaps to a broader set of demographic and vehicle market controls.

4. The report argues that a secondary pathway through which CAFE standards might impact VMT is through the overall size of the car market. The idea is that fuel economy standards will cause people to buy more cars because fuel efficiency standards lower the cost of driving, which thus increases the value of owning a car, holding prices constant. (This is the difference between M and $\widehat{M}$ in the report.) This argument is present in much of the related literature.

   I find this objectionable from a theoretical point of view. In a standard market model, the imposition of fuel economy standards could not *raise* the value of cars (net of price) on average. The market should be offering cars that have a bundle of attributes that maximizes private value to consumers. The introduction of fuel economy standards forces automakers to alter the mix of attributes they offer—perhaps through changes in technology or through a shift from size and

[1] The price sensitivity of miles driven for work is likely different than miles driven for personal reasons because of the difference in who is paying for fuel and whether time is uncompensated. The data used on VMT do not distinguish these types of driving.
[2] The time cost of driving is a function of the opportunity cost of driving and of the flow utility of being in the car. More comfortable cars with improved media, and cell phones, may substantially lower the cost of driving in that dimension.

performance to economy. If standards force this mix to be altered, it is counter to theory to suggest that this will create attribute mixes that consumers prefer, conditional on price (which is controlled for in the regressions).

This reasoning could be wrong if another market failure exists, such as the idea that consumers are myopic and thereby underappreciate the value of fuel economy. In that case, consumers could conceivably have increased private utility from the standard. But, even this scenario does not rationalize an increase in the size of the vehicle market because, if consumers are myopic, then they won't *recognize* that the new vehicle fleet is preferable—the market was providing the fleet that *seemed to be* value maximizing. This suggests that the market should shrink. It seems to me that the final effect on market size depends on whether the standards raise or lower producer mark-ups over marginal cost in equilibrium, which is theoretically ambiguous.

Importantly, the report de-emphasizes this channel, which is found to be quite small. So, while I disagree at points with the report on this issue, I do not think it has an important impact on the final projections.

5. This report introduces measures of media attention, which are new to the literature. This is used in two ways, one is as an additional regressor, another is as an auxiliary data series useful for aiding interpretation. I agree with the latter usage, but not the former. Media mentions of gasoline prices is not well motivated as an *independent* regressor from a theoretical standpoint. It is meant, I believe, as a measure of the salience of gasoline prices. But, the media surely reflects public attention as much as it dictates it. Thus, it is fundamentally endogeneous. As such, I prefer models that do not include it as a regressor.

   At other times in the report, the media mention series is looked at by itself as an interesting time series that might help interpretation. I think it is appropriate to use in this sense—if it is a proxy for an endogenous measure of salience or awareness, then it may be useful to look at this series and see if it happens to line up with the time pattern of coefficient estimates from the baseline model, as a way of perhaps interpreting what is going on in the main estimates. In the end, the report does not emphasize these results over others, which mitigates my concern.

6. One weakness of the aggregated data used in this report is that it provides no immediate way of modeling the relationship between vehicle age and VMT. Given the lack of data on this, it seems appropriate for the report to abstract from such issues, but this points to another area where the odometer microdata could be useful. Those data could be used to detail the age-VMT relationship and to see how it changes over time and in response to fuel price shocks and regulation. Such information might be especially useful in refining the dynamic rebound effects emphasized in the report.

Element 2: *Is the implementation of the authors' methodology appropriate for producing estimates of the VMT rebound effect? Specifically, are the input data and the methodology used to prepare the data appropriate? Are sound econometric procedures used? Does the model appropriately reflect underlying uncertainties associated with the assumptions invoked and the parameters derived in the model?*

7. The report suffers from crucial data limitations, of which the author and the literature more broadly are well aware. The key problem is that most of the dependent variables are not independently measured, but are instead imputed based on possibly inconsistent procedures across states and over time and through a methodology that is not well explained by the Federal Highway Administration. To recap, states generally have good data on gallons of fuel sold, because they collect taxes by the gallon. States themselves, or the FHWA, use some estimate of fuel efficiency of the vehicles on the road to translate gallons sold into VMT (M), by calculating that M = F / E-hat, where E-hat is their estimate and F is fuel consumed. The fuel intensity is measured in the report as 1/E = F/M, where again VMT is imputed based on E-hat. Then, Gas Price per mile is calculated as Gas Price / E = Gas Price * M/F = Gas Price / E-hat. Thus, the measurement of all of the most important variables depends on some estimate of efficiency that states are using, which may be inconsistent across states and over time, or that the FHWA is using, which at best is based on surveys 5-years apart and may be wiping out differences across states by using national averages for imputation. Any errors in measuring E are being passed through the system because it is an input into all of the relevant variables, which may create mechanical correlations across all of the variables of interest.

   The author is aware of these issues and articulates them (although much of the discussion is found only in the working paper version of Small and Van Dender), so raising the issue would be belaboring the point, but for three reasons. One is that this fundamental concern about data is an argument for shifting regulatory impact analysis from the type of methodology used here and towards a reliance on the new odometer-based microdata sooner rather than later.

   A second is that it raises some concerns about the CAFE variable used in the paper, which is imputed based on the relationship between fuel economy and VMT in the years before CAFE. What were states or the FHWA doing to impute fuel economy before EPA ratings existed in 1978? This is especially important because the CAFE variable used in the paper, which is theoretically very clever, is based entirely on a projection forward from data on fuel economy demand for the period before CAFE was in place, which is a period in which there were no government measures of fuel economy. How could states have had meaningful estimates of the on-road fuel economy of the vehicles in their state prior to those years? Why do we think that consumer demand for fuel economy would be the same before and after labels were introduced? How did they even know how efficient were the models in the earlier years?

A third is that it is worth pointing out that the relationship between gallons of gasoline consumed (the only thing actually measured directly) and VMT depends on average *on-road* fuel economy, not EPA ratings. As driving conditions vary, the relationship between VMT and on-road economy will differ. In particular, in observations with greater urbanization and greater congestion, the more miles will be spent in settings that garner lower average mpg for a given vehicle. A recent working paper by Ashley Langer and Shaun McRae suggests that there is huge variation in on-road fuel economy for identical vehicles.

8. There are some important differences in the estimates depending on whether or not the latest years of data are included. I think it is arguably preferable to omit the financial crisis, which would include both 2008 and 2009 in annual data. The paper does not report results that omit only those two years. One might make the case that the baseline specification should include data only up to 2007.

Element3: *The methodology used in this report attempts to account for asymmetric responses to increases vs. decreases in per mile fuel costs (and fuel prices). Does the report's finding of an asymmetric response seem reasonable given the methodology that the authors employed? In particular, do the authors' preferred model specifications (3 .21 b and 4.21 b) seem appropriate for capturing driver response to an increase in fuel efficiency?*

In brief, I agree with the choice of models 3.21b and 4.21b as the preferred model.

9. There are two types of asymmetry discussed in the analysis. One is that drivers may respond differently to changes in fuel economy than to changes in fuel prices, so that price-per-mile is not a sufficient measure of the price to which consumers respond. I am sympathetic to the idea that there could be a difference, primarily because of the salience of the fuel price. However, I think that the appropriate null hypothesis, based on theory, is that consumers make decisions based on price-per-mile. In the absence of compelling evidence that consumers react differently to the two components of price, I think that the report should focus on estimates that assume symmetry in this dimension. This is what the report chooses to do, and it is reflected in the preferred models of 3.21b and 4.21b.

10. The second type of asymmetry is in whether the rebound effect is different for price-per-mile increases as compared to decreases. The report ultimately favors a model in which fuel price increases yield larger responses than fuel price decreases, and it is deemed preferable to use a model based on asymmetry of fuel price, not asymmetry of price per mile.

    Here, I think the preferred specification is more ambiguous than with regard to the other symmetry question, but I am in agreement with the author on the preferred methodology. There does seem to be sufficiently strong evidence of an asymmetric response, in this paper and throughout the literature, to use a model that allows for this difference.

Theoretically, it is sensible to assume that the asymmetry lies in increases or decreases in the cost per mile (e.g., model 3.29), but the added econometric challenge of solving the additional endogeneity problem that is induced by this specification leads me to conclude that models based on asymmetry in fuel prices (not price per mile) are preferable, for practical reasons. Thus, I agree with the report's choice of models 3.21b and 4.21b as the baseline preferred model.

Element 4: *The report describes a methodology for projecting the VMT rebound effect for light-duty vehicles forward in time. The concept of dynamic rebound is introduced to quantify the rebound effect over the period of a vehicle lifetime, during which time the variables that influence the rebound effect are changing. Is this methodology reasonable and appropriate, given the inherent uncertainty in making projections about how future drivers will respond to a change in the fuel efficiency of their vehicles?*

In summary, I think that the paper makes defensible projections. That is, all of the assumptions used in the models that are projected out to 2035 are reasonable. I agree with the report that the baseline statistic should be the dynamic rebound effect, which is the most theoretically relevant statistic for most applications.

11. I do think, however, that an appealing alternative is to simply take the best available estimates of the rebound effect from recent years, say 2000 to 2007, and project this forward as a constant rebound effect over all future years without conditioning on changes in income and other interacted variables. This alternative is dubious in that it assumes that whatever conditions are at work in the most recent decade of data will continue to be true in the future. But, it avoids dangers of extrapolating out of context. That is, in the face of the inherent uncertainty in making projections two decades into the future, a conservative methodology is to simply take the best available recent estimate and assume that it will be constant in the future. If I were the author of the report, I would provide such an estimate alongside the dynamic rebound effects that are reported. An additional benefit of this alternative is that it would allow for direct comparison to the projections that would come from using odometer microdata estimates of the rebound effect, which could be used for this "straight line" projection, but may be harder to integrate into the dynamic estimates emphasized in the report.

12. I do have a question/concern about the way that fuel price volatility is represented in the projections. My understanding is that the AEO projects a smooth gasoline price into the future. This is fine for models that do not include asymmetry, but for models that do include asymmetry, a smoothly evolving gasoline price series and an alternative that has the same average trend but experiences movement up and

down around the trend will not produce the same rebound effect.

If this correct, then it is important for the models using asymmetry of adjustment to fuel price increases and decreases to be based on some reasonable projection of volatility. (I have in mind using the AEO projection of gasoline prices and the annual volatility around a trend from the last 20 or 30 years to draw random forecasted paths of the gasoline price, and then averaging the rebound effect projections that result over many such paths.) I suspect that this will increase the rebound effect for the asymmetric models, but that the effect on the forecasts will be small.

13. With rising income, the rebound effect is driven to zero in the projections, but the effect is truncated at zero so that it cannot become negative. Might it be preferable to truncate at a value above zero? Even as average income rises in the next two decades, many individuals will remain at lower income levels and would therefore be expected to remain responsive to fuel costs. Thus, it is hard to see the logic in expecting that the average rebound effect could go all the way to zero in the near future, so that some baseline above zero may be a more appropriate point of truncation. It would be ad hoc to choose some point, but 0 is actually an ad hoc point itself, given that it is meant to represent an average.

14. There is a great deal of uncertainty surrounding the final projections, due to uncertainty in the estimated coefficients, the possibility of model error, and the uncertainty in the forecasted inputs (like the price of gasoline and future income). The report lists point estimates for forecasts and includes a few different specifications and three forecasted futures that vary the path of the future price of oil. Additional representations of uncertainty might be appropriate.

    A first possibility is to include standard errors around the forecasted values that reflect the sampling uncertainty in the model estimation (i.e., the standard errors on the coefficients). This should be conceptually straightforward, though it multiplies the number of numbers that must be reported in a table by two (though it is just shading in a figure).

    The price of oil makes a substantial difference to the bottom line estimates. Thus, depending on what the EPA foresees as the final use of this report, it may be worth providing additional detail about the oil price scenarios that the AEO is using (are these meant to represent extremes of a spectrum of plausible paths? Or are they likely scenarios?). Or perhaps additional results should be presented. That would depend on the intentions of the user of the report.

    A fuller version way of representing forecast and coefficient uncertainty is to model the uncertainty in the forecasted variables and provide a collection of different model results based on random draws of these variables. I think this would be useful in making clearer which parameters are really pivotal, so users know where to draw their attention. If, for example, all that really matters is income growth relative to oil price growth, then I would like to see a focus on that relative parameter and to have spelled out for me why the range of estimates actually span the useful set of scenarios to study. I recognize that this is a tall order and would perhaps require a substantial separate analysis.

In terms of model error, which is more difficult to represent, the report lists projections for several different specifications, which is useful. The one thing that could perhaps be useful is to provide some explicit comparison, along the lines mentioned above, of how these projections differ from a projection that uses just the VMT elasticity estimate taken from the most recent decade of data and projected forward without reducing it based on income and other demographic trends (the straight line projection).