Fuel Efficiency and Motor Vehicle Travel: The Declining Rebound Effect

Kenneth A. Small and Kurt Van Dender***

We estimate the rebound effect for motor vehicles, by which improved fuel efficiency causes additional travel, using a pooled cross section of US states for 1966-2001. Our model accounts for endogenous changes in fuel efficiency, distinguishes between autocorrelation and lagged effects, includes a measure of the stringency of fuel-economy standards, and allows the rebound effect to vary with income, urbanization, and the fuel cost of driving. At sample averages of variables, our simultaneous-equations estimates of the short- and long-run rebound effect are 4.5% and 22.2%. But rising real income caused it to diminish substantially over the period, aided by falling fuel prices. With variables at 1997-2001 levels, our estimates are only 2.2% and 10.7%, considerably smaller than values typically assumed for policy analysis. With income and starting fuel efficiency at 1997-2001 levels and fuel prices 58 percent higher, the estimates are still only 3.1% and 15.3%, respectively.

1. INTRODUCTION

It has long been realized that improving energy efficiency releases an economic reaction that partially offsets the original energy saving. As the energy efficiency of some process improves, the process becomes cheaper, thereby providing an incentive to increase its use. Thus total energy consumption changes less than proportionally to changes in physical energy efficiency. This "rebound effect" is typically quantified as the extent of the deviation from proportionality. It has been studied in many contexts, including residential space heating and cooling, appliances, and transportation (Greening, Greene, and Difiglio, 2000).

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* Department of Economics, University of California, Irvine, Irvine, CA 92697-5100, e-mail: ksmall@uci.edu.

** Department of Economics, University of California, Irvine, Irvine, CA 92697-5100, e-mail: kvandend@uci.edu. Corresponding author. Tel: 949-824-9698; Fax 949-824-2182.

For motor vehicles, the process under consideration is use of fuel in producing vehicle-miles traveled (VMT). When vehicles are made more fuel-efficient, it costs less to drive a mile, so VMT increases if demand for it is downward-sloping. That in turn causes more fuel to be used than would be the case if VMT were constant; the difference is the rebound effect. Obtaining reliable measures of it is important because it helps determine the effectiveness of measures intended to reduce fuel consumption and because increased driving exacerbates congestion and air pollution. For example, the rebound effect was an issue in the evaluation of recently adopted greenhouse-gas regulations for California (CARB, 2004, Sect. 12.3-12.4). It has played a prominent role in analyses of the Corporate Average Fuel Economy (CAFE) regulations in the US and of proposals to strengthen them.

This paper presents estimates of the rebound effect for passenger-vehicle use that are based on cross-sectional time-series data at the U.S. State level. It adds to a sizeable econometric literature, contributing four main improvements. First, we use a longer time series (1966-2001) than was possible in earlier studies. This increases the precision of our estimates, enabling us (among other things) to determine short- and long-run rebound effects and their dependence on income. Second, the econometric specifications rest on an explicit model of simultaneous aggregate demand for VMT, vehicle stock, and fuel efficiency. The model is estimated directly using two- and three-stage least squares (2SLS and 3SLS); thus we can treat consistently the fact that the rebound effect is defined starting with a given change in fuel efficiency, yet fuel efficiency itself is endogenous. Third, we measure the stringency of CAFE regulation, which was in effect during part of our sample period, in a theoretically motivated way: as the gap between the standard and drivers' desired aggregate fuel efficiency, the latter estimated using pre-CAFE data and a specification consistent with our behavioral model. Fourth, we allow the rebound effect to depend on income and on the fuel cost of driving. The dependence on income is expected from theory (Greene, 1992), and is suggested by micro-based estimates across deciles of the income distribution (West, 2004). Just like income changes, changes in fuel prices affect the share of fuel costs in the total cost of driving, and so we also expect them to influence the rebound effect.

Our best estimates of the rebound effect for the US as a whole, over the period 1966-2001, are 4.5% for the short run and 22.2% for the long run. The 2SLS and 3SLS results are mostly similar to each other but differ from ordinary least squares (OLS) results, which are unsatisfactory as they strongly depend on details of the specification. While our short-run estimate is at the lower end of results found in the literature, the long-run estimate is similar to what is found in most earlier work. Additional estimation results, like the long-run price-elasticity of fuel demand (-0.43) and the proportion of it that is caused by mileage changes (52%), are similar to those in the literature.

This agreement is qualified, however, by our finding that the magnitude of the rebound effect declines with income and, with less certainty, increases with the fuel cost of driving. These dependences substantially reduce the magnitude that applies to recent years. For example, using average values of income, urbanization and fuel costs measured over the most recent five-year period covered in our data set (1997-2001), our results imply short- and long-run rebound effects of just 2.2% and 10.7%, roughly half the average values over the longer time period. Similarly, the long-run price elasticity of fuel demand declines in magnitude in recent years and so does the proportion of it caused by changes in amount of motor-vehicle travel. These changes are largely the result of real income growth and lower real fuel prices. Future values of the rebound effect depend on how those factors evolve.

The structure of the paper is as follows. Section 2 defines the rebound effect and reviews some key contributions toward measuring it. Section 3 presents our theoretical model and the econometric specification, and section 4 presents estimation results. Section 5 concludes.

2. BACKGROUND

The rebound effect for motor vehicles is typically defined in terms of an exogenous change in fuel efficiency, E. Fuel consumption F and motor-vehicle travel M – the latter measured here as VMT per year – are related through the identity $F=M/E$. The rebound effect arises because travel M depends (among other things) on the variable cost per mile of driving, a part of which is the per-mile fuel cost, $P_M \equiv P_F/E$, where P_F is the price of fuel. This dependence can be measured by the elasticity of M with respect to P_M, which we denote $\varepsilon_{M,PM}$. When E is viewed as exogenous, it is easy to show that fuel usage responds to it according to the elasticity equation: $\varepsilon_{F,E} = -1 - \varepsilon_{M,PM}$. Thus a non-zero value of $\varepsilon_{M,PM}$ means that F is not inversely proportional to E: it causes the absolute value of $\varepsilon_{F,E}$ to be smaller than one. For this reason, $-\varepsilon_{M,PM}$ itself is usually taken as the definition of the rebound effect.

Two of our innovations relate directly to limitations of this standard definition of the rebound effect. First, this definition postulates an exogenous change in fuel efficiency E. Yet most measurements of the rebound effect rely heavily on variations in the fuel price P_F, in which case it is implausible that E is exogenous. This is suggested by the substantial differences in empirical estimates of the fuel-price elasticities of fuel consumption, $\varepsilon_{F,PF}$, and of travel, $\varepsilon_{M,PF}$.[1] They are related by $\varepsilon_{F,PF} = \varepsilon_{M,PF} \bullet (1 - \varepsilon_{E,PF}) - \varepsilon_{E,PF}$, where $\varepsilon_{E,PF}$ measures the effect of fuel price on efficiency. Thus the observed difference between $\varepsilon_{F,PF}$ and $\varepsilon_{M,PF}$ requires that $\varepsilon_{E,PF}$ be considerably different from zero. Ignoring this dependence of E on P_F, as is done in many studies, may cause the rebound effect to be overestimated if unobserved factors that cause M to be large (e.g. an unusually long commute) also cause E to be large (e.g. the commuter chooses fuel-efficient vehicles to reduce commuting costs).

A second limitation of the standard definition is that fuel cost is just one of several components of the total cost of using motor vehicles. Another important component is time cost, which is likely to increase as incomes grow. If consumers' response to fuel costs is related to the proportion of total cost accounted for

1. USDOE (1996, chap 5); Graham and Glaister (2002); Parry and Small (2005).

by fuel, then $|\varepsilon_{M,PM}|$ should increase with fuel cost itself and diminish with income (Greene, 1992). Our specification allows for such dependences. Furthermore, time costs increase with traffic congestion; we account for this indirectly by allowing the rebound effect to depend on urbanization, although empirically this turns out to be unimportant. An extension, not attempted here, would be to allow congestion to be endogenous within the system that determines amount of travel.

Some empirical studies of the rebound effect have used aggregate time-series data. Greene (1992) uses annual U.S. data for 1957-1989 to estimate the rebound effect at 5 to 15% both in the short and long run, with a best estimate of 12.7%. He also finds not accounting for autocorrelation – which he estimates at 0.74 – results in spurious measurements of lagged values and to the erroneous conclusion that long-run effects are larger than short-run effects. Greene also presents evidence that the fuel-cost-per-mile elasticity declines over time, consistent with the effect of income just discussed; but the evidence has only marginal statistical significance.

Jones (1993) re-examines Greene's data, adding observations for 1990 and focusing on model-selection issues in time-series analysis. He finds that although Greene's autoregressive model is statistically valid, so are alternative specifications, notably those including lagged dependent variables. The latter produce long-run estimates of the rebound effect that substantially exceed the short-run estimates (roughly 31% vs. 11%).[2] Schimek (1996) uses data from a still longer time period and finds an even smaller short-run but a similarly large long-run rebound effect (29%). Schimek accounts for federal CAFE regulations by including a time trend for years since 1978; he also includes dummy variables for the years 1974 and 1979, when gasoline-price controls were in effect, resulting in queues and sporadic rationing at service stations. These controls reduce the extent of autocorrelation in the residuals.

These aggregate studies highlight the possible importance of lagged dependent variables (inertia) for sorting out short-run and long-run effects. But they do not settle the issue because they have trouble disentangling the presence of a lagged dependent variable from the presence of autocorrelation. Their estimates of these dynamic properties are especially sensitive to the time period considered and to their treatment of the CAFE regulations.

Another type of study relies on pooled cross-sectional time-series data at a smaller geographical level of aggregation. Haughton and Sarkar (1996) construct a data set for the 50 U.S. States and the District of Columbia, from 1970 to 1991. Fuel prices vary by state, primarily but not exclusively because of different rates of fuel tax, providing an additional opportunity to observe the effects of fuel price on travel. The authors estimate equations for VMT per driver and for fuel intensity, obtaining a rebound effect of about 16% in the short run and 22% in the long run. Autocorrelation and the effects of a lagged dependent variable are measured with sufficient precision to distinguish them; they obtain a statisti-

2. Pointers to the precise figures used in calculating these and other numbers in this section are in the notes to Small and Van Dender (2006).

cally significant coefficient on the lagged dependent variable, implying a substantial difference between long and short run. Tackling yet another dynamic issue, Haughton and Sarkar find that fuel efficiency is unaffected by the current price of gasoline unless that price exceeds its historical peak – a kind of hysteresis. In that equation, CAFE is taken into account through a variable measuring the difference between the legal minimum in a given year and the actual fuel efficiency in 1975. However, that variable is so strongly correlated with the historical maximum real price of gasoline that they omit it in most specifications, casting doubt on whether the resulting estimates, especially of hysteresis, really control adequately for the CAFE regulation.

It appears that the confounding of dynamics with effects of the CAFE regulation is a limiting factor in many studies. There is no agreement on how to control for CAFE, and results seem sensitive to the choice. This is partly because the standards were imposed at about the same time that a major increase in fuel prices occurred. But it is also because the control variables used are not constructed from an explicit theory of how CAFE worked. We attempt to remedy this in our empirical work.

Studies measuring the rebound effect using micro data show a wider disparity of results than those based on aggregate data, covering a range from zero to about 90%. Two recent such studies use a cross section for a single year. West (2004), using the 1997 Consumer Expenditure Survey, estimates a rebound effect that diminishes strongly with income (across consumers) but is 87% on average, much higher than most studies. By contrast, Pickrell and Schimek (1999), using 1995 cross-sectional data from the Nationwide Personal Transportation Survey (NPTS), obtain a rebound effect of just 4%. There are a number of reasons to be cautious about these results. West obtains an extremely low income-elasticity for travel, namely 0.02, in the theoretically preferred model which accounts for endogeneity between vehicle-type choice and vehicle use. Pickrell and Schimek's results are sensitive to whether or not they include residential density as an explanatory variable, apparently because residential density is collinear with fuel price. We think the value of cross-sectional micro data for a single year is limited because measured fuel prices vary only across states, and those variations are correlated with unobserved factors that also influence VMT—e.g. residential density, congestion, and market penetration of imports. We eliminate the spurious effects of such cross-sectional correlations by using a fixed-effects specification, i.e. by including a dummy variable for each state.

Two recent studies use micro data covering several different years, thereby taking advantage of additional variation in fuel price and other variables. Goldberg (1998) estimates the rebound effect using the Consumer Expenditure Survey for the years 1984-1990, as part of a larger equation system that also predicts automobile sales and prices. When estimated by Ordinary Least Squares (OLS), her usage equation implies a rebound effect (both short- and long-run, because the equation lacks a lagged variable) of about 20%. Greene, Kahn, and Gibson (1999) use micro data from the Residential Transportation Energy Consumption Survey and its predecessor, for six different years between 1979 and 1994. Their usage equation is part of a simultaneous system including vehicle type choice and actual fuel price paid by the individual. They estimate the rebound effect at 23% for all households (short- and long-run assumed identical), with a range from 17% for three-vehicle households to 28% for one-vehicle households.

Several micro studies, e.g. Train (1986), Hensher et al. (1992), Goldberg (1998), and West (2004), estimate model systems in which vehicle type and usage are chosen simultaneously, thereby accounting for the endogeneity of fuel efficiency.[3] Mannering (1986) explicitly addresses the bias resulting from such endogeneity; his estimate of $|\varepsilon_{M,PM}|$ becomes considerably *greater* when endogeneity is taken into account, which could indicate that people who drive more spend more time in stop-and-go traffic or that they invest more in fuel-consuming amenities.

Thus prior literature shows that aggregate estimates of the rebound effect, especially of the long-run effect, are sensitive to specification — in particular to the treatment of time patterns and CAFE standards. Disaggregate studies tend to produce a greater range of estimates; but those that exploit both cross-sectional and temporal variation are more consistent, finding a long-run rebound effect in the neighborhood of 20-25 percent. These results parallel those of three more comprehensive reviews, which report rebound estimates from numerous studies with means of 10-16 percent for short-run and 26-31 percent for long-run rebound effect.[4] Overall, we would regard long-run estimates of anywhere between 20 and 30 percent as compatible with previous studies, but we see less consensus on short-run estimates.

3. THEORETICAL FOUNDATIONS AND EMPIRICAL SPECIFICATION

3.1 System of Simultaneous Equations

Our empirical specification is based on a simple aggregate model that simultaneously determines VMT, vehicles, and fuel efficiency. We assume that consumers in each state choose how much to travel accounting for the size of their vehicle stock and the per-mile fuel cost of driving (among other things). They choose how many vehicles to own accounting for the price of new vehicles, the cost of driving, and other characteristics. Fuel efficiency is determined jointly by consumers and manufacturers accounting for the price of fuel, the regulatory environment, and their expected amount of driving; this process may include manufacturers' adjustments of the relative prices of various models, consumers' adjustments via purchases of various models (including light trucks), consumers' decisions about vehicle scrappage, and driving habits.

3. We earlier quoted Goldberg's OLS results, rather than her instrumental-variable results, because the latter display huge standard errors on the vehicle-type dummies, suggesting to us insufficient variation in the data to satisfactorily identify this two-way causality.

4. De Jong and Gunn (2001); Graham and Glaister (2002); Goodwin, Dargay, and Hanly (2004). The National Research Council (2002), without distinguishing short from long run, quotes 10-20 percent.

These assumptions lead to the following structural model:

$$\begin{aligned} M &= M(V, P_M, X_M) \\ V &= V(M, P_V, P_M, X_V) \\ E &= E(M, P_F, R_E, X_E) \end{aligned} \tag{1}$$

where M is aggregate VMT per adult; V is the size of the vehicle stock per adult; E is fuel efficiency; P_V is a price index for new vehicles; P_F is the price of fuel; $P_M \equiv P_F/E$ is the fuel cost per mile; X_M, X_V and X_E are exogenous variables (including constants); and R_E represents regulatory measures that directly or indirectly influence fleet-average fuel efficiency. Section 3.3 provides an overview of the main variables contained in the estimated system of equations.

The standard definition of the rebound effect can be derived from a partially reduced form of (1), which is obtained by substituting the second equation into the first and solving for M. Denoting the solution by $\hat{M}$, this produces:

$$\hat{M} = M\,[V(\hat{M}, P_V, P_M, X_V), P_M, X_M] \equiv \hat{M}\,(P_M, P_V, X_M, X_V). \tag{2}$$

We call this equation a "partially reduced form" because V but not E has been eliminated (E being part of the definition of P_M); thus we still must deal with the endogeneity of P_M as a statistical issue. The rebound effect is just $-\varepsilon_{\hat{M}, PM}$, the negative of the elasticity of $\hat{M}(\cdot)$ with respect to P_M. By differentiating (2) and rearranging, we can write this elasticity in terms of the elasticities of structural system (1):

$$\varepsilon_{\hat{M}, PM} \equiv \frac{P_M}{M} \cdot \frac{\partial \hat{M}}{\partial P_M} = \frac{\varepsilon_{M, PM} + \varepsilon_{M, V}\varepsilon_{V, PM}}{1 - \varepsilon_{M, V}\varepsilon_{V, M}}. \tag{3}$$

Strictly speaking, the estimation of a statistical model proves associations, not causation. However, one advantage of a structural model is that it makes explicit the pathways by which those associations occur and thus allows more informed judgments about whether causality is at work. It seems to us that the key relationships we are interested in, involving VMT, vehicle stock, fuel efficiency, income, and fuel price, are plausibly represented by interpreting each equation in (1) as causal. We therefore adopt this interpretation in describing our results.

3.2 Empirical Implementation

While most studies reviewed in section two are implicitly based on (2), we estimate the full structural model based on system (1). We generalize it in two ways to handle dynamics. First, we assume that the error terms in the empirical equations exhibit first-order serial correlation, meaning that unobserved factors influencing usage decisions in a given state will be similar from one year to the next: for example, laws governing driving by minors. Second, we allow for behavioral inertia by including the one-year lagged value of the dependent variable as a right-hand-side variable. We specify the equations as linear in parameters and with most variables in logarithms, leading to the following system:

$$\begin{aligned}
&(vma)_t = \alpha^m \cdot (vma)_{t-1} + \alpha^{mv} \cdot (vehstock)_t + \beta_1^m \cdot (pm)_t + \beta_3^m X_t^m + u_t^m \\
&(vehstock)_t = \alpha^v \cdot (vehstock)_{t-1} + \alpha^{vm} \cdot (vma)_t + \beta_1^v \cdot (pv)_t + \beta_2^v \cdot (pm)_t \\
&\quad + \beta_3^v X_t^v + u_t^v \\
&(fint)_t = \alpha^f \cdot (fint)_{t-1} + \alpha^{fm} \cdot (vma)_t + \beta_1^f \cdot (pf)_t + \beta_2^f \cdot (cafe)_t + \beta_3^f X_t^f + u_t^f
\end{aligned} \tag{4}$$

with autoregressive errors:

$$u_t^k = \rho^k u_{t-1}^k + \varepsilon_t^k, \;\; k=m,v,f. \tag{5}$$

Here, lower-case notation indicates that the variable is in logarithms. Thus *vma* is the natural logarithm of VMT per adult; *vehstock* is the log of number of vehicles per adult; and *fint* is the log of *fuel intensity*, defined as the reciprocal of fuel efficiency. Variable *pf* is the log of fuel price; hence log fuel cost per mile, *pm*, is equal to *pf+fint*. Variable *pv* is the log of a price index of new vehicles. The variable *cafe* measures fuel-efficiency regulation, as described in Section 3.3.3. The individual variables in each vector X_t^k may be in either levels or logarithms. Subscript t designates a year, and u and ε are error terms assumed to have zero expected value, with ε assumed to be "white noise".

Each lagged dependent variable can be interpreted as arising from a lagged adjustment process, in which the dependent variable moves slowly toward a new target value determined by current independent variables. The inertia of such movement can arise due to lack of knowledge, frictions in changing lifestyles, or slow turnover of the vehicle stock.

In system (4), equation (3) becomes:

$$-b^S = \varepsilon_{\hat{M},PM} = \frac{\varepsilon_{M,PM} + \alpha^{mv}\beta_2^v}{1 - \alpha^{mv}\alpha^{vm}} \tag{6}$$

where b^S designates the short-run rebound effect. If variable *pm* were included only in the form shown in (4), the structural elasticity $\varepsilon_{M,PM}$ would just be its coefficient in the usage equation, β_1^m. However, we include some variables in X^m that are interactions of *pm* with income, urbanization, and *pm* itself. Thus the elasticity, defined as the derivative of *vma* with respect to *pm*, varies with these measures. For convenience, we define the interaction variables in such a way that $\varepsilon_{M,PM} = \beta_1^m$ when computed at the mean values of variables in our sample. Since the other terms in (6) are small, this means that $-\beta_1^m$ is approximately the short-run rebound effect at those mean values.

To compute the long-run rebound effect, we must account for lagged values. The coefficient α^m on lagged *vma* in the usage equation indicates how much a change in one year will continue to cause changes in the next year. If α^{mv} were zero, we could identify $\varepsilon_{M,PM}$ as the short-run rebound effect and $\varepsilon_{M,PM}/(1-\alpha^m)$ as the long-run rebound effect. More generally, the long-run rebound b^L is defined by the following equation (Small and Van Dender, 2005, Section 5.1):

$$-b^L = \varepsilon^L_{\hat{M},PM} = \frac{\varepsilon_{M,PM} \bullet (1-\alpha^v) + \alpha^{mv}\beta^v_2}{(1-\alpha^m)(1-\alpha^v) - \alpha^{mv}\alpha^{vm}} \tag{7}$$

Similarly, the short- and long-run elasticities of vehicle usage with respect to new-car price are:

$$\varepsilon^S_{\hat{M},PV} = \frac{\alpha^{mv}\beta^v_1}{1-\alpha^{mv}\alpha^{vm}}; \quad \varepsilon^L_{\hat{M},PV} = \frac{\alpha^{mv}\beta^v_1}{(1-\alpha^m)(1-\alpha^v) - \alpha^{mv}\alpha^{vm}} \tag{8}$$

and the short- and long-run elasticities of fuel intensity with respect to fuel price are approximately:[5]

$$-\varepsilon^S_{\hat{E},PF} = \frac{\beta^f_1 + \alpha^{fm}\varepsilon_{M,PM}}{1-\alpha^{fm}\varepsilon_{M,PM}}; \quad -\varepsilon^L_{\hat{E},PF} = \frac{\beta^f_1 \bullet (1-\alpha^m) + \alpha^{fm}\varepsilon_{M,PM}}{(1-\alpha^f)(1-\alpha^m) - \alpha^{fm}\varepsilon_{M,PM}}. \tag{9}$$

Our data set is a cross-sectional time series, with each state observed 36 times. We use a fixed effects specification, which a Hausman test strongly favors over random-effects. We allow for autocorrelation by transforming each equation to a nonlinear one with no autocorrelation but with additional lags.[6]

3.3 Variables

This section describes the main variables in (4) and their rationale. We identify each using both the generic notation in (1) and the variable name used in our empirical specification. Variables starting with lower case letters are logarithms of the variable described. All monetary variables are real. Data sources are given in Small and Van Dender (2006).

5. The elasticities defined in (9) are those of $\tilde{E}(P_F, P_V, R_E, X_M, X_V, X_E)$, the fully reduced-form equation for E obtained by solving (1) for M, V, and E. The formulas given are approximations that ignore the effect of *pf* on *fint* via the effect of vehicle stock on vehicle usage combined with the effect of vehicle usage on fuel intensity. This combined effect is especially small because it involves the triple product $\beta^v_2\alpha^{mv}\alpha^{fm}$, all of whose values are small.

6. Suppose we write the original model as $y_t = x'_t\beta + u_t$, with x_t including the lagged dependent variable and u_t first-order serially correlated with parameter ρ. The transformed model is then $y_t = \rho y_{t-1} + (x_t - \rho x_{t-1})'\beta + \varepsilon_t$ with ε_t serially uncorrelated. This transformation is standard for autocorrelated models in the computer package Eviews 5: see Quantitative Micro Software (2004), equation (17.10). It is better here than the Cochrane-Orcutt procedure because the latter is statistically biased when the model contains a lagged dependent variable (Davidson and MacKinnon, 1993, p. 336).

3.3.1 Dependent Variables

M: Vehicle miles traveled (VMT) divided by adult population, by state and year (logarithm: *vma*, for "vehicle-miles per adult").

V: Vehicle stock divided by adult population (logarithm: *vehstock*).

$1/E$: Fuel intensity, F/M, where F is highway use of gasoline (logarithm: *fint*).

3.3.2 Independent Variables other than CAFE

P_M: Fuel cost per mile, P_F/E. Its logarithm is denoted $pm \equiv \ln(P_F) - \ln(E) = pf + fint$. For convenience in interpreting interaction variables based on *pm*, we have normalized it by subtracting its mean over the sample.

P_V: Index of real new vehicle prices (1987=100) (logarithm: *pv*).[7]

P_F: Price of gasoline, deflated by consumer price index (1987=100) (cents per gallon). Variable *pf* is its logarithm normalized by subtracting the sample mean.

X_M, X_V, X_E: See Appendix A. X_M includes $(pm)^2$ and interactions between normalized *pm* and two other normalized variables: log real income (*inc*) and fraction urbanized (*Urban*). All equations include time trends to proxy for unmeasured systemwide changes such as residential dispersion, other driving costs, lifestyle changes, and technology.

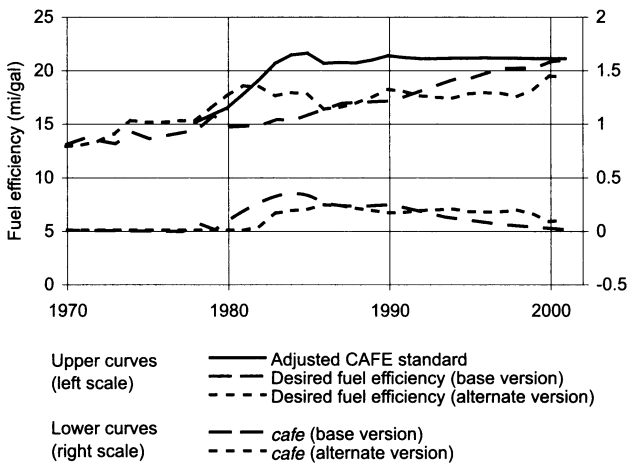
3.3.3 Variable to Measure CAFE Regulation (R_E)

We define a variable measuring the tightness of CAFE regulation, starting in 1978, based on the difference between the mandated efficiency of new passenger vehicles and the efficiency that would be chosen in the absence of regulation. The variable becomes zero when CAFE is not binding or when it is not in effect. In our system, we interpret this variable as helping to explain the efficiency of new passenger vehicles, while the lagged dependent variable in the fuel-intensity equation captures the inertia due to slow turnover of the vehicle fleet.

The calculation proceeds in four steps, described more fully in Appendix B. First, we estimate a reduced-form equation explaining log fuel intensity from 1966-1977. Next, this equation is interpreted as a partial adjustment model, so that the coefficient of lagged fuel intensity enables us to form a predicted desired fuel intensity for each state in each year, including years after 1977. Third, for a given year, we average desired fuel intensity (in levels, weighted by vehicle-miles traveled) across states to get a national desired average fuel intensity. Finally, we compare the reciprocal of this desired nationwide fuel intensity to the minimum

7. We include new-car prices in the second equation as indicators of the capital cost of owning a car. We exclude used-car prices because they are likely to be endogenous; also reliable data by state are unavailable.

Figure 1. Desired and Mandated Fuel Efficiencies and Corresponding *cafe* Variables



efficiency mandated under CAFE in a given year (averaged between cars and light trucks using VMT weights, and corrected for the difference between factory tests and real-world driving). The variable *cafe* is defined as the difference between the logarithms of mandated and desired fuel efficiency, truncated below at zero.

The comparison is shown in Figure 1. We see that the desired efficiency of new vehicles (upper curve with long dashes) was mildly increasing over much of our time period, especially 1975-1979 and 1984-1997. There were one-year upticks in 1974 and 1979, presumably due to queues at gasoline stations,[8] and some leveling in 1988-1991, 1998, and 2001 due to decreases in real fuel prices. The CAFE standard exhibited a very different pattern, rising rapidly from 1978-1984 and then flattening out. We can see that by this criterion, the CAFE standard has been binding throughout its time of application, but that its tightness rose dramatically during its first six years and then gradually diminished. This pattern, shown in the lower part of the figure (curve with long dashes) is obviously quite different from a trend starting at 1978 and from the CAFE standard itself, both of which have been used as a variable in VMT equations by other researchers.

Underlying our approach is a view of the CAFE regulations as exerting a force on every state toward greater fuel efficiency of its fleet, regardless of the desired fuel efficiency in that particular state. This reflects the fact that the CAFE standard applies to the nationwide fleet average for each manufacturer; the manufacturer therefore has an incentive to use pricing or other means to improve fuel efficiency everywhere, not just where it is low.

8. The uptick in 1979 is due to our assumption that the gasoline queues in 1979 would have the same effect on desired efficiency as those in 1974, which are captured by the 1974 dummy variable in the equation for fuel intensity fit on 1966-1977 data.

Also shown in Figure 1 is an alternate calculation of desired fuel efficiency (upper curve with short dashes). For this calculation, also explained more fully in Appendix B, we first construct a preliminary *cafe* variable as just described except omitting the trend variable. We then re-estimate the reduced-form equation for desired log fuel intensity using the entire sample period, and including this preliminary *cafe* variable. Desired fuel efficiency is then computed as before but without the effect of *cafe*. While in this alternate version the equation for desired efficiency is estimated with greater precision, it is less robust with respect to inclusion or omission of trend variables, so we prefer our original ("base") version for subsequent statistical analysis. As we shall see, they give nearly identical results for the rebound effect.

3.3.4 Data Summary

Table 1 shows summary statistics for the data used in our main specification. We show them for the original (unlogged) version of variables; we also show the logged version, after normalization, of those variables that enter the specification through interactions.

4. RESULTS

4.1 Structural Equations

The results of estimating the structural system are presented in Tables 2-4, excluding the fixed-effect coefficients. Each table shows two different estimation methods: three-stage least squares (3SLS) and ordinary least squares (OLS).[9]

The VMT equation (Table 2) explains the amount of driving by the average adult for a constant vehicle stock. Most coefficients are measured with good precision and demonstrate strong and plausible effects. We discuss those involving *pm* in Section 4.2. The income-elasticity of vehicle travel (conditional on fleet size and efficiency), at the mean value of *pm*, is 0.11 in the short run and 0.11/(1-0.79)=0.53 in the long run. An adult tends to travel more if there is a larger road stock (negative coefficient on *adults/road-mile*) and if that adult is responsible for more total people (*pop/adult*). Our measure of urbanization (*Urban*) has a statistically significant negative effect on driving; but the effect is small, perhaps

9. We also carried out estimations by two-stage least squares (2SLS) and generalized method of moments (GMM), as discussed in Small and Van Dender (2006). For GMM, 3SLS and 2SLS, the list of instrumental variables includes one lagged value of each exogenous variable and two lagged values of each dependent variable; see Fair (1984, pp. 212-213) or Davidson and MacKinnon (1993, section 10.10). In addition, the inclusion in our specification of pm^2 $\equiv (pf+fint)^2$ requires including as instruments those combinations of variables that appear when *fint* is replaced by its regression equation and $(pf+fint)^2$ is expanded. Following Wooldridge (2002), we do not include every combination separately, but instead use combinations of the composite variable *fint_inst*, defined as the predicted value of *fint* based on the coefficients of an OLS estimate of the *fint* equation.

Table 1. Summary Statistics for Selected Variables

Name	Definition	Mean	Std. Dev.	Min.	Max.
Vma	VMT per adult	10,929	2,538	4,748	23,333
Vehstock	Vehicles per adult	0.999	0.189	0.453	1.743
Fint	Fuel intensity (gal/mi)	0.0615	0.0124	0.0344	0.0919
Pf	Fuel price, real (cents/gal)	108.9	23.5	60.3	194.9
pf	log Pf, normalized	0	0.2032	-0.5696	0.6033
Pm	Fuel cost/mile, real (cents/mi)	6.814	2.275	2.782	14.205
pm	log Pm, normalized	0	0.3490	-0.8369	0.7935
Income	Income per capita, real	14,588	3,311	6,448	27,342
inc	log Income, normalized	0	0.2275	-0.7909	0.6538
Adults/road-mile	Adults per road mile	57.73	68.27	2.58	490.20
Pop/adult	Population per adult	1.4173	0.0901	1.2265	1.7300
Urban	Fraction of pop. in urban areas	0.7129	0.1949	0.2895	1.0000
Railpop	Fraction of pop. in metro areas served by heavy rail	0.0884	0.2073	0.0000	1.0000
Pv	Price of new vehicles (index)	1.066	0.197	0.777	1.493
Interest	Interest rate, new-car loans (%)	10.83	2.41	7.07	16.49
Licenses/adult	Licensed drivers per adult	0.905	0.083	0.625	1.149

Notes: Units are as described in Appendix A.
Variables with capitalized names are shown as levels, even if it is their logarithm that enters our specification. "Urban" is shown here unnormalized, but it is normalized when entering our specification.

indicating that *adults/road-mile* better captures the effects of congestion.[10] The availability of rail transit has no discernible effect, probably because it does not adequately measure the transit options available. The two years 1974 and 1979 exhibited a lower usage, other things equal.[11]

The negative effect of *adults/road-mile* can equivalently be viewed as confirmation that increasing road capacity produces some degree of induced demand, a result found by many other researchers. Our implied long-run elasticity of VMT with respect to road-miles, holding fleet constant, is 0.1. This is considerably smaller than the long-run elasticities with respect to *lane*-miles of 0.8 found by Goodwin (1996, p. 51) and Cervero and Hansen (2002, p. 484), probably because road-miles are an inadequate measure of capacity.

The coefficient on the lagged dependent variable implies considerable inertia in behavior, with people adjusting their travel in a given year by just 21 percent of the ultimate response to a permanent change. The equation exhibits only mild autocorrelation, giving us confidence that our specification accounts for most influences that move sluggishly over time.

10. The long-run difference in log(VMT) between otherwise identical observations with the smallest and largest urbanization observed in our sample is 0.19, whereas the corresponding variation with *adults/road-mile* is = 0.51.

11. We get nearly identical coefficients if we include separate dummy variables for 1974 and 1979, so we combine them for parsimony and to simplify the construction of the variable *cafe*.

Table 2. Vehicle-Miles Traveled Equation

	Estimated Using 3SLS		Estimated Using OLS	
Variable	**Coefficient**	**Stndrd. Error**	**Coefficient**	**Stndrd. Error**
vma(t-1)	**0.7907**	0.0128	**0.7421**	0.0158
vehstock	**0.0331**	0.0110	**0.0478**	0.0126
pm	**-0.0452**	0.0048	**-0.0852**	0.0051
pm^2	-0.0104	0.0068	*0.0152*	0.0088
pm*inc	**0.0582**	0.0145	**0.0768**	0.0194
pm*Urban	**0.0255**	0.0106	0.0159	0.0144
inc	**0.1111**	0.0141	**0.1103**	0.0157
adults/road-mile	**-0.0203**	0.0049	**-0.0178**	0.0068
pop/adult	**0.1487**	0.0461	0.0238	0.0513
Urban	**-0.0548**	0.0202	**-0.0514**	0.0226
Railpop	-0.0056	0.0063	-0.0002	0.0089
D7479	**-0.0442**	0.0035	**-0.0367**	0.0035
Trend	0.0004	0.0004	**-0.0009**	0.0004
constant	**1.9950**	0.1239	**2.5202**	0.1522
rho	**-0.0942**	0.0233	-0.0147	0.0295
No. observations	1,734		1,734	
Adjusted R-squared	0.9801		0.9809	
S.E. of regression	0.0317		0.0311	
Durbin-Watson stat	1.9181		1.9927	
Sum squared resid	1.6788		1.6156	

Notes: Bold or italic type indicates the coefficient is statistically significant at the 5% or 10% level, respectively. Estimates of fixed effects coefficients (one for each state except Wyoming) not shown.

OLS here means single-equation least squares accounting for autocorrelation but with no instrumental variables. It is estimated non-linearly (see note 6).

Variables *inc*, *Urban*, and the components of *pm* are normalized by subtracting their sample mean values, prior to forming interaction variables. Thus the coefficient of any non-interacted variable gives the effect of that variable on *vma* at the mean values of the other variables.

OLS overestimates the rebound effect, possibly because it ignores reverse causation between VMT and cost per mile. In this particular model, OLS overestimates the absolute value of the structural coefficient of cost per mile by 88%.

In the vehicle stock equation (Table 3), the cost of driving a mile has no significant effect. New-car price and income do have significant effects, as do road provision (*adults/road-mile*), the proportion of adults having drivers' licenses (*licences/adult*), and credit conditions (*interest*). As expected, there is strong inertia in expanding or contracting the vehicle stock, as indicated by the coefficient 0.845 on the lagged dependent variable. This means that any short-run effect on vehicle ownership, for example from an increase in income, will be magnified by a factor of 1/(1-0.845) = 6.45 in the long run. This presumably reflects the transaction costs of buying and selling vehicles as well as the time needed to adjust planned travel behavior.

Table 3. Vehicle Stock Equation

	Estimated Using 3SLS		Estimated Using OLS	
Variable	**Coefficient**	**Stndrd. Error**	**Coefficient**	**Stndrd. Error**
vehstock(t-1)	**0.8450**	0.0148	**0.8397**	0.0152
vma	0.0238	0.0161	**0.0434**	0.0148
pv	**-0.0838**	0.0383	**-0.0792**	0.0391
pm	-0.0009	0.0065	0.0065	0.0065
inc	**0.0391**	0.0155	**0.0330**	0.0156
adults/road-mile	**-0.0228**	0.0070	**-0.0214**	0.0072
interest	**-0.0143**	0.0071	**-0.0176**	0.0073
licenses/adult	**0.0476**	0.0191	**0.0525**	0.0197
Trend	**-0.0015**	0.0008	*-0.0014*	0.0008
constant	-0.0618	0.1581	*-0.2480*	0.1463
rho	**-0.1319**	0.0281	**-0.1238**	0.0290
No. observations	1,734		1,734	
Adjusted R-squared	0.9645		0.9645	
S.E. of regression	0.0360		0.0360	
Durbin-Watson stat	1.9487		1.9548	
Sum squared resid	2.1668		2.1639	

Notes: Bold or italic type indicates the coefficient is statistically significant at the 5% or 10% level, respectively. Estimates of fixed effects coefficients (one for each state except Wyoming) not shown.

OLS here means single-equation least squares accounting for autocorrelation but with no instrumental variables. It is estimated non-linearly (see note 6).

The results for fuel intensity (Table 4) show a substantial effect of annual fuel cost (*vma+pf*) in the expected direction.[12] This is consistent with prior strong evidence that people respond to fuel prices by altering the efficiency of new-car purchases. The results also suggest that CAFE regulation had a substantial effect of enhancing the fuel efficiency of vehicles – at its maximum value of 0.35 in 1984, the *cafe* variable increased long-run desired fuel efficiency by 21 percent.[13] Urbanization increases fuel efficiency, perhaps due to a preference for small cars in areas with tight street and parking space. The time trends show a gradual tendency toward more fuel-efficient cars, starting in 1974 and accelerating in 1980—probably reflecting the gradual development and dissemination of new automotive technology in response to the fuel crises in those years. Like vehicle stock, fuel intensity demonstrates considerable inertia.

12. With this rich specification, attempting to identify separate effects of the two components of annual fuel cost, namely *pf* and *vma*, produces numerical problems. If we omit one or more interactions in the equation for *vma*, the numerical problems disappear and we then find that the effect of fuel price entered separately remains strong.

13. The alternative version of the cafe variable, described earlier and depicted in Figure 1, reaches its maximum value in 1986, at which time it increases long-run desired fuel efficiency by 18% as calculated from the estimates presented in Appendix Table B2.

Table 4. Fuel Intensity Equation

	Estimated Using 3SLS		Estimated Using OLS	
Variable	**Coefficient**	**Stndrd. Error**	**Coefficient**	**Stndrd. Error**
fint(t-1)	**0.8138**	0.0137	**0.7894**	0.0162
vma+pf	**-0.0460**	0.0069	**-0.0934**	0.0075
cafe	**-0.1011**	0.0115	**-0.1018**	0.0144
inc	0.0025	0.0163	0.0082	0.0172
pop/adult	-0.0111	0.0691	0.0607	0.0814
Urban	**-0.1500**	0.0522	**-0.1528**	0.0663
D7479	**-0.0105**	0.0045	-0.0056	0.0046
Trend66-73	0.0006	0.0010	0.0015	0.0013
Trend74-79	**-0.0024**	0.0010	0.0006	0.0012
Trend80+	**-0.0037**	0.0004	**-0.0047**	0.0005
constant	-0.1137	0.0809	**0.2357**	0.0903
rho	**-0.1353**	0.0236	**-0.0966**	0.0292
No. observations	1,734		1,734	
Adjusted R-squared	0.9604		0.9611	
S.E. of regression	0.0398		0.0394	
Durbin-Watson stat	1.9515		2.0571	
Sum squared resid	2.6424		2.5961	

Notes: Bold or italic type indicates the coefficient is statistically significant at the 5% or 10% level, respectively. Estimates of fixed effects coefficients (one for each state except Wyoming) not shown.

OLS here means single-equation least squares accounting for autocorrelation but with no instrumental variables. It is estimated non-linearly (see note 6).

4.2 Rebound Effects and Other Elasticities

Table 5 shows the cost-per-mile elasticity of driving (the negative of the rebound effect) and some other elasticities implied by the structural models. Our best estimate of the average rebound effect in this sample is 4.5% in the short run and 22.2% in the long run.

Use of OLS overestimates the short- and long-run rebound effects by 88% and 53%, respectively. The short-run OLS estimate (8.5%) is well within the consensus of the literature, whereas our 3SLS estimate is somewhat below the consensus. This comparison might suggest that many estimates in the literature are overstated because of endogeneity bias; but the difference could also be caused by the sensitivity that we observed in the OLS results with respect to slight changes in specification—whereas 2SLS and 3SLS results are quite robust.

The model for vehicle usage discerns additional influences on the rebound effect. The coefficient on *pm*inc* in Table 2 shows that a 0.1 increase in *inc* (i.e. a 10.5 percent increase in real income) reduces the magnitude of the short-run rebound effect by about 0.58 percentage points. This appears to confirm the theoretical expectation that higher incomes make people less sensitive to fuel costs. Urbanization has a smaller effect: a 10 percentage-point increase in urbanization

Table 5. Rebound Effect and Other Price Elasticities

	Estimated Using 3SLS		Estimated Using OLS	
	Short Run	**Long Run**	**Short Run**	**Long Run**
Elasticity of VMT with respect to fuel cost per mile: (a)				
At sample average	-0.0452	-0.2221	-0.0850	-0.3398
	(0.0048)	(0.0238)	(0.0052)	(0.0251)
At US 1997-2001 avg. (b)	-0.0216	-0.1066	-0.0806	-0.3216
	(0.0090)	(0.0433)	(0.0109)	(0.0438)
At US 1997-2001 avg. except *Pf* 58% higher (c)	-0.0311	-0.1531	-0.0666	-0.2648
	(0.0060)	(0.0299)	(0.0068)	(0.0311)
Elasticity of VMT with respect to new veh price	-0.0028	-0.0876	-0.0038	-0.0964
	(0.0016)	(0.0500)	(0.0021)	(0.0549)
Elasticity of fuel intensity with respect to fuel price:				
At sample average	-0.0440	-0.2047	-0.0861	-0.3480
	(0.0067)	(0.0338)	(0.0070)	(0.0404)
Elasticity of fuel consumption with respect to fuel price:				
At sample average	-0.0892	-0.4268	-0.1711	-0.6878
	(0.0058)	(0.0355)	(0.0084)	(0.0436)
At US 1997-2001 avg. (b)	-0.0667	-0.3340	-0.1671	-0.6754
	(0.0091)	(0.0451)	(0.0122)	(0.0503)
At US 1997-2001 avg. except *Pf* 58% higher (c)	-0.0758	-0.3715	-0.1543	-0.6360
	(0.0068)	(0.0384)	(0.0094)	(0.0466)

Notes:
(a) The rebound effect is just the negative of this number (multiplied by 100 if expressed as a percent).
(b) Elasticities measured at the average 1997-2001 values of *pm*, *inc*, and *Urban* for all US.
(c) Same as (b) but raise *Pm* by 58.1% (equivalently, set *Pf* =\$1.93 at 1997-2001 prices but hold *E* at its 1997-2001 observed value).

Asymptotic standard errors in parentheses are calculated from the covariance matrix of estimated coefficients using the Wald test procedure for an arbitrary function of coefficients in Eviews 5.

reduces the rebound effect by about 0.25 percentage points. Finally, fuel cost itself raises the rebound effect as expected (coefficient of *pm*^2 in Table 2), but only modestly and without statistical significance.

To get an idea of the implications of such variations, we compute the short- and long-run rebound effects for values of income, urbanization, and fuel costs of driving equal to those of the average state over the most recent five-year period covered in our data set, namely 1997-2001. Using the 3SLS results, we see that the short-run rebound effect is reduced to 2.2% and the long-run effect to 10.7% (second row in Table 5). If fuel prices in 1997-2001 had been 58 percent higher, corresponding roughly to the \$2.35 nominal price observed in the first two months of 2006, these figures would be 3.1% and 15.3%, around two-thirds the values at the sample average.[14]

As we shall see in the next subsection, we find a similarly dramatic decline in the rebound effect if we exclude *pm*^2 from the specification, but then it is virtually all explained by rising income. This alternative explanation, justified not by theory but solely by our inability to estimate the coefficient of *pm*^2 with high precision, would be less conservative than our base specification in its implications for future scenarios. We think most analysts expect a continuation of the rise in real incomes, but a reversal of the fall in real fuel prices, that occurred over most of our sample period. In our base specification, these expected trends offset each other to some degree in their effects on the rebound effect, ameliorating the large continued decline projected to occur due to rising incomes.

The second panel of Table 5 shows that higher new car prices reduce travel, but only by a small amount, with a long-run elasticity of -0.09. The third and fourth panels provide information about how fuel prices affect fuel intensity and overall fuel consumption. The fuel-price elasticity of fuel intensity, given by equation (9), is estimated with good precision thanks to the small standard error on the coefficient of *vma+pf* in Table 4. Adding to it the elasticity of vehicle-miles traveled gives the total price-elasticity of fuel consumption, shown in the last panel of the table. The long-run estimate is -0.43, close to the middle of recent studies. In fact, our estimates both of this elasticity and of the proportion of it due to changes in vehicle travel (0.2221/0.4268 = 52%) are in line with the literature as reviewed by Parry and Small (2005).[15] Note, however, that the proportion caused by changes in vehicle travel decreases to 32% in the last five years of our sample.

Our results suggest that the response to fuel prices has become increasingly dominated by changes in fuel efficiency rather than changes in travel. Whether this remains the case after 2001 depends on how incomes and fuel costs of driving evolve.

4.3 Robustness Checks and Caveats

In this section, we discuss the sensitivity of our results to several assumptions and point to certain caveats. These issues are further elaborated in Small and Van Dender (2006).

The first robustness check concerns state population data, which are used in several key variables of our specification. The data published by the US Census Bureau for non-census years are estimates that take no account of the subsequent census counts when they become available. As a result they contain anomalous jumps in census years. We therefore apply a correction by assuming that the census

14. This scenario happens to put *pm* at its sample average, and thus enables us also to see the effect of rising income without falling fuel prices.

15. They choose as the best consensus an elasticity equal to -0.55, with 40% of it caused by mileage changes. Our alternative version of the cafe variable produces a long run price elasticity of fuel consumption of -0.60, of which 40% is caused by mileage changes (Appendix Table B2).

counts are accurate and that the estimation errors between census years grow linearly over that ten-year time interval. If we instead use the published data directly, or if we use a linear interpolation instead of our correction, the impact on results is noticeable but not major: they yield a long-run rebound effect of 25.8% and 20.6%, respectively, which bracket the result of 22.2% using our preferred data.[16]

Second, our finding that the rebound effect declines over time is supported by estimates using three twelve-year time periods instead of the full time series, although the estimates are less precise and less robust. Specifically, the point estimates for 1966-77 and 1978-89 produce long run rebound effects of approximately 19%, while the result from the 1990-2001 sample is 8.3%. This can be compared to point estimates from the full model of 32.3%, 23.6%, and 10.1% for the three time periods.

Third, in order to check the dependence of our rebound effect estimates on the imprecisely estimated coefficient on *pm*^2, we estimate a model where that term is omitted (despite the theoretical justification for its inclusion). The two specifications lead to similar estimates of the rebound effect, both for the sample average conditions and for 1997-2001. But as noted earlier, the inclusion of *pm*^2 reduces the extent to which the rebound effect declines with income, making our preferred specification more conservative when evaluating the impact of future income growth.

Fourth, we estimated the model using a Generalized Method of Moments estimator, as Bertrand et al. (2004) suggest that 3SLS may underestimate standard errors when there is heteroscedasticity or there are unobserved time-varying effects. However, we find that GMM produces very similar standard errors and, for statistically significant coefficients, similar point estimates.

While the robustness of our results is encouraging, a number of major caveats remain. First, the methods for collecting state VMT data are notoriously imperfect and differ among states (some rely on sporadic vehicle counts, others use information on fuel efficiency and fuel consumption). However, we think these problems do not bias our results. The sources of measurement error are mostly unrelated to the independent variables, and persistent errors within a given state are accounted for by state fixed effects. Correlation between measurement errors in VMT and fuel efficiency, which could appear in states where the VMT data are derived from fuel consumption data, would bias estimates from OLS but not from 2SLS or 3SLS.

A second caveat is that our study, like virtually all others, imposes the theoretical restriction that people choosing how much to drive care about the fuel cost of driving a mile, but not separately about its individual components (fuel price and fuel efficiency). Unfortunately, we find that this restriction is not supported by a model that relaxes it – in fact, the latter model suggests that the amount of driving responds to fuel prices but not to changes in fuel efficiency.

16. This difficulty in measuring adult population, by state and year, discourages us from seeking to refine our specification with additional variables measuring the age distribution of the adult population, even though age is known to affect vehicle ownership and travel.

Furthermore, in the unrestricted model the fuel-price elasticities are essentially identical to those in the restricted model, including the way they vary with income and fuel cost. However, the overall performance of this unrestricted model is unsatisfactory, especially its dynamic properties. This leads us to conclude that the theoretical constraint is justified although unproven.

Another caveat is that the short time series that we use for constructing the variable *cafe* does not allow us to precisely estimate all factors deemed relevant. In particular, the separate role of fuel prices and time trends in shaping the pattern of desired fuel efficiency is hard to determine. While the results concerning the rebound effect are hardly affected when alternative versions of the variable are used, the estimates of the price elasticity of fuel demand are somewhat sensitive, as is the strength of the *cafe* variable itself. We conclude that the degree to which the CAFE regulations have affected fleet fuel efficiency remains uncertain, but probably is bracketed by the results using our two versions of the regulatory variable *cafe* (Appendix Table B2). The effect of CAFE remains an interesting area for future research, and we believe our approach to it is an improvement over previous attempts. To make further progress probably requires disaggregating the passenger-vehicle fleet into at least the two categories, cars and light trucks, that are regulated differently under CAFE.

Finally, traffic congestion is an endogenous part of the system explaining reactions to changes in fuel efficiency. Presumably, any increased congestion would curtail the increased travel predicted by our model. To say how much, one could combine a model of congestion formation with a model like ours but containing an explicit congestion variable. This would go beyond current definitions of the rebound effect by incorporating not only vehicle manufacturers but road conditions into the system assumed to respond to energy regulation.

5. CONCLUSION

Our study supports many earlier findings that the long-run rebound effect, i.e. the elasticity by which changes in fuel efficiency affect the amount of driving, was 20-25% in the U.S. over the last third of the 20th century. What is new is evidence that the rebound effect diminishes with income, and possibly increases with the fuel cost of driving. Since incomes have risen and real fuel costs have fallen, the rebound effect has declined considerably over time. For example, our results suggest it was less than half as large in the years 1997-2001 than over the entire sample. The rebound effect is likely to diminish still further as rising incomes reduce the significance of fuel costs in decisions about travel, although this may be offset to some extent by increases in fuel prices.

This result is relevant to policy. For example, the recent debate over whether to strengthen fuel-efficiency standards has emphasized the potential adverse effects on traffic congestion (e.g. Portney et al., 2003). If the rebound effect has become smaller over time, these adverse effects will be smaller than has been thought. More generally, quantity standards are relatively more attractive compared to fuel taxes if the secondary effects of the standards on other consumer decisions are small. Put differently, if most of the elasticity of fuel consumption with respect to price reflects changes in the fuel efficiency of vehicles, as our results imply, then it is easier to design standards whose effects on fuel consumption and driving are similar to those of taxes. Their effects on fuel tax revenues, of course, are still different.

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APPENDIX A: CONTROL VARIABLES

Control Variables in (4):

X_M: Real personal income per capita at 1987 prices, in log form and normalized by subtracting the sample mean (*inc*);[17] number of adults divided by public road mileage (logarithm: *adults/road-mile*) as a rough measure of potential congestion; ratio of total population to adults (logarithm: *pop/adult*) as a measure of family size; fraction of state's population living in metropolitan statistical areas, normalized by subtracting its mean in the sample (*Urban*); fraction of the state's population living in metropolitan statistical areas with a heavy-rail transit system (*Railpop*); a dummy variable to represent gasoline supply disruptions in 1974 and 1979 (D7479); and a time trend measured in years since 1966 (*Trend*), intended to capture changes in technology and consumer preferences that we are unable to specify quantitatively. X_M also includes two interaction variables: *pm·inc* and *pm·Urban*.

X_V: This set of variables includes *inc*, *adults/road-mile*, and *Trend*, already defined in X_M. In addition: the national interest rate for auto loans (logarithm: *interest*); and the ratio of licensed drivers to adults (logarithm: *licences/adult*).

X_E: These variables include six of the variables in X_M, namely *inc*, *adults/road-mile*, *pop/adult*, *Urban*, *Railpop*, and *D7479*. In addition we allow for the possibility of three distinct time trends in fuel efficiency: one before the OPEC embargo (1966-1973), another between the embargo and the Iranian revolution (1974-1979), and a third after the Iranian revolution (1980-2001). The rationale is that these events changed people's perception of long-term prospects for oil supplies and therefore may have affected research and development efforts related to fuel efficiency. On the assumption that changes in technology cannot happen immediately, these variables (*Trend66-73*, *Trend74-79*, *Trend80+*) are specified in such a way that there is a break in the slope of the trend line but not a sudden "jump" from one regime to another. Specifically, *Trend66-73* = Min(*Trend*,7); *Trend74-79* = Max[0, Min[(*Trend*-7),6]}; and *Trend80+* = *Trend -Trend66-73 - Trend74-79*.

17. Using disposable instead of personal income produces results barely distinguishable from those presented here.

APPENDIX B: VARIABLE MEASURING STRENGTH OF CAFE REGULATION

Base Version

We use the following steps to create the variable *cafe*.

1. We first estimate the reduced-form equation explaining fuel intensity on data only from 1966-1977, with no regulatory variable included (since there was no regulation then). This equation should in principle include all exogenous variables from all three models (including P_V for the V equation); we simplify it by dropping the variable *Railpop*, which has little effect. Like our other equations, it also includes one lag of the dependent variable, and allows for fixed effects and autocorrelated errors. It does not include other endogenous variables, either current or lagged; the reason is that, unlike in an instrumental variables regression, our objective is to estimate a predictive model for what fuel intensity would have been in the absence of CAFE regulation and therefore we cannot use information about what actually happened to the endogenous variables. In theory, this equation could include any number of lagged values of independent variables, because they would be present in a complete solution of system (1) for the time path of *fint*; however on this very short time series it is impractical to estimate so many parameters, especially of variables that are highly correlated as current and lagged values are likely to be. For the same reason of parsimony, we included only a single time trend in this predictive equation.

We denote this equation by:

$$(fint)_{i,t} = \alpha^{fR} \cdot (fint)_{i,t-1} + \beta^{fR} X_{it}^{fR} + u_{it} \tag{B.1}$$

where i designates a state, superscript R indicates the reduced form, and X^{fR} denotes the set of all exogenous variables used, including prices, as described above. The results of this estimation are shown in the first column of Table B1. The statistically significant coefficients are those of $(fint)_{t-1}$, $D7479$, and pv. The price of fuel is not statistically significant but has the reasonable value of -0.021.

2. The coefficient α^{fR} of the lagged dependent variable is interpreted as arising from the following partial adjustment model:

$$(fint)_{i,t} = (fint)_{i,t-1} + \gamma \cdot \{(fint)^*_{i,t} - (fint)_{i,t-1}\} + u_{it} \tag{B.2}$$

where $(fint)^*_{i,t}$ denotes a long-run desired value for the logarithm of fuel intensity. That is, users and manufacturers basing decisions in year t desire to shift the vehicle stock toward one with fuel efficiency $(fint)^*_{i,,t}$ but they can do so only part way by changing a portion γ of the stock in that year. Thus it is natural to interpret $(fint)^*_{i,t}$ as the target fuel efficiency for new car purchases and γ as the fraction of the fleet that turns over each year. It is easy to see that (B.2) is the same as (B.1) if we choose $\gamma = 1 - \alpha^{f}$ and

$$(fint)^{*}_{i,t} = \frac{\beta^{fR} X^{fR}_{it}}{1 - \alpha^{fR}}. \tag{B.3}$$

3. From the estimated values of $(fint)^{*}_{i,,t}$, for each state and year, we compute the US average desired fuel intensity, averaged the same way as vehicles are averaged under CAFE regulations: namely,

$$(FintUS)^{*}_{t} = \frac{\sum_{i} M_{it} \exp\,(fint)^{*}_{i,t}}{\sum_{i} M_{it}}. \tag{B.4}$$

where M_{it} is aggregate VMT for state i in year t.

4. Finally, we assume CAFE is binding whenever the desired efficiency $E^{*}_{t} \equiv 1/FintUS^{*}_{t}$ is less than the minimum mandated efficiency, $\bar{E}_{t}$. The latter is computed as a weighted average of the CAFE standards for light trucks and cars, the weights being current nationwide light truck and car VMT, reduced by 16%, which is an estimate of the difference between fuel efficiency achieved in real driving and that achieved on the tests used to enforce the CAFE standard (Harrington, 2003). A measure of the strength of CAFE regulation is then $R_{E} = \max\,\{(\bar{E}_{t} / E_{t}), 1\}$ or its logarithm,

$$cafe = \max\,\{(\bar{e}_{t} - e^{*}_{t}), 0\} \tag{B.5}$$

where and $\bar{e}_{t} = \ln\,(\bar{E}_{t})$ and $e^{*}_{t} = \ln(E^{*}_{t})$. Note this measure is nationwide, not state-specific.

Alternate Version

For our alternate version of variable *cafe*, we begin with a slightly modified version of the variable just described (*cafe_prelim*). The modification is that we omitted the time trend (*Trend* in Table B1), which plays a dominant role in producing the generally upward slope of the desired fuel-efficiency variable shown in Figure 1. We interpret the effect of *Trend* as the result of technological changes making fuel efficiency easier to achieve; but there is some risk in projecting such a trend, estimated on 1966-1977 data, forward to 2001. Here, instead of relying on *Trend*, we estimate the full reduced-form model on the longer time period.

Specifically, we estimate the same reduced-form model as before, but with three changes: we use the full data set, delete *Trend*, and add *cafe_prelim*. As can be seen from Table B1, the precision of estimates is much better and both fuel price (*pf*) and CAFE regulation (*cafe_prelim*) have statistically significant effects in the expected direction. The coefficient of the price of new vehicles

Table B1. Fuel Intensity Equation: Reduced Form for Estimating Desired Fuel Efficiency

	Base Version (1966-1977)		Alternate Version (1966-2001)	
Variable	**Coefficient**	**Std. Error**	**Coefficient**	**Std. Error**
fint(t-1)	**0.6523**	0.0434	**0.8667**	0.0148
pf	-0.0253	0.0204	**-0.0231**	0.0087
Inc	0.0081	0.0291	-0.0152	0.0173
adults/road-m	0.0377	0.0268	-0.0002	0.0075
pop/adult	-0.1848	0.1626	0.0720	0.0703
Urban	-0.2465	0.2294	*-0.1265*	0.0662
D7479	**-0.0212**	0.0060	-0.0013	0.0046
Trend	**-0.0123**	0.0027		
pv	**-0.2251**	0.0797	**0.0844**	0.0246
Interest	0.0294	0.0301	*-0.0130*	0.0077
licences/adult	0.0294	0.0255	**0.0537**	0.0178
cafe_prelim			**-0.0545**	0.0150
constant	**-0.9340**	0.2124	**-0.4356**	0.0704
Rho	**-0.1374**	0.0614	**-0.1585**	0.0281
No. of observations	1734		1734	
Adjusted R-squared	0.8967		0.9582	
S.E. of regression	0.0253		0.0409	
Sum squared resid	0.2858		2.7914	
Durbin-Watson stat	1.9975		1.9703	

Note: 50 constants for individual states are not shown.

(*pv*) now has the opposite sign; apparently this variable, which trends downward throughout the period, now takes on the job of explaining long-term trends. Since we have no prior belief about the sign of this coefficient, we cannot say which result is more plausible.

We then calculate our alternate version of desired fuel efficiency with these estimated coefficients, using Step 2 above except setting the values of variable *cafe_prelim* to zero, (to represent the counter-factual absence of pressure from CAFE regulations). The rest of the calculation proceeds as in Steps 3 and 4 above.

Table B2 compares selected results of estimating our structural model with the two versions of the *cafe* variable. They are very similar except for the *fint* equation. The coefficient of *vma+pf* is much larger and more precisely estimated using the alternate version. The coefficient of *cafe* is also larger (though not as precisely estimated) and the adjusted R-squared value is slightly larger. On the negative side, we find that with our alternate calculation procedure, our estimates of desired fuel efficiency are not robust to adding trend variables in the reduced-form equation itself. In the end, we can offer no judgment about which version of *cafe* better depicts the tightness of regulations as perceived by market participants.

Table B2. Comparison of Selected Structural Estimates: 3SLS

	Using Base Version of *cafe*		Using Alternate Version of *cafe*	
Variable	**Coefficient**	**Std. Error**	**Coefficient**	**Std. Error**
Vehicle-Miles Traveled Equation (Table 2)				
vma(t-1)	**0.7907**	0.0128	**0.8081**	0.0127
pm	**-0.0452**	0.0048	**-0.0445**	0.0048
pm^2	-0.0104	0.0068	-0.0074	0.0069
pm*inc	**0.0582**	0.0145	**0.0560**	0.0148
Fuel Intensity Equation (Table 4)				
fint(t-1)	**0.8138**	0.0137	**0.8075**	0.0154
vma+pf	**-0.0460**	0.0069	**-0.0813**	0.0099
cafe	**-0.1011**	0.0115	**-0.1368**	0.0219
D7479	**-0.0105**	0.0045	-0.0015	0.0044
Trend74-79	**-0.0024**	0.0010	-0.0012	0.0011
Trend80+	**-0.0037**	0.0004	**-0.0029**	0.0004
rho	**-0.1353**	0.0236	**-0.1121**	0.0245
Rebound Effect and Other Price Elasticities				
Elasticity	**Short Run**	**Long Run**	**Short Run**	**Long Run**
Elasticity of VMT with respect to fuel cost per mile:				
At sample average	-0.0452	-0.2221	-0.0446	-0.2398
At US 1997-2001 avg.	-0.0216	-0.1066	-0.0242	-0.1308
Elasticity of fuel consumption with respect to fuel price:				
At sample average	-0.0892	-0.4268	-0.1226	-0.5993
At US 1997-2001 avg.	-0.0667	-0.3340	-0.1037	-0.5207

Note: Not all coefficients are listed here. For additional results, see Small and Van Dender (2006), App. B.