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The rebound effect in road transport: A meta-analysis of empirical studies☆

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ABSTRACT

The rebound effect is the phenomenon underlying the disproportionality between energy efficiency improvements and observed energy savings. In road transport, the effect reveals the extent to which energy savings from improved fuel efficiency are forgone due to additional car travel. We present a meta-analysis of 74 primary studies containing 1120 estimates of the direct rebound effect in road transport to evaluate its magnitude and identify its determinants. We find that the short-run rebound effect is, on average, about 10–12%, whereas the long-run effect about 26–29%. However, variation of estimates is large and can mainly be explained by differences in the time horizon considered, the elasticity measure used, and the type of data and econometric approach employed in primary studies. We also find that the rebound effect is declining over time and that lower per capita incomes, higher gasoline prices and higher population density are associated with larger rebound effects.

1. Introduction

Road transport is responsible for important negative externalities, including air pollution, emissions of greenhouse gases (GHG), noise and traffic congestion. It accounted for more than 17% of global energy-related GHG emissions in 2013, and is one of the few sectors of economic activity where emissions are still increasing (IEA, 2015; Sims et al., 2014). Road transport is also one of the major sources of emissions of harmful air pollutants, such as nitrogen oxides and particulate matter, and is responsible for about half of the costs of premature deaths and health problems caused by outdoor air pollution in OECD countries (OECD, 2014; Parry et al., 2007). At the same time, road traffic congestion is estimated to cost humanity billions of dollars annually from time losses. In the more congested countries, these losses can equal more than 1% of GDP (OECD/ECMT, 2007).

Governments use a wide array of policies to induce road users to internalise the external costs of their travel decisions, including both regulatory and market-based instruments. Motor fuel taxes are perhaps the most frequently used instrument to this end, with varying levels of stringency across countries. Motor vehicle taxes, including one-off and recurrent taxes, are also used in many countries, while congestion pricing has also been introduced in a number of cities (e.g. London, Milan, Singapore and Stockholm). In addition, policymakers frequently rely on regulatory approaches to address the external costs of road transport. Fuel efficiency and CO_2 emissions standards are among the most popular regulatory instruments used to this end. However, GHG emissions from transport have continued to rise since 2007, despite the increased use of more fuel efficient vehicles (Sims et al., 2014).

This paper investigates an unintended consequence of fuel efficiency improvements: the *rebound effect*. The rebound effect explains why energy efficiency improvements usually lead to less than proportional reductions in energy consumption. It stems from the increased use of an energy service (in this case, travel) following an improvement in efficiency (Gillingham et al., 2016; Khazzoom, 1980). Increased efficiency of a service effectively results in a lower (per unit) service price, which can have direct and indirect effects. This study focuses only on the *direct* rebound effect, which refers to changes in efficiency and use

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of a particular energy service.[1] In road transport, the direct rebound effect implies that people respond to higher fuel efficiency by driving more.

The rebound effect is at the core of the debate on the relative efficacy of fuel efficiency standards in comparison with market-based instruments. Although fuel efficiency standards are often established with similar end-goals as price instruments, the intermediate effects of the two policy approaches may, in fact, be the opposite (see also Parry et al., 2014). A motor fuel tax increases the cost of driving per mile, thereby reducing travel demand. In contrast, improved fuel efficiency decreases the cost of driving per mile, resulting in an increase in driving. Induced travel from improvements in fuel efficiency has important implications.[2] First, it partially offsets expected energy savings. Second, it contributes to mileage-related externalities, such as higher levels of non-exhaust air pollution, noise and congestion (see also van Dender and Crist, 2011). Thus, the rebound effect plays an important role in the choice of the appropriate policy instrument, or combination of instruments, to address road transport externalities.[3]

Earlier literature has shown that considering the change in travel demand from an *increase in fuel efficiency* is the most straightforward measure of the direct rebound effect in road transport (see e.g. Frondel et al., 2008; Sorrell and Dimitropoulos, 2008). However, a far more popular measure in the literature is the change in travel induced by a *reduction in the cost of driving per distance unit* (kilometre or mile). Many empirical studies also resort on changes in travel from a *reduction in fuel prices* to estimate the rebound effect in road transport. The last approach is usually followed due to difficulties in finding reliable data on fuel efficiency or due to econometric concerns (Frondel et al., 2012).

Although there is a general consensus in the literature that the rebound effect exists in road transport, empirical estimates vary widely, ranging from negative values (fuel efficiency improvements result in reduced travel) to greater than 100% (implying that improvements in fuel efficiency increase fuel use – a phenomenon often denoted as *backfire*). Indeed, Gillingham et al. (2016) note that estimates of the rebound effect show considerable variation, most likely caused by differences in the definitions employed, and in the data and empirical methods used by the authors. The literature still suffers from a lack of clear-cut definitions and guidelines for measurement, sometimes even leading to significantly different estimates of the rebound effect obtained from the same data source (Gavankar and Geyer, 2010).

This paper presents a meta-analysis of 74 primary studies measuring the direct rebound effect in road transport in order to provide a useful synthesis of past work. The scope of the analysis is narrowed as much as possible to passenger transport, as elasticities in the commercial sector are influenced by different factors from the ones affecting elasticities in passenger transport. Meta-regression analysis provides insights into cross-country differences in the magnitude of the rebound effect by considering factors such as differences in income, gasoline prices and population density.

The rest of the paper is structured as follows. Section 2 provides background on the theoretical and empirical literature related to the rebound effect. Section 3 explains why meta-analysis is an appropriate approach to synthesise past empirical literature on the rebound effect in road transport. Section 4 provides a statistical summary of the collected empirical estimates, while Section 5 presents the results of the meta-regression analysis. Section 6 concludes and discusses policy implications.

2. Background

A phenomenon first suggested by Jevons (1865) and revisited by Khazzoom (1980), the rebound effect is empirically measured as an elasticity of demand. Letting *e* denote fuel efficiency, *t* travelled distance (in kilometres or miles) and *f* the amount of fuel consumed (in litres or gallons), note first the identity $e = t/f$.[4] It can be shown that $E_e^f = E_e^t - 1$, where $E_e^f = \frac{\partial f}{\partial e}\ \frac{e}{f}$ is the elasticity of *fuel demand* with respect to energy efficiency and $E_e^t = \frac{\partial t}{\partial e}\ \frac{e}{t}$ is the elasticity of *travel demand* with respect to fuel efficiency (Sorrell and Dimitropoulos, 2008). If E_e^t is greater than zero, E_e^f is lower in absolute terms than unity, implying that an increase in fuel efficiency will lead to a less than proportional reduction in fuel demand. In fact, the *elasticity of travel demand with respect to fuel efficiency* reflects exactly the deviation of this reduction from proportionality and can, therefore, serve as a straightforward measure of the rebound effect in road transport (see also Greene et al., 1999; Hymel et al., 2010; Wheaton, 1982).

Considering, however, *c* as fuel cost per unit of travel (e.g. mile) and *p* as fuel price, the identity $c = p/e$ holds (Small and Van Dender, 2007). Earlier literature has shown that under specific assumptions (further explained below), the elasticity of travel demand with respect to fuel efficiency is equal to the *negative of the elasticity of travel demand with respect to fuel costs per unit of travelled distance*, $E_c^t = \frac{\partial t}{\partial c}\frac{c}{t}$. As mentioned above, the intermediate effect of an improvement in fuel efficiency is to decrease the fuel cost of driving. Therefore, the elasticity with respect to this cost is often used to empirically measure the rebound effect, although perhaps in a less direct way than the elasticity with respect to fuel efficiency (see, for example, Greene, 1992; Jones, 1993; Small and Van Dender, 2007).

In addition, data on fuel efficiency are often less abundant, harder to measure, or lacking in variation. This leads authors to exploit more readily available data on fuel prices. This has resulted in a third measure of the rebound effect (see, for example, Frondel et al., 2012; Greene et al., 1999; Munk-Nielsen, 2015): *the negative of the elasticity of travel demand with respect to fuel price*, $E_p^t = \frac{\partial t}{\partial p}\frac{p}{t}$.

For one to consider that these three elasticities are just different measures of the same underlying phenomenon, a set of assumptions must be made.[5] A first assumption is that fuel costs is what ultimately matters for consumers, and thus their response is identical if a certain cost reduction stems from an increase in fuel efficiency or a reduction in the fuel price. The second assumption is that fuel prices are exogenously determined (see e.g. Sorrell and Dimitropoulos, 2008). Estimates of the rebound effect based on the elasticity with respect to fuel price further require that fuel efficiency is held constant (see e.g. Frondel et al., 2012). Still, if empirical interest lies in the effect of changes in fuel efficiency, this last assumption may seem counterintuitive.

The empirical literature provides mixed evidence of the equivalence of these three definitions. Some papers find no statistical difference between any of the above elasticities, indicating that any definition provides a valid estimate of the rebound effect (e.g. Frondel et al., 2008; Frondel and Vance, 2014). Greene et al. (1999) conclude that the data

[1] One can also consider the *indirect* rebound effect, where a change in the price of an energy service results in changes in the demand for other goods and services (Sorrell and Dimitropoulos, 2008). For a more general typology of rebound effect definitions, see Gillingham et al. (2016).

[2] In this paper, the term *induced travel* is used to denote increases in travel demand stemming from improvements in fuel efficiency. The use of the term should not be confused with induced travel from road capacity expansions (cf. e.g. Cervero and Hansen; Hymel et al., 2010).

[3] When estimating the impacts of fuel efficiency standards, policy makers often take rebound effects and their implications into consideration. Even in that case, however, the relative economic efficiency of fuel efficiency standards compared to price instruments is undermined by rebound effects.

[4] The terms fuel efficiency and fuel economy (i.e. the ratio of travelled distance to the amount of fuel consumed by the vehicle to cover this distance – e.g. miles per gallon, kilometres per litre) are used interchangeably in this paper. In some countries, efficiency is measured by the inverse of this ratio, usually termed fuel consumption or fuel intensity (e.g. litres per 100 km). However, following the majority of the empirical literature on the rebound effect, we focus on fuel economy, as it provides a more direct measure of the amount of energy input required to maintain a specific level of an energy service.

[5] While the elasticities of travel with respect to cost and fuel price are expected to be negative, the elasticity with respect to fuel efficiency should, at least theoretically, be positive.

generally do not contradict the hypothesis that consumers respond symmetrically to proportionate changes in fuel price and fuel efficiency. However, others (e.g. De Borger et al., 2016; Greene, 2012; Hymel and Small, 2015) find that reductions in fuel price have a much larger impact on travel demand than increases in fuel efficiency. On the other hand, Linn (2016) finds that the estimated elasticity with respect to fuel economy is systematically larger in absolute terms than the one with respect to fuel price: however, in his empirical setup the former would better be viewed as a long-run elasticity, whereas the latter as a short-run one.

A fourth elasticity suggested by the literature as an upper bound measure of the rebound effect is the elasticity of *fuel consumption* with respect to fuel price (e.g. Sorrell and Dimitropoulos, 2008; Sorrell et al., 2009). This measure does not fall within the scope of this meta-analysis, as we were concerned that it may provide inflated estimates of the rebound effect, and as a number of comprehensive reviews and meta-analyses have already focused on it (see e.g. Brons et al., 2008; Espey, 1998).

To the best of our knowledge, no meta-analysis of elasticities of *travel demand* has been conducted to provide insights into the magnitude and determinants of the rebound effect. Previous comprehensive reviews of the rebound effect literature had a much wider scope than road transport and did not provide a formal statistical analysis of rebound effect estimates (Greening et al., 2000; Sorrell et al., 2009). At the same time, existing meta-analyses and surveys of road transport elasticities did not explicitly focus on the rebound effect and were therefore mainly concerned with different elasticities from the ones of interest here (Brons et al., 2008; Goodwin et al., 2004; Graham and Glaister, 2004).

Both streams of studies also date back to a time when much fewer primary studies were conducted in this area. In particular, more than two-thirds of the estimates included in our meta-analysis are derived from studies published after 2008. Given that both economic and environmental conditions have changed considerably over the past decades and that the theoretical understanding of the rebound effect and econometric techniques have advanced, a fresh look at the rebound effect literature is warranted.

It is useful, however, to provide an overview of these important earlier contributions to the literature. Greening et al. (2000) survey relevant studies from the United States and conclude that the magnitude of the rebound effect is low to moderate. Their survey investigates the rebound effect in different sectors and takes also into account indirect and economy-wide effects. In contrast, the review of Sorrell et al. (2009) focuses on the direct rebound effect in energy services in the household sector. The scope of services considered in their review is not limited to passenger transport; it extends to heating, cooling and other household services. The review encompasses 17 studies on passenger transport which allows Sorrell et al. (2009) to conclude that the long-run direct rebound effect is between 10 and 30%. They also classify studies according to the type of data used (cross-sectional and time series vs. panel) and their level of aggregation (aggregate vs. household survey data). They caution that estimates based on disaggregate data and ones based on cross-sectional or time series variation in energy prices may overestimate the rebound effect. They also express scepticism about the validity of the assumption underlying the equivalence of the three rebound effect measures presented earlier, i.e. that consumer responses to fuel price changes are symmetric to responses to fuel efficiency changes.

Other relevant reviews and meta-analyses include Brons et al. (2008) and parallel blind studies from Goodwin et al. (2004) and Graham and Glaister (2004). Brons et al. (2008) perform a meta-analysis of the price elasticity of gasoline demand using a Seemingly Unrelated Regression (SUR) approach with cross-equation restrictions. Their meta-analysis also collects 13 estimates of travel demand (either aggregate or per car) with respect to fuel price which are used in the estimation of their system of equations. The elasticity of aggregate travel demand with respect to fuel price is, on average, 0.03 in the short run (opposite sign than expected) and −0.32 in the long run.

Goodwin et al. (2004) provide a review of elasticities of fuel consumption, road traffic and vehicle stock, with respect to price and income (see also Hanly et al., 2002). Their review draws on 69 studies from the UK, or other countries broadly comparable to the UK, which inter alia provide about 20 estimates of the elasticity of travel demand with respect to fuel price. They distinguish between estimates from dynamic and static regression models, and between models using aggregate versus vehicle-level data. They find that the elasticity of travel demand with respect to fuel price is, on average, about −0.1 in the short-run and −0.3 in the long-run when the model is dynamic. When the model is static, however, average estimates range from −0.27 to −0.69 depending on the type of data and level of aggregation used. Graham and Glaister (2002, 2004) also focus on demand elasticities for fuel and road traffic, but extend the scope of their review to other countries. Again, one of the most significant differences in estimates comes from long-run versus short-run elasticities. They find that the elasticity of travel demand with respect to fuel price is, on average, −0.15 in the short run and −0.31 in the long run.

The type of data used to estimate the rebound effect varies substantially across empirical studies. To some extent, this is associated with the elasticity measure(s) adopted in each of them. Most studies estimating the rebound effect based on *fuel efficiency* do not rely on aggregate data to estimate the elasticity of interest, as variation in fuel efficiency is usually insufficient. In contrast, they mostly use disaggregate data and exploit variation in travelled distances and fuel efficiency across vehicles. Authors usually consider published or on-road corrected fuel efficiency ratings for each vehicle, or an average rating for a household with multiple vehicles. Many studies estimating the rebound effect based on fuel efficiency also include fuel price as a control variable, thus providing a way to directly compare the magnitude of the elasticity with respect to fuel efficiency with the one of the elasticity with respect to fuel price.

Studies considering the elasticity with respect to *fuel costs per unit of travelled distance* or the one with respect to *fuel price* are relatively diverse. Some of the studies using the former elasticity measure use panels of microdata, but the majority utilise aggregate time series or panel data (e.g. at the national or state level). Fuel costs per unit of travelled distance are usually calculated by the authors, combining data on fuel prices with data on average fleet fuel efficiency, or with data on fuel demand and travel demand.[6] Studies estimating the elasticity of travel demand with respect to fuel price largely rely on panel data to utilise variation in fuel prices over space and time.

Another important difference between studies lies in the treatment of the potential endogeneity of the independent variable of interest. On this issue, an influential study in the rebound effect literature is Small and van Dender (2007), which focuses on the elasticity of vehicle miles travelled (VMT) with respect to fuel cost per mile. Their approach considers that VMT, number of vehicles and fuel efficiency are simultaneously determined. This accounts for endogenous changes in fuel efficiency, and therefore provides a more reliable estimate of the rebound effect. Other approaches to treating the potential endogeneity of fuel efficiency are presented in e.g. De Borger et al. (2016) and Linn (2016). However, many studies seem to completely neglect this potential source of bias in elasticity estimates.

Estimates of the rebound effect may also be influenced by the treatment of the relationship between fuel efficiency and other car attributes. For example, it is likely that fuel efficiency is positively correlated with capital (car purchase) costs. Failing to control for changes in capital costs following fuel efficiency improvements can lead to an overestimation of the rebound effect (see also Sorrell and Dimitropoulos, 2008).

[6] Some studies using aggregate data on travel demand note the possible caveat of significant measurement error. This is the case particularly regarding US data on vehicle miles travelled, as they are generally reported independently by each state, and often different states have different methodologies for measurement and aggregation (see e.g. Small and Van Dender, 2007).

Similarly, more fuel efficient cars may have characteristics less desirable for consumers, such as being smaller or less comfortable, having less horsepower, or lower safety ratings (see also West et al., 2015).[7] Such differences may have an impact on the magnitude of the rebound effect.

3. Meta-analysis

Given the diversity in definitions, data and methodological approaches used to estimate the rebound effect in road transport, we conduct a meta-analysis of relevant empirical studies. Meta-analysis is a rigorous statistical approach to synthesise the findings of a narrowly defined collection of primary empirical studies (Glass, 1976). Even though originating from medical research, the approach has become popular in economics and other social sciences in the last decades. Meta-analysis has been widely applied to energy economics (see e.g. Brons et al., 2008; Espey, 1998; Havranek and Kokes, 2015; Liu and Shumway, 2016). This is, to the best of our knowledge, the first meta-analysis conducted on the rebound effect in road transport.

Generally, there are at least three purposes that can be served by a meta-analysis. First, to summarise existing empirical evidence with a view to provide valid estimates of the underlying *effect size* (here, the magnitude of the rebound effect); second, to pinpoint the sources of heterogeneity in estimates of the effect size across studies; and third, to provide an empirical framework suitable for making predictions of the effect of interest in other contexts (Nelson and Kennedy, 2009). It is especially the last two goals that account for economists' appreciation of *meta-regression analysis*, which uses econometric methods to synthesise the empirical results of previous studies (see e.g. Stanley and Jarrell, 1989; Weitzman and Kruse, 1990). In the meta-regression, each primary study estimate of the effect size of interest is a unique observation. An empirical model is then developed to estimate the impact of contextual and methodological characteristics of primary studies on effect size estimates. Primary study characteristics are usually modelled with the help of dummy variables.

The meta-regression analysis presented in Section 5 of this paper focuses on disentangling the determinants of heterogeneity in rebound effect estimates. The econometric models constructed for the meta-regression analysis investigate how: (i) differences in the elasticity measure used to compute the rebound effect, (ii) differences in primary study design, methodology and data, and (iii) differences in time- and country-specific factors (gasoline prices, GDP per capita and population density) influence rebound effect estimates.

Three main statistical challenges arise when conducting a meta-analysis: sample heterogeneity, heteroskedasticity of effect-size variances and correlation between effect size estimates (Nelson and Kennedy, 2009). Heterogeneity in primary studies and rebound effect estimates can be controlled for by introducing explanatory variables capturing differences in the design and methodology of the study in the meta-regression analysis. Heteroskedasticity of effect-size variances can be treated by using heteroskedasticity-robust variance-covariance estimators and by weighting each primary estimate by the inverse of its variance to give more reliable estimates greater weight.[8] Correlation between estimates may stem from the use of multiple estimates from each study. In addition, estimates could be correlated across papers with the same authors, or even across studies with different authors but with the same data source (which is often the case for US estimates). Several techniques can be used to treat this problem. One possible solution is to take only one estimate from each paper. However, this is often undesirable due to the resulting small sample size and the frequent absence of clear benchmarks allowing the identification of the preferred estimate from each study. Another way to address this problem is to test the robustness of the findings using a variety of econometric methods, ranging from generalised least squares to panel data techniques. In what follows, our analysis mainly focuses on a subset of 255 preferred estimates (see the following section for details on how these estimates were identified). We also use panel data empirical methods to address remaining concerns over correlation among estimates.

Another possible concern when conducting a meta-analysis is publication bias. Publication bias signifies the idea that journals tend to favour statistically and economically significant results which are consistent with economic theory (Stanley and Jarrell, 1989; Stanley and Doucouliagos, 2012, pp. 51–79). This implies that a meta-analysis which relies only on studies published in academic journals is likely to overestimate the magnitude of the effect of interest. A first step towards addressing the potential issue of publication bias is to collect estimates from other sources, such as discussion papers, manuscripts, or conference presentations (Nelson and Kennedy, 2009). About 20% of the estimates used in this meta-analysis (15% for the subset of preferred estimates) are derived from sources other than academic journals, including book chapters, PhD theses, and working and conference papers.

4. Descriptive analysis

In general, it is crucial for a meta-analysis to be as wide-reaching and inclusive as possible in the collection of primary studies in order to strengthen the robustness of the regression results and avoid potential biases. Therefore, this meta-analysis aimed to collect as many studies of the rebound effect in passenger transport as possible, including articles published in academic journals and books, as well as discussion, working and conference papers, and policy reports.

As a starting point, widely cited papers, prior narrative reviews and surveys of existing literature were consulted; this allowed identification of other important pieces of work on the topic. This step was followed by an online database search of ScienceDirect, EconLit, Wiley, IngentaConnect, Google Scholar, JSTOR, and NBER, as well as a detailed search for papers from conferences of relevant associations of economists (AERE, EAERE, IAEE, ITEA). Additionally, a search of relevant policy reports was made in the websites of ministries of environment, energy, and transport and environmental protection agencies of several countries. Individual researchers and national experts were also contacted on an ad hoc basis to widen the scope of the search.

Initially, more than 100 studies were collected. However, in order to be included in this meta- analysis, studies must conduct an econometric analysis to estimate one of the elasticities presented in Section 2. Studies focusing on freight transport were not further considered, as elasticities in the transport of goods are influenced by different factors from the ones affecting elasticities in passenger transport. In the end, our database contains 1120 estimates from 74 studies.[9] The top and bottom percentile of these estimates was not included in the empirical analysis, as their magnitude was considered implausible. The complete list of studies used in the analysis can be found in Table A.I of Appendix A.

[7] Improved fuel efficiency need not always be negatively correlated with other technical attributes. For example, diesel cars tend to be more fuel efficient (despite emitting higher levels of NO_x and particulate matter) than their gasoline counterparts, often without significant differences in other vehicle attributes valued by consumers (e.g. safety, reliability, comfort, performance). Similarly, in recent years, much progress has been made in the design of hybrid cars; today, many car manufacturers offer the same model in both gasoline and hybrid variants, with few – sometimes hardly noticeable – physical or aesthetic differences between the two. However, diesel and hybrid variants are usually more expensive than their gasoline counterparts, an example of how increases in fuel efficiency entail increases in capital costs.

[8] Alternatively, e.g. in case of missing standard errors, estimates can be weighted by the sample size of the primary study (see e.g. Nelson and Kennedy, 2009).

[9] We collected all estimates of elasticities of travel demand with respect to fuel efficiency, fuel costs and fuel price, regardless of whether they were explicitly denoted as estimates of the rebound effect. Apart from leading to a larger sample size, this approach is also less likely to suffer from publication bias.

Summary statistics of rebound effect estimates by elasticity measure and primary study are presented in Tables A.II to A.IV in Appendix A. The tables also present the country and time period on which estimates are based. The number of estimates derived per study ranges from 1 to 89, with a median of 10 estimates. Some studies produce estimates for more than one elasticity measure or more than one country. Estimates are concentrated on a relatively small number of countries: Australia, Canada, China, India, Israel, Japan, the United States and several European countries. The majority of estimates are for the United States (about 64%), while a relatively large number of estimates are also available for the United Kingdom (8.8%), Denmark (8.5%) and Germany (5.4%).

The last columns of Tables A.II to A.IV present summary statistics by study for a set of preferred estimates, which have been identified as follows. The full database contains inter alia estimates for certain population groups (e.g. by income or by annual distance travelled) or for different regions within a country, as well as estimates from models which have been shown to lead to inconsistent or inferior estimates in the primary studies (e.g. models which do not account for the endogeneity of an independent variable while they should have, or models whose underlying assumptions are rejected by a formal test). To ensure that we provide a summary of the most reliable empirical evidence, we construct a smaller database that contains only the subset of estimates which: (i) stem from model specifications identified as the preferred ones by the authors of primary studies, or from the most rigorous model specifications; and (ii) refer to the least possible extent to population subgroups that introduce additional sources of heterogeneity that are difficult to account for in a meta-regression framework.[10] This database contains 255 preferred estimates of the rebound effect which are used in the statistical analysis of this section and the meta-regression analysis of Section 5. Summary statistics and meta-regression results for the full sample of 1120 estimates are presented in Appendix B.

Fig. 1 depicts the kernel density of the preferred estimates of the rebound effect separately for the three elasticity measures. The distribution is skewed to the right regardless of the elasticity measure under consideration. Estimates of the elasticity with respect to fuel costs (long-dashed line) show lower dispersion and are much more concentrated in the interval between 0 and 20% than estimates of the other two measures.[11] Estimates of the elasticity with respect to fuel efficiency (solid line) seem to peak at a very similar magnitude of the rebound effect with estimates of the elasticity with respect to fuel costs. Estimates of the elasticity with respect to fuel price (short-dashed line) peak at a somewhat greater magnitude and show the highest dispersion.

Table I presents the corresponding summary statistics by elasticity measure (see Table B.I in Appendix B for a similar analysis for the full sample). The unweighted mean of estimates of the elasticity with respect to fuel efficiency (see third column) is 24%. Estimates based on the elasticity of travel with respect to fuel costs are slightly lower (around 23%), whereas estimates relying on the elasticity of travel with respect to fuel price are higher, close to 30%. However, the relatively high standard deviations of rebound effect estimates unveil considerable variation, even among estimates using the same elasticity measure. While unweighted statistics may provide a general idea of the magnitude of the rebound effect, they should be interpreted with great caution, as they neither account for the heterogeneity of estimates nor for the precision with which effects are estimated.

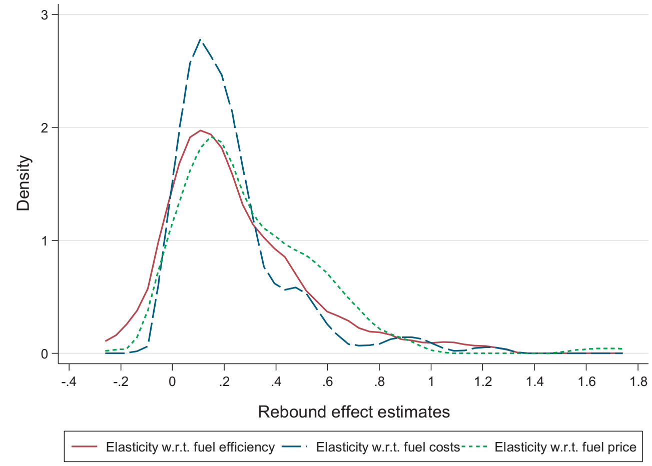

Fig. 1. Distribution of estimates of the rebound effect by elasticity measure.
Note: Sample of 255 preferred estimates. The figure illustrates the distribution of 57 estimates of the elasticity with respect to fuel efficiency, 116 estimates of the elasticity with respect to fuel costs and 82 estimates of the elasticity with respect to fuel price.

A remedy to the second issue is to consider averages whereby each estimate is weighted by a measure of its precision. Columns 6–8 of Table I present summary statistics of estimates weighted by the size of the sample used in the primary study. This is to take into account that, ceteris paribus, studies with a larger sample size tend to produce more precise results.[12] Weighting each estimate by a measure of its precision also helps to deal with the issue of heteroskedasticity mentioned in the previous section (Nelson and Kennedy, 2009).

Summary statistics based on weighted estimates imply noticeably lower values of the rebound effect than those based on unweighted estimates. The divergence between unweighted and weighted estimates is particularly acute when focusing on the elasticities with respect to fuel efficiency and fuel price. The elasticity with respect to fuel efficiency now provides the most conservative estimate of the average rebound effect among the three measures, at about 9%. The weighted average of the elasticity with respect to fuel price is about 17%, about 56% of its unweighted value, while the weighted average of the elasticity with respect to fuel costs is also about 10% lower than its unweighted counterpart. One could be inclined to conclude that less precise estimates tend to inflate the magnitude of the rebound effect, but this probably explains only partially the divergence observed here. Another cause of this divergence lies in the heterogeneity of estimates. In particular, weighted statistics may place more emphasis on certain types of demand response times (e.g. short-run estimates), as the latter may be estimated with larger sample sizes.

It is, thus, useful to draw an important distinction between estimates referring to different demand response times. Short- and long-run estimates usually stem from dynamic models; i.e. econometric specifications in primary studies include at least one lagged value of the dependent variable in the set of explanatory variables. In few cases, estimates are explicitly designated as short- or long-run by the authors of the primary study, despite the absence of a dynamic model in the empirical analysis. In these cases, we follow the classification provided by the authors. Short-run estimates refer to drivers' responses in the first period (usually a year) following the fuel efficiency improvement. In contrast, long-run estimates take into account the time required by consumers to change their capital stock, i.e. to change

[10] A number of studies provide only estimates for different population subgroups (e.g. studies based on quantile regression analyses). In those cases, we include all the relevant estimates in the meta-analysis. For studies providing estimates for different elasticity measures and/or different response times (e.g. both short- and long-run), we take into consideration at least one estimate per measure and type of response time.

[11] Note that figures and tables present estimates in *proportions*, whereas the interpretation of the results is made in *percentage* terms, as is common in the literature on the rebound effect.

[12] Typically, the preferred measure of precision is the variance of each estimate. In this meta-analysis, however, the standard error was not provided and could not be retrieved for a significant portion of the estimates. Following standard practice in the field (see e.g. Nelson and Kennedy, 2009), each rebound effect estimate was instead weighted by the sample size used in the primary study.

Table I
Summary statistics of empirical estimates of the rebound effect in road transport.

Rebound effect definition	Observations	Unweighted			Weighted		
		Mean	Median	Std. dev.	Mean	Median	Std. dev.
Elasticity w.r.t. fuel efficiency	57	0.243	0.172	0.261	0.091	0.023	0.121
Elasticity w.r.t. fuel costs	116	0.227	0.166	0.218	0.207	0.097	0.151
Elasticity w.r.t. fuel price	82	0.299	0.229	0.266	0.166	0.099	0.131

Note: Sample of 255 preferred estimates. For elasticities with respect to fuel costs and fuel price, the table presents the negative of the estimates derived from the primary study. To compute the weighted statistics, each estimate is weighted by the size of the sample used in the primary study.

vehicles.[13] However, not all studies use dynamic models to estimate the elasticities of interest or explicitly define the demand response time. These "unspecified response time" estimates are treated with different approaches in related meta-analyses and reviews, such as e.g. those of gasoline price and income elasticities (see e.g. Brons et al., 2008; Espey, 1998). In our analysis, such estimates are reported separately from short- and long-run estimates.

Fig. 2 depicts the distribution of rebound effect estimates by response time. Short-run estimates are concentrated around a rebound effect value slightly lower than 10% and are much less dispersed than long-run estimates and estimates with unspecified response time. In contrast, long-run estimates peak above 20%, pointing to a stronger demand response. Estimates with unspecified response time show the largest dispersion, reflecting the diversity of methods and data used to estimate them.

Table II presents the corresponding summary statistics of rebound effect estimates by demand response time (see Table B.II in Appendix B for a similar analysis for the full sample). The table reveals a significant divergence between short-run estimates and the other two categories. The weighted average of the estimates implies that the short-run rebound effect is in the area of 10–12%. The weighted average of the estimates of the most direct measure of the rebound effect, i.e. the elasticity with respect to fuel efficiency, point to a value closer to 12%, but the number of estimates is probably too small to rely on this figure. Instead, estimates of the short-run elasticities with respect to fuel costs and fuel price point to a rebound effect of 10%.

As long-run estimates take into account the time required by consumers to change their vehicle stock, they are particularly useful for policies aiming to increase the fuel efficiency of new cars. Weighted averages of long-run estimates are substantially higher than those of short-run ones, revealing a long-run rebound effect in the area of 26–29%. Once again, the small number of estimates of the elasticity with respect to fuel efficiency does not allow us to conclude that the long-run effect is necessarily on the high side of that interval. Estimates of elasticities with unspecified response time show mixed results. In the case of the elasticity with respect to fuel efficiency, they seem to reflect a short-term demand response, whereas estimates of the other two elasticities point to a long-term one. This is perhaps related to the sources of variation used in primary static models to estimate the elasticities of interest.[14]

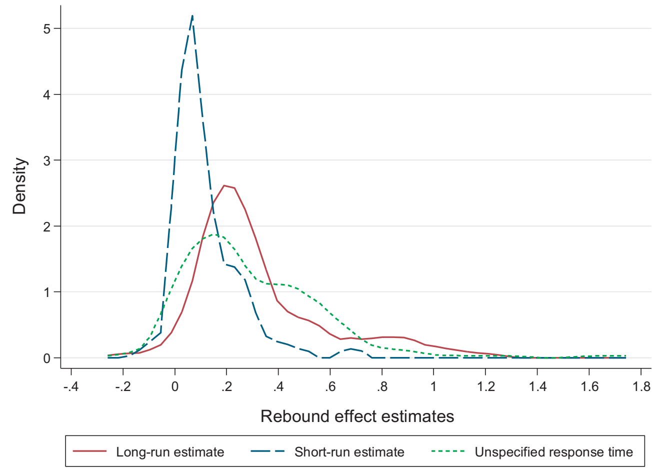

Fig. 2. Distribution of estimates of the rebound effect by time of response.
Note: Sample of 255 preferred estimates. The figure illustrates the distribution of 64 estimates of long-run elasticities, 70 estimates of short-run elasticities, and 121 estimates of elasticities with unspecified response time.

5. Meta-regression analysis

Summary statistics of empirical evidence are informative, but another objective of this meta-analysis is to unravel the sources of variation in rebound effect estimates. The large standard deviations presented in Table I indicate a high degree of heterogeneity in elasticity estimates. Several sources of heterogeneity among primary studies have been identified in the relevant literature (see e.g. Gillingham et al., 2016; Greening et al., 2000; Sorrell and Dimitropoulos, 2008). Differences in the geographical and time coverage of the study are common suspect causes of heterogeneity. Heterogeneity can also exist, however, due to differences in the type of data used (e.g. cross-sectional, time series or panel; disaggregate or aggregated) or in the quality of data: some datasets are more prone to measurement error than others.[15] Other methodological differences may be present, including differences in the econometric technique deployed, the control variables included in primary studies' empirical models, or the treatment of endogenously determined variables.

Our meta-regression analysis is based on the following econometric model:

$$R_i = \alpha + \beta_1 \mathbf{T}_i + \beta_2 \mathbf{M}_i + \beta_3 \mathbf{X}_i + \beta_4 \mathbf{C}_i + \beta_5 \mathbf{Y}_i + \varepsilon_i \quad (1)$$

where R denotes the rebound effect estimate from the primary study, **T** is a vector of dummy variables reflecting the demand response time (short-run, long-run, unspecified), and **M** is a vector of dummy

[13] For the purposes of this meta-analysis, estimates defined in a primary study as medium-run (Gillingham, 2014) are reclassified as long-run elasticities, as it seems likely that individuals can change vehicles within a two-year timeframe. Hanly et al. (2002) outline three main ways individuals can adapt to increases in the cost of driving: (i) changing driving styles (e.g. less heavy acceleration and braking); (ii) shifting the pattern of journeys such that more of them occur in fuel-efficient contexts (e.g. light traffic at moderate speeds); and (iii) shifting to more fuel-efficient vehicles. While (i) and (ii) can be enacted in the short run, changing vehicles can only occur in the long run. As individuals have the flexibility to choose the fuel efficiency of their vehicle, one would indeed expect greater increases in travel demand in the long run.

[14] In Table B.III of Appendix B, we compare means of rebound effect estimates published in academic journals with means of estimates extracted from other sources (e.g. book chapters, conference and working papers and PhD theses). Comparisons are made by demand response time and are based on both unweighted and weighted means. The analysis of the subset of 255 preferred estimates shows that estimates extracted from other sources are generally of larger magnitude. However, differences are not statistically significant, with one exception: the weighted mean of estimates with unspecified response time is about 8 percentage points lower in studies published in academic journals.

[15] For example, Weber and Farsi (2014) compare data for Switzerland. In one of their models, they use data on self-reported travel for a reference day to project annual vehicle kilometres travelled, while in another they use very accurate GIS travel data. They find a very weak correlation of 0.2 between the two measures of distance travelled, indicating that there may be significant differences between self-reported and rigorously measured travel data.

Table II
Summary statistics of estimates by time horizon and elasticity measure.

	No. of estimates	Unweighted mean	Weighted mean	Min	Max
Elasticity w.r.t. fuel efficiency					
Short-run	5	0.154	0.116	−0.032	0.684
Long-run	6	0.351	0.291	−0.143	1.152
Unspecified	46	0.238	0.072	−0.203	0.953
Elasticity w.r.t. fuel costs					
Short-run	47	0.103	0.097	−0.040	0.360
Long-run	45	0.311	0.256	0.040	0.994
Unspecified	24	0.313	0.411	0.065	1.220
Elasticity w.r.t. fuel price					
Short-run	18	0.145	0.099	−0.108	0.476
Long-run	13	0.345	0.283	0.128	0.801
Unspecified	51	0.342	0.338	0.035	1.686

Note: Sample of 255 preferred estimates. For elasticities with respect to fuel costs and fuel price, the table presents the negative of the estimates derived from the primary study. Estimates defined in the primary study as medium-run are considered here as long-run elasticities. To compute the weighted mean, each estimate is weighted by the size of the sample used in the primary study.

variables indicating the elasticity measure estimated in the primary study (see Section 2 for definitions of the elasticity measures used in this paper). The vector of variables $\mathbf{X}$ contains elements related to various study characteristics (e.g. type of data and econometric technique used), and vector $\mathbf{C}$ denotes specific macro-level variables affecting the rebound effect in the country and time period analysed in the primary study, such as income, gasoline prices and population density. Vector $\mathbf{Y}$ controls for global time trends that may have an impact on the rebound effect estimate. Parameter α and (vectors of) parameters $\boldsymbol{\beta}_{(.)}$ are to estimated, and ε is the error term of the model.

Table III presents the definition and summary statistics of the explanatory variables used in the meta-regression analysis, while Table IV presents the results of four meta-regression models. In addition to a simple OLS specification, provided for comparison purposes, Table IV shows the results of a weighted least squares (WLS) model, where variables are weighted by the sample size used in the primary study.[16]

The last two columns of Table IV present results from the estimation of fixed and random effects panel data models. These models assume that each group of primary studies provides a panel of rebound effect estimates. Two studies are assigned to the same group if they use the same dataset or a similar dataset from the same source, and share at least one co-author.[17] On the basis of this criterion, 58 groups of studies have been constructed. Panel data methods can more effectively take into account the potential correlation of estimates coming from the same study or group of studies (see also Nelson and Kennedy, 2009) than methods which do not explicitly account for multiple sampling. The panel data equivalent of Eq. (1) is as follows:

$$R_{jk} = \gamma_j + \boldsymbol{\delta_1}\mathbf{T}_{jk} + \boldsymbol{\delta_2}\mathbf{M}_{jk} + \boldsymbol{\delta}_3\mathbf{X}_{jk} + \boldsymbol{\delta}_4\mathbf{C}_{jk} + \boldsymbol{\delta_5}\mathbf{Y}_{jk} + u_{jk} \tag{2}$$

where R_{jk} denotes the kth rebound effect estimate from group of studies j. The group-specific parameter γ and (vectors of) parameters $\boldsymbol{\delta}_{(.)}$ are to be estimated. The error term of the model is denoted by u.

For readers interested in the meta-regression analysis of the full sample of 1120 estimates, the relevant summary statistics and meta-regression results are presented in Tables B.IV and B.V (Appendix B) respectively. Those results are in the same direction, and for most variables of similar magnitude, to the ones presented in this section.

Our empirical results are discussed in the context of the WLS and the panel data models: between the fixed- and the random-effects specification, we have a preference for fixed-effects, as the results of statistical tests reveal that the assumptions underlying the random-effects model are not adequately supported by empirical outcomes.

A first important assumption that was interesting to test was whether long-run estimates are systematically different from short-run ones and from those with an unspecified response time. The results of our econometric models reveal that short-run estimates of the rebound effect, as well as those with an unspecified response time, are consistently and significantly lower than long-run ones (reference category). Furthermore, estimates with an unspecified response time are statistically indistinguishable from short-run ones. The magnitude of the difference between long-run and short-run estimates slightly exceeds 18 percentage points, while the one between long-run estimates and estimates with an unspecified response time is slightly smaller.

Section 2 discussed the various definitions of the rebound effect and the assumptions underlying their use. As already mentioned, the empirical literature has not reached a consensus on how the estimated magnitude of the rebound effect varies with the elasticity measure used in the primary study. Our meta-regression results provide evidence that the elasticities of travel demand with respect to fuel costs and fuel price result in higher estimates than the elasticity with respect to fuel efficiency (reference category), even though the statistical and economic importance of the finding varies across specifications. On the one hand, WLS results imply that the former two elasticities result in a remarkable overestimation of the rebound effect by 30 percentage points. On the other hand, the panel data models do not provide equally worrisome evidence: for example, the fixed-effects model shows that estimates of the elasticity with respect to fuel costs are about 8 percentage points higher than estimates of the elasticity with respect to fuel efficiency. Despite the point estimate of the coefficient of the elasticity with respect to fuel price being higher than that of the elasticity with respect to fuel costs across specifications, the two coefficients are not different from each other in statistical terms. The empirical evidence does not alleviate concerns over the validity and implications of the assumptions underlying the theoretical equivalence of the three definitions, so the most direct measure of the rebound effect – the elasticity of travel demand with respect to fuel efficiency – should be preferred to other measures whenever this is possible.

Similar to meta-analyses of gasoline price elasticity (e.g. Brons et al., 2008; Espey, 1998), our models also test for differences in rebound effect estimates between studies using different types of data. Earlier studies found, for example, that cross-sectional data tended to inflate the gasoline price elasticity. Our empirical findings do not lead to similar conclusions: when the level of data aggregation (aggregate data vs. microdata) is controlled for, cross-sectional and time series data are associated with lower estimates of the rebound effect than estimates from models using panel or cross-sectional time-series data (reference category). Differences are not statistically significant in the panel data models, but are sizeable and significant in the WLS model.

The use of microdata, mainly stemming from household surveys, is associated with higher estimates of the rebound effect, especially when data cover relatively short time periods. The large effect on estimates could possibly be attributed to an assumption commonly made in empirical studies using microdata. This assumption is that vehicle fuel economy is uncorrelated with other vehicle and household characteristics (Linn, 2016). For example, econometric models in primary studies may fail to control for vehicle attributes like vintage which are correlated with fuel efficiency. These studies may overestimate the rebound effect, as they will attribute the effect of e.g. driving a newer, and thus more comfortable, car on VMT to improved fuel efficiency. Rebound effects are overestimated especially when data cover relatively short time periods which leave little room to fuel efficiency and fuel prices to vary significantly. The WLS model shows that each additional year of data is associated with a 4.9 percentage-point reduction in the estimated magnitude of the rebound effect, while the rest of the models point to a substantially smaller effect.

[16] The sample size used for the estimation of different models often varies within primary studies.

[17] An exception to this definition is Jones (1993), which is grouped together with Greene (1992, 2012), as it extends the analysis of Greene (1992).

Table III
Description and summary statistics for the variables used in the meta-regression analysis.

Explanatory variable	Description	Observations	Mean	Std. dev.	Min	Max
Short-run estimate	=1, if estimate refers to the short run (usually 1 time period); =0, otherwise.	255	0.275	–	0	1
Unspecified response time	=1, if estimate cannot be classified as short- or long-run; =0, otherwise.	255	0.475	–	0	1
Elasticity w.r.t. fuel costs	=1, if estimate of elasticity w.r.t. fuel costs in primary study; =0, otherwise.	255	0.455	–	0	1
Elasticity w.r.t. fuel price	=1, if estimate of elasticity w.r.t. fuel price in primary study; =0, otherwise.	255	0.322	–	0	1
Cross-sectional data	=1, if primary data are cross-sectional; =0, otherwise.	255	0.298	–	0	1
Time series data	=1, if primary data are time series; =0, otherwise.	255	0.153	–	0	1
Microdata	=1, if primary study uses micro-level (e.g. survey) data; =0, otherwise.	255	0.604	–	0	1
Length of period covered by the data	Length of time period covered by the data used in the primary study (in years).	255	15.592	15.093	1	45
Empirical model at the vehicle level	=1, if econometric model is developed at the vehicle level; =0, if model at the household level or on aggregate data.	255	0.451	–	0	1
Vehicle capital costs	=1, if vehicle capital costs taken into account in empirical model; =0, otherwise.	255	0.306	–	0	1
Single car	=1, if elasticity estimate is specific to households with one car; =0, otherwise.	255	0.157	–	0	1
Country-specific	=1, if estimates are based on an analysis for a single country; =0, if estimates are based on a cross-country analysis.	255	0.965	–	0	1
Trend	Time trend based on the average year of the period used to estimate the elasticity (base year is 1971)	255	24.814	11.358	0	39
Percentage of years in oil crisis	Percentage of years in the period 1974–1981 in the total time period considered in the study.	255	14.803	27.708	0	100
GDP per capita	Average GDP per capita (1000s of 2010 USD PPP) in the time period covered by the data	246	37.711	8.511	4.363	51.156
Gasoline price	Average gasoline price per litre (2010 USD PPP) in the time period covered by the data	246	0.939	0.457	0.420	3.600
Land per 100 people	Average land area (in sq. kilometres) per 100 people in the time period covered by the data	246	4.207	8.367	0.205	51.037

Note: Data on GDP per capita are extracted from the OECD National Accounts database. Population density data used to compute land per 100 people are extracted from the World Development Indicators database of the World Bank. Data on gasoline prices are calculated from IEA energy price data. Databases were last accessed in January 2016.

Several studies estimate empirical models at the vehicle level. These models could underestimate the magnitude of the rebound effect, because they do not consider that the elasticity of car ownership with respect to fuel efficiency is likely to be positive. Put differently, higher fuel efficiency induces the ownership and use of more cars, as driving becomes cheaper. The kilometres driven in these extra cars will be

Table IV
Meta-regression results for the subset of preferred estimates.

	OLS		WLS		Fixed effects		Random effects	
	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error
Short-run estimate	−0.188***	(0.029)	−0.271***	(0.019)	−0.184***	(0.031)	−0.188***	(0.030)
Unspecified response time	−0.136**	(0.052)	−0.190**	(0.089)	−0.179**	(0.068)	−0.130***	(0.050)
Elasticity w.r.t. fuel costs	0.110**	(0.054)	0.296***	(0.046)	0.080*	(0.046)	0.093*	(0.051)
Elasticity w.r.t. fuel price	0.112*	(0.066)	0.300***	(0.048)	0.113	(0.070)	0.109*	(0.066)
Cross-sectional data	−0.124*	(0.069)	−0.252***	(0.030)	–	–	−0.001	(0.073)
Time series data	−0.096	(0.059)	−0.417**	(0.184)	–	–	−0.098	(0.061)
Microdata	0.274***	(0.081)	0.664***	(0.132)	–	–	0.255***	(0.064)
Microdata × length of period covered by the data	−0.018***	(0.006)	−0.049***	(0.008)	−0.003	(0.006)	−0.017***	(0.005)
Empirical model at the vehicle level	0.044	(0.050)	0.060	(0.064)	−0.079*	(0.045)	−0.031	(0.044)
Vehicle capital costs	−0.057	(0.049)	−0.302**	(0.128)	0.066	(0.085)	−0.005	(0.052)
Single car	0.111**	(0.052)	0.119**	(0.059)	0.069**	(0.031)	0.085**	(0.033)
Trend	−0.008***	(0.002)	−0.041***	(0.013)	−0.007***	(0.002)	−0.005***	(0.002)
Percentage of years in oil crisis	−0.003***	(0.001)	−0.012***	(0.004)	−0.0002	(0.002)	−0.002*	(0.001)
Country-specific	−0.049	(0.059)	0.080	(0.183)	0.014	(0.022)	−0.024	(0.028)
Country-specific × (GDP per capita)$^{-1}$	0.494	(0.760)	0.371	(1.189)	1.583**	(0.663)	0.135	(0.782)
Country-specific × ln(gasoline price)	0.238***	(0.072)	0.336***	(0.116)	0.072	(0.092)	0.203***	(0.071)
Country-specific × land per 100 people	−0.002	(0.001)	−0.017*	(0.010)	−0.052***	(0.007)	−0.003**	(0.001)
Constant	0.534***	(0.127)	1.401***	(0.453)	0.664***	(0.092)	0.439***	(0.105)
Observations		255		255		255		255
R-squared		0.374		0.818		0.656		0.324[a]
Adjusted R-squared		0.329		0.805		0.522		–
Intraclass correlation[b]		–		–		–		0.487

Note: Robust standard errors, clustered by group of primary studies (58 groups), in parentheses. ***, ** and * indicate that the parameter is statistically significant at the 1%, 5% and 10% level respectively. Types of data used (cross-sectional, time series and microdata) do not vary within groups of studies, so their effect is unidentifiable in the fixed-effects model. The Sargan-Hansen test for overidentifying restrictions suggests that the fixed-effects model should be preferred to the random-effects one (p-value = 0.00).

[a] Weighted average of R-squared within and between panels.

[b] The intraclass correlation shows the proportion of the variance attributed to differences between panels.

neglected by these models and thus the overall rebound effect could be underestimated. Our results do not provide robust empirical support for this argument: the effect is only significant at the 10% level in the fixed-effects model.

Fuel efficiency improvements are rarely costless. Failing to take into account changes in vehicle capital costs in the empirical model used to estimate the rebound effect may lead to its overestimation (see e.g. Sorrell and Dimitropoulos, 2008). We test this assumption by adding a dummy variable capturing whether the primary econometric model controls for changes in car capital costs.[18] The relevant coefficient has the expected sign in most models (with the exception of the fixed-effects one), but only the WLS model points to a significant overestimation of the rebound effect by studies not taking into consideration changes in capital costs.

Households owning only one car may respond differently to fuel efficiency improvements from households owning multiple cars. Indeed, the fixed-effects model suggests that rebound effect estimates for one-vehicle households are about 7 percentage points higher than estimates for multi-vehicle ones and estimates pooling all households together. This finding can be explained by two factors. First, one-vehicle households drive, on average, fewer kilometres than multi-vehicle ones (see e.g. Feng et al., 2013; Linn, 2016). Hence, a certain increase in kilometres travelled in response to a fuel efficiency improvement (e.g. 100 km) will be reflected in a larger increase in *percentage* terms – and, thus, a higher rebound effect – for one-vehicle households than for multi-vehicle ones. Second, when the analysis is conducted at the individual vehicle level (vs. the household level), rebound effects can be more accurately estimated for one-car households. In their case, behavioural responses to fuel efficiency improvements are concentrated on one car and, therefore, the rebound effect observed for that car also reflects the household's full response. On the contrary, multivehicle households can substitute the use of less fuel-efficient cars with the use of more fuel-efficient ones, and rebound effect estimates will depend on the car being analysed.[19]

Several primary studies have argued that the rebound effect in road transport has declined over time and attribute this pattern mainly to an increase in real incomes and a reduction of real fuel costs (e.g. Hymel et al., 2010; Small and van Dender, 2007). We test for the impact of income and fuel price changes on rebound effect estimates (see discussion below), but we were also interested in testing whether there is any time effect independently of macroeconomic factors. To this end, we have included a variable capturing the difference between the average year of the period used to estimate the elasticity and a reference year: in this case, 1971 (see also Brons et al., 2008). The fixed-effects model reveals that the rebound effect declines by about 0.7 percentage points per year, after controlling for changes in real incomes and fuel prices.

Relevant elasticities, and therefore rebound effects, may have also been lower during the oil crises of the 1970s, as fuel supply was constrained. The models use the percentage of years in the period of the oil crises (1974–1981) in the total number of years considered in the primary study to test this assumption (cf. Brons et al., 2008; Espey, 1998). For instance, the WLS model points to a 1.2 percentage-point reduction of the rebound effect for every 10-point increase in the percentage of years in the oil crisis.

Rebound effect estimates vary by country, but it is more interesting to investigate particular country characteristics which may be responsible for this variation. To this end, rebound effect estimates were matched with macroeconomic, demographic, energy, and transport infrastructure characteristics.[20] Estimates are matched with GDP per capita, population density, the percentage of population living in urban areas, gasoline prices, railway density, and other variables which may theoretically affect the magnitude of the rebound effect. Out of these variables, only GDP per capita, gasoline prices and land per capita (the inverse of population density) appeared to both significantly influence rebound effect estimates and not raise multicollinearity concerns and are, thus, included in the presented specifications.

Several studies of the rebound effect suggest that it decreases with income (e.g. Hymel et al., 2010; Small and van Dender, 2007). At least two theoretical arguments exist in support of their findings. First, demand for car travel is closer to saturation for higher-income households than for lower-income ones and, thus, rebound effects for the former group should be smaller. Second, the relative importance of the time costs of driving – vs. e.g. fuel costs or other car operating costs – increases as incomes grow. The opportunity cost of time increases with income and, thus, richer households are likely to take advantage of improved fuel efficiency to a *lesser* extent than less well-off households (see also Small and Van Dender, 2007; Sorrell and Dimitropoulos, 2008; Sorrell et al., 2009). Empirical support for the inverse relationship between income – captured here by GDP per capita – and rebound effect estimates is not always strong in our analysis: coefficients do have the expected sign, but the relationship is statistically significant only in the fixed-effects model.[21]

Higher gasoline prices and population densities may imply higher rebound effect estimates for at least two reasons. First, higher gasoline prices and population densities are, ceteris paribus, associated with less intensive car use. Thus, even if individuals respond to improved fuel efficiency by increasing travelled distances by the same level (e.g. 1000 km), the increase will be higher in *percentage* terms in locations where gasoline prices and population density are higher. Second, individuals may be tempted to increase private car travel to a larger extent (in absolute terms) in places with higher gasoline prices and population density. Before the fuel efficiency improvement, it was more attractive to make some trips by other transport modes (e.g. public transport) than by car. This is much more likely to have occurred in places where gasoline prices and population density are high, as the ratio of the costs of travelling e.g. by public transport over the costs of travelling by car should be lower there. After the fuel efficiency improvement, private car travel becomes more competitive and households are more likely to substitute the car for other modes for those trips.

In agreement with the arguments above, the models show that higher average gasoline prices and population density (the inverse of land per capita) in the period analysed in the primary studies are associated with higher rebound effects (see also Sims et al., 2014). For example, the WLS model suggests that a 1% increase in gasoline prices (constant prices and PPPs) is associated with an increase of the rebound effect of 0.34 percentage points. Gasoline prices are to an important extent determined by excise taxes on gasoline, which vary significantly among countries. However, our findings provide no ground to argue that the rebound effect may be less of a concern where gasoline taxes are lower. Indeed, the effect on distances travelled and therefore on emissions, pollution, congestion and noise may be larger in *absolute*

[18] When the econometric model is based on a system of equations, the vehicle capital costs dummy is set equal to one regardless of whether capital costs are controlled for in the VMT equation or in another equation of the model (e.g. the equation explaining the fleet size/number of cars).

[19] For a given car body type, size and level of comfort, the rebound effect is likely to be underestimated when the analysis is based on the more fuel-efficient car, whereas overestimated when it is based on the less efficient one. However, if the less fuel-efficient car is the one in which more kilometres are driven – e.g. because it is larger and more comfortable – then the opposite line of reasoning might hold: the rebound effect is likely to be underestimated when the analysis focuses on the less fuel efficient (but more comfortable) car, whereas overestimated when it focuses on the more fuel-efficient (but less comfortable) car.

[20] For each estimate of a primary study, the matching approach considers the time coverage of the data used to produce that estimate. The average of each macro-level variable of interest for that time period and country is then calculated (ignoring possible gaps in time series). The resulting averages of macroeconomic and other country characteristics are finally matched back to the rebound effect estimates produced by primary studies.

[21] Assuming an initial value of GDP per capita of 20,000 USD (in 2010 PPP), the fixed-effects model implies a reduction of about 0.4 percentage points in the rebound effect for a 1000 USD increase in GDP per capita.

terms in countries where gasoline taxes are lower (and average distances travelled by car are higher).

The specifications presented above are the outcome of extensive exploratory analysis, where the influence of several other explanatory factors was tested. However, those variables were dropped from the final specifications due to poor statistical significance and multicollinearity concerns. Nevertheless, some of these factors are of particular interest, because they have been considered as important determinants of the magnitude of the rebound effect. For example, we examined whether rebound effect estimates varied with the treatment of the potential endogeneity of the independent variable of interest – fuel efficiency, fuel costs or fuel price. The results of the relevant econometric specifications are presented in Table C.I of Appendix C.[22] Models considering that the variable of interest is endogenous seem to lead to higher rebound effect estimates than models considering it to be exogenous (see the results for the "endogeneity treated" coefficient). However, the effect is only statistically significant in the random-effects model, while multicollinearity concerns are raised in the WLS model, where the variable indicating whether endogeneity is treated is strongly correlated with the variable indicating whether the rebound effect estimate refers to the short run.[23]

To further illustrate differences in the magnitude of the rebound effect between countries or regions with different demographic and macroeconomic characteristics and fuel prices, we also use the results of our meta-regression models to provide estimates of the long-run effect in diverse contexts. Long-run rebound effect estimates are based on the results of the fixed-effects model and are presented in Table V. Estimates refer to the year 2017 and to various levels of GDP per capita and gasoline prices (both in 2010 USD PPP), and population density.

Three levels are considered for each of these variables: for GDP per capita, values range from USD 10,000 – somewhat lower than e.g. the GDP per capita of Indonesia – to USD 60,000, close to the GDP per capita of Norway (OECD, 2018). Gasoline prices range from USD 0.50 per litre, a value below the price of gasoline in OECD countries (but relatively close to the average price of gasoline in the USA in 2016), to USD 3.00 per litre, slightly below the gasoline price in Turkey (IEA, 2018). Population density ranges from 20 people per km^2, a value slightly below average population density in Sweden or the USA to 300 people per km^2, slightly above population density in the United Kingdom (World Bank, 2018).

The range of long-run rebound effect estimates is wide, demonstrating the importance of considering the demographic, macroeconomic and energy price context of a country before assuming a certain value of the rebound effect. Point estimates of long-run rebound effects range from about 15% for a combination of low gasoline prices (50 US cents) and population density (20 people/km^2), and high GDP per capita (60,000 USD), to 65% for a combination of relatively high gasoline prices and population density and a GDP per capita of USD 10,000.

Table V
Predicted long-run rebound effect values for different levels of GDP per capita, gasoline price and population density, 2017: fixed-effects model.

Population density (people/km^2)	Gasoline price per litre (2010 USD PPP)					
	0.50		1.50		3.00	
	Pred. value	Std. error	Pred. value	Std. error	Pred. value	Std. error
GDP per capita (2010 USD PPP) = 10,000						
20	0.282	(0.166)	0.362	(0.107)	0.412	(0.109)
120	0.497	(0.190)	0.577	(0.118)	0.627	(0.102)
300	0.523	(0.193)	0.602	(0.119)	0.653	(0.102)
GDP per capita (2010 USD PPP) = 30,000						
20	0.176	(0.134)	0.256	(0.100)	0.306	(0.125)
120	0.391	(0.155)	0.471	(0.100)	0.521	(0.108)
300	0.417	(0.158)	0.497	(0.100)	0.547	(0.106)
GDP per capita (2010 USD PPP) = 60,000						
20	0.150	(0.127)	0.230	(0.101)	0.280	(0.131)
120	0.365	(0.147)	0.445	(0.098)	0.495	(0.112)
300	0.391	(0.149)	0.470	(0.098)	0.521	(0.110)

Note: Linear predictions, based on the estimates produced by the fixed-effects model. The predictions are computed by setting the values of all variables equal to zero, except for vehicle capital costs (set equal to 1), the time trend variable (equal to 46), the dummy for country-specific estimates (set equal to 1) and its interaction effects with the inverse of GDP per capita, the logarithm of gasoline prices and land per 100 people.

Where incomes are lower and gasoline prices and population density are higher, travellers drive on average relatively few kilometres, so even small behavioural changes in response to fuel efficiency improvements will be recorded as large changes in percentage terms.

6. Conclusions and policy implications

This paper presents a meta-analysis of 74 primary studies including 1120 estimates of the direct rebound effect in road transport. Its aim is to provide a useful synthesis of past work and inform ongoing discussions about the effect's magnitude and its determinants. Empirical results based on a subset of 255 preferred estimates from these studies reveal that the magnitude of the rebound effect in road transport is, on average, around 10–12% in the short run and 26–29% in the long run. In the case of fuel efficiency standards in particular, the long-run estimate is the most relevant, as the fuel efficiency of vehicles cannot be significantly changed in the short run (Linn, 2016).

Variation in rebound effect estimates is, however, large and can be attributed to both contextual and methodological factors. For example, rebound effect estimates vary with the elasticity used to measure the rebound effect and the type of data used in the primary study. The most direct measure of the rebound effect – the elasticity of travel with respect to fuel efficiency – results in more conservative estimates than measures exploiting variation in fuel prices. Furthermore, studies using microdata (e.g. survey data) lead to significantly higher estimates of the rebound effect than studies using aggregate data, especially when they cover relatively short time periods which leave little room to fuel efficiency and fuel prices to vary significantly.

The meta-analysis further reveals significant differences in the estimates of the rebound effect across countries. Cross-country differences can be to some extent attributed to variation in real per capita income, gasoline prices, and population density. Lower GDP per capita, higher gasoline prices and higher population density are associated with larger rebound effects. Therefore, caution should be exercised when assuming a certain magnitude of the rebound effect, as it may vary substantially across regions.

The rebound effect has important policy implications. A 10% increase in fuel efficiency results on average in a circa 2.6% increase in travel demand in the long run. This induced travel partially offsets the expected energy savings from the increase in fuel efficiency, in addition to contributing to mileage-related externalities, like higher levels of

[22] We also tried to test for differences between studies for Europe, North America (Canada and USA) and other regions after controlling for differences in GDP per capita, gasoline prices and population density. However, the relevant region dummies are very strongly correlated with the variables included in the model and raise multicollinearity concerns. Furthermore, estimates based on self-reported data have been suggested to be larger in absolute terms than estimates based on odometer readings (see e.g. Weber and Farsi, 2014). Therefore, we also tested whether estimates based solely on self-reported data are different from estimates based on odometer readings or a mix of data sources. Nevertheless, differences were not significant in any of our specifications. We also tried to introduce a term for correction for publication bias in the regression models equal to the inverse of the sample size used in the primary study. However, the term was only significant in the fixed-effects model and was not further considered in the analysis (cf. Liu and Shumway, 2016).

[23] The positive relationship between endogeneity treatment and rebound effect estimates is not generally in agreement with arguments presented in the existing literature, as studies not treating endogeneity have been suggested to lead to an overestimation of the rebound effect (see Small and Van Dender, 2007; Sorrell and Dimitropoulos, 2008). Such a positive relationship could exist if e.g. drivers of longer distances sort themselves in larger and less fuel-efficient cars and, thus, failing to take this relationship into account leads to an underestimation of the rebound effect.

non-exhaust air pollution, noise and congestion. Induced travel implies that even in the presence of stringent fuel efficiency standards, the implementation of price instruments is key to ensure that road transport externalities are effectively addressed. In addition to motor fuel taxation, it is timely to reconsider the implementation of distance-based road taxes, which can provide for an efficient means of addressing mileage-related externalities. As these external costs (e.g. congestion) vary across space and over time, distance-based taxes will be more efficient if they are space- and time-variant (see also Johansson and Schipper, 1997).

In addition to identifying the factors responsible for the variation in rebound effect estimates, the meta-regression model developed in this paper can serve as a tool to assist policy analysis in contexts where rebound effect estimates are missing. This can be especially useful in countries where it is difficult to collect data on travel demand and resources for relevant analyses are scarce. Importing data on macro-level variables which are more readily available (GDP per capita, gasoline prices, population density), the analyst can derive estimates of potential rebound effects in such contexts. However, such estimates should be treated with caution, especially when they are derived for countries with very different macroeconomic characteristics, transport infrastructure and rates of new technology adoption than the ones analysed here.

It will also be useful to collect and use national data to estimate the rebound effect in other countries. Estimates from developing countries, in particular, are lacking in the existing literature. Due to numerous differences in household income, road and public transport infrastructure, travel behaviour, car ownership and technology adoption between developed and emerging economies, there is little reason to assume a consistent magnitude of the rebound effect across regions. Provided that car travel demand is expected to increase considerably in emerging economies in the following years – with potentially serious environmental consequences – it is useful to collect more empirical evidence of the impact of improved fuel efficiency on car ownership and use in those contexts. This could be a promising avenue for future research.

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Appendix A. Summary statistics of rebound effect estimates by study

Table A.I
List of primary empirical studies used in the analysis.

Ajanovic and Haas (2012)	Gillingham et al. (2015a)	Liu (2011)
Ajanovic et al. (2012)	Gillingham et al. (2015b)	Mannering (1983, 1986)
Barla et al. (2009), Lamonde (2007)	Gillingham and Munk-Nielsen (2015)	Mannering and Winston (1985)
Bento et al. (2009)	Goldberg (1996, 1998)	Matiaske et al. (2012)
Bergel et al. (2002)	Gonzalez and Marrero (2012)	Mayo and Mathis (1988)
Cervero and Hansen (2002)	Greene (1992, 2012)	Mizobuchi (2008)
Chugh and Cropper (2014)	Greene and Hu (1984)	Munk-Nielsen (2015)
Concas (2012)	Greene et al. (1999)	Noland (2001)
Dargay (2007)	Greening et al. (1995)	Noland and Cowart (2000)
De Borger et al. (2016)	Hansen and Huang (1997)	Odeck and Johansen (2016)
De Jong (1996)	Haughton and Sarkar (1996)	Pickrell and Schimek (1999)
De Jong et al. (2009)	Hensher et al. (1990)	Pirotte and Madre (2012)
D'Haultfœuille et al. (2014)	Hensher and Smith (1986)	Puller and Greening (1999)
Dillon et al. (2015)	Hymel and Small (2015)	Rentziou et al. (2012)
Feng et al. (2013)	Hymel et al. (2010)	Schimek (1996)
Ficano and Thompson (2014)	Johansson and Schipper (1997)	Small and Van Dender (2007)
Fridstrøm (1998)	Jones (1993)	Stapleton et al. (2016)
Frondel et al. (2017)	Kemel et al. (2011)	Steren et al. (2016)
Frondel et al. (2007, 2008)	Knittel and Sandler (2010, 2011)	Su (2011, 2012, 2015)
Frondel et al. (2012)	Lee (2015)	Wang and Chen (2014)
Frondel and Vance (2009, 2011, 2013)	Leung (2015)	Weber and Farsi (2014)
Gately (1990)	Li et al. (2014)	Wheaton (1982)
Gillingham (2014)	Linn (2016)	Yu et al. (2016)

Note: More than one study has been produced by Frondel and Vance (3), Greene (2) and Su (3). Lamonde (2007), Goldberg (1996), Frondel et al. (2007), Knittel and Sandler (2010), and Mannering (1983) provide additional estimates and information for Barla et al. (2009), Goldberg (1998), Frondel et al. (2008), Knittel and Sandler (2011), and Mannering and Winston (1985) and Mannering (1986) respectively.

Table A.II
Summary statistics for studies estimating the elasticity of travel with respect to fuel efficiency.

Primary study	Country	Data years	All estimates				Sample of preferred estimates			
			N	Mean	Min	Max	N	Mean	Min	Max
De Borger et al. (2016)	Denmark	2001–2011	14	0.094	0.054	0.174	8	0.088	0.076	0.100
Dillon et al. (2015)	United States	2008–2009	3	0.019	0.004	0.045	1	0.045	0.045	0.045
Frondel et al. (2007, 2008)	Germany	1997–2005	5	0.716	0.575	1.152	3	0.807	0.585	1.152
Frondel et al. (2012)	Germany	1997–2009	2	0.506	0.418	0.594	1	0.418	0.418	0.418
Frondel and Vance (2009)	Germany	1997–2006	2	0.517	0.515	0.518	2	0.517	0.515	0.518
Frondel and Vance (2013)	Germany	1997–2012	4	0.612	0.188	0.953	2	0.571	0.188	0.953
Gillingham and Munk-Nielsen (2015)	Denmark	1998–2011	2	−0.006	−0.035	0.023	1	0.023	0.023	0.023

(continued on next page)

Table A.II (*continued*)

Primary study	Country	Data years	All estimates				Sample of preferred estimates			
			N	Mean	Min	Max	N	Mean	Min	Max
Greene (2012)	United States	1966–2007	6	−0.019	−0.143	0.144	2	−0.088	−0.143	−0.032
Greene and Hu (1984)	United States	1978–1981	24	0.348	0.119	0.479	6	0.347	0.307	0.413
Hymel and Small (2015)	United States	1966–2009	1	0.023	0.023	0.023	1	0.023	0.023	0.023
Linn (2016)	United States	2008–2009	34	0.348	0.103	0.793	2	0.290	0.208	0.371
Liu (2011)	United States	2000–2001	10	0.159	−0.292	0.369	1	0.172	0.172	0.172
Matiaske et al. (2012)	Germany	1997–2003	1	−0.032	−0.032	−0.032	1	−0.032	−0.032	−0.032
Mizobuchi (2008)	Japan	2007	4	0.254	0.166	0.411	1	0.223	0.223	0.223
Munk-Nielsen (2015)	Denmark	1997–2006	2	0.499	0.279	0.718	1	0.279	0.279	0.279
Schimek (1996)	United States	1950–1994	2	0.130	0.050	0.210	2	0.130	0.050	0.210
Stapleton et al. (2016)	United Kingdom	1969–2011	24	−0.134	−0.643	0.309	1	0.309	0.309	0.309
Steren et al. (2016)	Israel	2007–2011	3	0.355	0.129	0.535	3	0.355	0.129	0.535
Su (2015)	United States	2008	8	0.131	0.090	0.172	7	0.127	0.090	0.172
Wang and Chen (2014)	United States	2008–2009	5	0.100	−0.203	0.700	5	0.100	−0.203	0.700
Weber and Farsi (2014)	Switzerland	2010	4	0.528	0.187	0.814	2	0.501	0.187	0.814
Wheaton (1982)	Cross-national	1972	3	0.074	0.057	0.103	2	0.083	0.063	0.103
Yu et al. (2016)	Japan and China	2009	40	0.475	−0.411	1.329	2	0.356	0.287	0.425
All			203	0.267	−0.643	1.329	57	0.243	−0.203	1.152

Table A.III
Summary statistics for studies estimating the elasticity of travel with respect to per unit fuel costs.

Primary study	Country	Data years	All estimates				Sample of preferred estimates			
			N	Mean	Min	Max	N	Mean	Min	Max
Ajanovic and Haas (2012)	6 EU countries	1970–2007	14	0.329	0.050	0.880	14	0.329	0.050	0.880
Ajanovic et al. (2012)	12 EU countries	1980–2007	2	0.290	0.160	0.420	2	0.290	0.160	0.420
Barla et al. (2009), Lamonde (2007)	Canada	1990–2004	12	0.210	0.080	0.569	4	0.134	0.080	0.240
Bento et al. (2009)	United States	2001	1	0.340	0.340	0.340	1	0.340	0.340	0.340
Chugh and Cropper (2014)	India	2010	2	0.800	0.670	0.930	2	0.800	0.670	0.930
Concas (2012)	United States	1980–2005	6	0.582	0.086	1.453	2	0.583	0.171	0.994
Dargay (2007)	United Kingdom	1976–1995	10	0.131	0.090	0.180	2	0.120	0.100	0.140
De Jong (1996)	Netherlands	1992	1	0.320	0.320	0.320	1	0.320	0.320	0.320
De Jong et al. (2009)	Netherlands	2008	1	1.220	1.220	1.220	1	1.220	1.220	1.220
D'Haultfœuille et al. (2014)	France	2007–2008	1	0.530	0.530	0.530	1	0.530	0.530	0.530
Feng et al. (2013)	United States	1996–2000	10	0.054	0.024	0.117	5	0.039	0.024	0.070
Frondel et al. (2008)	Germany	1997–2005	3	0.587	0.581	0.596	1	0.596	0.596	0.596
Frondel et al. (2012)	Germany	1997–2009	2	0.540	0.459	0.620	1	0.459	0.459	0.459
Frondel and Vance (2009)	Germany	1997–2006	2	0.506	0.490	0.521	2	0.506	0.490	0.521
Gately (1990)	United States	1966–1988	2	0.080	0.070	0.090	1	0.090	0.090	0.090
Gillingham et al. (2015a)	Denmark	1996–2009	2	0.542	0.419	0.665	1	0.419	0.419	0.419
Gillingham et al. (2015b)	United States	2000–2010	3	0.108	0.076	0.150	1	0.097	0.097	0.097
Goldberg (1996, 1998)	United States	1984–1990	17	0.043	−0.280	0.240	1	−0.040	−0.040	−0.040
Greene (1992)	United States	1956–1989	15	0.178	0.059	0.450	1	0.134	0.134	0.134
Greene (2012)	United States	1966–2007	4	0.108	0.035	0.204	2	0.097	0.037	0.157
Greene et al. (1999)	United States	1979–1994	6	0.228	0.175	0.280	1	0.230	0.230	0.230
Greening et al. (1995)	United States	1990	17	0.304	0.133	0.574	1	0.292	0.292	0.292
Haughton and Sarkar (1996)	United States	1970–1991	12	0.204	0.074	0.580	2	0.193	0.156	0.230
Hensher et al. (1990)	Australia	1981–1982	8	0.268	0.065	0.389	4	0.248	0.065	0.389
Hensher and Smith (1986)	Australia	1981–1982	6	0.203	0.092	0.311	2	0.180	0.099	0.260
Hymel and Small (2015)	United States	1966–2009	38	0.075	0.008	0.309	12	0.083	0.008	0.295
Hymel et al. (2010)	United States	1966–2004	44	0.123	0.024	0.322	8	0.103	0.026	0.241
Johansson and Schipper (1997)	12 OECD countries	1973–1992	8	0.212	0.061	0.470	1	0.120	0.120	0.120
Jones (1993)	United States	1966–1990	14	0.161	0.108	0.313	2	0.211	0.108	0.313
Kemel et al. (2011)	France	1999–2007	2	0.369	0.278	0.460	2	0.369	0.278	0.460
Knittel and Sandler (2010, 2011)	United States	1998–2010	19	0.229	0.096	0.440	1	0.224	0.224	0.224
Linn (2016)	United States	2008–2009	5	0.442	0.125	0.894	2	0.493	0.174	0.811
Liu (2011)	United States	2000–2001	10	0.289	0.026	0.867	1	0.232	0.232	0.232
Mannering (1983, 1986)	United States	1979–1980	24	0.186	−0.264	0.543	4	0.289	0.132	0.543
Mannering and Winston (1985)	United States	1978–1980	20	0.210	0.004	0.911	4	0.166	0.059	0.279
Mayo and Mathis (1988)	United States	1958–1984	2	0.241	0.221	0.261	1	0.221	0.221	0.221
Munk-Nielsen (2015)	Denmark	1997–2006	7	0.362	0.158	0.744	1	0.300	0.300	0.300
Schimek (1996)	United States	1950–1994	6	0.185	0.050	0.410	2	0.180	0.070	0.290
Small and Van Dender (2007)	United States	1966–2001	12	0.138	0.022	0.340	8	0.104	0.022	0.240
Stapleton et al. (2016)	United Kingdom	1969–2011	34	0.182	0.023	1.420	1	0.245	0.245	0.245
Su (2011)	United States	2001–2009	8	0.084	0.028	0.196	2	0.069	0.028	0.110
Su (2012)	United States	2008–2009	33	0.154	0.097	0.224	8	0.151	0.106	0.193
All			445	0.196	−0.280	1.453	116	0.227	−0.040	1.220

Table A.IV
Summary statistics for studies estimating the elasticity of travel with respect to fuel price.

Primary study	Country	Data years	All estimates				Sample of preferred estimates			
			N	Mean	Min	Max	N	Mean	Min	Max
Bergel et al. (2002)	France	1981–1999	8	0.173	0.081	0.249	1	0.128	0.128	0.128
Cervero and Hansen (2002)	United States	1976–1997	2	0.201	0.179	0.223	2	0.201	0.179	0.223
De Borger et al. (2016)	Denmark	2001–2011	13	0.554	−0.184	1.026	2	0.711	0.565	0.856
Dillon et al. (2015)	United States	2008–2009	3	0.132	0.066	0.171	1	0.066	0.066	0.066
Ficano and Thompson (2014)	United States	2008–2009	14	0.640	0.255	1.625	2	0.692	0.605	0.778
Fridstrøm (1998)	Norway	1973–1994	2	0.183	0.109	0.257	2	0.183	0.109	0.257
Frondel et al. (2017)	Germany	2000–2014	6	0.607	0.314	1.420	2	0.458	0.447	0.468
Frondel et al. (2007, 2008)	Germany	1997–2005	5	0.616	0.476	0.801	3	0.633	0.476	0.801
Frondel et al. (2012)	Germany	1997–2009	8	0.655	0.551	0.898	1	0.574	0.574	0.574
Frondel and Vance (2009)	Germany	1997–2006	2	0.467	0.406	0.528	2	0.467	0.406	0.528
Frondel and Vance (2011)	Germany	1997–2009	12	0.420	−0.027	0.689	4	0.518	0.448	0.584
Frondel and Vance (2013)	Germany	1997–2012	4	0.498	0.438	0.573	2	0.490	0.439	0.541
Gillingham (2014)	United States	2001–2009	33	0.303	0.120	0.690	1	0.220	0.220	0.220
Gillingham et al. (2015a)	United States	2000–2010	17	0.120	0.007	0.411	1	0.099	0.099	0.099
Gillingham and Munk-Nielsen (2015)	Denmark	1998–2011	51	0.354	0.228	0.866	1	0.304	0.304	0.304
Gonzalez and Marrero (2012)	Spain	1998–2006	18	0.287	−0.030	0.615	2	0.444	0.282	0.607
Greene (2012)	United States	1966–2007	6	0.107	0.004	0.299	2	0.140	0.051	0.229
Greene and Hu (1984)	United States	1978–1981	24	0.199	−0.001	0.517	6	0.217	0.118	0.343
Hansen and Huang (1997)	United States	1973–1990	4	0.093	0.080	0.100	1	0.090	0.090	0.090
Hymel and Small (2015)	United States	1966–2009	1	0.054	0.054	0.054	1	0.054	0.054	0.054
Kemel et al. (2011)	France	1999–2007	2	0.230	0.200	0.260	2	0.230	0.200	0.260
Knittel and Sandler (2010, 2011)	United States	1998–2009	11	0.444	0.288	0.625	1	0.440	0.440	0.440
Lee (2015)	United States	2002–2011	6	0.060	0.046	0.068	3	0.060	0.048	0.067
Leung (2015)	United States	2008–2009	24	0.104	−0.033	0.265	1	0.087	0.087	0.087
Li et al. (2014)	United States	1995–2001	6	0.251	−0.108	0.497	2	0.142	−0.108	0.391
Linn (2016)	United States	2008–2009	25	0.150	0.093	0.587	2	0.112	0.107	0.116
Liu (2011)	United States	2000–2001	7	1.328	0.974	1.686	1	1.686	1.686	1.686
Matiaske et al. (2012)	Germany	1997–2003	1	0.329	0.329	0.329	1	0.329	0.329	0.329
Munk-Nielsen (2015)	Denmark	1997–2006	2	0.504	0.282	0.725	1	0.282	0.282	0.282
Noland (2001)	United States	1984–1996	67	0.093	−0.409	0.365	2	0.104	0.049	0.158
Noland and Cowart (2000)	United States	1982–1996	10	0.015	−0.135	0.080	2	0.049	0.045	0.052
Odeck and Johansen (2016)	Norway	1980–2011	4	0.213	0.110	0.358	2	0.173	0.110	0.235
Pickrell and Schimek (1999)	United States	1995	6	0.147	0.040	0.340	1	0.040	0.040	0.040
Pirotte and Madre (2012)	France	1985–2007	8	0.108	0.090	0.139	1	0.092	0.092	0.092
Puller and Greening (1999)	United States	1980–1990	4	0.728	0.690	0.770	1	0.750	0.750	0.750
Rentziou et al. (2012)	United States	1998–2008	6	0.158	0.034	0.310	2	0.062	0.035	0.088
Schimek (1996)	United States	1950–1994	2	0.160	0.060	0.260	2	0.160	0.060	0.260
Stapleton et al. (2016)	United Kingdom	1969–2011	31	0.130	−0.080	1.020	1	0.197	0.197	0.197
Steren et al. (2016)	Israel	2007–2011	1	0.661	0.661	0.661	1	0.661	0.661	0.661
Su (2015)	United States	2008	8	0.145	0.040	0.265	7	0.159	0.086	0.265
Wang and Chen (2014)	United States	2008–2009	5	0.241	0.094	0.406	5	0.241	0.094	0.406
Wheaton (1982)	Cross-national	1972	3	0.529	0.500	0.547	2	0.521	0.500	0.541
All			472	0.266	−0.409	1.686	82	0.299	−0.108	1.686

Appendix B. Summary statistics and meta-regression results for all estimates

Table B.I
Summary statistics of empirical estimates of the rebound effect in road transport; all estimates.

Rebound effect definition	Observations	Unweighted			Weighted		
		Mean	Median	Std. dev.	Mean	Median	Std. dev.
Elasticity w.r.t. fuel efficiency	203	0.267	0.245	0.333	0.177	0.103	0.207
Elasticity w.r.t. fuel costs	445	0.196	0.150	0.195	0.199	0.154	0.129
Elasticity w.r.t. fuel price	472	0.266	0.200	0.263	0.252	0.233	0.160

Note: Statistics of 1120 estimates. For elasticities with respect to fuel costs and fuel price, the table presents the negative of the estimates derived from the primary study. To compute the weighted statistics, each estimate is weighted by the size of the sample used in the primary study.

Table B.II
Summary statistics of estimates by time horizon and elasticity measure; all estimates.

	No. of estimates	Unweighted mean	Weighted mean	Min	Max
Elasticity w.r.t. fuel efficiency					
Short-run	21	−0.131	0.046	−0.372	0.684
Long-run	51	0.264	0.356	−0.643	1.152
Unspecified	131	0.331	0.064	−0.411	1.329
Elasticity w.r.t. fuel costs					
Short-run	189	0.103	0.092	−0.280	0.574
Long-run	170	0.272	0.239	−0.264	1.453
Unspecified	86	0.252	0.393	0.026	1.220

(continued on next page)

Table B.II (*continued*)

	No. of estimates	Unweighted mean	Weighted mean	Min	Max
Elasticity w.r.t. fuel price					
Short-run	120	0.122	0.128	−0.108	0.587
Long-run	111	0.277	0.298	−0.080	1.020
Unspecified	241	0.333	0.350	−0.409	1.686

Note: Statistics of 1120 estimates. For elasticities with respect to fuel costs and fuel price, the table presents the negative of the estimates derived from the primary study. Estimates defined in the primary study as medium-run are considered here as long-run elasticities. To compute the weighted mean, each estimate is weighted by the size of the sample used in the primary study.

Table B.III
Comparison of means of estimates published in academic journals with estimates extracted from other sources.

	No. of estimates		Unweighted mean				Weighted mean			
	Journals	Other types	Journals	Other types	Difference	*p-value*	Journals	Other types	Difference	*p-value*
Preferred estimates										
Short-run	64	6	0.102	0.281	−0.178	*0.141*	0.098	0.097	0.001	*0.906*
Long-run	54	10	0.289	0.500	−0.212	*0.140*	0.255	0.295	−0.039	*0.300*
Unspecified	98	23	0.272	0.404	−0.132	*0.112*	0.261	0.341	−0.080	*0.042*
All estimates										
Short-run	288	42	0.089	0.133	−0.043	*0.274*	0.122	0.091	0.030	*0.000*
Long-run	274	58	0.258	0.340	−0.081	*0.209*	0.256	0.288	−0.033	*0.202*
Unspecified	331	127	0.287	0.397	−0.110	*0.075*	0.321	0.344	−0.022	*0.705*

Note: Statistics for 255 preferred estimates (upper panel) and all 1120 estimates (lower panel). Estimates defined in the primary study as medium-run are considered here as long-run elasticities. To compute the weighted mean, each estimate is weighted by the size of the sample used in the primary study. The p-values show the lowest level of statistical significance at which the hypothesis that the difference between the two means is equal to zero can be rejected. The p-values are computed on the basis of robust standard errors, clustered by group of primary studies.

Table B.IV
Description and summary statistics for the variables used in the meta-regression analysis; all estimates.

Explanatory variable	Description	Observations	Mean	Std. dev.	Min	Max
Short-run estimate	=1, if estimate refers to the short run (usually 1 time period); =0, otherwise.	1120	0.295	–	0	1
Unspecified response time	=1, if estimate cannot be classified as short- or long-run; =0, otherwise.	1120	0.409	–	0	1
Elasticity w.r.t. fuel costs	=1, if estimate of elasticity w.r.t. fuel costs in primary study; =0, otherwise.	1120	0.397	–	0	1
Elasticity w.r.t. fuel price	=1, if estimate of elasticity w.r.t. fuel price in primary study; =0, otherwise.	1120	0.421	–	0	1
Cross-sectional data	=1, if primary data are cross-sectional; =0, otherwise.	1120	0.313	–	0	1
Time series data	=1, if primary data are time series; =0, otherwise.	1120	0.157	–	0	1
Microdata	=1, if primary study uses micro-level (e.g. survey) data; =0, otherwise.	1120	0.601	–	0	1
Length of period covered by the data	Length of time period covered by the data used in the primary study (in years).	1120	14.871	14.213	1	45
Empirical model at the vehicle level	=1, if econometric model is developed at the vehicle level; =0, if model at the household level or on aggregate data.	1120	0.442	–	0	1
Vehicle capital costs	=1, if vehicle capital costs taken into account in empirical model; =0, otherwise.	1120	0.207	–	0	1
Single car	=1, if elasticity estimate is specific to households with one car; =0, otherwise.	1120	0.113	–	0	1
Country-specific	=1, if estimates are based on an analysis for a single country; =0, if estimates are based on a cross-country analysis.	1120	0.984	–	0	1
Trend	Time trend based on the average year of the period used to estimate the elasticity (base year is 1971)	1120	24.766	10.820	−7.5	39
Percentage of years in oil crisis	Percentage of years in the period 1974–1981 in the total time period considered in the study.	1120	14.439	28.002	0	100
GDP per capita	Average GDP per capita (1000s of 2010 USD PPP) in the time period covered by the data	1102	37.056	8.896	4.363	51.156
Gasoline price	Average gasoline price per litre (2010 USD PPP) in the time period covered by the data	1099	0.880	0.392	0.420	3.600
Land per 100 people	Average land area (in sq. kilometres) per 100 people in the time period covered by the data	1102	3.476	6.293	0.205	51.037

Note: Data on GDP per capita are extracted from the OECD National Accounts database. Population density data used to compute land per 100 people are extracted from the World Development Indicators database of the World Bank. Data on gasoline prices are calculated from IEA energy price data. Databases were last accessed in January 2016.

Table B.V
Meta-regression results; all estimates.

	OLS		WLS		Fixed effects		Random effects	
	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error
Short-run estimate	−0.195***	(0.027)	−0.257***	(0.028)	−0.200***	(0.027)	−0.198***	(0.027)
Unspecified response time	−0.066*	(0.035)	−0.138	(0.097)	−0.122***	(0.035)	−0.101***	(0.025)
Elasticity w.r.t. fuel costs	0.092	(0.060)	0.179**	(0.083)	0.098	(0.061)	0.098*	(0.058)
Elasticity w.r.t. fuel price	0.100	(0.060)	0.241***	(0.075)	0.146*	(0.080)	0.142*	(0.077)
Cross-sectional data	−0.106**	(0.046)	−0.228***	(0.046)	–	–	−0.007	(0.062)
Time series data	−0.090	(0.067)	−0.356*	(0.207)	–	–	−0.091	(0.063)
Microdata	0.294***	(0.075)	0.652***	(0.136)	–	–	0.216***	(0.068)
Microdata × length of period covered by the data	−0.013**	(0.005)	−0.043***	(0.010)	−0.007	(0.006)	−0.010*	(0.005)
Empirical model at the vehicle level	0.022	(0.034)	0.092**	(0.041)	−0.019	(0.048)	−0.025	(0.035)
Vehicle capital costs	−0.040	(0.040)	−0.208	(0.151)	0.019	(0.022)	0.005	(0.024)
Single car	0.112***	(0.041)	0.141**	(0.056)	0.082**	(0.034)	0.083***	(0.032)
Trend	−0.004	(0.003)	−0.036**	(0.015)	−0.003***	(0.001)	−0.003**	(0.001)
Percentage of years in oil crisis	−0.003***	(0.001)	−0.011***	(0.004)	0.001	(0.001)	−0.001**	(0.001)
Country-specific	−0.212***	(0.075)	0.038	(0.189)	−0.129***	(0.018)	−0.145***	(0.045)
Country-specific × (GDP per capita)$^{-1}$	3.170**	(1.272)	0.783	(1.495)	5.035***	(0.489)	3.522***	(1.234)
Country-specific × ln(gasoline price)	0.102*	(0.058)	0.257*	(0.129)	0.135*	(0.074)	0.147***	(0.056)
Country-specific × land per 100 people	−0.0003	(0.001)	−0.009	(0.008)	−0.045***	(0.007)	−0.002	(0.002)
Constant	0.436***	(0.104)	1.183***	(0.419)	0.498***	(0.051)	0.339***	(0.070)
Observations		1120		1120		1120		1120
R-squared		0.316		0.493		0.498		0.277[a]
Adjusted R-squared		0.305		0.485		0.465		–
Intraclass correlation[b]		–		–		–		0.447

Note: Robust standard errors, clustered by group of primary studies (58 groups), in parentheses. ***, ** and * indicate that the parameter is statistically significant at the 1%, 5% and 10% level respectively. Types of data used (cross-sectional, time series and microdata) do not vary within groups of studies, so their effect is unidentifiable in the fixed-effects model. The Sargan-Hansen test for overidentifying restrictions suggests that the fixed-effects model should be preferred to the random-effects one (p-value = 0.00).

[a] Weighted average of R-squared within and between panels.

[b] The intraclass correlation shows the proportion of the variance attributed to differences between panels.

Appendix C. Additional estimation results

Table C.I
Results of meta-regression models considering whether endogeneity of variable of interest is treated in primary empirical model; subset of preferred estimates.

	OLS		WLS		Fixed effects		Random effects	
	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error
Short-run estimate	−0.193***	(0.031)	−0.459***	(0.034)	−0.185***	(0.031)	−0.194***	(0.031)
Unspecified response time	−0.131**	(0.057)	−0.217***	(0.058)	−0.183**	(0.069)	−0.126**	(0.054)
Elasticity w.r.t. fuel costs	0.111**	(0.055)	0.309***	(0.045)	0.081*	(0.046)	0.091*	(0.048)
Elasticity w.r.t. fuel price	0.117*	(0.069)	0.314***	(0.049)	0.113	(0.069)	0.111*	(0.066)
Cross-sectional data	−0.133*	(0.073)	−0.214***	(0.038)	–	–	−0.028	(0.072)
Time series data	−0.071	(0.074)	−0.348**	(0.169)	–	–	−0.064	(0.066)
Microdata	0.289***	(0.087)	0.597***	(0.104)	–	–	0.301***	(0.071)
Microdata × length of period covered by the data	−0.020***	(0.006)	−0.044***	(0.007)	−0.018	(0.017)	−0.021***	(0.005)
Endogeneity treated	0.067	(0.061)	0.216***	(0.036)	0.108	(0.103)	0.101**	(0.051)
Empirical model at the vehicle level	0.056	(0.050)	0.072	(0.052)	−0.031	(0.079)	−0.028	(0.044)
Vehicle capital costs	−0.083	(0.052)	−0.367***	(0.109)	−0.016	(0.136)	−0.040	(0.054)
Single car	0.097	(0.060)	−0.039	(0.046)	0.055	(0.034)	0.067*	(0.038)
Trend	−0.009***	(0.003)	−0.042***	(0.012)	−0.008***	(0.003)	−0.007***	(0.002)
Percentage of years in oil crisis	−0.003**	(0.001)	−0.011***	(0.003)	−0.001	(0.002)	−0.002*	(0.001)
Country-specific	−0.064	(0.066)	0.097	(0.196)	0.021	(0.022)	−0.043	(0.031)
Country-specific × (GDP per capita)$^{-1}$	0.708	(0.849)	0.467	(0.874)	1.796***	(0.609)	0.515	(0.809)
Country-specific × ln(gasoline price)	0.245***	(0.070)	0.370***	(0.076)	0.042	(0.085)	0.201***	(0.071)
Country-specific × land per 100 people	−0.002	(0.001)	−0.022**	(0.010)	−0.054***	(0.007)	−0.004**	(0.002)
Constant	0.533***	(0.145)	1.414***	(0.430)	0.705***	(0.124)	0.436***	(0.128)
Observations		255		255		255		255
R-squared		0.384		0.849		0.659		0.315[a]
Adjusted R-squared		0.337		0.838		0.525		–
Intraclass correlation[b]		–		–		–		0.451

Note: Robust standard errors, clustered by group of primary studies (58 groups), in parentheses. ***, ** and * indicate that the parameter is statistically significant at the 1%, 5% and 10% level respectively. Types of data used (cross-sectional, time series and microdata) do not vary within groups of studies, so their effect is unidentifiable in the fixed-effects model. The Sargan-Hansen test for overidentifying restrictions suggests that the fixed-effects model should be preferred to the random-effects one (p-value = 0.00). Endogeneity is treated in 42.8% of the cases in the subset of preferred estimates. The correlation between the dummy indicating treatment of endogeneity and the dummy for a short-run estimate is approximately 0.93 in the WLS model.

[a] Weighted average of R-squared within and between panels.

[b] The intraclass correlation shows the proportion of the variance attributed to differences between panels.

Appendix D. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.eneco.2018.07.021.

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