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Conference Paper
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From fuel taxation to efficiency standards: A wrong turn in European climate protection?

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Abstract. Using household travel diary data collected in Germany between 1997 and 2012, we employ an instrumental variable (IV) approach that enables us to consistently estimate both fuel price and efficiency elasticities at once. The aim is to gauge the relative impacts of fuel economy standards and fuel taxes on distance traveled. We find that the magnitude of the elasticity estimates are statistically indistinguishable: higher fuel prices reduce driving by the same degree as higher fuel efficiency increases driving. This finding indicates an offsetting effect of fuel efficiency standards on the effectiveness of fuel taxation, calling into question the efficacy of the European Commission’s current efforts to legislate CO2 emissions limits for new cars given prevailing high fuel taxes. The ecological implications of this legislation for emissions reductions is explored through a simple numerical simulation using the econometric estimates.

JEL classification: D12, Q41.

Key words: Automobile travel, instrumental variable approach, rebound effect.

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Acknowledgements: We are grateful for invaluable comments and suggestions by Christoph M. SCHMIDT, as well as an anonymous reviewer. This work has been partly supported by the German Federal Ministry of Education and Research (BMBF) within the framework of the project “Social Dimensions of the Rebound Effect” and by the Collaborative Research Center “Statistical Modeling of Nonlinear Dynamic Processes” (SFB 823) of the German Research Foundation (DFG), within the framework of Project A3, “Dynamic Technology Modeling”.


1 Introduction

Until a few years ago, the United States and the European Union pursued markedly different policies to contain emissions from automobile transport, with the US relying on the corporate average fuel economy (CAFE) standards and the EU relying on fuel taxation. These policies began to converge in April 2009 when the European Commission passed legislation requiring automakers to reduce the average per-kilometer carbon dioxide (CO$_2$) emissions of newly registered automobiles to 130g/km by 2015 (EC, 2009), with an even stricter standard of 95g/km now set for 2020.

As illustrated in Figure 1, the implementation of the 130g standard in 2009, which is equivalent to roughly 42 (48) miles per gallon of petrol (diesel) fuel (EC, 2014),\(^1\) marked the end of a 10-year period in which the fuel economy of the new car fleet in Europe increased substantially, rising nearly 20% from 33.5 miles per gallon (mpg) in 2000 to 40.1 mpg in 2009 (ODYSSEE, 2014).\(^2\) In the US, the same time interval saw an increase in fuel efficiency that was considerably more modest, rising 10% from 22.4 to 24.8 mpg (EPA, 2012). Of course, such a comparison does not unequivocally point to the superiority of one policy instrument over the other, but it does raise the question of whether the EU’s coupling of an efficiency standard with a system of high fuel taxes – one within which the efficiency of the car fleet has risen relatively rapidly – makes economic sense.

According to a press release published by the Commission in 2007, the CO$_2$ limits in the new legislation would “reduce the average emissions of CO$_2$ from new passenger cars in the EU from around 160 grams per kilometer to 130 grams per kilometer,” which would “translate into a 19% reduction of CO$_2$ emissions” (EC, 2007). But whether a CO$_2$ reduction of this magnitude in fact materializes depends fundamentally on the behavioral response of motorists to increased efficiency. Presuming that mobility is a conventional good, a decrease in the cost of driving due to an improvement in fuel

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\(^1\)The 130g standard requires 5.6 liters of petrol per 100 km and 4.9 liters of diesel per 100 km.

\(^2\)Expressed in metric units, the increase was from from 14.2 km/liter to 17.0 km/liter. Note that 1 mile equals 1.609 kilometers and 1 gallon equals 3.785 liters.
efficiency would result in an increased demand for car travel. This demand increase is referred to as the rebound effect (KHAZZOOM, 1980), as it offsets – at least partially – the reduction in energy demand that would otherwise result from an increase in efficiency. Though the existence of the rebound effect is widely accepted, its magnitude remains a contentious issue (e.g. BROOKES, 2000; BINSWANGER, 2001; SORRELL and DIMITROPOULOS, 2008).

Figure 1: **Average Fuel Economy of New Passenger Cars and EU Standards for New Diesel and Petrol Cars by 2015** (BMW, 2014; ODYSSEE, 2014; EPA, 2012).

Proponents of increased efficiency standards generally play down the magnitude of the rebound effect, arguing that the standards not only decrease emissions and dependence on imported oil, but also reduce motorists’ fuel expenses. Opponents argue that standards are a costly way to reduce gasoline consumption because, unlike a fuel tax, they fail to harness price signals (AUSTIN and DINAN, 2005; CRANDALL, 1992; KARPLUS et al., 2013; KLEIT, 2004; MANKIW, 2013; STERNER, 2007). This paper scrutinizes both viewpoints and gauges the relative impacts of fuel economy standards and fuel taxes on distance traveled by using detailed household travel diary data collected in Germany between 1997 and 2012 to consistently estimate both fuel price and efficiency elasticities at once.
Germany provides an interesting case study on this question: despite having one of the highest car ownership rates in Europe, the country has reduced CO\textsubscript{2} emissions from transport by 6% between 1990 and 2009, thereby bucking the 27% increase in transport emissions in the EU as a whole (EEA, 2011a). One contributing factor to this reduction has been the high German fuel taxes, whose rates of 65.45 cents per liter for petrol and 47.07 cents per liter for diesel are among the highest in the EU. These high tax rates result in high prices at the pump: An average German driver pays roughly double the price per gallon of fuel as a U. S. driver and drives less than half the distance, about 11,000 kilometer per year compared with 24,000 kilometers, respectively.

An immediate challenge in consistently estimating the rebound effect is endogeneity bias. Contrasting with fuel prices, which can generally be regarded as exogenous to households, fuel efficiency is potentially endogenous owing to unobserved household characteristics that affect both the decision on the distance driven and the fuel economy of the vehicle when it is purchased. Unobserved environmental preferences, for example, may trigger the purchase of a car with a high fuel efficiency, but may also lead to low driving distances. These preferences may therefore be correlated with regressors capturing fuel efficiency. Moreover, simultaneity biases may result from the fact that drivers who are prepared to drive longer distances, for instance, because of a job change, may tend to purchase more fuel-efficient cars.

Several features of our approach ameliorate these potential problems. First, the panel dimension of our data allows the inclusion of fixed effects to control for the influence of unobserved heterogeneity that stays fixed over time. Second, we address the endogeneity problem that potentially plagues the estimate of the fuel efficiency elasticity by employing the vehicle tax rate per 100 \textit{cm}^3 engine capacity as an instrumental variable (IV). This vehicle tax, which is administered by the federal government in Germany and derived from a combination of the cubic capacity of the vehicle and its emissions, is argued to be correlated with fuel efficiency, but uncorrelated with mileage, thereby fulfilling both identification assumptions of IV estimators. Other potential IVs are also explored, specifically the fuel prices at the time of the purchase of the
vehicle and the average CO$_2$ emissions per kilometer of the fleet of the car manufacturer. For both these IV candidates, one can safely expect that they do not determine the contemporaneous driving distances of individual households. Our empirical evidence suggests, however, that these IV candidates are very weakly correlated with the variable to be instrumented, rendering them weak instruments. Last, we approach the estimation of the rebound effect from two angles, one of which indirectly infers the rebound effect from estimating the fuel price elasticity, while the other relies on the estimation of the fuel efficiency elasticity. This provides for a direct estimate of the rebound, because the identification of the rebound effect via the estimation the fuel efficiency elasticity does not hinge on a series of restrictive assumptions (see Section 3).

By employing an IV approach, the present study advances our earlier work (FRONDEL, PETERS, VANCE, 2008; FRONDEL, RITTER, VANCE, 2012; FRONDEL, VANCE, 2013) by simultaneously relaxing two assumptions that, as pointed by LINN (2013), have been commonly invoked in the literature on the rebound effect. The first assumption is that increases in fuel prices and fuel efficiency have opposing impacts on vehicle miles traveled (VMT), but these impacts are equal in magnitude. If true, this equality would seriously undermine the case for Europe’s turn to efficiency standards, since by increasing driving, the standards would offset the effectiveness of existing fuel taxes. The second assumption is that fuel efficiency is uncorrelated with unobserved attributes of the motorist or car that affect the utility of driving. Hence, to test the first assumption requires controlling for any such correlation, accomplished here through the joint application of fixed effects and instrumental variables.

Two main results emerge from our analysis. First, the rebound estimates obtained here for single-vehicle households are in the range of 44 to 71%, which is in line with our earlier studies for Germany (e.g. FRONDEL, RITTER, VANCE, 2012; FRONDEL, VANCE, 2013). As these studies do not instrument for efficiency, but rather rely on fuel price elasticity estimates to infer the size of the rebound effect, they cannot formally test whether the response to increased efficiency is equal in magnitude to the response to increased fuel prices. In this regard, our second key finding is that the magnitudes of
the price and efficiency elasticities are statistically indistinguishable: Higher fuel prices reduce driving by the same degree as higher fuel efficiency increases driving, suggesting an offsetting effect of fuel efficiency standards on the effectiveness of fuel taxation. To our knowledge, in the context of panel data, this study is the first attempt to relax both assumptions.

The following section describes the panel data set. Section 3 offers a concise overview of the common definitions of the direct rebound effect and motivates our choice of definitions for estimation purposes, followed by a description of the estimation method. The presentation and interpretation of the results is given in Section 4. Section 5 undertakes a simple simulation that explores the implications of the rebound estimates for the long-run emissions reductions resulting from the EU legislation. The last section summarizes and concludes.

2 Data

The data used in this research is drawn from the German Mobility Panel (MOP 2013) and covers sixteen years, spanning 1997 through 2012 (see FRONDEL, PETERS, and VANCE (2008) for more details on this survey). By focusing on single-car households, which comprise roughly 62% of all car-owning households in Germany (RITTER and VANCE, 2013), we abstract from complexities associated with the substitution between cars in multi-vehicle households, thereby ensuring that we obtain results that are comparable to our former studies. The resulting estimation sample comprises a total of 2,596 observations covering 1,124 households, which is much larger than the sample underlying our first study. In fact, relative to FRONDEL, PETERS, and VANCE (2008), we employ seven additional survey waves including the years 2006 to 2012.

Travel survey information, which is recorded at the level of the automobile, is used to derive the dependent and explanatory variables. The dependent variable is given by the total monthly distance driven in kilometers (Table 1). Corresponding to the two alternate definitions of the rebound effect that are elaborated below, the key
explanatory variables for identifying the direct rebound effect are efficiency $\mu$ and the real price $p$ paid for fuel per liter.\(^3\)

**Table 1: Variable Definitions and Descriptive Statistics**

<table>
<thead>
<tr>
<th>Variable Name</th>
<th>Variable Definition</th>
<th>Mean</th>
<th>Std. Dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>$s$</td>
<td>Monthly kilometers driven</td>
<td>1,119</td>
<td>686</td>
</tr>
<tr>
<td>$s_d$</td>
<td>Monthly kilometers driven with a diesel car</td>
<td>1,579</td>
<td>839</td>
</tr>
<tr>
<td>$s_p$</td>
<td>Monthly kilometers driven with a petrol car</td>
<td>1,011</td>
<td>595</td>
</tr>
<tr>
<td>$\mu$</td>
<td>Fuel efficiency in kilometers per liter</td>
<td>13.1</td>
<td>2.9</td>
</tr>
<tr>
<td>$\mu_d$</td>
<td>Efficiency of diesel cars in kilometers per liter</td>
<td>15.4</td>
<td>3.0</td>
</tr>
<tr>
<td>$\mu_p$</td>
<td>Efficiency of petrol cars in kilometers per liter</td>
<td>12.5</td>
<td>2.6</td>
</tr>
<tr>
<td>$p$</td>
<td>Real fuel price in € per liter</td>
<td>1.18</td>
<td>0.15</td>
</tr>
<tr>
<td>$p_d$</td>
<td>Real diesel price in € per liter</td>
<td>1.01</td>
<td>0.15</td>
</tr>
<tr>
<td>$p_p$</td>
<td>Real petrol price in € per liter</td>
<td>1.14</td>
<td>0.14</td>
</tr>
<tr>
<td>diesel car</td>
<td>Dummy: 1 if the car is a diesel</td>
<td>0.19</td>
<td>–</td>
</tr>
<tr>
<td>tax rate</td>
<td>motor vehicle tax rate per 100 cm(^3) in € per year</td>
<td>6.66</td>
<td>3.22</td>
</tr>
<tr>
<td># children</td>
<td>Number of children younger than 18 in the household</td>
<td>0.26</td>
<td>0.62</td>
</tr>
<tr>
<td># employed</td>
<td>Number of employed household members</td>
<td>0.75</td>
<td>0.78</td>
</tr>
<tr>
<td>income</td>
<td>Real Household income in 1,000 €</td>
<td>2.27</td>
<td>0.79</td>
</tr>
<tr>
<td># high school diploma</td>
<td>Number of household members with a high school diploma</td>
<td>0.62</td>
<td>0.74</td>
</tr>
<tr>
<td>job change</td>
<td>Dummy: 1 if an employed household member changed jobs within the preceding year</td>
<td>0.09</td>
<td>–</td>
</tr>
<tr>
<td>vacation with car</td>
<td>Dummy: 1 if household undertook vacation with car during the survey period</td>
<td>0.22</td>
<td>–</td>
</tr>
<tr>
<td>urban area</td>
<td>percentage of area classified as urban</td>
<td>0.19</td>
<td>0.18</td>
</tr>
<tr>
<td>$(\text{mesh}_{eff})^{-1}$</td>
<td>landscape fragmentation, see formula (1)</td>
<td>0.95</td>
<td>1.07</td>
</tr>
</tbody>
</table>

Given an average efficiency of $\mu_d = 15.4$ kilometers per liter for diesel cars, i.e. $\mu_d = 36.2$ miles per gallon, versus $\mu_p = 12.5$ kilometers per liter ($\mu_d = 29.4$ mpg),\(^3\)

\(^3\)The price series was deflated using a consumer price index for Germany obtained from Destatis (2012).
for petrol cars, the well-known fact that the efficiency of diesel cars is substantially higher than that of comparable petrol cars is confirmed by the data. Furthermore, in Germany, diesel fuel is significantly cheaper per liter than petrol due to a lower tax rate of diesel that is about 18 € cent less per liter than that of petrol fuel. These are the two major reasons for the fact that the average distance driven is larger for diesel than for petrol cars. To control for potentially further differences between diesel and petrol cars beyond those that are already captured by the price and efficiency variables \( p \) and \( \mu \), respectively, we include a diesel dummy as additional regressor, thereby assuming that the (conditional) correlation with the error term is vanishing given that we control for fuel price \( p \) and efficiency \( \mu \): \( \text{Cov}(\text{diesel car}, \nu | \mu, p) = 0 \), where \( \nu \) denotes the error term. This exogeneity assumption seems warranted, as the primary appeal of diesel cars for households with large annual VMT should originate from the fuel price and efficiency advantages of diesel vehicles.

The suite of additional control variables that are hypothesized to influence the extent of motorized travel encompass, among others, the demographic composition of the household, its income, the surrounding landscape pattern, and dummy variables indicating whether any employed member of the household changed jobs in the preceding year and whether the household undertook a vacation with the car in the year of the survey.

Last, two landscape measures, which are derived from satellite imagery for the years 2000 and 2006 and linked with the MOP data using a Geographic Information System, deserve brief elaboration. Urban area is measured as the percent of area classified in the imagery as urban in the zipcode within which the household resides. We hypothesize that households located in areas characterized by a larger share of urban area are less dependent on the automobile because of the shorter travel distances separating origin from destination for standard activities like shopping, recreation and work. The second measure of landscape pattern is a metric commonly used in ecology:

\[
\text{mesh}_{\text{eff}} = \frac{1}{A_{\text{total}}} \sum_{i=1}^{n} A_i^2,
\]

where the subscript \( i \) indexes the patch and \( A_i \) measures its area. As described fur-
ther in JAEGER (2000), the effective mesh size defined by (1) provides a quantitative expression of landscape connectivity, one that has been widely implemented by various European countries as an indicator for environmental monitoring (EEA, 2011b). Following KELLER and VANCE (2013), we use the inverse of the effective mesh size, interpreted by ecologists as a measure of landscape fragmentation. The sign of this variable is ambiguous. To the extent that fragmented landscapes reflect a mix of uses, they may reduce car travel by decreasing the distance between destinations serving different purposes. Conversely, this variable may be positively associated with car travel given that highly fragmented landscapes typically necessitate longer travel distances over circuitous routes.

3 Methodological Issues

According to e. g. SORRELL and DIMITROPOULOS (2008), there are three conventional, but not equivalent definitions of the rebound effect:

**Definition 1:** $\eta_\mu(s) := \frac{\partial \ln s}{\partial \ln \mu}$, the elasticity of the demand for a particular energy service, such as lighting or mobility, in the amount of $s$ with respect to energy efficiency $\mu$, where in an individual mobility context $s$ is measured in vehicle kilometers or miles and energy efficiency $\mu$ is defined by (e. g. BINSWANGER, 2001:121):\(^4\)

$$\mu = \frac{s}{e} > 0. \quad (2)$$

**Definition 2:** $-\eta_{ps}(s)$, the negative of the elasticity of service demand $s$ with respect to service price $p_s := p_e / \mu$, which for given efficiency $\mu$ is proportional to the energy price $p_e$, and

\(^4\)This efficiency definition reflects the fact that the higher the efficiency $\mu$ of a given technology, the less energy $e = s / \mu$ is required for the provision of a service. The definition assumes proportionality between service level $s$ and energy input $e$ 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 between $s$ and $e$. 
**Definition 3:** $-\eta_{pe}(e)$, the negative of the price elasticity of energy demand $e$.

Definition 1 is the most natural definition of the direct rebound effect (BERKHOUT *et al.*, 2000), as, formally, the service demand response to energy efficiency changes is described by the elasticity of service demand with respect to efficiency. However, due to the likely endogeneity of energy efficiency (SORRELL, DIMITROPOULOS, SOMMERVILLE, 2009:1361), FRONDEL, RITTER, and VANCE (2012) argue that none of these definitions should be applied\(^5\) and instead suggest a fourth rebound definition that is based on the negative of the energy price elasticity of service demand, $\eta_{pe}(s)$:

**Definition 4:**

$$-\eta_{pe}(s) = -\frac{\partial \ln s}{\partial \ln pe}.$$  

(3)

Although not plagued by potential endogeneity problems, Definition 4 nonetheless rests on a series of strong assumptions that have to be invoked to ensure that it is equivalent to Definition 1. As elaborated by FRONDEL, RITTER, and VANCE (2012), these assumptions are threefold: distance traveled $s$ solely depends on $p_s$, fuel prices $p_e$ are exogenous, and energy efficiency $\mu$ is constant. As a consequence, while simultaneously identifying the rebound effect via Definition 4, here we focus on Definition 1 and employ IV methods to cope with the endogeneity of $\mu$.

In line with this focus, we estimate the following model specification, where the logged monthly vehicle-kilometers traveled, $\ln(s)$, is regressed on logged fuel prices, $\ln(p_e)$, logged fuel efficiency, $\ln(\mu)$, and a vector of control variables $x$ described in the previous section:

$$\ln(s_{it}) = \alpha_0 + \alpha_\mu \cdot \ln(\mu_{it}) + \alpha_{pe} \cdot \ln(p_{eit}) + \alpha_x^T \cdot x_{it} + \zeta_i + \nu_{it}.$$  

(4)

Subscripts $i$ and $t$ are used to denote the observation and time period, respectively. $\zeta_i$ denotes an unknown individual-specific term, and $\nu_{it}$ is a random component that varies over individuals and time. On the basis of this specification and Definition 1, the rebound effect can be identified by an estimate of the coefficient $\alpha_\mu$ on the logged fuel

\(^5\)An extensive discussion on why Definitions 1-3 appear to be inappropriate for both theoretical and empirical reasons can be found in FRONDEL, RITTER, and VANCE (2012).
efficiency, whereas Definition 4 implies that, if equivalent to Definition 1, the rebound effect can be obtained by the negative estimate of the coefficient $a_{p_e}$ on the logged fuel price.

To simultaneously estimate the rebound effect via both definitions requires an IV approach in which at least one instrumental variable is employed for the likely endogenous variable $\mu$. For an IV approach to be a reasonable identification strategy, any instrumental variable $z$ is required to be correlated with fuel efficiency $\mu$, i.e. $\text{Cov}(\mu, z) \neq 0$ (Assumption 1), while it should not be correlated with the error term $\varepsilon$: $\text{Cov}(\mu, \varepsilon) = 0$ (Assumption 2), where the components of $\varepsilon$ are given by $\varepsilon_{it} := \xi_i + \nu_{it}$. If either of these two identification assumptions is violated, employing $z$ as an instrument for $\mu$ is not a viable approach.

Our use of the tax rate per 100 cm$^3$ cubic capacity would seem to fulfill these requirements, although the second assumption is principally not testable. In Germany and elsewhere in Europe, the declared aim of this lump-sum tax is to privilege cars with low emissions. In fact, as of 2009, the tax was restructured to be based not only on cubic capacity, which is correlated with emissions, but also directly on emissions themselves. Petrol cars, for example, incur a tax of 2.00 € per 100 cm$^3$ cubic capacity, augmented by an additional tax penalty of 2.00 € for every gram of CO$_2$ emitted per kilometer beyond a threshold of 110 grams (ADAC, 2014). The corresponding figures for diesel are 9.50 € per 100 cm$^3$ cubic capacity with the same 2.00 € penalty for each gram exceeding the 110 gram threshold. Being negatively correlated with the endogenous variable fuel efficiency, but independent of annual driving distances, the motor vehicle tax rate per 100 cm$^3$ should consequently be an appropriate instrument, as it should not affect the dependent variable distance driven, nor the error term.

Apart from motor vehicle tax rates per 100 cm$^3$, we explored additional instruments, such as fuel prices at the time of the purchase of the vehicle (LINN, 2013) and the average vehicle efficiency prevailing in the household’s zip code of residence, all of which turned out to be very weakly correlated with fuel efficiency in terms of partial correlation coefficients. This leaves us with a single instrument for a single endogenous
variable, thereby obviating the need for over-identification tests. In this just-identified case, alternative estimators, such as two-stage least squares (2SLS) and the more general methods of moments estimator (GMM), reduce to the IV estimator (CAMERON, TRIVEDI, 2009:174,175).

Although the IV estimates should be estimated from a one-stage regression to obtain correct standard errors (WOOLDRIDGE, 2006:526), it is illuminating to conceive the IV estimation as a two-stage estimation procedure. In the first stage of such a two-stage least squares (2SLS) panel estimation approach, the following reduced form is estimated using ordinary fixed- or random-effects estimation methods:

\[
\ln(\mu_{it}) = \beta_0 + \beta_p \cdot \ln(p_{eit}) + \beta_z \cdot \ln(z_{it}) + \beta_x^T \cdot x_{it} + \eta_{it},
\]

where vector \( x \) includes the same control variables as in structural equation (4) and \( z \) is called the excluded instrument, because \( z \) represents our single instrumental variable tax rate that does not appear in specification (4). On the basis of the predictions \( \hat{\ln}(\mu) \) resulting from the first-stage estimation, the IV estimates are obtained in a second stage by estimating structural equation (4) using the predicted instead of the observed values of \( \ln(\mu) \). It bears noting that performing a t- or an F test on the coefficient \( \beta_z \) of the instrument \( z \) in the first stage would allow for testing the validity of Assumption 1.

An important drawback of IV estimates is that the related standard errors are typically larger than those of the respective OLS, fixed- or random effects estimates (BAUER, FERTIG, SCHMIDT, 2009:327). That is, if a variable that is deemed to be endogenous were actually to be exogenous, IV estimators would still be consistent, but less efficient than the OLS, fixed- or random effects estimators. Moreover, if an instrument is only weakly correlated with an endogenous regressor, the standard errors of IV estimators are even much larger, so that the loss of precision will be severe. Even worse is that with weak instruments, IV estimates are inconsistent and biased in the same direction as OLS estimates (CHAO and SWANSON, 2005). Most disconcertingly, as is pointed out by BOUND, JAEGGER, and BAKER (1993; 1995), when the excluded instruments are only weakly correlated with the endogenous variables, the cure in the form of the IV approach can be worse than the disease resulting from biased and inconsistent OLS
estimates. Given these potential problems, it is reasonable to perform an endogeneity test that examines whether a potentially endogenous variable is in fact exogenous, a question we take up in the following section.

4 Empirical Results

To provide for a reference point for the results obtained from our IV approach, we estimate structural model (4) using standard panel estimation methods, thereby ignoring the endogeneity of the fuel efficiency variable. Starting with the fixed-effects estimator, several features bear highlighting. First, noting from the discussion in the previous section that, according to Definition 4, the rebound effect can be identified by the negative of the coefficient of $\ln(p_e)$, the relevant estimate suggests that some 44% of the potential energy savings due to an efficiency improvement is lost to increased driving (see Table 2). In contrast, on the basis of Definition 1, which recurs on coefficient $\alpha_\mu$, the rebound effect is estimated to amount to about 71%.

From a statistical point of view, however, one cannot reject the hypothesis that both rebound effects, irrespective of whether identified according to Definition 1 or Definition 4, are identical. In fact, at any conventional level, the null hypothesis $H_0 : \alpha_{p_e} = -\alpha_\mu$ that the magnitudes of the fuel efficiency elasticity and the fuel price elasticity are equal cannot be rejected, as the test statistic of $F(1; 1, 123) = 3.75$ is less than the corresponding critical value of $F(1; \infty) = 3.84$ at the 5% significance level. This finding confirms former results obtained by Frondel, Peters, and Vance (2008). The equality of the size of the coefficients $\alpha_\mu$ and $\alpha_{p_e}$ reflected by $H_0$ is highly intuitive: for constant fuel prices $p_e$, raising the energy efficiency $\mu$ 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 $\mu$.

With respect to the remaining fixed-effects estimates, it is perhaps not surprising that many are statistically insignificant. This is clearly the result of very low variability of time-persistent variables, such as the number of children or the number of employed
household members. Three exceptions are the car vacation dummy and the landscape metrics measuring urban area and landscape fragmentation, the former two of which have positive and negative signs, respectively. The negative sign on the measure for landscape fragmentation is consistent with the notion of mixed land uses in reducing the need for travel.

Table 2: Fixed-Effects Estimation Results for Travel Demand of Single-Vehicle Households.\footnote{To correct for the non-independence of repeated observations from the same households over the years of the survey, observations are clustered at the level of the household, and the presented standard errors are robust to this survey design feature.}

<table>
<thead>
<tr>
<th></th>
<th>Ordinary Fixed Effects</th>
<th>IV Approach</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coeff.s</td>
<td>Std. Errors</td>
</tr>
<tr>
<td>( \ln(p_e) )</td>
<td>-0.438</td>
<td>(0.109)</td>
</tr>
<tr>
<td>( \ln(\mu) )</td>
<td>0.707</td>
<td>(0.092)</td>
</tr>
<tr>
<td>diesel car</td>
<td>-0.180</td>
<td>(0.105)</td>
</tr>
<tr>
<td># children</td>
<td>-0.058</td>
<td>(0.031)</td>
</tr>
<tr>
<td>income</td>
<td>0.002</td>
<td>(0.026)</td>
</tr>
<tr>
<td># employed</td>
<td>0.021</td>
<td>(0.030)</td>
</tr>
<tr>
<td># high school diploma</td>
<td>0.024</td>
<td>(0.033)</td>
</tr>
<tr>
<td>job change</td>
<td>0.061</td>
<td>(0.035)</td>
</tr>
<tr>
<td>vacation with car</td>
<td>**0.266</td>
<td>(0.028)</td>
</tr>
<tr>
<td>urban area</td>
<td>**-0.931</td>
<td>(0.355)</td>
</tr>
<tr>
<td>( (mesh_{eff})^{-1} )</td>
<td>**-1.512</td>
<td>(0.369)</td>
</tr>
<tr>
<td>tax rate</td>
<td>-</td>
<td>–</td>
</tr>
</tbody>
</table>

\( H_0 : \alpha_{p_e} = -\alpha_{\mu} \)

\( F(1; 1,123) = 3.75 \)  \( \chi^2(1) = 0.89 \)

Note: * denotes significance at the 5 % -level and ** at the 1 % -level, respectively.

Observations used for estimation: 2,596. Number of Households: 1,124.

Of course, interpretation of all the estimates from the standard fixed-effects regression is subject to the caveat that they may be biased from the potential endogeneity.
of μ. To explore this possibility, we follow Wooldridge (2006:532) in testing whether the error term η of the first-stage equation explaining efficiency is correlated with the error term ν of the structural equation. Although both η and ν cannot be observed, one can employ the residuals of the first- and second-stage regressions and test whether they are correlated. Alternatively, one can plug the residual ˆη as an additional regressor into structural equation (4) and test its statistical significance. In fact, this is the essential idea of the Durbin-Wu-Hausman test for endogeneity (Cameron, Trivedi, 2009:183). With a t statistic of 9.47 for the fixed-effects estimation using a cluster-robust covariance estimator, this test clearly rejects the hypothesis that ln(μ) is exogenous.

While this outcome suggests the application of the IV approach, its validity depends on the strength of our instrument. An initial indication is given by the highly significant coefficient estimate of the motor vehicle tax rate originating from the first-stage regression in the middle column of Table 2. We obtain the expected result that the tax rate is negatively correlated with the fuel efficiency of cars, reflecting the intention of the legislator to privilege cars with low emissions and, hence, high fuel efficiencies. A more formal gauge of the strength of the instrument is given by the rule of thumb of Staiger and Stock (1997), according to which the F statistic for the coefficient βz of the first-stage regression should exceed the threshold of 10 (Baum, Schaffer, Stillmann, 2007:490, Murray, 2006). With an F statistic of $F(1; 992) = 17.82$ resulting from the first-stage estimation using a heteroskedasticity-robust covariance estimator, we reject the hypothesis that the second-stage equation is weakly identified.\footnote{This rule accounts for the fact that, as Bound, Jaeger, and Baker (1995), Staiger and Stock (1997) and others have shown, the weak-instruments problem can arise even if the endogenous variables and the instruments are correlated at conventional significance levels of 5 and 1 % and the researcher is using a large sample (Baum, Schaffer, Stillmann, 2007:489).}

Moreover, the IV approach is based on the assumption that the excluded instruments affect the dependent variable only indirectly, through their correlations with the
included endogenous variables. Yet, if an excluded instrument exerts both direct and indirect influences on the dependent variable, the exclusion restriction must be rejected. This can be readily tested by including an excluded instrument as a regressor in the structural equation. Upon adding our instrumental variable \( z \), the tax rate per 100 cm\(^3\), as an additional regressor to the structural model (4), for the fixed-effects estimation, the resulting t statistics amounts to \( t = -0.46 \) when calculating heteroscedasticity-robust standard errors (not presented). This result does not allow for rejecting the hypothesis that \( z \) exerts no effect on the dependent variable, the logged monthly vehicle-kilometers traveled. With random-effects estimations, we come up with the same conclusion.

Turning to the IV regression in the final column, apart from the coefficient estimate of the fuel efficiency variable, the estimates do not differ substantially from those of the ordinary fixed-effects estimation. Specifically, the estimate of -0.439 on the fuel price coefficient shows that the rebound effect identified via Definition 4 is virtually identical to the rebound estimate of 0.438 resulting from the standard fixed-effects estimator.

Taking the drastic increase of the standard error of the instrumented variable \( \mu \) into account, a phenomenon that is rather typical for IV regressions, the estimate for \( \alpha_\mu \) of 0.953 is not statistically different from the fixed-effects estimate of 0.707, nor does the low chi-square statistic of \( \chi^2(1) = 0.89 \) indicate that the equal-size condition given by \( H_0 : \alpha_{p_e} = -\alpha_\mu \) is violated. In short, although our instrument passes the test on weak identification, the statistical insignificance of \( \alpha_\mu \) suggests that in our case the IV approach is not a successful strategy to identify the direct rebound effect on the basis of the most natural Definition 1. This is particularly unfortunate, as this identification strategy does not hinge on the additional assumptions that are required by Definitions 2 to 4 (see Section 3).

A similar pattern of results emerges from the random-effects estimates presented in Table 3. While the IV estimate for \( \alpha_\mu \) is not statistically different from zero, the IV estimate of the rebound effect according to Definition 4 is fairly close to that of the
standard random-effects estimation, which in turn is almost identical to the rebound estimate of 59.8% resulting from Definition 1. Again, for the ordinary random-effects estimation, the null hypothesis $H_0: \alpha_{p}\chi^2(1) = 0.06$ cannot be rejected, suggesting that from an empirical point of view, it is irrelevant whether the rebound effect is identified via Definition 1 or Definition 4.\(^9\)

Table 3: Random-Effects Estimation Results for Travel Demand of Single-Vehicle Households.

<table>
<thead>
<tr>
<th></th>
<th>Random Effects</th>
<th>1. Stage GLS</th>
<th>IV Random Effects</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coeff. Std. Errors</td>
<td>Coeff. Std. Errors</td>
<td>Coeff. Std. Errors</td>
</tr>
<tr>
<td>$\ln(p_e)$</td>
<td><strong>-0.573 (0.081)</strong></td>
<td><strong>-0.091 (0.029)</strong></td>
<td><strong>-0.541 (0.086)</strong></td>
</tr>
<tr>
<td>$\ln(\mu)$</td>
<td><strong>0.598 (0.069)</strong></td>
<td>-</td>
<td>0.188 (0.158)</td>
</tr>
<tr>
<td>diesel car</td>
<td><strong>0.148 (0.048)</strong></td>
<td><strong>0.038 (0.025)</strong></td>
<td><strong>0.240 (0.052)</strong></td>
</tr>
<tr>
<td># children</td>
<td>0.018 (0.018)</td>
<td>-</td>
<td>0.005 (0.023)</td>
</tr>
<tr>
<td>income</td>
<td><strong>0.061 (0.018)</strong></td>
<td><strong>-0.022 (0.006)</strong></td>
<td><strong>0.048 (0.018)</strong></td>
</tr>
<tr>
<td># employed</td>
<td><strong>0.117 (0.018)</strong></td>
<td>-</td>
<td>0.036 (0.020)</td>
</tr>
<tr>
<td># high school diploma</td>
<td>0.030 (0.019)</td>
<td>*0.014 (0.006)</td>
<td>0.036 (0.020)</td>
</tr>
<tr>
<td>job change</td>
<td>*0.067 (0.033)</td>
<td>*0.025 (0.011)</td>
<td>*0.079 (0.032)</td>
</tr>
<tr>
<td>vacation with car</td>
<td><strong>0.305 (0.024)</strong></td>
<td><strong>0.035 (0.007)</strong></td>
<td><strong>0.320 (0.023)</strong></td>
</tr>
<tr>
<td>urban area</td>
<td>*-0.231 (0.093)</td>
<td>-</td>
<td><strong>-0.241 (0.090)</strong></td>
</tr>
<tr>
<td>$(mesh_{eff})^{-1}$</td>
<td>-0.136 (0.119)</td>
<td>-</td>
<td>-0.146 (0.134)</td>
</tr>
<tr>
<td>tax rate</td>
<td>-</td>
<td><strong>-0.030 (0.001)</strong></td>
<td>-</td>
</tr>
</tbody>
</table>

$H_0: \alpha_{p}\chi^2(1) = \alpha_{\mu}$

$\chi^2(1) = 4.44$

Note: * denotes significance at the 5 %-level and ** at the 1 %-level, respectively.

Observations used for estimation: 2,596. Number of Households: 1,124.

In sum, although the IV estimates related to efficiency $\mu$ are imprecisely estimated, the other estimates of the rebound effect, which lie between 44 and 71%, are quite

\(^9\)A key reason for the high elasticities obtained across the models in Tables 2 and 3 might be that the elasticities from household-level data are generally larger than those from aggregate time series data (WADUD, GRAHAM, NOLAND, 2010:65). It also bears noting that much of the research on this topic, particularly that using household level data, is drawn from the US, where elasticity estimates may be lower because of longer driving distances and fewer alternative modes.
close to the rebound range of 57 to 67% estimated by FRONDEL, PETERS, and VANCE (2008) for the sub-sample of single-vehicle German households observed between 1997 and 2005 using standard panel estimation methods. The range of rebound effects obtained here even fits better to that identified by FRONDEL and VANCE (2013), who estimate rebound effects in the range of 46 to 70% for the sub-sample of single-vehicle households observed between 1997 and 2009. With our standard panel estimations thus confirming our former outcomes, we conclude that for IV estimations to be a sensible identification strategy, it seems most likely that the number of observations has to be drastically larger than in our case in order to improve the precision of the IV estimates of the fuel efficiency coefficient.

5 Implications for Emissions Reductions

Moving beyond the estimates of the rebound effects presented in the preceding section, we now explore their implications for the 2009 EU legislation limiting CO₂ emissions of new cars to 130g/km. We undertake a straightforward comparison of emissions reductions with and without the rebound effect, the aim of which is to gauge the extent to which the rebound effect offsets the emissions reductions that would otherwise be attained in its absence.

Following a similar analysis by LINN (2013) for the U. S. context, we begin by assuming a long-run time horizon in which every household of our sample as of 2009, the year the legislation went into effect, replaces its vehicle with one meeting the 130g/km target. This corresponds to a fuel economy of 17.9 km per liter for petrol cars and 20.4 km per liter for diesel cars or fuel consumption rates of 5.6 and 4.9 liters per 100 km, respectively (EC 2014). The result is an increase in the average fuel economy of the vehicles in the sample by 46.8%.

To get the corresponding emissions reduction that would occur in the absence of the rebound effect, we first calculate the actual emissions generated by the household based on the distance driven and the fuel efficiency of the vehicle. We then subtract
from this figure the hypothetical emissions that would be generated using the same
distance driven, but assuming that the vehicle is in compliance with the standard of
130g/km. Taking the average across the households in the data, the emissions reducti-
on from this calculation is on the order of 29.4%.

We next calculate the effect of the fuel economy increase on the distance driven
and emissions under alternative assumed values of the rebound effect taken from our
econometric estimates presented in the previous section. These are bounded by a ma-
ximum value of 0.707 from the fixed-effects estimate of Table 2 and a minimum value
of 0.188 from the IV random-effects estimate reported in Table 3. While the estimate
of 0.188 is not statistically significant, it serves as a lower bound reference point, being
the smallest of the estimated rebound coefficients.

Table 4 presents the average values from these calculations. Given a rebound ef-
effect of 70.7%, households in the data increase distance driven by an average of 33.1%,
which is obtained by multiplying the rebound effect with the percentage change in fuel
economy for each household in the data and taking the average over households. This
has the effect of reducing emissions by 8.6%, instead of the 29.4% that would be achie-
ved in the absence of the rebound effect. The corresponding figures for a rebound of
0.188 are 8.8% and 23.8%, respectively. Relative to the decrease in emissions in the no-
rebound case, we find that the rebound effect reduces the emissions decrease by about
19.0% to 70.7%, which, apart from rounding errors, perfectly reflects our rebound effect
estimates reported in the third row of Table 4.

This exercise thus indicates that with respect to emissions, the rebound is precise-
ly the same as the rebound effect with respect to fuel efficiency. Denoting the emission
factor of any fuel by \( f \), be it gasoline or diesel, this can be seen as follows: From energy
efficiency definition (2) and the emissions output \( \text{CO}_2 = f \cdot e = f \cdot s/\mu \) resulting from
the fuel consumption in the amount of \( e \), it follows that

\[
\frac{\partial \ln \text{CO}_2}{\partial \ln \mu} = \frac{\partial \ln f}{\partial \ln \mu} + \frac{\partial \ln s}{\partial \ln \mu} - \frac{\partial \ln \mu}{\partial \ln \mu} = \frac{\partial \ln s}{\partial \ln \mu} - 1, \tag{6}
\]

as \( \frac{\partial \ln f}{\partial \ln \mu} \) is vanishing due to the fact that emission factor \( f \) is a constant. Only if there
is no rebound effect, that is, only if \( \frac{\partial \ln s}{\partial \ln \mu} = 0 \) (see Definition 1), will a fuel efficiency
improvement result in a perfectly proportional emission reduction: \( \frac{\partial \ln \text{CO}_2}{\partial \ln \mu} = -1 \). Yet, if there is a rebound effect with respect to fuel efficiency, i.e. \( \frac{\partial \ln s}{\partial \ln \mu} > 0 \), there will be a rebound effect with respect to emissions of the same degree:

\[
R_{\text{CO}_2} := \frac{-1 - \frac{\partial \ln \text{CO}_2}{\partial \ln \mu}}{-1} = 1 + \frac{\partial \ln \text{CO}_2}{\partial \ln \mu} = \frac{\partial \ln s}{\partial \ln \mu'},
\tag{7}
\]

where the last equation follows from equation (6) and the emissions rebound \( R_{\text{CO}_2} \) is defined as relative deviation from the no-rebound case.

**Table 4**: Long-run Effect of the European Commission’s 130 g/km Emissions Standard on Driving Distances and Emissions.

<table>
<thead>
<tr>
<th>Without Rebound Effect</th>
<th>Accounting for the Rebound Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percentage Increase in Distance Driven</td>
<td>0 0 0 0</td>
</tr>
<tr>
<td>Percentage Decrease in Emissions</td>
<td>29.4 29.4 29.4 29.4</td>
</tr>
<tr>
<td>Rebound Effect Estimates in per cent</td>
<td>18.8 43.8 59.8 70.7</td>
</tr>
<tr>
<td>Percentage Increase in Distance Driven</td>
<td>8.8 20.5 29.0 33.1</td>
</tr>
<tr>
<td>Percentage Decrease in Emissions</td>
<td>23.8 16.5 11.8 8.6</td>
</tr>
<tr>
<td>Emissions Rebound in per cent</td>
<td>19.0 43.9 59.9 70.7</td>
</tr>
</tbody>
</table>

If, for instance, the fuel efficiency rebound effect amounts to 100\%, \( \frac{\partial \ln s}{\partial \ln \mu} = 1 \), no fuel is conserved and, hence, no emissions are reduced: \( \frac{\partial \ln \text{CO}_2}{\partial \ln \mu} = 0 \). In this case, the emissions rebound also amounts to 100\%: \( R_{\text{CO}_2} = 1 \), as can be seen from equation (7). Even more extreme is a backfire example in which the fuel efficiency rebound amounts to, say, \( \frac{\partial \ln s}{\partial \ln \mu} = 1.2 \), for which emissions increase by 20\%, rather than decrease, and, hence, the emissions rebound amounts to 1.2 as well. It bears noting that the same reasoning holds for fuel consumption, which is relevant for the negative external effects of driving other than CO\(_2\) emissions.
6 Summary and Conclusion

Using a panel of household travel diary data collected in Germany and an instrumental variable approach to deal with the endogeneity of fuel efficiency, we have simultaneously estimated fuel price and fuel efficiency elasticities to provide a basis for assessing the policy impacts of both fuel taxes and fuel economy standards on distance traveled. Both these elasticities allow for estimating the direct rebound effect, the behaviorally induced offset in the reduction of energy consumption following efficiency improvements. While the IV approach does not provide for any further insights on the size of the rebound effect in individual mobility, most likely due to the very ambitious data requirements of this approach, the rebound estimates resulting from our panel estimations range between 44 to 71% for single-car households, meaning that between 44 to 71% of the potential energy saving from efficiency improvements in Germany is lost to increased driving.

Hence, while proponents of efficiency standards argue that a monetary benefit of higher efficiency to motorists is decreased per kilometer costs of driving (EC, 2007), our estimation results indicate that an immediate consequence of this benefit is that motorists drive more. We additionally find that the magnitude of the rebound effect is statistically indistinguishable from that of the fuel price elasticity, which suggests that efficiency standards offset the effects of reduced vehicle travel from fuel taxes. Taken together, these results call into question the effectiveness of both the European Commission’s current emphasis on efficiency standards as a pollution control instrument (Frondel, Schmidt, Vance, 2011), as well as the U. S. corporate fuel economy (CAFE) standards.

While an assessment of welfare effects from fuel taxation and efficiency standards extends beyond the scope of the present study, our findings complement a long of line of simulation studies finding negative welfare impacts from fuel efficiency standards. Karplus and colleagues’ (2013) recent estimates from a computable general equilibrium model, for example, suggest that fuel efficiency standards are at least six times more expensive than a tax on fuel, verifying other studies finding that fuel taxes may
be a more effective measure of reducing gasoline consumption (e.g. AUSTIN, DINAN, 2005; CRANDALL, 1992; KLEIT, 2004; LI, LINN, MUEHLEGGER, 2012). That these studies all originate from the US, where the responsiveness to fuel costs are likely to be low relative to other parts of the globe (BRONS et al., 2008), highlights the potential for even costlier welfare consequences in the German context, a point warranting further investigation.

Notwithstanding the political advantages of efficiency standards, whose costs to consumers and the economy are largely obscured, we would argue that the economic logic in favor of standards is wanting given the large rebound effects identified in this study. It is therefore regrettable that European policy-makers have proceeded down this path, and have recently set an even stricter CO₂ standard of 95 grams/km by 2020. Our results suggest that the efficiency standards introduced with the 2009 legislation will blunt what had been a highly effective climate protection policy based on fuel taxation, one under which the efficiency of the car fleet has grown substantially.
References


