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Publication Date
2005-09-21

Energy Policy and Economics 014

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September 2005

This paper is part of the University of California Energy Institute's (UCEI) Energy Policy and Economics Working Paper Series. UCEI is a multi-campus research unit of the University of California located on the Berkeley campus.

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The Effect of Improved Fuel Economy on Vehicle Miles Traveled: Estimating the Rebound Effect Using U.S. State Data, 1966-2001

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This version: September 2, 2005

Abstract:

We estimate the rebound effect for motor vehicles, by which improved fuel efficiency causes additional travel, using a panel 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 interacts the rebound effect with income. At sample averages of variables, our 3SLS estimates of the short- and long-run rebound effect are 4.7% and 22.0%. But they decline substantially with income: with variables at 1997-2001 levels they become 2.6% and 12.1%, considerably smaller than typically assumed for policy analysis.

JEL-codes: Q0, D5, R4, C2

Keywords: carbon dioxide, fuel economy, travel demand, motor vehicle use, rebound effect

Acknowledgment:

This paper is based on research sponsored by the California Air Resources Board and the California Energy Commission. We would like to thank S. Jun and C.K. Kim for excellent research assistance. Earlier stages of this work have benefited from comments by D. Brownstone, D. Greene, W. Harrington, C. Kavalec, C. Lave, R. Mahdavi, D. Pickrell, and C. Shulock, among others. All errors, shortcomings, and interpretations are our responsibility. Nothing in this paper has been endorsed by or represents the policy of the California Air Resources Board or the California Energy Commission.

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).

For motor vehicles, the energy input is fuel and the associated service is travel, typically measured as 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 affects 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. The rebound effect is also relevant to the problem of instrument choice: if it is large, then price instruments become relatively more effective compared to technology standards because higher energy prices counteract the rebound effect.

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, as would be expected from theory (Greene, 1992) and from micro-based estimates across deciles of the income distribution (West, 2004).

Our best estimates of the rebound effect for the US as a whole, over the period 1966-2001, are 4.7% for the short run and 22.0% for the long run. The 2SLS and 3SLS results are similar except in terms of precision, and differ strongly from ordinary least squares (OLS) results. The latter are not satisfactory overall, 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 overall price-elasticity of fuel demand (-0.41) and the proportion of it that is caused by mileage changes (54%), are similar to those in the literature.

This agreement is qualified, however, by our finding that the rebound effect depends strongly and negatively on income. This dependence substantially reduces the magnitude that is relevant for current policy. For example, using average values of income and urbanization 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 2.6% and 12.1%, 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.

The structure of the paper is as follows. Section 2 introduces the definition of the rebound effect and reviews some key contributions on estimating it. Section 3 presents the 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 a definition of the rebound effect.

Two of our innovations relate directly to limitations of this standard definition of the rebound effect. First, the standard definition postulates an exogenous change in fuel efficiency E. Yet most empirical measurements of the rebound effect rely heavily on variations in the fuel price P_F, [1] in which case it is implausible that E be exogenous. This can be seen by noting the substantial differences in empirical estimates of the fuel-price elasticities of fuel consumption, $\varepsilon_{F,PF}$, and of travel, $\varepsilon_{M,PF}$.[2] As shown by USDOE (1996: 5-11), they are related by $\varepsilon_{F,PF} = \varepsilon_{M,PF} \cdot (1 - \varepsilon_{E,PF}) - \varepsilon_{E,PF}$, 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 the cost of that commute).[3]

A second limitation of the standard definition is that fuel cost is just one of several components of the cost of using motor vehicles. Another is time cost, which is likely to increase as incomes grow. That increase should make $|\varepsilon_{M,PM}|$ diminish with income (Greene, 1992), a dependence allowed by our specification. A related extension is to recognize that traffic congestion may be affected by the VMT changes that create the rebound effect. If congestion is substantially increased, the rebound effect would be diminished, yet even a smaller rebound may

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[1] Most studies assume that that travel responds to fuel price P_F and efficiency E with equal and opposite elasticities, as implied by the definition of the rebound effect based on the combined variable $P_M=P_F/E$. See for example Schimek (1996), Table 2 and Greene et al. (1999), fn. 6.

[2] See USDOE (1996, pp. 5-14 and 5-83 to 5-87); Graham and Glaister (2002, p. 17); and the review in Parry and Small (2005).

[3] This seems the most likely direction of bias, although it could be the opposite. For example, the person with a long commute may register a *lower* average fuel economy on a given vehicle because a higher proportion of it is used during stop-and-go traffic than someone who mostly uses the car for off-peak or vacation travel. Also, someone with a long commute might invest in fuel-consuming amenities like air conditioning or a heavier vehicle.

be of greater concern due to the costly nature of congestion. We account for this by allowing the rebound effect to depend on urbanization, although empirically this turns out to be unimportant. We also allow it to be influenced by road provision, which does have the expected effect.

One set of empirical studies of the rebound effect uses aggregate time-series data. Greene (1992) uses a U.S. time series (1957-1989) on fuel prices and fuel efficiency to measure the effect of P_M on VMT: he finds the rebound effect to be between 5 and 15% both in the short and long run, with a best estimate of 12.7%. According to Greene, failing to account 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.[4] 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%).[5] Schimek (1996) uses data from a longer time period than Greene (1992) and finds an even smaller short-run rebound effect (7%); but he obtains a larger long-run rebound effect (29%), similar to Jones (1993).[6] 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 rationing was in effect. 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 as they have trouble disentangling the presence of a lagged dependent variable from the presence of autocorrelation. In particular, their estimates of these dynamic properties are sensitive to the time period considered and treatment of the CAFE standards.

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[4] Another study that found autocorrelation is that by Blair, Kaserman, and Tepel (1984). They obtain a rebound effect of 30%, based on monthly data from Florida from 1967 through 1976. They did not estimate models with lagged variables.

[5] These figures are from the linear lagged dependent variable model (model III in Table 1). Estimates for the log-linear model are nearly identical.

[6] These figures are his preferred results, from Schimek (1996), p. 87, Table 3, model (3).

Another type of study relies on a panel series of data at a smaller geographical level of aggregation. Haughton and Sarkar (1996) construct a cross-sectional time series 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 its effects on the amount of motor-vehicle travel. The authors estimate equations both for VMT per driver and for fuel intensity (the inverse of fuel efficiency), obtaining a rebound effect of about 16% in the short run and 22% in the long run.[7] Here, autocorrelation and the effects of a lagged dependent variable are measured with sufficient precision to distinguish them; they obtain a statistically 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 CAFE regulation.

It appears that the confounding of dynamics with effects of 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 National Personal Transportation Survey (NTPS),

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[7] This paragraph is based on models E and F in their Table 1, p. 115. Their variable, “real price of gasoline per mile,” is evidently the same as fuel cost per mile.

obtain a rebound effect of just 4%.[8] 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 model that 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 by the fact that measured fuel prices vary only across states, and those variations are correlated with unobserved factors that also influence VMT – factors such as residential density, congestion, and market penetration of imports. In our work, we eliminate the spurious effects of such cross-sectional correlations by using panel data with a fixed-effects specification.

Two other 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 over the years 1984-1990, as part of a larger equation system that also predicts automobile sales and prices. When estimated by Ordinary Least Squares, her usage equation implies a rebound effect (both short- and long-run, because the equation lacks a lagged variable) of about 20%.[9] 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 (again, both short- and long-run) at 23% overall, with a range from 17% for three-vehicle households to 28% for one-vehicle households.

Several micro studies estimate model systems in which vehicle choice and usage are chosen simultaneously. Thus they can account for the endogeneity of fuel efficiency.[10] Mannering (1986) explicitly addresses the bias resulting from such endogeneity, finding it to be

[8] Their model 3, with odometer readings as dependent variable. They actually measure the elasticity of VMT with respect to gasoline price, $\varepsilon_{M,PF}$, which is equal to $\varepsilon_{M,PM}$ as defined here.

[9] When estimated using instrumental variables to account for the endogeneity of vehicle type, Goldberg's estimate diminishes to essentially zero. But in that model the variables representing vehicle type attain huge yet statistically insignificant coefficients (see her Table I), casting doubt in our minds on the ability of the data set to adequately account for simultaneity.

[10] Examples include Train (1986), Hensher *et al.* (1992), Goldberg (1998), and West (2004).

large, although in the direction opposite to what we expect: his estimate of $|\varepsilon_{M,PM}|$ becomes considerably *greater* when endogeneity is taken in to account.

In summary, prior literature shows that aggregate estimates of the rebound effect, especially of the long-run effect, are sensitive to specification, especially 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 find a long-run rebound effect in the neighborhood of 20-23%, consistent with several of the aggregate studies.

3. Theoretical Foundations and Empirical Specification

3.1 System of Simultaneous Equations

Our empirical specification is based on a simple aggregate model of simultaneous demand for 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 to the relative prices of various models, consumers' adjustments via purchases of various models (including light trucks), consumers' decisions about vehicle scrappage, and driving habits.

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.

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. This produces:

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

We call this form of the demand equation “partially reduced” because *V* but not E has been eliminated (*E* being part of the definition of P_M). 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):

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

3.2 Empirical Implementation

While most studies reviewed in the previous section 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-degree 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. Finally, we specify the equations as linear in parameters and with most variables in logarithms. Thus we estimate the following system:

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

with autoregressive errors:

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

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 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”. In the third equation, we impose the further restriction $\alpha^{fm} = \beta_1^f$, which implies that fuel price and amount of travel affect the fuel efficiency choice only through their product, which is fuel expenditure; this restriction improves the OLS estimation of the fuel intensity equation and has little effect on estimates that account for simultaneity among equations.

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

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

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 or urbanization. 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 $\varepsilon_{M,PM}$ does equal β_1^m when computed at the mean values of income and urbanization 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 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, due perhaps to people’s inability to make fast adjustments in lifestyle. 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:[11]

$$-b^L = \varepsilon^L_{\tilde{M},PM} = \frac{\varepsilon_{M,PM}\cdot(1-\alpha^v) + \alpha^{mv}\beta_2^v}{(1-\alpha^m)(1-\alpha^v) - \alpha^{mv}\alpha^{vm}}. \tag{7}$$

The same considerations apply to other elasticities. It can be shown that the short- and long-run elasticities of vehicle usage with respect to new-car price are:

$$\varepsilon^S_{\tilde{M},PV} = \frac{\alpha^{mv}\beta_1^v}{1-\alpha^{mv}\alpha^{vm}}; \qquad \varepsilon^L_{\tilde{M},PV} = \frac{\alpha^{mv}\beta_1^v}{\left(1-\alpha^m\right)\left(1-\alpha^v\right)-\alpha^{mv}\alpha^{vm}} \tag{8}$$

[11] Derivations of equations (7)-(9) can be found in Small and Van Dender (2005), section 5.1.

and the short- and long-run elasticities of fuel intensity with respect to fuel price are approximately:[12]

$$-\varepsilon^{S}_{\tilde{E},PF} = \frac{\beta_1^f + \alpha^{fm}\varepsilon_{M,PM}}{1-\alpha^{fm}\varepsilon_{M,PM}}; \qquad -\varepsilon^{L}_{\tilde{E},PF} = \frac{\beta_1^f \cdot (1-\alpha^m) + \alpha^{fm}\varepsilon_{M,PM}}{(1-\alpha^f)(1-\alpha^m) - \alpha^{fm}\varepsilon_{M,PM}}. \qquad (9)$$

Our data set is a cross-sectional time series, with each state observed 36 times. We use a fixed effects specification, which is easily favored over random effects by a standard Hausman test. The commonly used two-step Cochrane-Orcutt procedure to estimate autocorrelation is known to be statistically biased when the model contains a lagged dependent variable, as ours does (Davidson and MacKinnon, p. 336). Therefore we instead transform the model to a nonlinear one with no autocorrelation but with additional lags, and estimate it using nonlinear least squares.[13]

3.3 Variables

This section describes the main variables in (4) and their rationale. We identify each variable 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. Data sources are given in Appendix A.

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*).

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[12] Equations (9) 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 $\beta_2^v \alpha^{mv} \alpha^{fm}$.

[13] This is accomplished using the computer package Eviews 5. In the first stage of 3SLS and 2SLS, each equation contains as variables the contemporary exogenous variables, one lagged value of each exogenous variable, and two lagged values of each of the three endogenous variables. See Fair (1984, ch. 6) or Davidson and MacKinnon (1993, ch. 10) for an explanation.

3.3.2 Independent Variables other than CAFE

P_M: Fuel cost per mile, P_F/E. Its logarithm is denoted $pm \equiv \ln(P_F)–\ln(E) \equiv pf+fint$. 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*).

P_F: Price of gasoline, deflated by consumer price index (1987=1.00) (cents per gallon). Its logarithm (*pf*) is normalized by subtracting the mean in the sample.

X_M, X_V, X_E: See Appendix A. X_M includes interactions between normalized *pm* and two other normalized variables: log income (*inc*) and fraction urbanized (*Urban*).

3.3.3 Variable to Measure CAFE Regulation (R_E)

We define a variable measuring the tightness of CAFE regulation, starting in 1978, as the difference between the mandated efficiency of new passenger vehicles and the efficiency that would be chosen in the absence of regulation. This difference is truncated at zero, that is, the variable is zero when CAFE is not binding or when it is not in effect. This variable influences 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 γ 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 efficiency mandated under CAFE in a given year (corrected for the difference between factory tests and real-world driving). The variable *cafe* is defined 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 was mildly increasing over much of our time period, especially 1975-1979 and 1984-1997. There were one-year upticks in 1974 and 1979, due to queues at gasoline stations,[14] and some leveling in 1988-1991, 1998, and 2001 due to decreases in 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 definition, 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 until it is just barely binding in 2001. This pattern is obviously quite different from either a trend starting at 1978, or the CAFE standard itself, both of which have been used as a variable in VMT equations by other researchers.

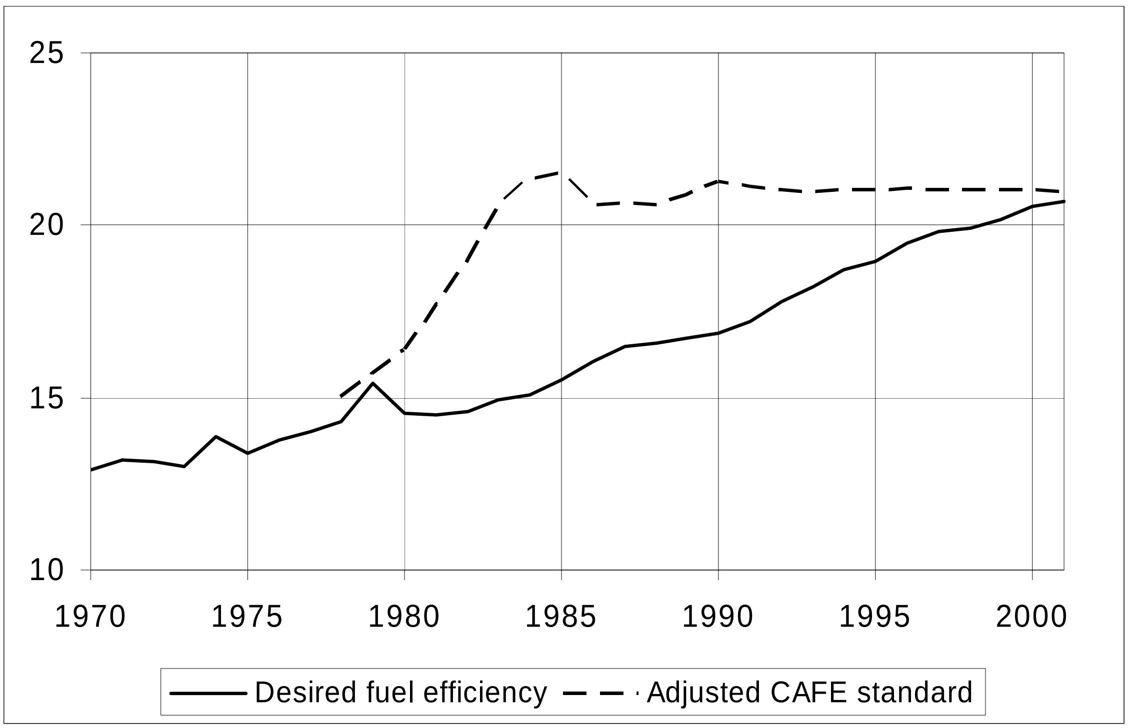

Figure 1. Desired and Mandated Fuel Efficiencies

Implicit in this definition 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. Our reason for adopting this view is that the CAFE standard applies to the

[14] 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.

nationwide fleet average for each manufacturer, who therefore has an incentive to use pricing or other means to improve fuel efficiency everywhere, not just where it is low.

3.3.4 State population data

Several variables of our specification, including the first two endogenous variables, make use of data on adult or total state population. Such data are published by the U.S. Census Bureau as midyear population estimates; they use demographic information at the state level to update the most recent census count, taken in years ending with zero. However, these estimates do not always match well with the subsequent census count, and the Census Bureau does not update them to create a consistent series. As a result, the published series contains many instances of implausible jumps in the years of the census count. For our preferred specification, we apply a correction assuming that the census counts are accurate and that the error in estimating population between them grows linearly over that ten-year time interval.[15]

We believe this approach is better than using the published estimates because it makes use of Census year data that were not available at the time the published estimates were constructed (namely, data from the subsequent census count). It should also be better than a simple linear interpolation between Census counts, because it incorporates relevant demographic information that is contained in the published population estimates.[16] The impact of using either of these alternative population estimates is noticeable but not major. The published data yield the highest estimates of the rebound effect (25.4% in the long run), while the linear interpolation produces the lowest estimate (20.8%).

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[15] We estimate this 10-year cumulated error by extrapolating from the ninth year's figure: namely, it is $(\Delta P_{10}) = [P_0+(10/9)\cdot(P_9-P_0)] - P_{10}$, where P_y is the published value in the *y-th* year following the most recent count. We then replace the published value P_y by $P_y^c = P_y - (y/10)\cdot\Delta P_{10}$.

[16] Our corrected value in year y can be written as $P_y^c = P_y^{\text{int}} + \{P_y - P_y^{\text{int}-9}\}$, where P_y^{int} is the interpolated value between census counts and $P_y^{\text{int}-9}$ is the interpolated value between years 0 and 9. In other words, it adjusts P_y^{int} by accounting for how the inter-census estimate P_y differs from the nine-year linear trend of inter-census estimates.

3.3.5 Data Summary

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

Table 1. Summary Stastistics 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 they enter our specification as logarithms.
Variable Urban is shown unnormalized, although it is normalized when entering our specification.

4. Results

4.1 Structural Equations

The results of estimating the structural system are presented in Tables 2-4, excluding the estimated fixed-effect coefficients. Each table shows two different estimation methods: three-stage least squares (3SLS) and ordinary least squares (OLS). We also carried out two-stage least squares (2SLS) estimation, with results nearly identical to 3SLS except for slightly less precision (but no change in the statistical significance of the important variables.) In light of the consistency between 3SLS and 2SLS, along with the lack of any obvious signs of mis-specification, we regard the 3SLS results as our best estimates.

The usage equation (Table 2) explains how much driving is done by the average adult, holding constant the size of the vehicle stock. The coefficients on fuel cost per mile (*pm*) and its interaction with income are precisely measured and in the expected direction; we discuss their magnitudes in the next subsection. Many other coefficients are also measured with good precision and demonstrate strong and plausible effects. 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/(1-0.78)=0.50 in the long run. Each adult tends to travel more if there is a larger road stock available (negative coefficient on *adults/road-mile*) and if the average 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 indicating that *adults/road-mile* better captures the effects of congestion.[17] The availability of rail transit has no discernable effect, probably because it does not adequately measure the transit options available. The two years 1974 and 1979 exhibited a lower usage, by about 4.4%, other things equal.

The coefficient on the lagged dependent variable implies considerable inertia in behavior, with people adjusting their travel in a given year by just 22 percent of the ultimate shift if a given change is maintained permanently. The equation exhibits only mild autocorrelation, giving us some confidence that our specification accounts for most influences that move sluggishly over time.

OLS overestimates the rebound effect, possibly because it attributes the relationship between VMT and cost per mile as the latter causing the former, whereas the full system shows that some of it is due to reverse causality. In this particular model, OLS overestimates the absolute value of the structural coefficient of cost per mile by 75%.

[17] The long-run difference in log(VMT) between otherwise identical observations with the smallest and largest urbanization observed in our sample (see Table 1) is only 0.0514 x 0.7105 / (1-0.7800) = 0.17; whereas the corresponding difference from *adults/road-mile* is 0.0229 x [ln(490.2)-ln (2.50)] / (1-0.7800) = 0.55.

Table 2. Usage Equation

Variable	Estimated Using 3SLS Coefficient	Estimated Using 3SLS Stndrd. Error	Estimated Using OLS Coefficient	Estimated Using OLS Stndrd. Error
vma(t-1)	**0.7800**	0.0127	**0.7480**	0.0152
vehstock	**0.0385**	0.0110	**0.0466**	0.0125
pm	**-0.0468**	0.0046	**-0.0819**	0.0048
pm*(inc)	**0.0887**	0.0168	**0.0691**	0.0184
pm*(Urban)	0.0105	0.0118	0.0210	0.0141
inc	**0.1100**	0.0143	**0.1103**	0.0157
adults/road-mile	**-0.0229**	0.0050	**-0.0174**	0.0068
pop/adult	**0.1758**	0.0458	0.0374	0.0487
Urban	**-0.0514**	0.0204	**-0.0498**	0.0224
Railpop	-0.0047	0.0064	-0.0003	0.0089
D7479	**-0.0442**	0.0035	**-0.0364**	0.0035
Trend	0.0006	0.0004	*-0.0008*	0.0004
constant	2.0898	0.1213	**2.4592**	0.1456
rho	-0.0893	0.0233	-0.0173	0.0292
No. observations	1,734		1,734	
Adjusted R-squared	0.9802		0.9809	
S.E. of regression	0.0317		0.0311	
Durbin-Watson stat	1.9105		1.9902	
Sum squared resid	1.6751		1.6182	

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) are not shown.
Variables *inc* , *Urban* , and the constituent variables in *pm* are normalized by subtracting their mean value in the sample, both in the variable itself and in any interactions it takes. As a result, the coefficient of any variable in its uninteracted form gives the effect of that variable on vma at the mean values of the other variables.

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*) and the proportion of adults having drivers' licenses (*licences/adult*). As expected, there is strong inertia in expanding or contracting the vehicle stock, as indicated by the coefficient 0.8445 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/(1-0.8445) = 6.43 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)	**0.8445**	0.0148	**0.8397**	0.0152
vma	0.0257	0.0162	**0.0434**	0.0148
pv	**-0.0828**	0.0383	**-0.0792**	0.0391
pm	-0.0007	0.0065	0.0065	0.0065
inc	**0.0384**	0.0155	**0.0330**	0.0156
adults/road-mile	**-0.0227**	0.0070	**-0.0214**	0.0072
Trend	**-0.0015**	0.0008	*-0.0014*	0.0008
interest	**-0.0140**	0.0071	**-0.0176**	0.0073
licenses/adult	**0.0481**	0.0191	**0.0525**	0.0197
constant	-0.0817	0.1592	*-0.2480*	0.1463
rho	**-0.1314**	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	1.9489		1.9548	
Sum squared resid	2.1665		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) are not shown.

The results for fuel intensity (Table 4) show a substantial effect of annual fuel cost, in the expected direction. The effect of fuel price remains strong even if we allow the two components of annual fuel cost, namely *pf* and *vma*, to have separate coefficients. 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 model suggests it reduced long-run desired fuel efficiency by 21 percent.[18] Urbanization appears to increase 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 – possibly reflecting the gradual development and dissemination of new

[18] Since it changed the logarithm of desired efficiency by +0.1045x0.35/(1-0.8100) = ln (1.21).

automotive technology in response to the fuel crises in those years. Like vehicle stock, fuel intensity demonstrates considerable inertia, presumably reflecting the slow turnover of vehicles.

Table 4. Fuel Intensity Equation

	Estimated Using 3SLS		Estimated Using OLS	
Variable	Coefficient	Stndrd. Error	Coefficient	Stndrd. Error
fint(t-1)	**0.8100**	0.0139	**0.7900**	0.0161
vma+pf	**-0.0461**	0.0071	**-0.0935**	0.0075
cafe	**-0.1045**	0.0117	**-0.1025**	0.0144
inc	0.0005	0.0164	0.0076	0.0172
pop/adult	-0.0261	0.0656	0.0434	0.0760
Urban	**-0.1629**	0.0526	**-0.1536**	0.0662
D7479	**-0.0109**	0.0045	-0.0056	0.0046
Trend66-73	0.0006	0.0010	0.0014	0.0013
Trend74-79	**-0.0025**	0.0010	0.0005	0.0012
Trend80+	**-0.0038**	0.0005	**-0.0047**	0.0005
constant	-0.1222	0.0793	**0.2463**	0.0876
rho	**-0.1325**	0.0237	**-0.0970**	0.0292
No. observations	1,734		1,734	
Adjusted R-squared	0.9604		0.9614	
S.E. of regression	0.0398		0.0392	
Durbin-Watson stat	1.9486		1.9550	
Sum squared resid	2.6440		2.5757	

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) are not shown.

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. The interactions through the simultaneous equations modify only slightly the numbers that can be read directly from the coefficients. In particular, the average cost-per-mile elasticity in the sample is -0.0467, nearly identical to the coefficient of *pm* in Table 2. Thus the average rebound effect in this sample is estimated to be approximately 4.7% in the short run, and 22.0% in the long run.

Table 5. Rebound Effect and Other Price Elasticities

	Estimated Using Three-Stage Least Squares		Estimated Using Ordinary Least Squares	
	Short Run	Long Run	Short Run	Long Run
Elasticity of VMT with respect to fuel cost per mile: (a)				
At sample average	-0.0469	-0.2199	-0.0818	-0.3341
	(0.0046)	(0.0231)	(0.0048)	(0.0254)
US 1997-2001 (b)	-0.0257	-0.1211	-0.0651	-0.2647
	(0.0063)	(0.0307)	(0.0066)	(0.0315)
Elasticity of VMT with respect to new veh price:	-0.0032	-0.0959	-0.0037	-0.0961
	(0.0017)	(0.0528)	(0.0021)	(0.0550)
Elasticity of fuel intensity with respect to fuel price:				
At sample average	-0.0440	-0.1910	-0.0865	-0.3005
	(0.0069)	(0.0335)	(0.0070)	(0.0406)
US 1997-2001	-0.0450	-0.2205	-0.0879	-0.3728
	(0.0070)	(0.0364)	(0.0071)	(0.0419)
Elasticity of fuel consumption with respect to fuel price:				
At sample average	-0.0909	-0.4109	-0.1683	-0.6346
	(0.0059)	(0.0351)	(0.0083)	(0.0438)
US 1997-2001	-0.0707	-0.3416	-0.1530	-0.6375
	(0.0071)	(0.0387)	(0.0093)	(0.0468)

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 *inc* and *Urban* for all US.
Standard errors, shown in parentheses, are calculated exactly from the covariance matrix of estimated coefficients using the Wald test procedure for an arbitrary function of coefficients in Eviews 5.

Use of OLS overestimates the short- and long-run rebound effects by 74% and 52%, respectively. This short-run OLS estimate (8.2%) 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 such a conclusion is speculative given the poor performance of the OLS specification. We found that OLS results vary strongly with slight changes in specification – in particular indicating implausibly high autocorrelation and implausibly small coefficients on lagged dependent variables – whereas 2SLS and 3SLS results are quite robust. Thus differences among OLS results in the literature may be caused as much by differences in specification as by the presence or absence of endogeneity corrections.[19]

The model for vehicle usage discerns an additional influence of real income on the rebound effect. The coefficient on *pm* interacted with logarithm of per-capita income shows that each increase in log income by 0.1 (roughly a ten percent increase in income) reduces the magnitude of the short-run rebound effect by just under one percentage point. This appears to confirm the theoretical expectation that higher incomes make people less sensitive to fuel costs. To get an idea of the implications of income for the rebound effect, we compute the relevant elasticity for values of income and urbanization 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 2.6% and the long-run effect to 12.1%.

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 -.10. The third and fourth panels provide information about how fuel prices affect fuel intensity and overall fuel consumption. The former effect, from equation (9), is estimated with precision, thanks to the small standard error on the coefficient of *vma+pf* in Table 4. Adding to it the elasticity due to vehicle-miles traveled gives the total price-elasticity of fuel consumption, shown in the last panel of the table. The long-run estimate is -0.41, 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 usage (0.2199/0.4109 = 54%) are in line with the literature as reviewed by Parry and Small (2005).[20] Note, however, that the proportion caused by changes in usage decreases to only 35% in the last five years of our sample. Thus, our results suggest that the response to fuel prices increasingly takes the form of changes in fuel efficiency more than changes in amount of motor-vehicle travel.

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[19] Estimating our model on a shorter sample (1966-1990) does not change the estimated rebound effect for a given value of income and urbanization. So our use of a longer time series does not explain the differences in results with other estimates using aggregate data. The longer time series does increase the precision of the estimates.

[20] They choose as the best consensus an elasticity equal to -0.55, with 40% of it caused by mileage changes.

4.3 Estimates on separate time periods

As noted, we find the rebound effect to be much smaller when computed for values of per capita income characterizing recent years than when computed for average values over the 36-year estimation period. Would we see this same fall in the estimated value if we just estimated a model with a constant rebound effect on different time periods? We answered this question using three twelve-year time periods. The resulting estimates are considerably less precise and less robust, especially for the fuel intensity equation: its estimated autocorrelation coefficient is uncomfortably large (0.47) during the middle time period (1978-1989), and the estimates of the effect of *cafe* (for the two periods where it was in effect) show implausible variations. Furthermore, the coefficient of lagged *vma* in the usage equation is considerably smaller (0.55 to 0.58) when estimated on these subsamples than when estimated on the full sample; this may reflect the inability of such short time periods to identify lagged coefficients well, especially given that two of the eleven years' data are lost because of using variables with two lags as instruments.

Nevertheless, the results clearly show the hypothesized decline in the rebound effect as we move from the first two periods to the last period, 1990-2001. Table 6 shows our 3SLS estimates of the short- and long-run rebound effect in each time period, and compares them with those predicted by our base model with variables set to their average values for that time period. Except for the middle period, the short-run estimates agree closely with these full-model predictions. Long-run estimates are mostly smaller than the prediction due to the smaller lag coefficient. The first column of the table also shows that the average rebound effect estimated over the entire period is nearly the same whether or not the model includes interaction terms. The same is true if the rebound effect is allowed to vary with a simple time trend but not income or urbanization, although in that model the interaction term is not significant (results not shown).

Thus this exercise lends support to our interpretation that the rebound effect has indeed declined between the time periods 1966-1989 and 1990-2001. Although we cannot say definitively that the reason is higher incomes, that explanation seems the most likely given its theoretically justification and good data fit.

Table 6. Rebound Effect Estimates from Different Time Periods

	Full Sample	Separate Subsamples		
	1966-2001	1966-1977	1978-1989	1990-2001
Short run:				
Estimated constant value	-0.0508	-0.0767	-0.0898	-0.0362
	(0.0046)	(0.0208)	(0.0101)	(0.0110)
Predicted from full model	-0.0469	-0.0639	-0.0458	-0.0309
	(0.0046)	(0.0053)	(0.0046)	(0.0057)
Long run:				
Estimated constant value	-0.2594	-0.1824	-0.2234	-0.0824
	(0.0249)	(0.0456)	(0.0291)	(0.0255)
Predicted from full model	-0.2199	-0.2996	-0.2147	-0.1454
	(0.0231)	(0.0263)	(0.0232)	0.0309

Notes: Estimated standard errors are in parentheses. Results shown as "estimated constant value" are from a model estimated without interaction terms. Results shown as "predicted from full model" are calculated from our base model in Tables 2-4 using the average values of *inc* and *Urban* for the time period shown.

4.4 Caveats

Despite the generally good performance of our equation system, we call attention to three limitations. First, there are well-known problems with the VMT data collected by the US Federal Highway Administration. These data are reported by states, which lack a uniform methodology for estimating them – for example, some rely on sporadic vehicle counts, while others multiply fuel consumption (measured from tax records) by an independent estimate of fleet fuel efficiency.[21]

However, we have no reason to think that these problems bias our results. The posited sources of measurement error are mostly unrelated to our independent variables; and even if they were, our use of fixed effects eliminates the spurious effect of any cross-state relationship that is consistent over time. One might worry that errors in measuring fuel consumption by state could appear in both VMT data (in those states where the VMT estimate is based on fuel consumption)

[21] VMT estimates in other data sets have problems as well. For example, the 1990 Nationwide Personal Transportation Survey (NPTS) changed their sampling method in a manner that apparently greatly exaggerated the measured 1983-1990 growth in VMT. Lave (1996) compares three sources in terms of national VMT growth rates, finding that the FHWA data set that we use (at the national level) agrees well with the other sources. For more recent years, it is generally believed that the FHWA methodology has improved.

and in fuel efficiency. This would bias OLS estimates, but not 2SLS and 3SLS, which are specifically designed to eliminate asymptotic bias resulting from correlated errors in the dependent variables.[22]

Second, our estimates, like those of most previous studies, rely on the theoretical restriction that people react to changes in cost per mile in the same way whether those changes arise from variations in fuel prices or in fuel efficiency. This restriction is critical to most studies because most data sets contain more variation in fuel prices than in fuel efficiency. Unfortunately, we are unable to confirm the restriction with our data and model; rather, we easily reject equality of coefficients on the two components of log price per mile, *pm* = *pf* + *fint*, when we enter them separately each time *pm* appears in our equations. These results are shown in Appendix C. In particular, the coefficient on the non-interacted variable *fint* is small and statistically insignificant, while that on *pf* retains about the same value as that on *pm* in the restricted model. This result is consistent with the observation that variations in fuel prices are mainly what identify the rebound effect.

Thus in the absence of theory we cannot prove that there is *any* rebound effect defined as a reaction to exogenous changes in fuel efficiency. However, the model with *pf* and *fint* entered separately does not perform very well. In the usage equation, the interactions of *fint* with *inc* and *Urban* become unstable with respect to inclusion or exclusion of other variables, making us think the equation is overfitting. With OLS, several coefficients of the usage equation, including that of *fint*, are implausible and erratic, and the equation portrays an extremely high value of autocorrelation, suggesting that the time-series properties of the usage equation are poorly identified when *pf* and *fint* are allowed to have separate effects. Thus we conclude that the best estimate of the rebound effect is obtained by imposing the theoretical constraint that equates the effects of fuel price and fuel intensity, and that our data are unable to test this constraint satisfactorily.

A third caveat is that the estimated role of fuel price in determining fuel efficiency is quite sensitive to details of how the *cafe* variable is defined. The prediction equation for desired fuel intensity, shown in Appendix B, is not very robust to attempts to add variables such as

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[22] Furthermore, we measure fuel intensity as fuel consumption divided by VMT, causing a cancellation of a common error in both. Thus measurement error in fuel consumption would appear in our VMT variable but not in our fuel intensity variable, which instead would be determined by the independent estimate of fleet fuel efficiency used in those states that estimate VMT from aggregate fuel consumption.

lagged values of variables already included – probably because it relies on data for only a short time period, 1966-78. When we tried this, the time pattern exhibited by the *cafe* variable was quite different, and its influence in the structural model for fuel efficiency diminished to statistical insignificance, as did that of fuel price. However, we believe that this richer specification is unreliable because it over-fits the data: coefficients on a variable and its lag are in several instances large and opposite in sign, and the predicted desired fuel intensity show implausible oscillations over time. Therefore, we believe our base specification is the most suitable one given the short time period over which we can observe pre-CAFE behavior.

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, however, is evidence that the rebound effect diminishes substantially with income, causing it to be considerably lower today. For example, our results suggest it was barely half as large in the years 1997-2001, and is likely to diminish still further as rising incomes reduce the significance of fuel costs in decisions about travel.

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 standards and taxes do not differ much from each other in their effects on fuel consumption and driving. Their effects on fuel tax revenues, of course, are still different.

Our model as estimated can be used to forecast the dynamic adjustment path resulting from specific policies. For example, in 2004 California adopted regulations that phase in efficiency controls over the period 2009-2016, under legislation mandating reduction of greenhouse gases from motor vehicles. Because our model has a dynamic component, it calculates the year-by-year response while taking into account ongoing changes in income. Its ability to do this is limited, however, by the need to project incomes outside the range observed in our sample. Thus there is a need for continued research into how consumers react to changes in fuel prices and fuel efficiency over coming years.

In urbanized areas, 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, we would need a supply model of congestion along with a model explaining how congestion affects the demand for travel. Our model makes a start on this by including as variables urbanization and population relative to road supply, but a more exact link to congestion would be a desirable addition.

For now, our conclusion is that policy analyses of regulations affecting fuel efficiency should assume that the rebound effect is considerably smaller today than has been measured in the past, and is likely to become smaller still as time goes on.

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Appendix A: Control Variables and Data Sources

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*);[23] 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.[24] X_M also includes two interaction variables: $pm{\cdot}inc$ and $pm{\cdot}Urban$.

X_V: This set of variables includes *inc*, *adults/road-mile*, and *Trend*, already defined in X_M. In addition there are two other variables: 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

[23] We also tried using disposable income, which excludes taxes, to see if it changed the strong influence that we find for income on the rebound effect. The results are barely distinguishable from those presented here.

[24] We experimented with replacing these trends by three technology variables: vehicle volume, engine horsepower, and top speed (each in the form a fractional change in that measure since 1975, the earliest year for which we have the measure, and zero prior to 1975) plus a trend variable *Techtrend* equal to min{(year-1975), 0} in order to capture the effects of any earlier changes (assumed linear) in these variables. This approach did not improve the estimation.

regime to another. Specifically, *Trend66-73* = Min(*Trend*,7); *Trend74-79* = Max[0, Min[(*Trend*-7),6]}; and *Trend80+ = Trend -Trend66-73 - Trend74-79*.

Data Sources:

Adult population
Definition: midyear population estimate, 18 years and over
U.S. Census Bureau (http://www.census.gov)
(http://eire.census.gov/popest/archives/1990.php and
http://eire.census.gov/popest/data/states/ST-EST2002-ASRO-02.php)
(*accessed 12/03/2004*). Corrected as described in text.

Corporate Average Fuel Economy Standard (Miles Per Gallon)
National Highway Traffic Safety Administration (NHTSA), *CAFE Automotive Fuel Economy Program, Annual update 2001*, Table I-1 (http://www.nhtsa.dot.gov/cars/rules/cafe/FuelEconUpdates/2001/Index.html). Passenger car and light truck standard are averaged in each year using nationwide VMT as weights (see VMT source).

Consumer price index – all urban consumers (1982-84=100)
Bureau of Labor Statistics (BLS), CPI (http://www.bls.gov/cpi/). Note: all monetary variables (gas tax, new passenger vehicle price index, price of gasoline, personal income) are put in real 1987 dollars by first deflating by this CPI and then multiplying by the CPI in year 1987. The purpose of using 1987 is for ease in replicating Haughton and Sarkar (1996).

Highway Use of Gasoline (millions of gallons per year)
1966-1995: FHWA, *Highway Statistics Summary to 1995*, Table MF-226
1996-2001: FHWA, *Highway Statistics*, annual editions, Table MF-21

Income per capita ($/year, 1987 dollars)
Primary measure: Personal income divided by midyear population
Personal income is from Bureau of Economic Analysis (BEA) (http://www.bea.doc.gov/)
Alternative measures:
(1) Disposable income per capita: Available from same web site as above, starting 1969; for 1966-68 we interpolated by assuming it bore the same ratio to per capita personal income as existed in the same state for 1969-78.
(2) Gross state product per capita: Available starting 1977; for 1966-1976 we interpolated by assuming it bore the same ratio to per capita personal income as existed in the same state for 1977-87.

Interest rate: national average interest rate for auto loans (%)
Definition: average of rates for new-car loans at auto finance companies and at commercial banks.
Source: Federal Reserve System, Economic Research and Data, Federal Reserve Statistical Release G.19 “Consumer Credit”

(http://www.federalreserve.gov/releases/g19/hist/cc_hist_tc.html). Available starting 1971 for auto finance companies, 1972 for commercial banks. For earlier years in each series, we use the predicted values from a regression explaining that rate using a constant and Moody's AAA corporate bond interest rate, based on years 1971-2001 (finance companies) or 1972-2001 (commercial banks).

New Car Price Index: price index for U.S. passenger vehicles, city average, not seasonally adjusted (1987=100)
Source: Bureau of Labor Statistics web site.
Note: Original index has 1982-84=100.

Number of vehicles: Number of automobiles and light trucks registered
1966-1995: FHWA, *Highway Statistics Summary to 1995*, Table MV-201
1996-2001: FHWA, *Highway Statistics*, annual editions, Table MV-1
Note: "Light trucks" include personal passenger vans, passenger minivans, utility-type vehicles, pickups, panel trucks, and delivery vans.

Price of gasoline (cents per gallon, 1987 dollars)
Data Set A: U.S. Department of Energy (USDOE 1977), Table B-1, pp. 93-94 (contains 1960-1977)
Data Set B: Energy Information Administration, *State Energy Data 2000: Price and Expenditure Data*, Table 5 (contains 1970-2000)
2001: Energy Information Administration, *Petroleum Marketing Annual*, Table A1.
Note: We use Data Set B for 1970-2000, and for the earlier years we use predicted values from a regression explaining Set B values for overlapping years (1970-1977) based on a linear function of Set A values.

Public road mileage: Total length of roads in state (miles)
1966-1979: FHWA, *Highway Statistics*, annual editions, Table M-1
1980-1995: FHWA, *Highway Statistics Summary to 1995*, Table HM-220
1996-2001: FHWA, *Highway Statistics*, annual editions, Table HM-20

Rail Transit Availability Index (Railpop)
Definition: Fraction of the state's population living in metropolitan statistical areas with a subway or heavy rail transit system.
Source for existence of rail by metro area: American Public Transportation Association (APTA) (http://www.apta.com)
Source for population by Metropolitan Statistical Areas: *Statistical Abstract of the United States*, section on "Metropolitan Statistics", various years.
Note: Data are missing for years 1969, 1971, 1974, 1979, 1981, 1982, 1989; for those years we interpolate between the nearest available years.

Number of Licensed Drivers
1966-1995: FHWA, *Highway Statistics Summary to 1995*, Table DL-201
1996-2001: FHWA, *Highway Statistics*, annual editions, Table DL-1C
Some outliers in this series were replaced by values given by a fitted polynomial of degree 3.

Urban Road Mileage (miles): Total municipal mileage

1966-1979: FHWA, *Highway Statistics*, annual editions, Table M-1
1980-1995: FHWA, *Highway Statistics Summary to 1995*, Table HM-220
1996-2001: FHWA, *Highway Statistics*, annual editions, Table HM-20

Urbanization: Share of total state population living in Metropolitan Statistical Areas (MSAs), with MSA boundaries based on December 2003 definitions.
Available starting 1969; for earlier years, extrapolated from 1969-79 values assuming constant annual percentage growth rate. Source: Bureau of Economic Analysis, Regional Economic Accounts (http://www.bea.doc.gov/bea/regional/reis/)

VMT (Vehicle Miles Traveled),in millions

1966-1979: FHWA, *Highway Statistics*, annual editions, Table VM-2
1980-1995: FHWA, *Highway Statistics Summary to 1885*, Table VM-202
1996-2001: FHWA, *Highway Statistics*, annual editions, Table VM-2

Appendix B: Variable Measuring Strength of CAFE Regulation

We followed the following steps to create the variable.

1. We first estimate the reduced-form equation explaining fuel intensity—i.e., the empirical counterpart of the third of equation set (1)—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 simplified it by dropping the variable *Railpop*, which seemed to have little effect in this short time series. 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:

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

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 Table B1. The statistically significant coefficients are those of $(fint)_{t-1}$, *D7479*, and *pv*. The price of fuel is not statistically significant (t-statistic -1.02) 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:

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

where $(fint)^*_{i,t}$ denotes a long-run desired value for the logarithm of fuel intensity. That is, users 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 γ=1-α^f and

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

This value is computed for each state and each year t.

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

$$(FintUS)^*_t \frac{\sum_i M_{it} \exp\left((fint)^*_{it}\right)}{\sum_i M_{it}} \tag{B.4}$$

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

4. Finally, we assume CAFE is binding whenever the desired efficiency $E^*_t \equiv \left(1/FintUS^*_t\right)$ is less than the minimum mandated efficiency, $\overline{E}_t$. 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

$$R_E \equiv \max\left\{\frac{\overline{E}_t}{E^*_t}, 1\right\}$$

or its logarithm,

$$cafe \equiv \max\left\{\left(\overline{e}_t - e^*_t\right), 0\right\} \tag{B.5}$$

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

Table B1. Fuel Intensity Equation: Reduced Form Estimated on 1966-1977 Data

Variable	Coefficient	Std. Error
fint(t-1)	0.6386	0.0443
pf	-0.0209	0.0204
Inc	0.0169	0.0288
adults/road-mile	0.0363	0.0273
pop/adult	0.0852	0.0910
Urban	-0.1974	0.2328
D7479	-0.0213	0.0060
Trend	-0.0097	0.0024
pv	-0.2209	0.0798
Interest	0.0213	0.0298
licences/adult	0.02605	0.0262
constant	-0.9822	0.3584
Rho	-0.1241	0.0625
No. of observations	510	
Adjusted R-squared	0.8967	
S.E. of regression	0.0253	
Sum squared resid	0.2858	
Durbin-Watson stat	1.9975	

Note: 50 constants for individual states are not shown.

Appendix C: Results of Model with Fuel Price and Fuel Intensity Entered Separately

Table C1. Usage Equation

	Estimated Using 3SLS		Estimated Using OLS	
Variable	Coefficient	Stndrd. Error	Coefficient	Stndrd. Error
vma(t-1)	0.8222	0.0157	0.0390	0.0184
vehstock	0.0377	0.0122	0.0857	0.0166
pf	-0.0583	0.0053	-0.0858	0.0067
pf*(inc)	0.0971	0.0230	0.1666	0.0322
pf*(Urban)	0.0554	0.0269	-0.0300	0.0330
fint	0.0174	0.0149	-0.5840	0.0150
fint*(inc)	0.0854	0.0505	0.3349	0.0558
fint*(Urban)	-0.1039	0.0572	-0.1159	0.0573
inc	0.0741	0.0164	0.3686	0.0238
adults/road-mile	-0.0141	0.0066	-0.0517	0.0161
pop/adult	0.0815	0.0608	1.1845	0.1125
Urban	-0.0523	0.0238	0.0479	0.0868
Railpop	-0.0061	0.0105	-0.0191	0.0180
D7479	-0.0475	0.0038	-0.0137	0.0020
Trend	0.0010	0.0004	-0.0002	0.0007
constant	1.6955	0.1476	9.0193	0.1846
rho	-0.0897	0.0231	0.8128	0.0138
No. observations	1,734		1,734	
Adjusted R-squared	0.9776		0.9882	
S.E. of regression	0.0337		0.0244	
Durbin-Watson stat	1.8853		2.0551	
Sum squared resid	1.8927		0.9967	

Notes:

Estimates of fixed effects coefficients (one for each state except Wyoming) are not shown.

Variables *inc* , *Urban* , and the constituent variables in *pm* are normalized by subtracting their mean value in the sample, both in the variable itself and in any interactions it takes. As a result, the coefficient of any variable in its uninteracted form gives the effect of that variable on vma at the mean values of the other variables.

Table C2. Vehicle Stock Equation

Variable	Estimated Using 3SLS Coefficient	Estimated Using 3SLS Stndrd. Error	Estimated Using OLS Coefficient	Estimated Using OLS Stndrd. Error
vehstock(t-1)	0.8352	0.0149	0.8277	0.0153
vma	0.0571	0.0178	0.0828	0.0166
pv	-0.1073	0.0383	-0.1120	0.0393
pf	-0.0149	0.0073	-0.0130	0.0074
fint	0.0567	0.0152	0.0705	0.0141
inc	0.0165	0.0161	0.0064	0.0163
adults/road-mile	-0.0214	0.0070	-0.0201	0.0071
Trend	-0.0009	0.0008	-0.0009	0.0008
interest	-0.0043	0.0075	-0.0040	0.0077
licenses/adult	0.0333	0.0195	0.0302	0.0200
constant	-0.4145	0.1778	-0.6655	0.1668
rho	-0.1356	0.0282	-0.1264	0.0291
No. observations	1,734		1,734	
Adjusted R-squared	0.9650		0.9651	
S.E. of regression	0.0357		0.0357	
Durbin-Watson stat	1.9475		1.9511	
Sum squared resid	2.1331		2.1300	

Notes:
Estimates of fixed effects coefficients (one for each state except Wyoming) are not shown.

Table C3. Fuel Intensity Equation

Variable	Estimated Using 3SLS Coefficient	Estimated Using 3SLS Stndrd. Error	Estimated Using OLS Coefficient	Estimated Using OLS Stndrd. Error
fint(t-1)	0.7663	0.0203	-0.0780	0.0197
vma	-0.0522	0.0234	-0.7635	0.0210
pf+vma	-0.0383	0.0072	-0.0739	0.0077
cafe	-0.1048	0.0131	-0.3018	0.0224
inc	0.0283	0.0186	0.3983	0.0280
pop/adult	0.0164	0.0687	0.7934	0.1432
Urban	-0.0780	0.0598	-0.2058	0.2312
D7479	-0.0103	0.0045	-0.0193	0.0025
Trend66-73	0.0007	0.0011	0.0057	0.0052
Trend74-79	-0.0026	0.0011	-0.0043	0.0024
Trend80+	-0.0040	0.0005	-0.0141	0.0009
constant	0.2303	0.2031	4.8929	0.2519
rho	-0.1299	0.0245	0.7930	0.0169
No. observations	1,734		1,734	
Adjusted R-squared	0.9624		0.9792	
S.E. of regression	0.0388		0.0288	
Durbin-Watson stat	1.9168		2.2404	
Sum squared resid	2.5071		1.3883	

Notes:
Estimates of fixed effects coefficients (one for each state except Wyoming) are not shown.