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Identifying the Rebound – Evidence from a German Household Panel

Abstract
Using a panel of household travel diary data collected in Germany between 1997 and 2005, this study assesses the effectiveness of fuel efficiency improvements by econometrically estimating the rebound effect, which measures the extent to which higher efficiency causes additional travel. Following a theoretical discussion outlining three alternative definitions of the rebound effect, the econometric analysis generates corresponding estimates using panel methods to control for the effects of unobservables that could otherwise produce spurious results. Our results, which range between 57% and 67%, indicate a rebound that is substantially larger than obtained in other studies, calling into question the efficacy of policies targeted at reducing energy consumption via technological efficiency.

JEL Classification: D13, Q41

Keywords: Automobile travel, rebound effect, panel models

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1 Introduction

The improvement of energy efficiency is often asserted to be one of the most promising options to reduce both the usage of energy and associated negative externalities, such as carbon dioxide emissions (CO_2). Ever since the creation of the Corporate Average Fuel Economy (CAFE) standards in 1975, this assertion has been a mainstay of energy policy in the United States. In recent years, it has also found increasing currency in Europe, as attested to by the voluntary agreement negotiated in 1999 between the European Commission (EC) and the European Automobile Manufacturers Association, stipulating the reduction of average emissions to a target level of 140g CO_2/km by 2008. The EC is additionally considering legislation that would set a target of 120g CO_2/km by 2012.

Although such technological standards undoubtedly confer benefits via reduced per-unit prices of energy services, the extent to which they reduce energy consumption, and hence pollution, remains controversial. It is plausible, for instance, that the owner of a more fuel-efficient car will *ceteris paribus* drive more in response to lower per-kilometer traveling costs relative to other modes. This increase in service demand from reduced energy prices is called the "rebound effect", alternatively referred to as "take back" of efficiency improvements. KHAZZOOM (1980) was among the first to study the rebound effect at the microeconomic level of households, focusing on the effects of increases in the energy efficiency of a single energy service, such as space heating and individual conveyance. The "rebound", however, is a general economic phenomenon, diminishing potential gains of time-saving technologies (e. g. BINSWANGER 2001) as well as of innovations that may reduce the usage of resources such as water.

The significance of the "rebound" has been hotly debated among energy economists ever since then – see e. g. BINSWANGER (2001), BROOKES (2000), and GREENING *et al.* (2000) for surveys of the relevant literature. Though the basic mechanism is widely accepted, the core of the controversy lies in the identification of the magnitude of the *direct rebound effect*, which describes the increased demand for an energy service whose price shrinks due to improved efficiency[1]. This substitution mechanism in favor of the energy service works exactly as would the price reduction of any commodity other than energy, and suggests that price elasticities are at issue when it comes to the estimation of direct rebound effects. Some analysts, most notably LOVINS (1988), maintain that these effects are so insignificant that they can safely be ignored (see also GREENE 1992 and SCHIPPER and GRUBB 2000). Other authors argue that they might be so large as to completely defeat the purpose of energy efficiency improvements (BROOKES 1990, SAUNDERS 1992, WIRL 1997).

Support for both views are found in the available empirical evidence. A survey by GOODWIN, DARGAY, and HANLY (2004), for example, cites rebound effects varying between 4% and 89% from studies using pooled cross-section/time-series data. Results from subsequent studies are equally wide-ranging. Using cross-sectional micro data from the 1997 Consumer Expenditure Survey, WEST (2004) finds a rebound effect that is 87% on average, while SMALL and VAN DENDER (2007), who use a pooled cross-section of US states for 1966-2001, uncover rebound effects varying between 2.2% and 15.3%.

Aside from differences in the level of data aggregation, one major reason for the diverging results of the empirical studies is that there is no unanimous definition of the direct rebound effect. Instead, several definitions have been employed as determined by the availability of price and efficiency data, making comparisons across studies difficult. The resulting variety of definitions used in the economic literature is summarized and analyzed in an illuminating way by DIMITROUPOULOS and SORRELL (2006), who argue that it is particularly due to the omission of potentially relevant factors, such as capital cost, that the size of the rebound effect might be frequently overesti-

[1]The indirect rebound effect and general equilibrium effects have also been distinguished in the literature (see, e. g. , GREENING and GREENE 1997, GREENE *et al*. 1999). The former arises from an income effect: lower per-unit cost of an energy service implies - *ceteris paribus* - that real income grows. The latter arises from innovations, such as James WATT's famous steam engine, that increase society's aggregate income potential. Given that both indirect and general equilibrium effects are difficult to quantify, the overwhelming majority of empirical studies confines itself to analyzing the direct rebound effect.

mated in empirical studies. GREENE, KAHN, and GIBSON (1999) and SMALL and VAN DENDER (2007) express similar reservations, noting in particular the shortcomings of cross-sectional or pooled approaches that fail to control for the time-invariant effects of neighborhood design, infrastructure, and other geographical features, which are likely to be strongly correlated with fuel economy and travel.

Departing from the theoretical grounds provided by BECKER's (1965) classical household production function approach and drawing on a panel of household travel data, this paper focuses on estimating the direct rebound effect from variation in the fuel economy of household vehicles. Several features distinguish our analysis. After cataloguing three commonly employed definitions of the direct rebound effect in the theoretical section of the paper, econometric estimates corresponding to each of the three definitions are provided in the empirical section. These estimates are generated from panel models of micro-level data, thereby bypassing aggregation problems while at the same time controlling for time-invariant omitted variables.

Our results, which range between 57% and 67%, indicate a "rebound" that is substantially larger than the typical effects obtained from the U.S. transport sector. Based on household survey data, GREENE, KAHN, and GIBSON(1999:1), for instance, find a long-run "take back" of about 20% of potential energy savings, confirming the results of other U.S. studies using national and or state-level data. While this issue has received relatively less scrutiny in the European context, our results are also substantially larger than those of WALKER and WIRL (1993), who estimate a long-run rebound effect of 36% for Germany using aggregate time-series data.

The following section presents three definitions of the direct rebound effect, building the basis for the empirical estimation. Section 3 describes the econometric specifications and estimators. Section 4 describes the panel data base used in the estimation, followed by the presentation and interpretation of the results in Section 5. The last section summarizes and concludes.

2 A Variety of Direct Rebound Effect Definitions

Along the lines of BECKER's seminal work on household production, we assume that an individual household derives utility from energy services, such as mobility or comfortable room temperature. A specific service is taken to be the output of a production function f:

$$s = f(e, t, k, o), \tag{1}$$

where f describes how households "produce" the service in the amount of s by using energy, e, time, t, capital, k, and other market goods o. Using this framework, we begin by drawing on the definition of energy efficiency typically employed in the economic literature (e. g. WIRL, 1997):

$$\mu = \frac{s}{e} > 0, \tag{2}$$

where the efficiency parameter μ characterizes the technology with which a service is provided. For the specific example of individual conveyance, parameter μ designates fuel efficiency that can be measured in terms of vehicle kilometers per liter of fuel input and may alter, for instance, if a household changes its vehicle. Efficiency definition (2) assumes proportionality between service level and energy input regardless of the level – a simplifying assumption that may not be true in general, but provides for a convenient first-order approximation of the relationship of s with respect to e.

Efficiency definition (2) reflects the fact that the higher the efficiency μ of a given technology, the less energy $e = s/\mu$ is required for the provision of a certain amount s of energy service. Hence, the concept of energy efficiency is perfectly in line with BECKER's idea of household production, according to which households are, ultimately, not interested in the amount of energy required for a certain amount of service, but in the energy service itself. Based on efficiency definition (2), it follows that the price p_s per unit of the energy service, given by the ratio of service cost to service amount, is smaller the higher the efficiency μ is:

$$p_s = \frac{e \cdot p_e}{s} = \frac{e}{s} \cdot p_e = \frac{p_e}{\mu} \; . \tag{3}$$

We now provide a concise summary of three widely known definitions of the *direct* rebound effect that are based on either efficiency, service price, or energy price elasticities. Using these definitions and data on fuel efficiency, fuel prices, distance driven, and fuel consumption for household vehicles originating from German household data, we will estimate each of the three rebound effects. The proofs of the propositions that complement the rebound definitions are given in the appendix.

Definition 1: The immediate and most general measure of the direct rebound effect – see e. g. BERKHOUT *et al.* (2000) – is given by $\eta_\mu(s) := \frac{\partial \ln s}{\partial \ln \mu}$, the elasticity of service demand with respect to efficiency, reflecting the relative change in service demand due to a percentage increase in efficiency.

Proposition 1: Having $\eta_\mu(s)$ in hand, we obtain the relative reduction in energy use due to a percentage change of efficiency:

$$\eta_\mu(e) = \eta_\mu(s) - 1 \ . \tag{4}$$

Only if $\eta_\mu(s)$ equals zero, that is, only if there is no direct rebound effect, $\eta_\mu(e)$ amounts to -1, indicating that 100 % of the potential energy savings due to an efficiency improvement can actually be realized.

Definition 2: Instead of $\eta_\mu(s)$, empirical estimates of the rebound effect are frequently based on $-\eta_{p_s}(s)$, the negative price elasticity of service demand – see e.g. BINSWANGER (2001) and GREENE *et al.* (1999). Major reasons for this preference are that data on energy efficiency is often unavailable or data provides only limited variation in efficiencies. The basis for this definition is given by the following proposition.

Proposition 2: If energy prices p_e are exogenous and service demand solely depends on p_s, then

$$\eta_\mu(s) = -\eta_{p_s}(s) \ . \tag{5}$$

That the rebound may be captured by $-\eta_{p_s}(s)$ reflects the fact that the direct rebound effect is, in essence, a price effect, which works through shrinking service prices p_s.

Definition 3: Empirical estimates of the rebound effect are sometimes necessarily based on $-\eta_{p_e}(e)$, the negative energy price elasticity of energy consumption, rather than on

$-\eta_{p_s}(s)$, because data on energy consumption and prices is more commonly available than on energy services and service prices. It was this definition of the rebound that was originally introduced by KHAZZOOM (1980:38) and is also employed by, e. g. , WIRL (1997:30).

Proposition 3: If the energy efficiency μ is constant, then

$$\eta_{p_e}(e) = \eta_{p_s}(s) \ . \tag{6}$$

It bears emphasizing that Definitions 2 and 3 are based on the assumption that service demand is solely a function of the energy input e, or alternatively of service price p_s, as is the conventional assumption in the literature. Contrasting with function (1), this assumption implies that service demand is independent of time t, capital k, and other market goods o. In practice, however, more energy efficient appliances frequently have higher fixed costs, but simultaneously reduce operating costs through lower fuel and time requirements, a point to which we return in Section 5. In other words, the possibility that efficiency improvements also determine other factors such as the time usage required by an energy service, the use of other commodities, or capital cost is not considered.

3 Methodology

Our empirical methodology proceeds with two principle aims: (1) to compare alternative model specifications that yield estimates corresponding to each of the three definitions of the rebound effect explicated in the theoretical discussion; (2) to generate these estimates using various panel data estimators that control for the omission of potentially relevant factors varying across observations and over time.

Referring to Definition 1, the first specification regresses the log of monthly kilometers traveled, $\ln(s)$, on the log of kilometers traveled per liter, $\ln(\mu)$, the coefficient of which yields the rebound effect, $\eta_\mu(s)$. As control variables, we additionally include the logged price of fuel per liter, $\ln(p_e)$, and a set of household- and car-level variables designated by the vector $\mathbf{x}$.

Model 1:

$$\ln(s_{it}) = \alpha_0 + \alpha_\mu \cdot \ln(\mu_{it}) + \alpha_{p_e} \cdot \ln(p_{e it}) + \alpha_{\mathbf{x}} \cdot \mathbf{x}_{it} + \xi_i + \nu_{it} \ , \tag{7}$$

Subscripts i and t are used to denote the observation and time period, respectively. ξ_i denotes an unknown individual-specific term, and ν_{it} is a random component that varies over individuals and time.

The second model generates estimates of the rebound corresponding to Definition 2, which involves regressing $\ln(s)$ on the logged price of fuel per kilometer, $\ln(p_s)$, and the vector of control variables $\mathbf{x}$. In this model, the rebound effect is obtained according to Proposition 2 by the negative coefficient of $\ln(p_s)$: $\eta_\mu(s) = -\eta_{p_s}(s) = -\alpha_{p_s}$.

Model 2:

$$\ln(s_{it}) = \alpha_0 + \alpha_{p_s} \cdot \ln(p_{s it}) + \alpha_{\mathbf{x}} \cdot \mathbf{x}_{it} + \xi_i + \nu_{it} \ . \tag{8}$$

Recognizing that $p_s = \frac{p_e}{\mu}$, and that $\ln(p_s) = \ln(p_e) - \ln(\mu)$, it can be seen that the specification of Model 2 is functionally equivalent to that of Model 1. In fact, if we impose the restriction

$$H_0 : \alpha_\mu = -\alpha_{p_e} \tag{9}$$

on Model 1, we exactly get Model 2. Hence, testing the null-hypothesis H_0 using Model 1 allows for a simple examination of whether both models are equivalent. Moreover, the anti-symmetry reflected by H_0 is intuitive: for constant fuel prices p_e, *raising* the energy efficiency μ should have the same effect on the service price p_s, and hence on the distance traveled, as *falling* fuel prices p_e given a constant energy efficiency μ. Lastly, testing the null is also a test whether $p_s = \frac{p_e}{\mu}$ and thus whether the proportionality assumption underlying efficiency definition (2) is appropriate.

Corresponding to our third definition of the rebound effect, the final specification regresses the logged monthly liters of fuel consumed, $\ln(e)$, on $\ln(p_e)$ and the vector of control variables $\mathbf{x}$.

Model 3:

$$\ln(e_{it}) = \alpha_0 + \alpha_{p_e} \cdot \ln(p_{e_{it}}) + \alpha_{\mathbf{x}} \cdot \mathbf{x}_{it} + \xi_i + \nu_{it} \,. \tag{10}$$

According to Propositions 2 and 3, the rebound effect results from the negative of the price coefficient: $\eta_\mu(s) = -\eta_{p_e}(e) = -\alpha_{p_e}$. It also bears noting that it is possible to examine whether Model 3 differs from Model 2 by testing the hypothesis

$$H_0 : \alpha_{p_e} = -1 \tag{11}$$

on the basis of the estimates of Model 3 and, additionally, by testing the hypothesis

$$H_0 : \alpha_{p_s} = -1. \tag{12}$$

on the basis of the estimates of Model 2. Only if both hypotheses were to hold, Model 2 and 3 would be identical, as can be seen by inserting restriction (11) into model formulation (10) and employing efficiency definition (2), $\mu = s/e$, and relationship (3), $p_s = p_e/\mu$. Of course, if one of these hypotheses is rejected, it follows that Model 3 and Model 1 are also not identical.

Panel data affords three principle approaches for econometric modeling: the fixed-, between-, and random effects estimators. The key advantage of using the fixed-effects estimator is that it produces consistent estimates even in the presence of time-invariant, unobservable factors (e.g. topography and urban form) that vary across observations and are correlated with the explanatory variables. This is a particularly useful feature for the analysis of the present data set given that we lack information on household income. To the extent that income remains relatively stable over the three-year survey period, its influence will be captured by the fixed effects.

In contrast to the fixed-effects estimator, in which dummy variables are included to capture the time-invariant, unobservable factors ξ_i that vary across observations, random effects treats these factors as part of the disturbances, thereby assuming that their correlation with the regressors is zero. If this assumption is met, the random-effects estimator is a viable alternative, as it confers the advantage of greater efficiency over the fixed-effects estimator. Violation of the assumption, however, implies biased estimates.

While most analyses neglect between effects, instead focusing on the choice between fixed and random effects, we see merit in applying all three estimators to the three model specifications. For starters, our relatively short panel of three years means that some of the regressors may have insufficient variability to be precisely estimated using fixed effects, a problem that does not afflict between effects given its reliance on cross-sectional information. Beyond this, the between-effects estimator, which is equivalent to an OLS regression of averages across time, conveys valuable economic content that is not otherwise revealed, telling us the cross-sectional effects of changes in an explanatory variable between subjects. Last but equally important, we distinguish between fixed and random effects using a test that, in essence, is based on the comparison of the fixed- and between effects.

This test is a slight variation of the HAUSMAN test commonly employed to test the null hypothesis that the fixed-effects are equal to the random-effects, which, if not rejected, would suggest adoption of the random-effects estimator due to its higher efficiency. Yet, testing the hypothesis that the fixed- and the random effects are equal is numerically identical to testing that the between- and fixed effects are equal – see e. g. BALTAGI 2005:67 – and thus that the inter-temporal within-subject effects are the same as the cross-sectional effects across subjects. As there is rarely a theoretical basis for this assumption, it must not be surprising if the null hypothesis of the HAUSMAN test is not found to withstand empirical scrutiny.

Exploiting the equivalence of between- and fixed effects under the null, we thus implement modified versions of our Models 1 to 3 that easily allow us to examine both the equality of the fixed- and between coefficients for individual variables as well as that of the whole range of coefficients (see Proposition 4 of the appendix). Chi square tests can then be used to determine for which variables the assumption of equivalence holds and which variables require separate specification of the fixed- and between effects. Using, say, Model 3, testing the null of the standard HAUSMAN test on the basis of the following specification,

$$\ln(e_{it}) = \alpha_0 + \alpha_{b_e} \cdot \ln(\bar{p}_{ei}) + \alpha_{w_e} \cdot (\ln(p_{eit}) - \ln(\bar{p}_{ei})) + \boldsymbol{\alpha}_{b_{\mathbf{x}}} \cdot \bar{\mathbf{x}}_i + \boldsymbol{\alpha}_{w_{\mathbf{x}}} \cdot (\mathbf{x}_{it} - \bar{\mathbf{x}}_i) + \xi_i + \nu_{it} \;, \quad (13)$$

translates to examining

$$H_0 : \alpha_{b_e} = \alpha_{w_e}, \boldsymbol{\alpha}_{b_{\mathbf{x}}} = \boldsymbol{\alpha}_{w_{\mathbf{x}}}. \tag{14}$$

Estimated using the random-effects estimator, specification (13) retrieves the entire set of fixed- and between-effects estimates, where α_{w_e} and $\boldsymbol{\alpha}_{w_{\mathbf{x}}}$ designate the fixed-effects coefficients and α_{b_e} and $\boldsymbol{\alpha}_{b_{\mathbf{x}}}$ the between-effects coefficients.

4 The German Mobility Panel Data Set

The data used in this research is drawn from the German Mobility Panel (MOP 2007), an ongoing travel survey that was initiated in 1994. The panel is organized in overlapping waves, each comprising a group of households surveyed for a period of one week in autumn for three consecutive years. All households that participate in the survey are requested to fill out a questionnaire eliciting general household information, person-related characteristics, and relevant aspects of everyday travel behavior. In addition to this general survey, the MOP includes another survey focusing specifically on vehicle travel among a sub-sample of randomly selected car-owning households. This survey takes place over a roughly six-week period in the spring, during which time respondents record the price paid for fuel, the liters of fuel consumed, and the kilometers driven with each visit to a gas station and for every car in the household.

The data used in this paper cover nine years of the survey, spanning 1997 through 2005, a period during which real fuel prices rose 3.2% per annum. To avoid complications of multiple car ownership due to substitution effects among cars, we focus on single-car households, which comprise roughly 53% of the households in Germany (MID 2007).[2] The resulting sample includes 574 households, 254 of which appear two years in the data and 293 of which appear in all three years of a panel-wave. To correct for the non-independence of repeated observations over the years of the survey, the regression disturbance terms are clustered at the level of the household, and the

[2]Of the remaining 47% of German households, 27% have more than one car and 20% have no car (MID 2007).

presented measures of statistical significance are robust to this survey design feature.

We used the survey information, which is recorded at the level of the automobile, to derive the dependent and explanatory variables required for estimating each of the three variants of the rebound effect. The two dependent variables, which are converted into monthly figures to adjust for minor variations in the survey duration, are the total monthly distance driven in kilometers (Definitions 1 and 2) and the total monthly liters of fuel consumed (Definition 3). The three explanatory variables for identifying the direct rebound effect are the kilometers traveled per liter (Definition 1), the price paid for fuel per kilometer traveled (Definition 2), and the price paid for fuel per liter (Definition 3).[3]

The remaining suite of variables selected for inclusion in the model measure the sociodemographic and automobile attributes that are hypothesized to influence the extent of motorized travel. Table 1 contains the definitions and descriptive statistics of all the variables used in the modeling. To control for the effects of quality (WIRL, 1997:14), the age of the automobile and a dummy indicating luxury models is included.[4] Although income is not directly measured, an attempt is made to proxy for its influence via measures of the number of employed residents and the number with a high school diploma living in the household. Finally, controls are included for household size, the presence of children, whether the household undertook a vacation with the car during the survey period, and whether any employed member of the household changed jobs in the preceding year.

[3]The price series was deflated using a consumer price index for Germany obtained from DESTATIS (2007).

[4]We also tested for quality-dependent rebound effects by interacting the efficiency measure with the luxury dummy, but found this to be insignificant in all of the specifications.

Table 1: Variable Definitions and Descriptive Statistics

Variable Definition	Variable name	Mean	Std. Dev.
Monthly kilometers driven	s	1156.82	714.55
Monthly fuel consumption in liters	e	94.82	59.33
Kilometers driven per liter	μ	12.51	2.76
Real fuel price in Euros per kilometer	p_s	0.08	0.02
Real fuel price in Euros per liter	p_e	0.96	0.13
Age of the car	*car age*	6.13	4.04
Dummy: 1 if fuel type is diesel	*diesel car*	0.10	0.30
Dummy: 1 if car is a sports- or luxury model	*premium car*	0.21	0.40
Number of household members	*household size*	1.98	1.04
Number of household members with a high school diploma	*high school diploma*	0.48	0.065
Number of employed household members	*# employed*	0.71	0.074
Dummy: 1 if household undertook car vacation during the survey period	*car vacation*	0.24	0.43
Dummy: 1 if children younger than 12 live in household	*children*	0.13	0.34
Dummy: 1 if an employed household member changed jobs within the preceding year	*job change*	0.10	0.30

5 Empirical Results

Our empirical analysis of the data involved the estimation of two sets of models, one in which the individual-specific component was specified at the level of the household and one in which it was specified at the level of the automobile. Noting that this distinction had little bearing on the qualitative conclusions of the analysis, the following discussion focuses on the estimates generated at the household level. This focus facilitates comparison of the three estimators as it ensures that each uses the same sample of observations. Were the individual component set at the level of the automobile, then observations in which the household changes automobiles from one year to the next would drop out in the case of the fixed-effects estimator.[5]

Table 2 presents estimates corresponding to Definition 1 of the rebound effect, in which fuel efficiency is regressed on the distance driven using fixed-, between-, and random-effects estimators. Several features of the results bear highlighting. First, we confirm that the impact of efficiency improvements on traveled distance is of the same order as the effect of fuel prices: As reported in the final row of the table, upon testing the null-hypothesis $H_0 : \alpha_\mu = -\alpha_{p_e}$, we cannot reject the anti-symmetry given by H_0 for any of the estimation techniques. Hence, there is no reason, neither on a theoretical nor an empirical basis, to assume that Model 1 and 2 are principally different, implying the conclusion that it is equally well-founded to estimate the direct rebound effect on the basis of either Definition (Model) 1 or Definition (Model) 2. Second, the estimated rebound effects are considerably higher than most estimates reported elsewhere in the literature, and suggest that some 58 % of the potential energy savings due to an efficiency improvement is lost to increased driving. Finally, these effects are of a strikingly similar magnitude across the three estimators, differing by less than a percentage point.

This similarity does not hold for many of the remaining coefficients. A particularly stark difference is seen for the effect of the number of employed household members, which has a counterintuitive and negative coefficient in the fixed-effects model, but is positive in the between- and random-effects models. All else equal, we would expect that a greater number of employed persons in the household would increase the dependency on the automobile. The negative fixed effects estimate is difficult to interpret, but may be the result of temporary disruptions associated with changes in the household labor force that reduce automobile travel.

The remaining control variables have either intuitive effects or are statistically insignificant. Referencing the random-effects coefficients, older cars are seen to be driven less, while premium cars are driven more. Another important determinant is whether a vacation with the car was undertaken over the survey period, which results in a roughly 35% (= exp(0.30) -1) increase in distance traveled. Aside from the fuel price

[5]Roughly 18% of households changed cars at least once over the three years of the survey.

and the number of employed household members, this is the only control variable also found to be significant in the fixed-effects model. We also explored models in which time dummies were included to control for autonomous changes in the macroeconomic environment. As these were found to be jointly insignificant across all of the models estimated, they were excluded from the final specifications.

Table 2: Estimation Results for Model 1 and the Rebound based on Definition 1.

	Fixed-Effects Estimator		Between-Group Estimator		Random-Effects Estimator		HAUSMAN Test
$\ln(s)$	Coeff.s	Std. Errors	Coeff.s	Std. Errors	Coeff.s	Std. Errors	χ^2 (1) Statistics
$\ln(\mu)$	** .585	(.122)	** .575	(.139)	** .584	(.105)	.01
$\ln(p_e)$	**-.622	(.164)	**-.595	(.211)	**-.588	(.127)	.02
car age	-.011	(.006)	**-.020	(.006)	**-.018	(.005)	.88
diesel car	-.232	(.182)	.120	(.102)	.014	(.090)	2.86
premium car	.140	(.147)	** .252	(.062)	** .220	(.052)	.47
household size	-.013	(.050)	.003	(.031)	*.040	(.025)	.05
# *high school diploma*	-.012	(.055)	** .095	(.036)	*.070	(.030)	2.82
# *employed*	**-.135	(.046)	** .208	(.037)	** .092	(.029)	** 33.8
vacation with car	** .275	(.037)	** .411	(.070)	** .300	(.031)	3.16
children	-.064	(.118)	.062	(.091)	.036	(.067)	.75
job change	.113	(.071)	.127	(.089)	** .145	(.055)	.03
constants	–	–	**7.768	(.284)	**7.854	(.219)	–
$H_0 : \alpha_\mu = -\alpha_{p_e}$	F(1, 545) = 0.03		F(1, 545) = 0.01		$\chi^2(1)$ = 0.02		
Standard HAUSMAN Test:			χ^2 (11) = 55.19**				

Note: * denotes significance at the 5 %-level and ** at the 1 %-level, respectively. Number of observations used for the estimations: 1,357. Number of households: 546.

Not unexpectedly, a HAUSMAN test rejects the null hypothesis that the fixed- and random-effects coefficients are jointly equal for all significance levels. Whether this result therefore implies that equality fails to hold for each of the variables individually is, however, not immediately clear. To pursue this issue further, we estimated the model in equation (13) and proceeded to test the equality restrictions using individual chi-square tests, the results for which are presented in the final column of Table 2. These findings confirm what was already evident from casual inspection: the difference between the fixed- and between-effects estimates of the rebound effect are statistically insignificant. In fact, this conclusion applies to several of the other explanatory variables, with the one clear exception being the number of employed people in the household.

Table 3 presents estimates of the rebound effect corresponding to Definition 2, based on a regression of distance traveled on the price of fuel per kilometer. As expected, the overall pattern is similar to that of Table 2. Again, the estimated rebound effects are high, roughly on the order of 59%. The remaining coefficient estimates are also similar to the first specification. The HAUSMAN test rejects equality of the fixed- and random-effects models for all significance levels. The only variable for which differences are clearly evident at the 1% level is again the number of employed household members.

Table 4 presents estimates of the rebound effect based on Definition 3, which is distinguished by the use of total fuel consumption as the dependent variable and the price of fuel per liter as the key regressor. That this model is not identical to Models 1 or 2 is confirmed by the rejection of the null hypotheses that the price coefficients in Models 3 and 2 are both -1. Despite these differences, the estimates in Table 4 are remarkably similar to those of Tables 3 and 2, albeit with a larger range across the fixed- and between-effects estimators. In this instance, the estimated rebound effect is seen to vary between 57% and 67%; but even here we cannot reject the hypothesis that the coefficients are equal based on the chi square test. Likewise, with the exception of the number of employed household members and the dummy indicating a car vacation, the other coefficients also appear to be equal despite the rejection of the HAUSMAN test for all significance levels.

We thus conclude that although our estimates of the rebound effect are high, they appear to be robust to both the estimator and the specification. Whether the model controls for time-invariant factors that vary across cases (as with the fixed effects estimator) or case-invariant factors that vary over time (as with the between effects estimator) has no substantial impact on the key results. Perhaps even more notable is the similarity of the estimates corresponding to Definition 3 with those of Definitions 1 and 2. While the latter definitions incorporate efficiency either directly via the kilometers per liter traveled or indirectly via the service price per kilometer, Definition 3 relies exclusively on the price mechanism, suggesting that this information can serve as a useful substitute in the absence of data on technology.

Table 3: Estimation Results for Model 2 and the Rebound based on Definition 2.

	Fixed-Effects Estimator		Between-Group Estimator		Random-Effects Estimator		HAUSMAN Test
$\ln(s)$	Coeff.s	Std. Errors	Coeff.s	Std. Errors	Coeff.s	Std. Errors	χ^2 (1) Statistics
$\ln(p_s)$	**-.596	(.099)	**-.581	(.122)	**-.585	(.084)	.02
car age	-.011	(.006)	**-.020	(.006)	**-.019	(.005)	.87
diesel car	-.226	(.180)	.121	(.098)	.017	(.089)	2.87
premium car	.140	(.147)	** .252	(.061)	** .220	(.051)	.48
household size	.013	(.050)	.003	(.031)	*.033	(.025)	.05
# high school diploma	-.012	(.055)	** .095	(.036)	*.072	(.030)	2.81
# employed	**-.135	(.046)	** .208	(.037)	**.082	(.029)	**33.8
vacation with car	** .274	(.037)	** .411	(.070)	**.306	(.032)	3.20
children	-.063	(.117)	.062	(.091)	.046	(.066)	0.74
job change	.113	(.071)	.126	(.088)	** .144	(.055)	.03
constants	–	–	**7.779	(.249)	**7.867	(.174)	–
$H_0 : \alpha_{p_s} = -1$	$F(1, 545)$ = 16.75**		$F(1, 545)$ = 11.88**		$\chi^2(1)$ = 24.37**		
Standard HAUSMAN Test:			χ^2 (11) = 55.31**				

Note: * denotes significance at the 5 %-level and ** at the 1 %-level, respectively. Number of observations used for the estimations: 1,357. Number of households: 546.

It also bears noting that in the appendix we distinguish between short-run and long-run rebound effects by estimating variants of our models that, as in WALKER AND WIRL (1993:185), include the lagged value of the dependent variable as a regressor. As is to be expected, the short-run effect directly obtained from the coefficient estimate of the lagged variable is smaller than the long-run rebound effect, registering nearly a two-fold increase in magnitude. Interestingly, the estimates of the rebound effects presented in Tables 2 to 4 are located somewhere in between the short- and long-run effects reported in the appendix.

Table 4: Estimation Results for Model 3 and the Rebound based on Definition 3.

	Fixed-Effects Estimator		Between-Group Estimator		Random-Effects Estimator		HAUSMAN Test
$\ln(e)$	Coeff.s	Std. Errors	Coeff.s	Std. Errors	Coeff.s	Std. Errors	χ^2 (1) Statistics
$\ln(p_e)$	**-.569	(.165)	**-.671	(.211)	** -.594	(.128)	.15
car age	-.010	(.007)	**-.019	(.006)	** -.018	(.005)	.84
diesel car	-.297	(.195)	.001	(.094)	-.076	(.091)	1.89
premium car	.149	(.149)	** .330	(.057)	** .285	(.050)	1.30
household size	.002	(.048)	.027	(.031)	*.045	(.024)	.16
# high school diploma	.002	(.055)	**.081	(.036)	*.065	(.029)	1.59
# employed	**-.117	(.044)	** .219	(.036)	** .093	(.029)	**33.4
vacation with car	** .254	(.036)	** .412	(.070)	** .289	(.032)	*4.19
children	-.020	(.118)	.053	(.092)	.055	(.066)	.25
job change	.116	(.070)	.103	(.089)	** .137	(.055)	.01
constants	–	–	**4.003	(.073)	** 4.098	(.063)	–
$H_0 : \alpha_{p_e} = -1$	F(1, 545) = 6.82**		F(1, 545) = 2.44		χ^2(1) = 10.09**		
Standard HAUSMAN Test:			χ^2 (11) = 57.57**				

Note: * denotes significance at the 5 %-level and ** at the 1 %-level, respectively. Number of observations used for the estimations: 1,357. Number of households: 546.

A few caveats should be recognized, perhaps the strongest of which is the assumption that automobile efficiency is exogenous. If, for example, individuals who drive more also select more fuel efficient vehicles, then we might expect an upward bias imparted on the rebound effect estimated by the coefficients of $\ln(\mu)$ and $\ln(p_s)$. However, there are two reasons why we do not deem endogeneity to be a serious concern here. First, any time-invariant unobservable factors that would otherwise induce correlation between the rebound effect and the error term (e.g. proximity to public transit, environmental attitudes) will be captured by the fixed effects model. Although we cannot exclude the possibility of relevant time-variant unobservables, we believe the range of included explanatory variables - the presence of young children, job changes, and the number of employed - provides reasonably good coverage of temporal changes whose absence could induce biases. Second, endogeneity problems relating to vehicle choice would not be expected to afflict the estimates from Definition 3 in Table 4, as these are based on the price of fuel. The fact that these estimates are of roughly the same magnitude as those from Definitions 1 and 2 provides some confirmation that any upward bias from endogeneity is negligible.

An additional caveat arises from the analysis' neglect of changes in capital costs due to efficiency improvements in automotive technology. Specifically, more costly equipment reduces disposable income, thereby triggering an income effect that mitigates the rebound. Several authors, such as HENLEY *et al.* (1988), therefore argue that neglecting capital cost would lead to an overestimation of the rebound when relying on definitions of the direct rebound effect, such as Definitions 1, 2, and 3. On the other hand, the correlation of energy and time efficiency may lead to an underestimation of the rebound: If more efficient cars also reduce transport time, the increased time for leisure, for example, may imply a higher energy consumption due to energy-intensive leisure activities. As the mobility data analyzed here includes no information on annualized capital cost k_i, nor time requirements, nor the consumption of other goods o_i, we are not able to pursue these issues. To date, only a few empirical studies, such as BRÄNNLUND, GHALWASH, NORDSTRÖM (2007), have availed data that allows for explicit consideration of income effects on the consumption of goods other than fuel, thereby enabling to estimate the net rebound including the indirect rebound effect. This is clearly an area warranting further study.

A final caveat pertains to the exclusion of multiple-car households from the models. To the extent that such households enjoy greater substitution possibilities, they would be better positioned to alter automobile efficiency in response to changes in fuel prices by switching cars. We might therefore expect to find a smaller rebound effect given that this strategy substitutes for reducing vehicle kilometers. While the relatively small share of multiple-car households precluded detailed analysis of this issue, we see little evidence for it compromising the generality of our findings.

6 Summary and Conclusion

Industrialized countries are increasingly struggling both to ensure their security of energy supply and to reduce emissions of greenhouse gases. It is commonly asserted that efficiency-increasing technological innovations, particularly in the transport sector, are an important pillar in this process. This assertion underpins the CAFE standards in the United States and the more recently reached voluntary agreement between the European Union and the European Automobile Manufacturers Association (ACEA) stipulating the reduction of average emissions in the new car fleet.

Although increased efficiency confers economic benefits in its own right, its effectiveness in reducing fuel consumption and pollution depends on how consumers alter behavior in response to cheaper per-unit energy prices due to improved efficiency. To the extent that service demand increases via rebound effects, gains in reducing environmental impacts and energy dependency will be offset. The results presented in this paper, based on the analysis of a German household panel, suggest that the size of this offset is potentially quite large, varying between 57% and 67%. Stated alternatively, the relative reduction in energy use due to a percentage change in efficiency is on the order of 33% to 43%.

While these estimates are considerably higher than those found elsewhere in the literature, with most empirical evidence originating from the U.S., our results are robust to both alternative panel estimators and to alternative measures of the rebound effect. Moreover, our results are consistent with recent anecdotal evidence from Germany. Between 2004 and 2005, fuel prices increased by 5% while average road milage decreased by 3% (MWV 2007), suggesting a sizeable price elasticity of -0.6. One possible explanation for the discrepancy between the estimates in this study and those from the U.S. may be superior access to public transport in Germany. Related to this, longer trip distances in the U.S., particularly for commuting (STUTZER, FREY 2007), likely decrease the responsiveness of mode choice to changes in automobile efficiency.

As this is one of the few studies to be conducted on this issue in a European context, it would be of interest to see whether the qualitative findings presented here are corroborated by studies using other data sets from within Germany and other European countries. If this is found to be the case, it would suggest that policy interventions targeted at technological efficiency - be they voluntary agreements or command and control measures - may have only muted effects in reducing fuel consumption. At the very least, our results indicate that the current emphasis on efficiency as the principal means for policy-makers to address environmental challenges (see *e.g.* BMU 2007) may be misplaced. Given the strong responses to prices found here, price-based instruments such as fuel taxes would appear to be a more effective policy measure.

Appendix A: Proofs

Proof of Proposition 1: Employing efficiency definition (2) and taking logarithms, we get $\ln e = \ln s - \ln \mu$. Logarithmic differentiation with respect to μ yields the claim:

$$\eta_\mu(e) = \frac{\partial \ln e}{\partial \ln \mu} = \frac{\partial \ln s}{\partial \ln \mu} - \frac{\partial \ln \mu}{\partial \ln \mu} = \frac{\partial \ln s}{\partial \ln \mu} - 1 = \eta_\mu(s) - 1 \ ,$$

thereby exploiting the fact that, for a desired amount of service, the input e of energy is, ultimately, a function of the parameter μ, making it meaningful to take the derivative of $s = f(e(\mu), k, o, t)$ with respect to μ. This is also seen when taking account of efficiency definition (2): $s = f(e(\mu), k, o, t) = e \cdot \mu \cdot g(k, o, t)$, where g is only a function of k, o, and t.

Proof of Proposition 2: Using (3), the chain rule, and the explicit assumption that the service amount s solely depends on the price p_s, we obtain

$$\begin{aligned} \eta_\mu(s) &= \frac{\partial \ln s}{\partial \ln \mu} = \frac{\partial \ln s}{\partial \ln p_s} \cdot \frac{\partial \ln p_s}{\partial \ln \mu} = \eta_{p_s}(s) \cdot \frac{\partial \ln(p_e/\mu)}{\partial \ln \mu} = \\ &= \eta_{p_s}(s) \cdot [\frac{\partial \ln p_e}{\partial \ln \mu} - \frac{\partial \ln \mu}{\partial \ln \mu}] = \eta_{p_s}(s) \cdot [\frac{\partial \ln p_e}{\partial \ln \mu} - 1]. \end{aligned}$$

If energy prices p_e are exogenous, $\frac{\partial \ln p_e}{\partial \ln \mu} = 0$, so that $\eta_\mu(s)$ equals $-\eta_{p_s}(s)$.

Proof of Proposition 3: Using the chain rule and the definition (2) of energy efficiency as well as of service prices, (3), we get

$$\begin{aligned} \eta_{p_e}(e) &= \frac{\partial \ln e}{\partial \ln p_e} = \frac{\partial \ln e}{\partial \ln p_s} \cdot \frac{\partial \ln p_s}{\partial \ln p_e} = \frac{\partial \ln(s/\mu)}{\partial \ln p_s} \cdot \frac{\partial \ln(p_e/\mu)}{\partial \ln p_e} = \\ &= [\eta_{p_s}(s) - \frac{\partial \ln \mu}{\partial \ln p_s}] \cdot [\frac{\partial \ln p_e}{\partial \ln p_e} - \frac{\partial \ln \mu}{\partial \ln p_e}] = [\eta_{p_s}(s) - \frac{\partial \ln \mu}{\partial \ln p_s}] \cdot [1 - \frac{\partial \ln \mu}{\partial \ln p_e}]. \end{aligned}$$

Hence, only if $\frac{\partial \ln \mu}{\partial \ln p_s} = 0$ and $\frac{\partial \ln \mu}{\partial \ln p_e} = 0$, which holds true if the energy efficiency μ is constant, both elasticities are equal: $\eta_{p_e}(e) = \eta_{p_s}(s)$.

Appendix B: Hausman Test for Individual Variables

The following Proposition 4 is a generalization of BALTAGI's (2005:76) exercise formulated in a bivariate context and allows for easily examining the null hypothesis of the classical HAUSMAN test that is typically employed for the distinction of fixed versus random effects. The null of this test is equivalent to the hypothesis that the fixed-effects estimator equals the between-effects estimator BALTAGI (2005:67):

$$H_0 : \boldsymbol{\beta}_w = \boldsymbol{\beta}_b.$$

In addition to testing H_0 using a Chi square test, representation (13) also allows for examining the equality of the fixed- and between coefficients for individual variables, when the random-effects estimator is used.

Proposition 4: Departing from a standard panel data model,

$$y_{it} = \beta_0 + \boldsymbol{\beta}^T \mathbf{x}_{it} + \mu_i + u_{it}, \quad i = 1, ..., N, t = 1, ..., T, \tag{15}$$

and estimating the regression model

$$y_{it} = \beta_0 + \boldsymbol{\beta}_w^T (\mathbf{x}_{it} - \bar{\mathbf{x}}_i) + \boldsymbol{\beta}_b^T \bar{\mathbf{x}}_i + \mu_i + u_{it}, \tag{16}$$

via OLS simultaneously yields estimates of the between- and fixed effects, where the OLS estimator of $\boldsymbol{\beta}_w$ provides for the fixed-effects estimator and the between-effects estimator is given by the OLS estimator of $\boldsymbol{\beta}_b$.

Proof of Proposition 4: First, the between-effects estimator of parameter vector $\boldsymbol{\beta}$ emerging from model (15) can be obtained by averaging (15) over time and estimating the result via OLS:

$$\bar{y}_i = \beta_0 + \boldsymbol{\beta}^T \bar{\mathbf{x}}_i + \mu_i + \bar{u}_i. \tag{17}$$

Second, the fixed-effects estimates of $\boldsymbol{\beta}$ is to be received by subtracting (17) from (15) and estimating the result via OLS:

$$y_{it} - \bar{y}_i = \boldsymbol{\beta}^T (\mathbf{x}_{it} - \bar{\mathbf{x}}_i) - \bar{u}_i + u_{it}. \tag{18}$$

Third, instead of estimating either (17) or (18), we alternatively suggest in Proposition 4 estimating (16) via OLS in order to at once get both the fixed- and between-effects estimates of $\boldsymbol{\beta}$. This can be seen as follows: Upon averaging (16) over time, the term related to $\boldsymbol{\beta}_w$ wipes out so that the result is, aside from the notation of the parameter vector, identical to (17):

$$\bar{y}_i = \beta_0 + \boldsymbol{\beta}_w^T \cdot \mathbf{0} + \boldsymbol{\beta}_b^T \bar{\mathbf{x}}_i + \mu_i + \bar{u}_i. \tag{19}$$

Hence, whether averaging either (15) or (16) over time and estimating the results via OLS must yield the same estimates, namely the between-effects estimates of $\boldsymbol{\beta}$. Next, inserting (19) into (16) yields (18) with $\boldsymbol{\beta}_w$ instead of $\boldsymbol{\beta}$ as parameter vector. In other words, either demeaning (15) or (16) and estimating the result via OLS provides for the same estimates of $\boldsymbol{\beta}$, namely the fixed-effects estimates.

Appendix C: Short- and Long-Run Rebound Effects

The difference between short- and long-run effects is explored by estimating specifications that include the lagged value of the dependent variable as a regressor (WALKER, WIRL 1993:185). As Model 1 and Model 2 differ only if the null hypothesis $H_0 : \alpha_\mu = -\alpha_{p_e}$ is invalid and we fail to reject the anti-symmetry given by H_0, we focus on Definitions 1 and 3 of the rebound effect using the random effects estimator:

Long-run Variant of Model 1:

$$\ln(s_{it}) = \alpha_0 + \alpha_1 \cdot \ln(s_{i(t-1)}) + \alpha_\mu \cdot \ln(\mu_{it}) + \alpha_{p_e} \cdot \ln(p_{eit}) + \alpha_{\mathbf{x}} \cdot \mathbf{x}_{it} + \xi_i + \nu_{it} \, . \tag{20}$$

Long-run Variant of Model 3:

$$\ln(e_{it}) = \beta_0 + \alpha_1 \cdot \ln(e_{i(t-1)}) + \beta_{p_e} \cdot \ln(p_{eit}) + \beta_{\mathbf{x}} \cdot \mathbf{x}_{it} + \zeta_i + \varepsilon_{it} \, , \tag{21}$$

where the coefficients α_μ and β_{p_e} are the short-run rebound effects, respectively.

On the basis of (20) and following (WALKER, WIRL 1993:185), the long-run rebound effect is to be determined as

$$\alpha_\mu/(1-\alpha_1), \tag{22}$$

since in the long run it may be assumed that $s_{it} = s_{i(t-1)} = s_i$. For Definition 1, the long-run effect is estimated to be 0.80, with an almost identical magnitude estimated for Definition 3. As expected, these effects are higher than the short-run estimates reported in Table C1, registering nearly a two-fold increase in magnitude.

Table C1: Short-Run Rebound Effects for Model 1 and 3.

	Model 1			Model 3	
$\ln(s)$	Coeff.s	Std. Errors	$\ln(e)$	Coeff.s	Std. Errors
$\ln(s_{t-1})$	** .406	(.040)	$\ln(e_{t-1})$	** .443	(.125)
$\ln(\mu)$	** .684	(.121)	–	–	–
$\ln(p_e)$	**-.476	(.128)	$\ln(p_s)$	**-.444	(.038)
car age	* -.009	(.005)	*car age*	$-.008	(.005)
diesel car	-.041	(.077)	*diesel car*	.033	(.069)
premium car	** .178	(.051)	*premium car*	** .147	(.046)
household size	-.020	(.024)	*household size*	.011	(.022)
# high school diploma	**.080	(.027)	*# high school diploma*	** .085	(.027)
# employed	.049	(.030)	*# employed*	.045	(.029)
vacation with car	** .281	(.044)	*vacation with car*	** .263	(.044)
children	.039	(.070)	*children*	.048	(.069)
job change	** .191	(.056)	*job change*	** .188	(.057)
constants	**5.289	(.363)	*constants*	**2.230	(.164)

Note: * denotes significance at the 5 %-level and ** at the 1 %-level, respectively. Number of observations used for the estimations: 816. Number of households: 535.

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