

# Meta-analyses on Environmental and Energy Economics

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## List of Abbreviations

<b>APE</b>	Absolute percentage error
<b>BMA</b>	Bayesian model averaging
<b>BPLM</b>	Breusch and Pagan Lagrangian multiplier
<b>BT</b>	Benefit transfer
<b>FAT</b>	Funnel Asymmetry Test
<b>FEML</b>	Fixed effects multilevel model
<b>GHG</b>	Greenhouse gas
<b>IQR</b>	Interquartile range
<b>LCA</b>	Life-cycle assessment
<b>MRA</b>	Meta-regression analysis
<b>NPL</b>	National Priority List
<b>OLS</b>	Ordinary least squares
<b>PIP</b>	Posterior inclusion probability
<b>PET</b>	Precision Effect Test
<b>PEESE</b>	Precision Effect Estimate with Standard Error
<b>PRISMA</b>	Preferred Reporting Items for Systematic Reviews and Meta-Analyses
<b>REML</b>	Random effects multilevel model
<b>WLS</b>	Weighted least squares

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*To Felix, whose eagerness for discovery and learning inspires me anew every day.*

# Introduction

*Ich suche Wahrheit,*

*Ich suche Klarheit.*

Prof. Ambronsius<sup>1</sup>

Empirical economic research seeks to explain real-world phenomena by applying economic theory to specific problems. In addition to general academic interest, the findings of economists are of great practical importance as they are intended to inform policy-making. It is therefore important to be able to rely on the accumulated empirical economic evidence on a policy-relevant issue.

However, the credibility of empirical economic evidence continues to be debated. In his seminal paper "Let's Take the Con out of Econometrics", Leamer (1983) argues that at-that-time contemporary econometric inference was largely subjective, criticizing especially the lack of sufficient sensitivity analyses to corroborate empirical findings. Sims (1980) and Hendry (1980) present similar lines of argumentation, both questioning the robustness of reported empirical results due to the multitude of researchers' modeling decisions and poor data quality. A little more than 25 years later, Angrist and Pischke (2010) reassessed the situation, testifying a "Credibility Revolution in Empirical Economics". Next to sensitivity analyses, the authors identify considerable improvements in empirical research design quality as the primary source for growth of trust in reported results. In particular, they argue that the increased reliance on methods explicitly aiming for causal inference, i.e., randomized control trials and quasi-experimental methods, helped to remedy the concerns of spurious findings mentioned above.<sup>2</sup>

Ioannidis and Doucouliagos (2013) challenge this optimism of increased trustworthiness, noting that even randomized control trials, as well as experiments, may suffer from publication bias, data selection, and confirmation bias to an unknown extent.<sup>3</sup> In fact, Brodeur et al. (2020) document that p-hacking and publication bias are even present in empirical economic causal analysis and Christensen and Miguel (2018, p. 920) conclude that "publication bias, inability to replicate and specification searching remain

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<sup>1</sup> From the musical "Tanz der Vampire" (Dance of the vampires) (Kunze and Steinman, 1998).

<sup>2</sup> In fact, his "methodological contributions to the analysis of causal relationships" gained Joshua D. Angrist the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 2021 (Nobel Prize Outreach AB 2023, 2023).

<sup>3</sup> Deaton and Cartwright (2018) summarize general limitations of randomized control trials.

widespread in the discipline." Similarly, Ioannidis et al. (2017) show that most empirical economic results require more statistical power to properly identify effects. Worse, they document a strong effect size bias, i.e., estimates are typically overestimated by a factor of two compared to an average based only on adequately powered estimates. Bartoš et al. (2022, p. 1) add to this evidence on publication bias in economic research, showing that the "median probability of the presence of an effect in economics decreased from 99.9% to 29.7% after adjusting for publication selection bias."

This brief summary of assessments of the overall credibility of empirical economic evidence draws a mixed picture, at best. While progress has been made over time, e.g., in research design and data availability, many challenges still need to be overcome (e.g., publication bias, specification searching, and poor statistical power), questioning the informative value of the accumulated empirical economic evidence. Fortunately, there are many promising recent developments that increase the credibility of economic research findings. I highlight the ongoing shift towards study registration, pre-analysis plans, journals' reporting standards, and data and code sharing policies that foster transparency and reliability of reported results (see also Christensen and Miguel, 2018, for a detailed overview). Likewise, the regular reliance on methods to address model uncertainty (e.g., Bayesian model averaging, general-to-specific approaches) increases the robustness and objectivity of results (Steel, 2020).

While these developments will probably increase the credibility of upcoming research, the existing body of empirical evidence is likely to be plagued with the aforementioned deficits. In this context, meta-analytic methods are particularly well suited to inform on the insights gained in a particular scientific field while taking the introduced biases into account. More specifically, meta-analyses meet the need for a quantitative summary of an area of research by combining the available evidence on a specific research question into a meta-dataset. Next to the calculation of average effect sizes for the analysed literature, meta-regressions allow to identify factors of research design that are accountable for heterogeneous findings in the primary literature.<sup>4</sup> In addition, meta-analyses can identify and correct the accumulated evidence for publication bias, quantify the effect of misspecification on the estimated effect size, and can be combined with model averaging techniques to account for model uncertainty (Havránek et al., 2020).

Given these virtues, this method has, unsurprisingly, gained popularity in economics in the last decades (Alinaghi and Reed, 2018; Havránek et al., 2020).<sup>5</sup> Guidelines for conducting and reporting meta-analyses in economics have been developed (Stanley et al., 2013) and revised (Havránek et al., 2020), reflecting the vast improvements in

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<sup>4</sup> In contrast, qualitative narrative reviews are useful to, e.g., provide structure to a research field and highlight best-practice examples. The insights gained remain, by definition, qualitative and can give - at best - anecdotal evidence about quantitative effects.

<sup>5</sup> Meta-analyses have their roots in medicine and psychology (Glass, 1976).

meta-analytic methodology in recent years. Where Nelson and Kennedy (2009) still identified many deficiencies in the meta-analyses they review, Havránek et al. (2020) acknowledge an overall improvement in quality and especially the development of new meta-analytic methods that address publication bias (see, e.g., Irsova et al., 2023; Bartoš et al., 2023; Andrews and Kasy, 2019; Bom and Rachinger, 2019; Furukawa, 2019; Ioannidis et al., 2017) as well as more frequent adoption of model averaging techniques (e.g., Havranek and Sokolova, 2020; Gechert et al., 2022; Matousek et al., 2022). In summary, the current meta-analytic toolkit allows for a reliable synthesis of existing empirical economic research, taking into account the credibility threats mentioned above.

In this thesis, I use meta-analytic techniques in three articles related to empirical questions in environmental and energy economics. In all three cases, the analysed literature is characterised by a large variation in the reported effect size as well as pronounced differences in the research design choices of the primary studies. Accordingly, the main focus of all articles is to calculate an average effect size and to identify study design factors that explain the observed heterogeneity of results. In addition, it is possible to assess the presence and severity of publication bias and to use model reduction techniques in two of the three articles (Chapters 1 and 2). For the third article (Chapter 3), these options are not feasible due to the nature of the data analysed, so it focuses instead on calculating average effect sizes for reasonable combinations of research designs often observed in primary studies. In the following, I highlight the main findings and contributions of each article.

**Chapter 1: Systematic Variation in Waste Site Effects on Residential Property Values: A Meta-Regression Analysis and Benefit Transfer** (published as Schütt, 2021)

This chapter examines the relationship between waste sites and nearby residential property values. The meta-analysis of 727 estimates from 83 hedonic pricing studies confirms that heavily contaminated waste sites negatively affect the value of nearby residential properties. For non-hazardous waste sites such as sanitary landfills and waste transfer stations, there is no support for such an effect on average. Correcting for publication bias is essential to arrive at an unbiased estimate of the average effect size. Publication bias is responsible for overestimating effect sizes in this literature by up to 38%. Depending on the method used to correct for publication bias, the adjusted average effect size translates into an increase in property value of between 1.5% and 2.9% per mile of increased distance from a waste site for a house one mile away.

In addition, the accumulated empirical evidence shows significant variation in effect size that can be explained by waste site and data characteristics, as well as the econometric specifications of the respective studies. In particular, the effect size is smaller for remediated sites and shows a distance decay, i.e., residential properties further away from hazardous waste sites are less affected. The confirmed distance decay for hazardous waste sites reconciles previous inconsistent findings in the literature. In addition, the

omission of relevant socio-economic control variables in the econometric specification of the primary studies leads to an upward bias in the reported estimates. Similarly, studies that include distance interaction terms report significantly smaller average effect sizes.

### **Chapter 2: Wind Turbines and Property Values: A Meta-Regression Analysis**

The second chapter aims to answer whether and under what conditions wind turbines affect nearby residential property values. Without a consensus in the empirical hedonic literature estimating this price-distance relationship, this first comprehensive meta-analysis of this literature combines 720 estimates from 25 studies to address this ambiguity. After correcting for publication and misspecification bias, the resulting effect size is equivalent to an average reduction in property values of 0.68%, for properties 1.89 miles away, which falls to zero beyond 2.8 miles.

The application of novel publication bias correction methods confirms a 25% overestimation of the effect due to selective reporting. These methods include in particular the recently developed RoBMA-PSMA framework (Bartoš et al., 2023), which simultaneously tests 36 competing publication bias correction models, weighting the result by the fit to the data using Bayesian model averaging. Bayesian model averaging techniques are also used to identify the primary study design elements responsible for the considerable variation in effect size estimates. Accordingly, studies can avoid misspecification bias by, for example, accurately calculating the distance between properties and turbines, using appropriate spatial controls, and relying on a difference-in-difference estimation design. Data characteristics, such as the distance of the properties from the wind turbines and the type of property price data analysed, may explain additional heterogeneity in effect sizes.

### **Chapter 3: How to Explain the Huge Differences in Rebound Estimates: A Meta-Regression Analysis of the Literature**

In the third chapter, co-authored with Anke Jacksohn, Tobias Möllney and Katrin Rehdanz, we review the literature on microeconomic rebound effects at the household level, i.e., the relative gap between potential and realised savings in resource use following efficiency improvements or sufficiency changes. In the first meta-analysis on this topic, we find factors that explain the widely varying empirical estimates and provide reliable information on the magnitude of rebound effects in different contexts. Based on a set of 43 primary studies with 1043 estimates, we document a total microeconomic rebound of about 36-43% on average. The variance can be explained by the different settings of the primary studies in terms of the type of data used, the setup of the rebound scenario and the assumptions used in estimating the rebound.

In particular, we show that rebound effects preceded by sufficiency-related behavioural changes are on average 24 percentage points higher than rebound effects following efficiency improvements. In addition, we find that rebound effects differ across consumption areas, e.g., measures in the transport area lead to rebound effects that are

on average 19 percentage points higher than in the residential energy area. We also confirm that the methodological choices in estimating of rebound effects as such has a strong influence on the estimated rebound effects. In particular, we find that studies that do not estimate substitution effects in addition to income effects, that estimate only indirect rebound effects, or that assume small direct rebound effects report smaller total rebound effects on average. In addition, we find only small absolute transfer errors, indicating good predictability of rebound effects using our meta-regression model. We thus provide guidance to policymakers who need reliable estimates of the effectiveness of resource-saving policies, taking into account both direct and indirect rebound effects.

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## Chapter 1

# Systematic Variation in Waste Site Effects on Residential Property Values: A Meta-Regression Analysis and Benefit Transfer

*Marvin Schütt*

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Data and code are available for replication via <https://doi.org/10.7910/DVN/828WUD>.

### **Abstract:**

This article presents a meta-analysis based on 727 estimates from 83 hedonic pricing studies to provide new insights on the effects of waste sites on residential property values. Relative to previous meta-analyses on this subject, estimates are corrected for publication bias and the ability of the meta-regression model to produce reliable benefit-transfer estimates is assessed. Proximity to severely contaminated waste sites has a supremely negative impact on residential property values, whereas on average the distance from non-hazardous waste sites has no effect. Correcting for publication bias has a sizeable impact, reducing the average effect size by up to 38%. Benefit-transfer errors based on the meta-regression model are fairly large and, in line with the broader literature, outperform simple value transfer when the underlying data sample is heterogeneous.

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## 1.1 Introduction

The world's annual generation of waste equalled two billion tonnes in 2016 and is expected to reach 3.4 billion tonnes by 2050 (Kaza et al., 2018). This poses serious threats to the environment in general and may generate externalities for residents living in close proximity to a waste site. These externalities include health risks, offensive smells, noises or unpleasant views (Giusti, 2009). However, well-administered waste sites may nullify these adverse effects, indeed they may even be perceived positively due to their employment potential. Accordingly, it is important to understand whether and under what circumstances waste sites significantly affect local residents.

The hedonic pricing framework pioneered by Rosen (1974) is a prominent method used to evaluate the effect of waste sites on property values.<sup>1</sup> Studies relying on this concept explain the price variation in residential properties as a combination of the value of their respective characteristics, e.g., distance from a waste site. Empirical evidence on this price-distance relationship, however, is ambiguous. The literature displays high variance in its assessment of the magnitude and significance of the effect as well as disagreement on its sign (see, e.g., Reichert et al., 1992; Du Preez et al., 2016; Poor et al., 2007 and Ready, 2010, for landfill effects). Accordingly, it remains unclear whether waste site externalities significantly affect local residents. In response to these open questions, I apply meta-analytic techniques (Stanley and Doucouliagos, 2012; Ringquist, 2013) to investigate the existence of adverse price effects on proximate residential property values at the aggregate level. The heterogeneity of the empirical results is discussed and explained with reference to the differences in methodological approaches and waste-site characteristics across the literature. Relative to previous meta-analyses on this subject (e.g., Braden et al., 2011; Simons and Saginor, 2006), estimates are corrected for publication bias and the ability of the meta-regression model to produce reliable benefit-transfer (BT) estimates is assessed. BT can be an especially valuable tool for policymakers in making predictions on effect sizes in areas where time, data, or money constraints do not permit primary studies (Johnston et al., 2015). The present meta-regression analysis (MRA) builds on a meta-sample of 727 observations from 83 studies, going beyond previous MRAs in this area by using 56 studies hitherto unconsidered. The large sample also enables me to add nine moderators to explore new factors explaining heterogeneity. In the framework of cost-benefit analyses, the MRA can support policymakers in making informed decisions on such things as the placement of new waste sites or clean-up activities for hazardous waste sites.

The results confirm that proximity to severely contaminated waste sites has a supremely negative impact on residential property values, whereas on average the distance from

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<sup>1</sup> There are other methods for eliciting this relationship. However, studies that use methods other than hedonic pricing (such as contingent valuation) are not considered here as they rely on different welfare measures, which is likely to lead to problems of incomparability in meta-analyses (Smith and Pattanayak, 2002).

non-hazardous waste sites has no effect. Correcting for publication bias has a sizeable impact, reducing the average effect size by up to 38%. The corrected average effect size translates into a 1.5% to 2.9% property value increase per mile of increased distance from a waste site for a house at the distance of one mile. Together with waste-site and study characteristics, the primary studies' econometric specifications are identified as important dimensions influencing effect size and explaining observed heterogeneity. In particular, the effect size is reduced for cleaned-up waste sites and for residential properties at greater distances from hazardous waste sites, thus reconciling inconsistent previous findings. BT errors based on the meta-regression model are fairly large and, in line with the broader literature, outperform simple value transfer when the underlying data sample is heterogeneous. While the acceptable level of transfer error is context-dependent (Rosenberger, 2015; Brander et al., 2006), the predicted levels of transfer error limit practical application accordingly.

The remainder of the paper is organised as follows: The next section presents an overview of results from previous MRAs on this subject, in addition illustrating the requirements for valid and reliable MRA and BT. Section 1.3 introduces the meta-dataset used in this paper. Section 1.4 demonstrates the selection of the appropriate model and publication bias control. The results are presented and discussed in Section 1.5. Section 1.6 concludes.

## 1.2 Literature Review

A meta-analysis is commonly described as a statistical analysis of previously reported research findings on a given empirical effect (Stanley and Doucouliagos, 2012). In contrast to qualitative reviews, meta-analytic methods can estimate average effect sizes, quantify the extent of variance observed and help explain heterogeneous results (Borenstein et al., 2011). Advances in meta-analytic methodology and the attendant guidelines (Nelson and Kennedy, 2009; Stanley et al., 2013; Nelson, 2015; Johnston et al., 2018; Stanley and Doucouliagos, 2012) have made current meta-analyses more reliable and useful for both academic and practical purposes. More precisely, meta-analyses now typically rely on large meta-sample sizes and substantial sets of moderators to explain heterogeneous results. They control for publication bias and assess the usefulness of meta-regression results for BT applications. Unsurprisingly, meta-analytic tools have enjoyed increasing popularity in economic research over the past decade (Alinaghi and Reed, 2018). Within the literature on environmental valuation, recent applications synthesise the empirical evidence pertaining to such things as water-quality improvements (Johnston et al., 2017; Klemick et al., 2018), river restoration (Chen et al., 2019; Brouwer and Sheremet, 2017), wetland values (Vedogbeton and Johnston, 2020; Chaikumbung et al., 2016) or flood risk (Beltrán et al., 2018).

Within the strand of literature estimating price-distance relationships between waste sites and residential properties, several studies have already analysed and summarised the empirical evidence in a systematic fashion. Table A.1 lists the most relevant MRAs, highlighting their findings and main study attributes. The brief discussion in the following section presents the estimated average effect sizes including identified moderators and the type of waste site considered. It also indicates potential limitations. For the MRAs closest to this study (Simons and Saginor, 2006; Lipscomb et al., 2013; Braden et al., 2011) I provide a more detailed overview.

### 1.2.1 Previous reviews and meta-analyses

The first reviews in this area (Farber, 1998; Zeiss, 1998; M. A. Boyle and Kiel, 2001; Jackson, 2001, and also Brinkley and Leach, 2019) were qualitative and aimed to identify moderators explaining the apparent heterogeneity of waste site-related property-price effects (e.g., mean distance from waste site in the primary study, employment opportunities at the waste site, type of waste considered). Later studies confirmed the relevance of some of these research dimensions and added other moderating variables in initial meta-analyses on the topic. They identified the functional form employed, the type of waste site examined and the mean distance from the waste sites as influential research dimensions (Walton et al., 2006; Chèze, 2007; Ready, 2010). In one of these initial meta-analyses, Ready (2010) reports an average increase in residential property values of 1.3% to 5.9% per mile of increased distance from a landfill depending on the size of the latter. By contrast, Walton et al. (2006) record an average price premium of 6.7% per mile of increased distance for a different set of landfill studies. Chèze (2007) reports an average discount of 3.8% (8.4%) for living one mile closer to non-hazardous (hazardous) landfills or incinerators. Although they contain important initial insights, all of these studies are restricted in value by low sample sizes. Walton et al. (2006) cover 17 estimates from seven studies; Ready (2010) considers 15 estimates from nine landfill studies; Chèze (2007) discusses 12 studies with 45 estimates. This drawback has motivated further research to validate the findings.

One of the largest meta-analyses in this area of research is Simons and Saginor (2006), with 290 observations from 75 articles, 42 of them with 164 observations using the hedonic pricing method. Their meta-dataset comprises studies dealing not only with contaminated waste sites but also with amenities, reliance on surveys, case studies or hedonic regression techniques. They report a 4% mean increase in property value for each mile of increased distance from the respective site (9.5% for studies focusing exclusively on a disamenity). Across specifications, they confirm that the geographic region, the mean distance from the waste site, the type of waste and the announcement of the closure of a site are important moderating dimensions. Although they acknowledge the importance of publication bias, the potential dependence of multiple observations

from the same study and differing levels of precision in the estimates, they do not explicitly accommodate these factors in their meta-regressions.

More recently, Lipscomb et al. (2013) have drawn upon 40 studies with 273 observations in their meta-analysis, about 227 of them from 33 articles using the hedonic pricing method (see Table A.1 for details). These observations differ not only in the type of disamenity (landfills, hazardous waste sites, power lines, railroad tracks, etc.) or amenity (proximity to water bodies, view, etc.) discussed but also in the methods employed (hedonic regression, travel cost method, contingent valuation, etc.). More than half of the observations are not concerned with price-distance relationships between waste sites and residential properties as the focus here is on an attempt to detect differences in valuation through the choice of elicitation methods. Hence, it deviates from the focus of the present study and will not be discussed any further.<sup>2</sup>

The study by Braden et al. (2011) is closest in spirit to this meta-analysis. They consider 46 hedonic studies with 129 estimates from various types of waste site, 114 of them from 38 studies of waste-site effects on residential property values. They use weighted least squares (WLS) and ordinary least squares (OLS) techniques with a large set of moderating regressors some of which are new to the relevant literature. They find that a one-mile increase in distance from a terrestrial (aquatic) hazardous waste site leads to an average increase in property values of 3.5% (15.9%). Nuclear and non-hazardous waste sites do not significantly influence property values, with the effects of non-hazardous waste sites being greater in magnitude (3.1% compared to -0.4%). Some of the newly added moderators help to significantly explain the variation in the data. These include control variables indicating the addition of socio-demographic variables in the primary studies' regressions, the use of sales data instead of assessed values and listing on the National Priority List (NPL).<sup>3</sup> To the surprise of the authors, remediating contaminated sites and the mean distance of properties from the waste site do not consistently influence effect sizes for hazardous and non-hazardous waste sites, thus contradicting in the latter case the "fundamental premise of hedonic property valuation of environmental quality" (Braden et al., 2011, p. 198). One possible explanation for these surprising results may be that they consider observations from discrete and continuous distance specifications simultaneously, also encompassing linear and quadratic distance specifications. These differences in distance definition may lead to incomparability of effect sizes. In addition, the distance-decay effect may only be detectable for obvious disamenities like hazardous waste sites. I discuss both aspects in more detail in my

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<sup>2</sup> There exist two additional studies that are close to this MRA. Both differ in some respects from the focus chosen here, so this review does not consider them in detail. Kiel and Williams (2007) use a probit regression meta-analysis to model the likelihood of being listed as a superfund site and hence do not focus on average effect sizes of superfunds. Saginor et al. (2011) synthesise the empirical evidence of environmental contamination effects on *non*-residential property values.

<sup>3</sup> The most severely contaminated waste sites in the USA are listed on the NPL. These sites have remediation priority (Environmental Protection Agency, 2019).

remarks on the study selection process in Section 1.3 and in the results section. Despite their methodological improvements over previous meta-analyses, Braden et al. (2011) do not correct for publication bias in their model.<sup>4</sup>

The meta-analyses reviewed provide valuable insights into the likely range of the average effect size and into heterogeneity aspects reflected in e.g., the type of waste site considered. However, none of these studies assess their meta-regression results for usage in BT applications or discuss validity and reliability requirements.<sup>5</sup> Similarly, the omission of publication bias controls and the (partly) small sample sizes impose restrictions on their explanatory power. In the present study I emphasise its inherent value for BT, which is also reflected in the study selection criteria. Additionally, I address the potential presence of publication bias in the development of the econometric specification. Further, a large meta-sample in combination with several sets of moderators enable me to assess the robustness of results and conduct subsample analyses. The next section discusses the requirements for consistent and reliable MRA and accurate BT.

### 1.2.2 Validity and reliability requirements

BTs use existing effect-size estimates from one or more previous studies to infer the effect size for a new policy application (K. J. Boyle et al., 2013). In principle, BT based on MRA is a form of function transfer, as the meta-equation can be calibrated to fit the new context (K. J. Boyle and Wooldridge, 2018). Importantly, BT can be a valuable tool in making predictions on effect sizes in areas where time, data or money constraints make primary studies impracticable (Johnston et al., 2015). Consequently, BTs based on MRAs have increasingly been applied in the context of non-market values in recent years, especially for applied cost-benefit analyses (Vedogbeton and Johnston, 2020). At the same time, many challenges remain unresolved, casting doubt on the validity and reliability of BT applications under certain conditions (Johnston et al., 2018; Rosenberger, 2015; Vedogbeton and Johnston, 2020). Arguably, one of the main requirements for a valid combination of studies on both MRA and BT is a “minimal degree of commodity consistency across metadata observations” (Vedogbeton and Johnston, 2020, p. 836). However, the pooling of observations from different studies with different attributes lies at the heart of any MRA. Hence, while commodity

<sup>4</sup> However, they include dummy variables to control for the effect of publication and significance status on the magnitude of the effect size. Whether this serves as a viable approximation is not clear.

<sup>5</sup> There exists, however, some anecdotal evidence. Walton et al. (2006) provide preliminary insights into the potential of BT for landfills but themselves question the validity of this due to the small sample size. Eshet et al. (2007a) assess transferability of results from four individual hedonic estimates of waste-transfer station effects from Eshet et al. (2007b). Again, the small number of estimates restricts external validity. Finally, Braden et al. (2010) use the MRAs from Braden et al. (2011) for BT applications to the Great Lakes Areas of Concern. Due to a different focus in the underlying meta-model, only one variable in the meta-function can be calibrated to distinguish between sites. This limited capacity for differentiating between sites prompts the authors to advise against using their results for out-of-sample predictions.

consistency is a commonly acknowledged requirement, a too narrow definition of the commodity under consideration can drastically reduce sample size and impose considerable restrictions on statistical analysis (Chaikumbung et al., 2016; Bergstrom and Taylor, 2006; Vedogbeton and Johnston, 2020). Alongside commodity consistency, welfare-measure consistency and outcome-variable consistency are often called for (Nelson and Kennedy, 2009; Vedogbeton and Johnston, 2020; Klemick et al., 2018, see also Rosenberger, 2015, for a detailed overview).

The trade-off between consistency and sample size is clearly reflected in the meta-analyses reviewed here. One part of the literature (Walton et al., 2006; Chèze, 2007; Ready, 2010) has synthesised very consistent sets of studies but is limited by small sample sizes restricting a detailed investigation of factors explaining heterogeneity. Other meta-analyses (Simons and Saginor, 2006; Braden et al., 2011; Lipscomb et al., 2013) have opted for larger sample sizes and aim to control for greater heterogeneity in their MRA via moderators. However, this happens at the expense of commodity consistency (waste sites combined with power lines or parks, etc.), welfare consistency (hedonic pricing studies combined with studies using contingent valuation or travel cost methods, etc.) or outcome consistency (effects on residential property values combined with effects on non-residential property values) of the pooled observations (see Table A.1 for details). In line with the last-named studies in this part of the literature, I argue that some diversity in waste sites is needed for insightful MRA (Vedogbeton and Johnston, 2020; Nelson, 2015). However, as the results of this MRA will also be assessed for BT, I aim for a higher degree of consistency (Bergstrom and Taylor, 2006; Rosenberger, 2015; Smith and Pattanayak, 2002). Hence, outcome validity and welfare validity are consistently upheld, only allowing the inclusion of observations from hedonic pricing studies reporting price-distance relationships for waste sites and residential properties. Similarly, waste sites are the only commodity allowed. The operative definition of waste here is “any substance or object which the holder discards or intends or is required to discard” (European Commission, 2008, Article 3). This definition encompasses different types of waste (e.g., hazardous or non-hazardous), disposed of at different facilities (e.g., landfills or incinerators) and affecting different elements (e.g., soil, air or water), thus allowing for a detailed analysis. The study selection criteria introduced in the next section are designed to meet these minimal consistency criteria.

Two observations from the literature motivate the procedure I have chosen to calculate BT errors. First, K. J. Boyle and Wooldridge (2018) emphasise that there is no single meta-analytic model necessarily appropriate for both purposes, i.e., explaining heterogeneity and providing low-error BT estimates. Thus, we can hardly expect any preferred model with major explanatory power to perform unusually well in terms of transfer error as well (Nelson, 2015). However, smaller transfer error-rates may be expectable for subsets of observations that have an even higher degree of commodity consistency (Eshet et al., 2007a), e.g., sharing the severity of pollution. To assess this eventuality, I thus

calculate transfer errors based on both the entire meta-dataset and on subsets of more homogeneous studies. This approach also addresses commodity consistency concerns independent of the study-selection criteria (Nelson, 2015; Chaikumbung et al., 2016). Second, though it is generally assumed that in terms of transfer error MRA perform better than value transfer (Rosenberger, 2015), some studies have shown that this is not necessarily the case (Lindhjem and Navrud, 2008; Klemick et al., 2018; Johnston et al., 2018). Intuitively, BT based on MRA is expected to be beneficial in the context of dissimilar sites (Bergstrom and Taylor, 2006; Bateman et al., 2011; Johnston et al., 2015), but counterexamples do exist (Rosenberger, 2015). Hence, I also calculate simple value transfer errors for both the entire meta-sample and subsets. This approach is conducive to insights in the way transfer errors depend on the degree of commodity consistency associated with the sample and transfer method chosen in this part of the literature.

## 1.3 Meta-Dataset

### 1.3.1 Selection of studies

The strategy employed for identifying relevant studies followed the MEAR-Net guidelines<sup>6</sup> for conducting and reporting meta-analyses (Stanley et al., 2013) and involved three steps. First, seven search engines suitable for the complexity of a predefined search query were used to identify initial records.<sup>7</sup> The search query was a combination of synonyms for residential property values, HPM and waste sites (see Table A.2 in the appendix for a full list). The search resulted in 2,000 initial records. Subsequently, I screened these initial records for eligibility, discarding all spuriously detected studies. Second, for all eligible studies the respective reference lists were checked for additional suitable material. Third, for the resulting record, I searched four previously unused databases (sciencedirect, JSTOR, EVRI and Google scholar) manually for studies citing the studies already identified. The second and third steps were repeated until no other relevant studies were found. At the end of this process, 325 studies were included in a preliminary meta-sample (see Figure A.1 in the appendix for a detailed PRISMA statement). I started the search in April 2018 and finished in December 2018, using the reference management software Citavi (version 5.7) to list the records identified.

For inclusion, the studies had to comply with the following criteria: (i) use of the basic hedonic pricing method, (ii) price of residential properties as dependent variable, (iii) distance from a waste site as independent variable, and (iv) report of all necessary

<sup>6</sup> In accordance with the guidelines referred to in Stanley et al. (2013), a second coder separately coded "a substantial proportion" of the final dataset, i.e., 32 studies. Reassuringly, coding ambiguities were attributable to the varying levels of reporting detail in the primary studies and were reconciled. Coding decisions in the face of ambiguities are commented on in the meta-dataset available in the data repository linked to in the online version of this article. Remaining ambiguity is indicated in the categorisation of moderator variables as 'unclear'.

<sup>7</sup> These were EBSCO (including EconLit), ISI Web of Science, RePEc, opengrey.eu, science.gov, worldwidescience.org and wiso-net.de.



information for standardising the respective regression coefficient and its measure of precision. These restrictions ensure that the studies included measure a common effect - fulfilling commodity, welfare and outcome consistency. Despite these study selection criteria, the studies included differ in terms of the waste-related activity they value, e.g., incineration, landfill, smelter. As discussed in the previous section, some degree of commodity diversity is inevitable and also necessary for meaningful statistical analysis. However, one might argue that these differences are sufficient to prohibit the combination and ultimately the meta-analysis of these different types of observation.<sup>8</sup> In this study, I prefer to synthesise the studies to explore the effect of heterogeneous types of waste site by means of appropriate control variables. This is in line with (and indeed rather conservative compared to) previous MRAs in this area of research (Simons and Saginor, 2006; Lipscomb et al., 2013) but is also in accordance with other MRAs in the field of environmental valuation (Chaikumbung et al., 2016) and with the recommendations of Stanley and Doucouliagos (2012). Further, my hypothesis is that it is not the activity conducted at the waste site that determines the (assumed) disamenity effect. Instead, I conjecture that the potential undesirability of waste sites stems from the type of resultant pollution. More precisely, I consider the hazardousness of the waste site and the element affected (soil, water, air) to be a set of attributes causing the effect of waste sites on residential property values to differ. Importantly, these factors cannot be ascribed to one particular type of waste site alone, i.e., hazardous waste can be both landfilled or incinerated, thus affecting soil or air (see, e.g., Affuso et al., 2010; Zegarac and Muir, 1998). Ultimately, this approach conserves the sample size and enables me to examine a wide spectrum of waste-site effects. However, as already indicated, subsample analyses are provided to assess the sensitivity of this decision. More formally, the study selection criteria ensure that studies included in the meta-sample report results from a variant of the following stylized hedonic pricing specification:

$$P = \alpha_0 + \beta_1 * DIST + \sum_{n=2}^N \gamma_n * X_n + u \quad (1.1)$$

with  $P$  being the residential property value,  $DIST$  the distance from a waste site,  $X_n$  a set of control variables (with  $\beta_1$  being the estimated coefficient of interest) and  $u$  a common error term. Consequently, several sets of studies included in the preliminary dataset of 325 studies had to be discarded. Table A.3 lists these studies with the reasons for exclusion.

Two sets of excluded studies are discussed in more detail to emphasise the importance of comparable estimates in the meta-sample. First, studies that use quadratic distance specifications were eliminated because of the absence of information on the estimates'

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<sup>8</sup> I would like to thank one anonymous reviewer for pointing to this important aspect.

precision. Given the stylised hedonic pricing specification,

$$P = \alpha_0 + \beta_1 * DIST + \beta_2 * DIST^2 + \sum_{n=3}^N \gamma_n * X_n + u \quad (1.2)$$

with all variables defined as above, the marginal effect of distance is given by

$$\frac{\partial P}{\partial DIST} = \beta_1 + 2 * \beta_2 * DIST \quad (1.3)$$

While it is frequently possible to calculate the marginal effect from the information given in the studies, the information required to calculate its standard error is usually not provided (in particular, the covariance between the linear and quadratic term is never reported). Gunby et al. (2017) argue convincingly that it is incorrect to include either of the coefficients individually because they represent incomplete information about the marginal effect of distance on the price of properties. Thus, nine studies were discarded. Second, I have excluded 40 studies using discrete distance specifications, i.e., defining the location of a house to be inside or outside a certain radius. Although this is the largest set of excluded studies, omission is unavoidable. First, the respective radius is defined differently across studies, so pooling studies with discrete distance definitions for meta-analytic purposes is only possible if the different measurement units can be matched. This would entail assumptions on the distribution of houses around the respective waste site and on the nature of the distance-decay effect, e.g., assuming that the effect vanishes linearly over distance and that houses are evenly distributed in concentric circles around the waste site (Debrezion et al., 2007). These assumptions would necessarily introduce measurement error unknown both in extent and in nature.<sup>9</sup> Second, discrete distance definitions cannot be matched with continuous measures (Ready, 2010) as a continuous effect size like a percentage increase in property prices per mile cannot be aligned with a dummy variable indicating, say, the value reduction of a house situated within a certain radius around a waste site compared to a house outside this radius. Hence, in contrast to Braden et al. (2011), I consider studies with discrete distance specifications to lead to mutually incomparable estimates and exclude them. As a result, the final dataset consists of 83 studies with 727 observations covering 13 countries and spanning approximately 40 years.<sup>10</sup> For a full list of the studies included, see Table A.4 in the appendix. To facilitate the overview, Table A.5 summarises the basic characteristics of the studies included.

<sup>9</sup> One reviewer recommended running a separate regression for this set of excluded studies. I agree that it would help strengthen my argument of incomparability and for exclusion. However, in the light of the necessary assumptions needed to convert the respective study estimates into a common effect size, I prefer not to combine these types of study in a separate MRA.

<sup>10</sup> Three observations generally met the inclusion criteria but were still discarded because the reported effect sizes far exceeded reasonable levels, which hints at typing errors or the like. These were two observations from Bilbao-Terol (2009) and the only observation from Li et al. (2015). They reported price-distance elasticities of 92 and 81 (Bilbao-Terol, 2009) and 521 (Li et al., 2015). Leaving these observations aside reduces the simple mean of the effect size from 0.991 to 0.042. Attempts to get an explanation for these seemingly bizarre results were unsuccessful.

The final dataset consists of studies with various model specifications. Accordingly, the standardisation of the respective regression coefficients to a common metric is required to reconcile different distance variable definitions across studies (e.g., feet or kilometres), the functional form used (e.g., linear or logarithmic) and the estimation strategy (e.g., OLS or spatial autocorrelation) (Nelson and Kennedy, 2009). In this meta-study, the common effect size selected is the distance elasticity of residential property prices, or more formally:

$$elasticity = \frac{\partial \log P}{\partial \log DIST} = \frac{\partial P}{\partial DIST} * \frac{DIST}{P} \quad (1.4)$$

If original estimates differ from this common effect size, they are consistently converted, see Table A.6 in the appendix for a detailed description. Elasticities resulting from such conversion are evaluated at the mean of the respective distance and price variables, if applicable. Similarly, the standardisation process for the measures of statistical precision in the original estimates (standard error, t-value or p-value) is summarised in Figure A.2 in the appendix. The effect size can be interpreted as the percentage change in the price of a residential property in response to a one-percent increase in its distance from a waste site and serves as the dependent variable in this meta-analysis. Given the results from the previous literature, the elasticity is expected to be positive, *viz.*, greater distance from a waste site is expected to be beneficial for residential property values.

### 1.3.2 Selection of moderators

The moderators, their respective definitions and descriptive statistics are summarised in Table 1.1. The selection of moderators is undertaken on the basis of previous findings as discussed in the literature review.<sup>11</sup> The asterisks (\*) indicate previously unconsidered moderators reflecting additional methodological particularities in the primary hedonic pricing studies. In general, the moderators can be grouped into three categories reflecting site characteristics, data characteristics and researcher decisions on methodology.

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<sup>11</sup> In some cases I adopt the naming conventions used by Braden et al. (2011) as I consider them informative and intuitive. I hope that this enhances the comparability of results.

**Table 1.1:** Summary statistics of moderators.

Moderator	Definition	Mean	SD	# observations [studies]
SE*	Standard error of effect size	0.07	0.12	727 [83]
Published	= 1 if study is published in a journal, 0 else	0.78	0.41	727 [83]
Year publish	Year the study is published	2006	7.74	727 [83]
<b>Site characteristics</b>				
Site m	= 1 if multiple sites were present and considered, 0 else	0.39	0.49	727 [83]
HDI*°	HDI-index for the country of the waste site in the year of the sampled data at the subnational level if available, on national level else	0.87	0.04	369 [70]
GDP*°	GDP p.c. in 2010 USD for the country of the waste site in the year of sampled data	38308	10022	469 [81]
<i>NPL</i>				
Not on NPL	= 1 if waste site is not on the NPL, 0 else	0.56	0.50	404 [45]
On NPL	= 1 if waste site is on the NPL, 0 else	0.25	0.44	184 [22]
Not USA	= 1 if waste site is not in the USA and hence not eligible for the NPL, 0 else	0.17	0.37	121 [26]
Unclear	= 1 if status on the NPL is unclear, 0 else	0.02	0.16	18 [4]
<i>Activity status</i>				
Inactive	= 1 if waste site is inactive, 0 else	0.68	0.47	494 [49]
Active	= 1 if waste site is active, 0 else	0.24	0.43	175 [45]
Unclear	= 1 if activity status is unclear, 0 else	0.08	0.27	58 [7]
<i>Employment opportunities</i>				
No employment opportunities	=1 if the waste does not offer employment, 0 else	0.31	0.46	226 [44]
Employment opportunities	=1 if the waste offers employment, 0 else	0.25	0.43	180 [41]
Unclear	=1 if employment at the waste site is unclear, 0 else	0.44	0.50	321 [7]
<i>Type of waste site</i>				
Hazardous	= 1 if waste is hazardous, 0 else	0.79	0.41	571 [54]
Non-hazardous	= 1 if waste is non-hazardous, 0 else	0.17	0.38	125 [30]
Nuclear	= 1 if waste is nuclear, 0 else	0.04	0.20	31 [6]
<i>Waste-affected element</i>				

**Table 1.1:** (continued).

Moderator	Definition	Mean	SD	# observations [studies]
Soil	= 1 if the soil is the waste-affected element, 0 else	0.58	0.49	421 [40]
Air	= 1 if the air is the waste-affected element, 0 else	0.13	0.34	98 [22]
Water	=1 if a water body is the waste-affected element, 0 else	0.11	0.31	79 [11]
Unclear	=1 if the waste-affected element is unclear, 0 else	0.18	0.38	129 [22]
<i>Continent*</i>				
North America	= 1 if waste site is in North America, 0 else	0.85	0.35	620 [61]
Asia	= 1 if waste site is in Asia, 0 else	0.08	0.27	59 [9]
Europe	= 1 if waste site is in Europe, 0 else	0.06	0.23	41 [10]
Other	= 1 if waste site is on another continent (Africa, Australia), 0 else	0.01	0.10	7 [3]
<i>Clean-up stage</i>				
Not recognised	= 1 if waste site is not yet recognized as polluted, 0 else	0.11	0.31	78 [13]
Recognised, no clean-up plan exists	= 1 if waste site is recognized as polluted but no remediation plan exists, 0 else	0.37	0.48	267 [31]
Clean-up begun, but not finished	= 1 If remediation plan for polluted waste site exists, but waste site is not remediated yet, 0 else	0.16	0.37	115 [20]
Cleaned-up	=1 if polluted waste site is remediated, 0 else	0.06	0.23	42 [11]
Unclear	= 1 if clean-up stage is nuclear, 0 else	0.14	0.34	100 [21]
<b>Data characteristics</b>				
Dist greater mean*	=1 if mean distance from the waste site is greater than the mean distance in the sample (4.29 miles), 0 else	0.40	0.49	727 [83]
Dist mean°	Mean distance from the waste site in the sample (in miles)	4.29	7.56	548 [82]
Miles km*	= 1 if the unit of distance is in miles or kilometres, 0 else	0.68	0.47	727 [83]
Sample	Sample size of the study (in 1000s)	6,02	19,95	727 [83]
Sale ind	= 1 if price is sales price on individual house level, 0 else	0.84	0.37	727 [83]
<b>Methodology: Econometric specification</b>				
Num sig var*°	Number of significant variables used (p<0.05)	15.10	9.44	396 [76]
Num expl	Number of explanatory variables used	21.76	14.19	727 [83]

**Table 1.1:** (continued).

Moderator	Definition	Mean	SD	# observations [studies]
Oth disamen	= 1 if other disamenities are present and considered, 0 else	0.29	0.45	727 [83]
Oth amen	= 1 if other amenities are present and considered, 0 else	0.29	0.45	727 [83]
Access	= 1 if accessibility options (highway, distance to CBD) are present and considered	0.46	0.50	727 [83]
Industry	= 1 if industrial activity is present and considered, 0 else	0.12	0.33	727 [83]
Demoecon	= 1 if socio-economic control variables are included, 0 else	0.75	0.43	727 [83]
Time control*	= 1 if a time control is included (e.g., dummy variable, inflation adjustment), 0 else	0.78	0.41	727 [83]
Direction*	= 1 if a direction-related variable is controlled for (prevailing wind / compass direction), 0 else	0.07	0.25	727 [83]
Interaction*	= 1 if any interaction terms with the distance variable exist, 0 else	0.10	0.30	727 [83]
<b>Methodology: Estimation strategy</b>				
Log log	= 1 if regression is of double-logarithmic type, 0 else	0.63	0.48	727 [83]
OLS spatial	= 1 if OLS or a spatial model specification is used, 0 else	0.97	0.17	727 [83]

*Notes:* The asterisk (\*) denotes moderators that have not been used in this context before. The circle (°) indicates moderators with missing information inducing a reduced number of observations. The categorical variables have mutually exclusive levels, e.g., ‘not on NPL’ signifies that the NPL status is clear and that the waste site is located in the USA. The number of studies within a category of moderators sometimes exceeds the number of studies in this meta-study. This reflects the strategy of some studies analysing a waste site, e.g., at different points in time with varying clean-up stages.

Considering site characteristics serves to distinguish the effects of different types of waste site, i.e., the severity of contamination, the element affected<sup>12</sup>, the activity status of the site or the continent on which the site is located. Similarly, the number of proximate waste sites and the clean-up stage of hazardous waste sites may influence the estimated effect size. Hazardous waste sites, for example, are expected to have more adverse effects on property values than non-hazardous sites due to their greater expected impact on people's health in their vicinity. Additionally, multiple waste sites are presumably related to greater effect sizes than the single-site case due to the greater likelihood of noise or offensive smells.

Data characteristics reflect some particularities of the respective sample of residential property values, such as mean distance from the waste site, sample size and whether the properties in question were sold rather than assessed. Only a subset of all observations reports the mean distance of houses from a waste site in the respective sample. Two newly introduced moderators serve as potential alternatives. First, a dichotomising moderator indicates studies with reported sample mean distances greater than the mean distance in this meta-sample (4.29 miles). Second, a dummy variable signals whether the definition of the distance variable is in miles or kilometres as opposed to, say, feet or metres. I presume that studies defining their distance variable in miles or kilometres will report smaller estimates than studies with, say, feet or metres as the chosen metric. Full information on both alternatives is available. All else being equal, greater mean distances can be expected to lead to smaller effect sizes. The choice of these dummy moderators is designed to shed more light on the surprising absence of a distance-decay effect in the findings by Braden et al. (2011), as discussed in Section 1.2.

Unique methodological approaches in estimation strategy or econometric specification are also suspected of systematically influencing results. Although there is no fixed set of prescribed variables for inclusion in a hedonic regression, estimates from regressions with a very small set of control variables are likely to suffer from misspecification bias. Consequently, the reported effect size may also be biased (Wooldridge, 2010; Phaneuf and Requate, 2017). As a response to this hazard, I include moderators that control for the number and type of explanatory variables. In the same vein, uncommon functional forms or model specifications such as Box-Cox transformations or inverse distance specifications may influence the effect size. Two control variables address these eventualities.

Finally, three moderators are included that do not belong to the categories referred to earlier. First, a dummy variable for the publication year controls for time-trend effects. Two additional moderators aim to address publication bias, potentially influencing

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<sup>12</sup> I collected the information for the affected element from the primary studies and the US EPA, where applicable (see <https://cumulis.epa.gov/superpad/cursites/srchsites.cfm>). If a site affected more than one element and the main affected element could not be inferred from these sources, the coding was based on the judgements of two separate coders. In ambiguous cases the element was coded as 'unclear'.

the effect sizes assembled. Publication bias occurs when the selection of results by a researcher or the selection of studies by a journal are dictated by statistical significance or theoretical expectations (Stanley and Doucouliagos, 2012). Accordingly, if publication bias is present, there will be larger and more significant findings in the accessible literature that do not reflect the true population parameter (Card and Little, 2016). Hence, there is good reason to believe that the standard error of the effect size is positively correlated with the effect size, which leads to its inclusion as a moderating variable. Additionally, the peer-review process itself may introduce changes in the set and composition of reported findings and this may also affect the effect size. Hence, a dummy moderator indicates estimates from studies published in peer-reviewed journals to control for this possibility. In Section 1.4 the potential presence of publication bias is reflected in the development of the econometric specification.

### 1.3.3 Summary statistics

The distribution of the effect size and its dependence on the conditions prevailing in the respective studies are of primary interest in this study. Figure A.3 and Table 1.2 illustrate these two aspects. In Figure A.3 the effect size is depicted in the form of a frequency distribution. With 194 of 727 estimates being negative, there is a tendency towards positive elasticity values in the meta-sample. However, approximately 70% of the estimates are between -0.1 and 0.1. This clearly indicates that the majority of estimated elasticities are clustered around zero. With only four observations greater than 1 in absolute terms, the price-distance relationship under investigation can be summarised as inelastic in almost all cases. In addition to the overall distribution of the effect size shown in Figure A.3, a more nuanced picture may provide an initial impression of the heterogeneity observed. For this purpose, the summary statistics of the effect size are illustrated in Table 1.2, where the unweighted mean, fifth and 95<sup>th</sup> percentiles are displayed for the whole sample and several sets of subsamples defined by selected site characteristics.

Table 1.2 indicates that the mean effect size is positive for the whole sample and in most of the subsamples. The magnitude of the effect lies in the range of the results of the MRAs discussed earlier. Bearing in mind the definition of the effect size as an elasticity, the unweighted mean effect size for a house one mile away from a waste site is a 4.2% increase in property value per mile increase in distance. Turning to the percentiles reveals major disparities in the observations. Observations at the fifth percentile are generally negative, whereas observations at the 95<sup>th</sup> percentile have consistently positive effect sizes. Moreover, the effect size from observations at the 95<sup>th</sup> percentile is approximately four to ten times larger than the mean, depending on the subsample considered. Comparing the effect size by study characteristics provides additional preliminary insights. Published studies apparently show a higher mean



**Table 1.2:** Distribution of effect size by selected study characteristics.

Study characteristics	# observations [studies]	Mean	P5	P95
Unweighted mean	727 [83]	0.042	-0.183	0.327
<i>Type of waste site</i>				
Hazardous	571 [56]	0.042	-0.177	0.327
Non-hazardous	125 [28]	0.038	-0.135	0.206
Nuclear	31 [6]	0.055	-0.343	0.404
<i>Waste-affected element</i>				
Soil	421 [40]	0.036	-0.295	0.387
Air	98 [22]	0.079	-0.004	0.269
Water	79 [11]	0.048	-0.136	0.318
Unclear	129 [22]	0.031	-0.011	0.108
<i>Continent</i>				
North America	620 [61]	0.039	-0.186	0.351
Asia	59 [9]	0.046	-0.295	0.244
Europe	41 [10]	0.075	-0.082	0.265
Other	7 [3]	0.071	0.030	0.084
<i>Clean-up stage</i>				
Not recognised	78 [12]	0.072	-0.140	0.429
Recognised, no clean-up plan exists	267 [31]	0.060	-0.203	0.404
Clean-up begun, but not finished	115 [20]	0.024	-0.267	0.236
Cleaned-up	42 [11]	-0.044	-0.387	0.115
Unclear	100 [21]	0.033	-0.018	0.132
<i>Publication status</i>				
Published	567 [69]	0.049	-0.190	0.366
Not published	160 [14]	0.016	-0.138	0.157

*Notes:* The number of studies within a category of moderators sometimes exceeds the number of studies in this meta-study. This reflects the strategy of some studies analysing a waste site, e.g., at different points in time with varying clean-up stages.

effect size than their unpublished counterparts. Cleaning up a contaminated site would seem to be beneficial for residential property values.

The purpose of inspecting summary statistics is to explore the data and to identify tendencies rather than to draw inferences. As Stanley and Doucouliagos (2012) emphasise, simple average effect sizes (weighted or unweighted) are distorted in the presence of publication bias because in that case the meta-sample would not be drawn at random from the underlying population. Additionally, using simple averages implicitly assumes that all observations are treated equally and ignores the potential interdependence of multiple observations per study, heterogeneity across studies and differences in statistical precision. These potential limitations motivate the choice of the meta-analytic model(s) in the following section.

## 1.4 Methodology

The choice of the appropriate meta-analytic model is a point of ongoing discussion in the literature (Nelson and Kennedy, 2009; Stanley and Doucouliagos, 2012; Ringquist, 2013). The core of the debate revolves around the best identification of, and correction for, publication bias and the justification for either random- or fixed-effects models (Stanley and Doucouliagos, 2017; Alinaghi and Reed, 2018). Here, I draw upon the variety of meta-analytic models and consider them an opportunity for ample robustness checks on the results. Accordingly, I begin with a brief overview of meta-model candidates discussed in the literature, and continue with an assessment of publication bias in the meta-dataset. The overview concentrates on the controversies related to fixed- and random-effects models, interdependence of observations and heteroscedastic error terms. It follows the decision pathways presented in more detail by Feld and Heckemeyer (2011) and by Stanley and Doucouliagos (2012).

### 1.4.1 Choice of the meta-analytic model

Multivariate meta-analytic models including moderating variables have become a standard framework to help explain the very likely presence of heterogeneity in effect sizes in applied economic research (Stanley and Doucouliagos, 2012; Ringquist, 2013).<sup>13</sup> Accordingly, I adopt a general multivariate model framework as a starting point, i.e.,

$$elasticity_i = \alpha_0 + \sum_{k=1}^K \alpha_k * C_{k,i} + \epsilon_i \quad i = 1, 2, \dots, M, \quad (1.5)$$

with elasticity being the standardised effect size,  $C_{k,i}$  representing the  $k^{th}$  study characteristic attributed to estimate  $i$  and  $\epsilon_i$  a random error term with  $\epsilon_i \sim N(0, \sigma_i^2)$ . Here,  $\alpha_0$  is an estimate of the genuine mean effect size conditional on the set of controls  $C_{k,i}$ , i.e., an estimate of the magnitude and significance of the price effect of waste-site proximity on residential properties. This type of model is commonly referred to as the fixed effects model (Ringquist, 2013).<sup>14</sup> It crucially assumes that any deviation from the mean that is not explained by the moderator variables is entirely random due to sampling error (Feld and Heckemeyer, 2011). By contrast, the random (also known as mixed) effects model introduces a second error term allowing for unobserved heterogeneity across observations, i.e.,

<sup>13</sup> Simple meta-analytic models omitting heterogeneity-explaining moderators have been shown to be biased in the presence of publication selection (Stanley and Doucouliagos, 2012). Later, in Subsection 1.4.2, a control for publication selection (FAT-PET-PEESE) is introduced, proceeding from this naïve framework to calculate corrected mean effect sizes (see Borenstein et al., 2011 and Ringquist, 2013 for in-depth discussions of meta-analytic models).

<sup>14</sup> Note that the terminology of fixed- and random-effects is different from their usage in panel models. The explanations in this section are intended to draw clear distinctions despite this ambiguity in wording.

$$elasticity_i = \alpha_0 + \sum_{k=1}^K \alpha_k * C_{k,i} + v_i + \epsilon_i, \quad (1.6)$$

with  $v_i \sim iid(0, \tau^2)$  depicting unobserved heterogeneity. A standard test for assessing the presence of unobserved heterogeneity is Cochran's Q-test (Borenstein et al., 2011). For these multivariate models, the null hypothesis of the Q-test assumes that all heterogeneity is explained by the moderating variables. If this can be rejected, the random effects model is generally favoured over its fixed effects counterpart (Feld and Heckemeyer, 2011). With many studies reporting multiple estimates, there is potential dependence among estimates from the same study through the study design, shared methodology or sample reuse (Stanley and Doucouliagos, 2012; Penn and Hu, 2019). One way to account for non-independent observations is to use panel-econometric techniques. In this type of model, a second study layer explicitly reflects the nested structure of estimates. Accordingly, the multilevel or hierarchical model is given by

$$elasticity_{ij} = \alpha_0 + \sum_{k=1}^K \alpha_k * C_{k,ij} + \lambda_j + \epsilon_{ij} \quad j = 1, 2, \dots, S, \quad (1.7)$$

with  $j$  indexing the study level. The Breusch and Pagan Lagrangian multiplier (BPLM) test helps in deciding whether a panel-type model is appropriate. If the null hypothesis of no study-level effect is rejected, there are again two modelling options. First, the study-level effect  $\lambda$  can be estimated as an unobserved study-level error term resulting in a random effects multilevel model (REML), or else it can be modelled explicitly by replacing  $\lambda$  with study dummies, which is known as a fixed effects multilevel model (FEML) (Stanley and Doucouliagos, 2012). The REML critically assumes that the unobserved study effect is uncorrelated with all regressors. If there is reason to suspect correlation, the FEML is the appropriate choice. A robust Hausman test serves as decision rule (Feld and Heckemeyer, 2011). Alternatively, if the BPLM test does not support a panel-type model, clustered standard errors can be calculated to correct correlated error terms at the study level. This is especially apposite if the number of clusters is high (Nelson 2015). A related approach assigns equal weights per study or equal weights per sample to avoid undue dominance of studies with many estimates over studies reporting only one (Penn and Hu, 2019).<sup>15</sup>

Regardless of choice, any model should be estimated with WLS rather than OLS (Feld and Heckemeyer, 2011). The meta-dataset includes studies with widely dispersed

<sup>15</sup> Another solution proposed is to evade the problem by using only one observation per primary study such as the 'preferred' estimate or 'best evidence' per study (Nelson, 2004). However, this inherent selection has been criticised as arbitrary, as greatly diminishing sample size, discarding valuable information and conserving publication bias (Nelson, 2015; Stanley and Doucouliagos, 2012). Alternatively, the mean effect size per study could be used (Stanley, 2001). While this also greatly reduces sample size and discards valuable information, the resulting average may still better reflect the 'true' effect size.

estimates and corresponding variances that induce heteroscedasticity in the error term(s). Hence, though estimating Equation 1.5, Equation 1.6 or Equation 1.7 by OLS would produce unbiased results, the estimates would be inefficient. Relying on WLS ensures efficient estimates of the coefficients (Wooldridge, 2010). The employed analytic weights are the reciprocal error-term variances, which vary in accordance with the type of model chosen. In the case of the fixed effects model, the variance  $\sigma_i^2$  is given or transformed information on precision in the regressions of the original studies (see Subsection 1.3.1). Hence, heteroscedasticity is easily accommodated by using analytic weights  $w_i = \frac{1}{\sigma_i^2} = \frac{1}{SE_i^2}$ , with  $SE$  being the standard error of each respective estimate.<sup>16</sup> In the case of random effects models, the weight changes to  $w_i = \frac{1}{\sigma_i^2 + \tau^2} = \frac{1}{SE_i^2 + \tau^2}$  to incorporate the additional variance introduced by unobserved heterogeneity. In contrast to  $\sigma_i^2$  the additional element of variance  $\tau^2$  is not known to the meta-analyst a priori and must be estimated in a first step.<sup>17</sup> In both cases, the weights chosen reflect the precision of the respective estimates and thus give greater weight to more precise estimates.<sup>18</sup> In contrast to the fixed effects weight, however, the random effects weight is typically more evenly distributed due to the added constant between-study variance.<sup>19</sup>

This setup identifies four classes of models, all of them estimated by WLS. Considering the wide range of notations and terminologies in the literature, I hope to clearly distinguish them by calling the fixed effects model in Equation 1.5 WLS-FE and its random effects counterpart in Equation 1.6 WLS-RE (following Alinaghi and Reed, 2018). As introduced above, their respective panel-type counterparts originating from Equation 1.7 are referred to as FEML and REML to underscore their multilevel nature (following Stanley and Doucouliagos, 2012). This structured decision process is the framework used for selecting the most appropriate meta-analytic model. Choice is determined entirely by the meta-dataset at hand. Starting from the general multivariate model, Cochran's Q-test provides the criterion for choosing either a WLS-FE or WLS-RE model. In the latter case, study-level effects reflecting the non-independence of

<sup>16</sup> Stata has several commands that estimate weighted regressions, with the same resulting regression coefficients and corresponding standard errors in all cases. With no statistical guidance for preferring one command to the other (Ringquist, 2013), I here use the simple and widely known `regress` command with manually weighted variables. This can be thought of as OLS applied to weighted observations.

<sup>17</sup> This is achieved by using the `metareg` package in Stata, which, for example, relies on residual maximum likelihood estimation.

<sup>18</sup> Alternatively, heteroscedasticity-robust standard errors may be calculated. However, as weighting the estimates also serves to give greater importance to more precise estimates, I prefer using WLS to OLS with heteroscedasticity-robust standard errors.

<sup>19</sup> There are major differences in the magnitudes of the effect sizes and their respective standard errors that eventually serve as weights. In the fixed effects framework, this means that some observations receive a lot of weight while others have barely any influence. While in principle this reflects the desired weighting mechanism, here it would lead to some observations dominating others. This could be handled by following Feld and Heckemeyer (2011) and excluding observations with extremely high weights. Here, however, there is no clear cut-off point, as there are many observations with both large and small weights simultaneously. Thus, the precision weight is logarithmised, or  $\log(\text{precision}) = \log(\frac{1}{SE_i})$ , as this transformation resulted in an approximately normal distribution of the precision variable ( $p < 0.001$ ). The subsequent regression results are close to the random effects framework, so that I consider the transformation a viable alternative in this case.

multiple estimates from the same study can explain unexplained heterogeneity. A BPLM test helps investigating the existence of such study-level effects, in which case a multilevel model is the appropriate choice. Finally, the robust Hausman test indicates the appropriateness of either the REML or FEML model.

### 1.4.2 Publication bias

A visual method commonly used to detect publication bias is the examination of a funnel plot. In a funnel plot, the effect sizes are plotted against their respective standard error. In an ideal setting without publication bias, the distribution of effect sizes from studies with large samples would cluster around the top of the plot (where precision is high), with estimates from studies with smaller samples (and lower precision, i.e., higher standard errors) spreading down into the bottom area, thus creating an inverted funnel shape (Borenstein et al., 2011). This would reflect the random deviation from the genuine mean effect due to sampling error. If the funnel plot is asymmetric, this may hint at publication selection biasing the results (Ringquist, 2013). Figure A.4 shows two funnel plots. The analysis of alternative funnel plots indicates the sensitivity of the visual impression. On the left-hand side, each point represents a single observation from the meta-sample. The points on the right-hand side are study means. Furthermore, the type of waste site examined in each study serves as a label for the points depicted: hazardous, non-hazardous and nuclear-waste sites. I consider waste-site category labels to reflect the possibility that publication selection only occurs for some types of waste site. The standard funnel plot on the left is a rather symmetric, homogenous plot with many precise estimates clustered around the top. Despite the symmetric impression, there is considerable dispersal of estimates at the top, suggesting that the monetary effect of waste sites on residential property values may be moderated by some study characteristics, such as the type of waste site. The funnel plot of study means on the right-hand side does not confirm the impression of a symmetric graph. Clearly, several estimates lie right to the centre, forming an asymmetric plot. Note, however, that taking study means narrows down the scales of the axes so that the two funnel plots cannot be compared directly. Turning to the waste-site labels in the funnel plot on the left-hand side, it becomes evident that studies examining hazardous waste sites report the most widely spread results. However, there is no clear visual evidence of differences in plot symmetry by waste-site labels. The funnel plot on the right-hand side generally supports this impression, with studies examining non-hazardous waste sites also reporting dispersed findings. The funnel plots taken as a whole suggest publication bias in the sample and show that the type of waste site may be one important explanatory factor for heterogeneity in the effect sizes observed. Considering study means instead of single estimates further supports the impression of an asymmetric plot.

Although funnel plots are an informative visual tool, their interpretation remains subjective. A regression-based formal test framework known as FAT-PET-PEESE builds on the rationale of the funnel plot, adding in the first place the standard error SE of the estimated effects to a simple version of Equation 1.5 or Equation 1.6, or

$$elasticity_i = \alpha_0 + \alpha_1 * SE_i + \epsilon_i \quad (1.8)$$

In this setting, the so-called Funnel Asymmetry Test (FAT) tests the hypothesis of  $\alpha_1 = 0$  with a conventional t-test, assuming that, in the absence of publication bias, the effect size will be uncorrelated with its standard error (Stanley and Doucouliagos, 2012). Thus, rejecting the FAT hypothesis confirms that publication selection places a bias on the estimates in the meta-sample. Similarly, the Precision Effect Test (PET) tests for the presence of a genuine average effect size beyond publication bias ( $H_0 : \alpha_0 = 0$ ) (Stanley, 2008; Stanley and Doucouliagos, 2017). In the case of a true non-zero effect confirmed by the PET, simulations have shown that the estimated average effect size ( $\alpha_0$ ) is often underestimated (Stanley and Doucouliagos, 2012; Stanley and Doucouliagos, 2017). In these cases, replacing the standard error SE in Equation 1.8 by its square produces less biased estimates of the true underlying effect, an approach known as Precision Effect Estimate with Standard Error (PEESE) (Stanley and Doucouliagos, 2014). However, if the mean effect is not significantly different from zero, the PET is shown to be the better choice. Accordingly, I estimate both alternative specifications for this meta-analysis. However, while the FAT-PET-PEESE framework is a commonly applied control for publication bias, some simulation studies question its performance under certain conditions (Alinaghi and Reed, 2018; Carter et al., 2019; Du et al., 2017). For this reason, I additionally check for the presence of publication bias by applying the publication bias control methods recently proposed by Andrews and Kasy (2019), Furukawa (2019), Ioannidis et al. (2017), Simonsohn et al. (2014), and Stanley et al. (2010).<sup>20</sup> For conciseness, these methods and their results are set out in detail in the appendix.

## 1.5 Results and Discussion

The presentation of results follows the shape of the remarks on model-selection strategy set out in the previous section. The results of the tests for publication bias come first, followed by the results for the models chosen, including subsample analyses and related robustness checks. The discussion of BT errors concludes this section.

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<sup>20</sup> See also Christensen and Miguel (2018) and van Aert and van Assen (2018) for a summary of approaches addressing publication bias.

**Table 1.3:** Results for tests of publication bias and genuine effect size.

	Meta-analytic model and publication bias control					
	Random effects			Fixed effects		
	No control	FAT-PET	PEESE	No control	FAT-PET	PEESE
Mean effect	0.030*** (0.006)	0.019*** (0.007)	0.029*** (0.006)	0.024*** (0.005)	0.015*** (0.005)	0.023*** (0.005)
SE		0.382*** (0.129)			0.426*** (0.280)	
SE <sup>2</sup>			0.660** (0.280)			0.857** (0.350)
<i>N</i>	727	727	727	727	727	727
<i>R</i> <sup>2</sup>	0.093	0.113	0.099	0.081	0.113	0.091

*Notes:* The table presents the results of WLS regressions from Equation 1.8. The FAT-PET framework uses *SE*, while the PEESE correction mechanism uses *SE*<sup>2</sup>. The weights are the logarithmised inverse of the squared standard error (FE) and a composite weight also reflecting between study variance (RE). Standard errors clustered at the study level are given in parentheses below the coefficients. \*\*\*, \*\* and \* denote coefficients significantly different from zero at the 1-, 5- and 10-percent levels.

### 1.5.1 Publication bias and corrected mean effect size

The results of the tests for publication bias are summarised in Table 1.3. In all cases, the weighted mean effect size is reported along with the respective coefficient controlling for publication bias, where applicable.

Regardless of the chosen method, the results show that publication bias clearly distorts the average effect size. In comparison to the unweighted average in Table 1.2 (0.042) and the weighted average reported in columns (1) and (4) of Table 1.3 (0.030 and 0.024), all methods correct the effect downwards, with estimates ranging from 0.015 to 0.029. As expected, the FAT-PET estimates (0.015 and 0.019) correct more strongly than the PEESE alternatives (0.023 and 0.029), with the PEESE estimates being less biased with regard to the significant mean effect size. In economic terms, the FAT-PET-PEESE range of estimates translates into an average increase of 1.5% to 2.9% in property values per mile of increased distance from a waste site for a house located one mile away from the waste site. For a house located 4.29 miles away from the waste site (the mean distance in this sample), the increase in value is 0.35% to 0.68%. These findings are at the lower bound of the results found in previous meta-analyses on the topic (see Section 1.2). In summary, Table 1.3 confirms a minor negative effect of waste sites on proximate residential property values at the aggregate level. Publication selection is present, however, resulting in an upward bias of up to 38% in this literature. This finding is corroborated by results for other publication bias control methods (see Figure A.8 and Table A.10 in the appendix).

Despite these findings at the aggregate level, the average effect size is based on a heterogeneous set of observations as indicated by Q-tests, funnel plots and summary statistics. I analyse the origin of this heterogeneity in the next section. As discussed previously, publication bias correction via the FAT-PET or the PEESE approach can easily be included in MRAs explaining heterogeneity. Where publication bias is clearly confirmed, fixed effects models should be the models of choice. On the other hand, Q-test statistics indicate a rejection of effect-size homogeneity that would appear to favour the random effects models.<sup>21</sup> In such a case, the literature provides no clear guidance on which estimator to prefer and the choice of the appropriate model follows the decision rules described in Subsection 1.4.1.

### 1.5.2 Heterogeneity of effect size

The meta-regression results are reported in Table 1.4. Column (1) shows the baseline WLS-RE PEESE model. Selection of this specification follows the results of a Q-test rejecting the null hypothesis of no heterogeneity at the estimate level and a subsequent BPLM test lending no support for the hypothesis of additional study-level heterogeneity.<sup>22</sup> In summary, the moderators included are sufficient to explain study-level heterogeneity so that there is no need to rely on panel-econometric models to reflect study-level effects.<sup>23</sup> For this baseline specification I consider all moderators with non-missing observations defined in Table 1.1 so as to conserve sample size. The PEESE publication bias control is included as this is the preferred choice with a significant mean effect size resulting in a smaller bias, as set out below. Results including FAT-PET publication bias control are reported in Table A.8 in the appendix.

I have checked the baseline model for normality of residuals, outliers, persistent heteroscedasticity and multicollinearity. The findings support inference validity; see Figure A.5 and Figure A.6 in the appendix for details on non-normality and outliers.<sup>24</sup> However, persistent heteroscedasticity was identified despite reliance on WLS. Consequently, cluster-robust standard errors are used for all regressions. Multicollinearity was only a minor concern that did not affect the regression results in any relevant way.<sup>25</sup> I explore the robustness of the results from a variety of perspectives.

<sup>21</sup> The Q-test statistics are summarized in Table A.7.

<sup>22</sup> Here, the BPLM test cannot be performed in its standard version. However, this can easily be resolved by slightly altering the approach; see the detailed explanations in the Do-files made available in the data repository linked to in the electronic version.

<sup>23</sup> Detailed test results are summarised in Table A.7.

<sup>24</sup> These exemplary diagnostic plots for the WLS-RE PEESE model detect six potentially outlying observations. I reassessed them for coding errors or any particularities justifying their exclusion. No obvious pattern was detected. As the regression results are robust to the exclusion of the outlying observations, they are not discarded. See Table A.8 in the appendix for details. Diagnostic plots for the other regression models based on the entire sample come up with almost identical results and are available on request.

<sup>25</sup> The mean variance inflation factor (VIF) is 3.7 with only a few levels of some categorical regressors indicating multicollinearity. These are to be expected as, e.g., the status on the NPL is, by design,



Column (2) represents the WLS-FE model, reflecting the discussion in the previous section on the preferred estimator in the presence of publication bias. The results for a reduced model are reported in column (3), following a general-to-specific (G-S) modelling approach recommended by Stanley and Doucouliagos (2012). This approach involves a stepwise removal of the least significant variable until only variables with a p-value less than 0.2 remain. Additional regressions based on trimmed datasets and reweighted observations confirm the robustness of the results. They are shown in Table A.8 in the appendix.

The majority of moderators are binary or categorical variables. Accordingly, their corresponding coefficients can be interpreted *ceteris paribus* as the expected change in mean effect size caused by a departure from the benchmark scenario. The benchmark scenario for the categorical moderators is the omitted category indicated in parentheses. For all binary regressors, the benchmark is the zero case. The remaining continuous variables are centred so that their coefficients can be interpreted as the effect of deviations from the mean.<sup>26</sup> Hence, the constant can be interpreted as the mean effect size for a reference study indicated by the benchmarks. Table 1.4 shows that the results are robust across specifications based on all observations. With only small quantitative differences for most coefficients, the following description focuses on the results of the WLS-RE model and only has recourse to the WLS-FE and G-S alternatives in the case of pronounced disagreement.

Most notably, publication bias is confirmed throughout the models. This squares with the results from Table 1.3 and Table A.10, finding evidence of a highly significant upward bias in the literature. The corrected mean effect sizes in columns (1) to (3) range from about 0.074 to 0.106 in magnitude and are significant in all cases. In comparison to mean effect sizes displayed in Table 1.2 and Table 1.3, mean effect sizes in the comprehensive meta-regressions are two to three times larger. This can be explained by the different cases reflected in the respective mean effect sizes.<sup>27</sup> The  $R^2$  shows little variance, ranging between 0.232 and 0.261. Turning to the explanatory variables, published and unpublished studies do not seem to differ in effect size magnitude when publication bias is controlled for. More recent publications tend to report greater effect sizes.

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correlated with the status North America. Reassuringly, the reduced model in column (3) and the clear majority of robustness regressions confirm the same set of significant moderators, so that multicollinearity does not affect the results in any relevant way.

<sup>26</sup> This excludes, of course, the publication bias control variables  $SE$  and  $SE^2$ .

<sup>27</sup> The reference study in the meta-regressions displayed in Table 1.4 is, e.g., unpublished, does not include socio-demographic controls in its regression(s) and models the effect of a European waste site, as indicated by the definition of the moderators. As the majority of the studies in the meta-dataset are published, include socio-demographic controls and focus on North American waste sites, the mean effect sizes in Table 1.2 and Table 1.3 reflect a different reference case.

**Table 1.4:** Meta-regression results.

Variables	Full sample			Subsamples			
	(1) WLS-RE PEESE	(2) WLS-FE PEESE	(3) G-S	(4) Mean distance	(5) Socio-economic controls	(6) Non-hazardous site	(7) On NPL
Mean effect	0.106*** (0.032)	0.102*** (0.031)	0.074*** (0.021)	0.098*** (0.034)	0.111** (0.045)	-0.187 (0.144)	0.424** (0.164)
SE <sup>2</sup>	0.847*** (0.279)	0.972*** (0.369)	0.856*** (0.271)	1.612** (0.790)	1.030*** (0.179)		1.282* (0.633)
SE						0.390 (0.906)	
Published	0.007 (0.009)	0.006 (0.006)		0.018** (0.009)	0.008 (0.010)	-0.031 (0.041)	0.097 (0.070)
Year publish	0.001* (0.001)	0.001* (0.001)	0.001** (0.001)	0.002** (0.001)	-0.004*** (0.001)	0.007* (0.003)	0.015* (0.007)
<b>Site characteristics</b>							
Multiple sites	0.037** (0.015)	0.037** (0.014)	0.027*** (0.008)	0.028** (0.014)	0.071* (0.036)	-0.012 (0.047)	-0.002 (0.064)
<i>NPL (reference category: Not USA)</i>							
Not on NPL	0.036 (0.039)	0.036 (0.033)		0.026 (0.035)	0.038 (0.038)		
On NPL	0.035 (0.044)	0.044 (0.036)		0.024 (0.040)	0.039 (0.040)		
Unclear	0.046 (0.046)	0.039 (0.039)		0.026 (0.040)	0.026 (0.041)		
<i>Activity status (reference category: Active)</i>							
Inactive	-0.023 (0.029)	-0.014 (0.025)	-0.026 (0.020)	-0.019 (0.028)	-0.072 (0.057)	-0.024 (0.046)	-0.340*** (0.045)
Unclear	-0.039 (0.033)	-0.018 (0.019)	-0.045*** (0.016)	-0.021 (0.031)	-0.066 (0.046)	-0.050 (0.092)	
<i>Employment opportunities (reference category: Employment opportunities)</i>							
No employment opportunities	-0.016 (0.024)	-0.020 (0.017)		-0.019 (0.025)	-0.023 (0.033)	0.120** (0.047)	-0.024 (0.121)
Unclear	-0.007 (0.031)	-0.021 (0.022)		-0.020 (0.032)	0.019 (0.039)		-0.075 (0.095)
<i>Type of waste site (reference category: Hazardous)</i>							
Non-hazardous	-0.042 (0.026)	-0.041* (0.024)	-0.036* (0.022)	-0.040 (0.025)	-0.087 (0.055)		
Nuclear	-0.009 (0.069)	0.009 (0.062)	-0.020 (0.059)	-0.067 (0.099)	0.104 (0.079)		
<i>Waste-affected element (reference category: Soil)</i>							
Air	0.042** (0.018)	0.038** (0.017)	0.042** (0.016)	0.022 (0.017)	0.011 (0.028)	-0.012 (0.033)	0.037 (0.052)
Water	-0.007 (0.018)	-0.005 (0.017)	-0.009 (0.018)	-0.029* (0.016)	0.005 (0.013)	-0.060 (0.058)	0.012 (0.008)

**Table 1.4:** (continued).

Variables	Full sample			Subsamples			
	(1) WLS-RE PEESE	(2) WLS-FE PEESE	(3) G-S	(4) Mean distance	(5) Socio-economic controls	(6) Non-hazardous site	(7) On NPL
Unclear	-0.018 (0.015)	-0.023 (0.015)	-0.016 (0.012)	-0.021 (0.014)	-0.060** (0.026)	-0.048* (0.026)	0.043 (0.042)
<i>Continent (Reference category: Europe)</i>							
North America	-0.033 (0.043)	-0.031 (0.037)		-0.013 (0.038)	-0.011 (0.056)	0.148 (0.097)	
Asia	-0.015 (0.027)	-0.017 (0.024)		-0.009 (0.027)	0.003 (0.044)	0.144 (0.121)	
Other	0.003 (0.024)	0.011 (0.020)		-0.015 (0.023)	0.016 (0.038)	-0.027 (0.099)	
<i>Clean-up stage (Reference category: Recognised, no clean-up plan exists)</i>							
Not recognised	-0.007 (0.014)	-0.001 (0.011)	-0.005 (0.012)	-0.020 (0.022)	-0.002 (0.013)		
Clean-up begun, but not finished	-0.007 (0.015)	-0.016 (0.015)	-0.006 (0.012)	-0.019 (0.020)	0.010 (0.018)		-0.012** (0.006)
Cleaned-up	-0.058** (0.028)	-0.056** (0.026)	-0.060** (0.026)	-0.081*** (0.028)	-0.051* (0.030)		-0.051 (0.044)
Unclear	0.005 (0.018)	0.007 (0.016)	0.006 (0.016)	-0.009 (0.016)	-0.010 (0.026)		0.232** (0.075)
<b>Data characteristics</b>							
Dist greater mean	-0.024* (0.014)	-0.020 (0.014)	-0.021 (0.013)		-0.035*** (0.011)	-0.053 (0.055)	-0.077*** (0.016)
Dist mean				0.001 (0.002)			
Sample	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	0.001 (0.002)	-0.002** (0.001)
Sale ind	-0.025 (0.018)	-0.020 (0.017)		-0.021 (0.018)	-0.022 (0.025)	0.029** (0.014)	-0.010 (0.129)
<b>Methodology: Econometric specification</b>							
Num expl	0.000 (0.000)	0.000 (0.000)		-0.000 (0.000)	0.001** (0.000)	-0.001 (0.003)	-0.002* (0.001)
Oth disamen	0.002 (0.011)	-0.000 (0.010)		-0.000 (0.012)	0.004 (0.020)	-0.108*** (0.033)	-0.026 (0.090)
Oth amen	-0.000 (0.017)	-0.003 (0.018)		0.004 (0.016)	0.006 (0.028)	0.021 (0.047)	0.029 (0.065)
Access	-0.010 (0.012)	-0.012 (0.011)		-0.016 (0.012)	-0.002 (0.015)	0.064* (0.035)	-0.046 (0.045)
Industry	-0.025 (0.017)	-0.021 (0.015)		-0.021 (0.016)	-0.035 (0.026)	0.074 (0.063)	0.086 (0.086)
Demoecon	-0.031** (0.014)	-0.027* (0.015)	-0.021* (0.011)	-0.025* (0.014)		-0.022 (0.063)	-0.017 (0.018)
Time control	0.022 (0.016)	0.018 (0.015)		0.024 (0.015)	-0.018 (0.026)	-0.010 (0.056)	0.007 (0.049)

**Table 1.4:** (continued).

Variables	Full sample			Subsamples			
	(1) WLS-RE PEESE	(2) WLS-FE PEESE	(3) G-S	(4) Mean distance	(5) Socio-economic controls	(6) Non-hazardous site	(7) On NPL
Direction	0.010 (0.017)	0.017 (0.016)		0.013 (0.015)	0.011 (0.017)	0.120** (0.055)	0.026 (0.023)
Interaction	-0.045*** (0.013)	-0.043*** (0.012)	-0.047*** (0.015)	-0.047*** (0.013)	-0.055*** (0.015)	-0.054 (0.057)	-0.016 (0.015)
<b>Methodology: Estimation strategy</b>							
Log log	-0.012 (0.012)	-0.011 (0.012)		-0.005 (0.012)	-0.020 (0.014)	0.000 (0.012)	0.039 (0.027)
OLS spatial	-0.009 (0.027)	-0.015 (0.025)		-0.009 (0.027)	0.001 (0.030)	0.131 (0.106)	-0.084 (0.089)
<i>N</i>	727	727	727	548	544	125	184
<i>R</i> <sup>2</sup>	0.261	0.232	0.246	0.343	0.277	0.610	0.613

*Notes:* Regression results are based on WLS. Standard errors clustered at the study level are given in parentheses next to the coefficients. \*\*\*, \*\* and \* denote coefficients significantly different from zero at the 1-, 5- and 10-percent levels. In columns (1) to (3), the full sample is used with different estimators and regressors, while columns (4) to (7) depict subsample results.

*Site characteristics*

As expected, studies with multiple sites in the proximity of residential properties report higher effect sizes on average. In other words, multiple waste sites affect residential property values more adversely than single sites. By contrast, there seem to be no significant differences in terms of effect size for waste sites with differing employment opportunities or status on the NPL. According to the reduced G-S model, sites with unclear activity status seem to have a less value-depressing impact than active sites. Surprisingly, the type of waste does not seem to influence effect size in any consistent way. Only in the WLS-FE and G-S model are the effects of non-hazardous waste sites significantly smaller than their hazardous counterparts (and only at the 10% significance level). I explore this result in more detail in the subsample regressions discussed below. Further, there seem to be distinctive differences in effect size depending on the element affected by waste. Waste sites emitting airborne pollutants clearly reduce residential property values more significantly than waste sites where the polluted element is unclear, whereas soil-polluting or water-polluting waste sites do not significantly differ from the latter in terms of effect size. The continent on which a waste site is located does not seem to be of relevance. In neither case is there any significant difference from a European waste site.<sup>28</sup> The clean-up stage seems to have a distinct impact on property values. Recalling the reference case of a waste site with recently discovered pollution, it transpires that, during the remediation phase, there is no significant recovery in residential property values. However, this recovery effect manifests itself once the clean-up has been completed. This finding, intuitive as it is, contrasts with the results of Braden et al. (2011), who report insignificant effects from clean-up activities.

*Data characteristics*

The data characteristics reveal that on average studies working with larger samples tend to report smaller estimates. By contrast, studies based on sales data collected at individual house level do not differ significantly from studies using assessed values or aggregated data. Apparently, studies with greater mean distances of the sampled houses from the respective waste site do not consistently report significantly smaller effect sizes than studies with smaller mean distances. Only in the WLS-RE model, the coefficient is significant, albeit weakly. This finding is seemingly at odds with the basic hedonic hypothesis that an environmental disamenity will be considered less serious the further away it is from the property in question. However, the effect may be obscured by the choice of the dummy variable "Dist greater mean" as it is only a crude measure of the distance-decay effect. In addition, reported effect sizes may be biased if primary studies fail to control for important confounding factors. Finally, if on average non-hazardous waste sites are not perceived as disamenities, this may also mask the distance-decay effect. I investigate these eventualities in the subsample analyses.

<sup>28</sup> One reviewer suggested replacing the continent dummies with a continuous measure like the HDI or GDP per capita. Both replacements are tested and reported in Table A.9. The results are similar to the baseline specification.

*Methodology*

Whereas the mere number of explanatory variables does not seem to be an important factor, some moderators reflecting the comprehensiveness or quality of the econometric specification do significantly affect the reported effect size.<sup>29</sup> First, not controlling for socio-demographic factors, such as the crime rate or median household income in a census tract, results in significantly higher estimates of waste-site effects. This shows that the omission of important control variables in the primary literature potentially leads to biased estimates of the effect of waste sites on residential property values. However, controlling for other amenities or non-waste disamenities in the vicinity does not on average appear to influence the reported effect size. Moreover, studies that use a distance-interaction term report considerably smaller average effect sizes. Evidently, the interaction term takes up some part of the effect size that would otherwise be reflected in the single term. By contrast, controlling for time-fixed effects via dummy variables or using price inflation adjustments does not seem to influence the reported effect size. Lastly, the estimation strategy is not a significant factor explaining variance. More precisely, neither the functional form nor the chosen estimator seem to influence the reported effect size in any given case.

With very similar results across specifications and estimators, the subsample analyses rely on the WLS-RE PEESE baseline model. Based on the results from Table 1.3 and Table 1.4, this is justifiable as the mean effect size consistently differs significantly from zero.

**1.5.3 Subsample analysis**

Several moderators are not included in the baseline regression shown in column (1) due to missing observations or because these moderators only serve as replacements for explanatory variables already included. In a first step, I add these moderators to the meta-regression separately, which naturally leads to a reduced sample size in each case. Moreover, the results from the baseline model identify sub-categories of observations that warrant further investigation. Accordingly, I use different subsample regressions to provide a more nuanced picture of the robustness of results. In most cases, the subsample analyses reveal only marginal differences from the baseline model. Reassuringly, however, expected differences manifest themselves as well. The results for four subsamples are summarised in columns (4) to (8) in Table 1.4. For other subsample regressions, see Table A.9 in the appendix. Results are generally robust, so again I only discuss notable differences from the baseline model. As expected, replacing "Dist greater mean" with "Dist mean" in column (4) makes very little difference to the

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<sup>29</sup> In a robustness check reported in Table A.9, the number of total explanatory variables is replaced by the number of significant variables, which is also insignificant. The overall results remain unchanged except for an increased  $R^2$ . This effect, however, is not due to the exchanged variable but rather related to the effect of a smaller, more consistent subsample.

overall results. Notably, the coefficient on "Dist mean" is insignificant, reproducing the counterintuitive finding of Braden et al. (2011). Hence, a continuous distance-decay effect cannot be confirmed for the full sample. When we turn to the subsamples in columns (5) to (7), the differences from the baseline model are more clear-cut. Controlling for socio-economic factors (column (5)) seems to be an important quality dimension in the primary study. While the overall results remain similar to the baseline regression, the distance-decay effect is now confirmed. This finding manifests the impression that the design of the primary study may be one factor concealing this pattern. This interpretation is supported by an additional subsample regression that omits effect sizes from interactions (shown in Table A.9).

For the subsample of non-hazardous waste-site observations in column (6), the mean effect size turns insignificant. On average, non-hazardous waste sites are apparently not value-depressing. This finding supports the hypothesis that waste-site effects on property values differ by waste categories that can be controlled for in MRAs. Similarly, no publication bias can be confirmed. Neither the dummy variable *Publish* nor the publication bias control variable *SE* significantly affect the average effect size. It appears that there are no prior expectations of the sign or significance of the effect size for this type of waste site. Moreover, in contrast to the baseline model, other disamenities in the vicinity significantly reduce the effect size of non-hazardous waste sites. In addition, a non-hazardous waste site offering no employment is clearly more value-depressing than otherwise. The  $R^2$  increases substantially compared to the baseline scenario (0.610 compared to 0.261).

Surprisingly, status on the NPL seemed to be of no relevance for the effect size in any of the previous regressions. Hence, in column (7) I explore the subsample of observations from waste sites on the NPL more closely. The mean effect size is approximately fourfold in magnitude, confirming the expected negative effects of highly contaminated waste sites. Moreover, the  $R^2$  increases substantially compared to the baseline scenario (0.613 compared to 0.261). The clean-up stage continues to be of importance. More precisely, for this subset of highly contaminated sites, an unclear clean-up stage increases effect size over and against a site with recently discovered contamination. In addition, the start of clean-up activities seems to be an important step towards remediation. As expected, the distance-decay effect can be confirmed.<sup>30</sup> Apparently, the type and level of contamination are determinants in detecting this pattern (see also the subsample regression on hazardous waste sites in Table A.9).

#### 1.5.4 Benefit transfer

As noted previously, one of the potential merits of MRA is the use of the resulting coefficients for BT applications. The usefulness for BT applications, however, depends

<sup>30</sup> For this subsample regression, the moderator "Dist mean" was also significant (not shown).

on the magnitude of the inherent transfer error. Table 1.5 shows the transfer errors to illustrate the predictive power of this MRA. I calculate the transfer errors following common practice, using the Absolute Percentage Error (APE) as metric (Nelson 2015). The APE is shown for the baseline specification that includes all observations as well as for the subsamples of non-hazardous waste sites and waste sites on the NPL, respectively.<sup>31</sup> Moreover, the APE is calculated using not only meta-functional transfer based on the respective results shown in Table 1.4, but also mean-value transfer based on a univariate WLS analogue of Equation 1.6. Following Brander et al. (2006) and Chaikumbung et al. (2016),<sup>32</sup> I calculate the transfer errors using n-1 out-of-sample regressions, i.e., omitting one observation at a time, re-estimating the model and calculating the estimated BT for the observation omitted.

**Table 1.5:** Benefit transfer errors.

	Mean APE (SD)	Median APE	Range of APE
<i>Meta-functional transfer</i>			
Full sample	543.41 (3228.37)	87.55	0.63 - 49363
Non-hazardous waste sites	463.90 (1708.55)	84.52	0.93 - 13253
Waste sites on the NPL	358.71 (2224.52)	90.45	0.19 - 29098
<i>Mean value transfer</i>			
Full sample	683.56 (4635.68)	85.92	0.03 - 72724
Non-hazardous waste sites	601.33 (2234.48)	79.72	2.78 - 15146
Waste sites on the NPL	133.08 (333.77)	75.92	1.30 - 3575

*Notes:* The meta-functional transfer calculations are based on the regressions in columns (1), (6) and (7) from Table 1.4. The mean value transfer is based on the univariate WLS-RE analogue of Equation 1.6.

The general impression gained from the results on transfer errors is in line with previous discussions in the literature. First, meta-functional transfer outperforms simple value transfer if the underlying sample is heterogeneous. If the sample consists of a more homogenous set of sites, e.g., waste sites on the NPL, simple value transfer results in smaller transfer errors. Second, regardless of the type of transfer, the APE is smaller if the pooled sites have a higher degree of commodity consistency. Third, the Mean APE is considerably larger than the Median APE, with BT errors ranging between 133% and 684% for the former, compared to values of 76% to 90% for the latter. A small number of outlying observations drives the mean APE upwards.<sup>33</sup> This pattern is also apparent in Figure A.7 in the appendix, showing the distribution of BT errors.

<sup>31</sup> In all cases, one observation with an extreme APE (770,261%) is omitted from the calculations and from the presentation in Table 1.5. Omitting this clearly outlying observation reduces, e.g., the Mean APE from 1,602% to 543% for the full sample, while the Median APE barely changes. Renewed perusal of the corresponding paper (Schmalensee, 1975) revealed no particularities.

<sup>32</sup> I calculated in-sample transfer error as well (not shown). They follow the same pattern as the out-of-sample predictions depicted in Table 1.5, albeit with smaller transfer errors throughout, as expected.

<sup>33</sup> Omitting 14 observations from one study (Walsh and Mui, 2017) that represent the vast majority of extreme APE reduces the Mean APE from 543% to 213%. Similarly, for the subset of observations on the NPL, four observations are clearly outlying. Omitting these observations reduces the corresponding Mean APE from 359% to 125%.



The results are in line with expectations, but the order of magnitude of the BT errors warrants further investigation. In general, moderate to high transfer errors are common in the valuation literature. Rosenberger (2015) reviews the transfer errors for 38 valuation studies, reporting an average of 65% (36%) for the Mean (Median) APE based on function transfer and a corresponding average of 140% (45%) for value-transfer errors. However, though the BT errors shown in Table 1.5 are greater than this average in most cases, this is not necessarily surprising as the complex model was designed to identify sources of heterogeneity rather than to calculate transfer errors (K. J. Boyle and Wooldridge, 2018; Nelson, 2015). Still, while the acceptable level of transfer error is context-dependent (Rosenberger, 2015; Brander et al., 2006), the predicted levels of transfer error will in most cases prohibit accurate policy applications. Apparently, waste-site effects on property values are difficult to predict reliably.<sup>34</sup> Nevertheless, the BT estimates may still be informative for broad cost-benefit analysis at policy sites where primary studies are not feasible.

## 1.6 Conclusion

This meta-study confirms the existence of adverse price effects on residential property values caused by the proximity to waste sites at the aggregate level. Correcting for publication bias has a sizeable impact, reducing the average effect size by up to 38%. The corrected average effect size translates into a 1.5% to 2.9% property value increase per mile of increased distance from a waste site for a house at a one-mile distance. These estimates are situated in the lower range of values produced by the previous literature. The results are generally robust across justifiable estimators, weighting schemes and the replacement of moderators. This need not necessarily hold in other circumstances, and future researchers would do well to adhere to the structured decision pathways that already exist (Feld and Heckemeyer, 2011; Stanley and Doucouliagos, 2012) in choosing the appropriate model for their respective meta-dataset.

Various site and data characteristics as well as the respective econometric specification are significant factors explaining the variation in the empirical findings. Notably, the distance decay of the waste-site effect is partly confirmed, e.g., for hazardous waste sites in general and waste sites on the NPL in particular. In addition, cleaning up a waste site is beneficial for residential property values, possibly restoring value formerly forfeited. This finding contrasts with previous insignificant findings by Braden et al. (2011), possibly due to smaller sample size or the absence of subsample analysis for hazardous waste sites. The subsample analyses revealed distinct differences for severely contaminated sites on the NPL and non-hazardous waste sites. As non-hazardous waste sites do not reduce property values on average, they are not considered a disamenity in

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<sup>34</sup> This interpretation is supported by additional calculations of BT errors for even more consistent subsamples like landfills or brownfields. The transfer errors were in the same range.

these average cases. By contrast, severely contaminated waste sites on the NPL clearly reduce residential property values on average, with an estimated mean effect size of 42.2%. Future MRAs in this context may want to concentrate separately on these types of waste site, as this might be a way of identifying more waste-type-specific moderators. This might also increase forecast accuracy for BT applications. The BT errors in this MRA are consistent with general findings in the literature, showing that meta-functional transfer performs better than value transfer if sites are heterogeneous. The practical applicability of the BT estimates, however, is limited due to comparatively high transfer errors. Hence, more specialised MRAs focusing on waste sites with a higher degree of similarity and corresponding waste-type-specific moderators are needed to forecast estimates more reliably.

There are at least two avenues for future research to explore. First, new moderators need to be identified that can help shed light on the remaining unexplained variance. Second, there is still no unambiguous picture of the presumed distance-decay effect. With occasional insignificance of the moderator controlling for the mean distance between houses and waste sites in at least two MRAs, this remains a partly unresolved issue. Clearly, it would be of great interest to find average distance cut-off points beyond which a waste site is no longer perceived as a disamenity.

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# Appendix

**Table A.1: Literature Review.**

Study	Number of observations (studies) <sup>a</sup>	Number of moderators	Average effect size	Moderators	Type of publication bias treatment	Type of waste site	Type of methodology in primary studies	Estimation
Walton et al. (2006)	17 (7)	11	6.7% per mile	Size of landfill, age of landfill, activity status, population density, income, maximum distance, samples size, time control, functional form	Sample size as moderator	Sanitary landfills	Hedonic pricing	OLS with heteroscedasticity-robust standard errors
Chèze (2007)	45 (12)	8	3.8% (8.4%) per mile for non-hazardous (hazardous) sites	Income, mean distance, hazardousness of waste site, maximum distance, functional form, other disamenities	-	Waste incinerators, sanitary landfills, hazardous waste landfills	Hedonic pricing	WLS / OLS with heteroscedasticity-robust standard errors
Ready (2010)	15 (9)	4	1.3% to 5.9% per mile (landfill size dependent)	Sample size, average house price, maximum distance, size of landfill (measured in volume of waste accepted)	Sample size as moderator, discussion of file-drawer problem	Landfills	Hedonic pricing	Maximum Likelihood
Simons and Saginor (2006)	290 (75), thereof 164 (42) <sup>b</sup> from hedonic pricing studies	28	9.5% per mile for negative disamenities. Information on hedonic-pricing-only average effect size are not reported	Average house price, location, activity and clean-up status, logged distance, type or source of contamination, litigation, information disclosure, unemployment rate, mortgage rate, urbanity, sample size, study methodology	Logged sample size as moderator, calculation of fail-safe-N	E.g., incinerators, landfills, nuclear waste sites, powerlines, superfunds, parks	Hedonic pricing, surveys, case studies, pre- and post-analysis research	OLS, subsample analysis with a maximum of five observations per article allowed



Table A.1: (continued).

Study	Number of observations (studies) <sup>a</sup>	Number of moderators	Average effect size	Moderators	Type of publication bias treatment	Type of waste site	Type of methodology in primary studies	Estimation
Braden et al. (2011)	129 (46) post-outlier, thereof 114 (38) from residential properties	32	3.5% (15.9%) per mile for terrestrial (aquatic) hazardous waste sites	Type of disamenity, location, National Priority List, On-site Employment, clean-up stage, property type, house data type, geographical range, number of sites, sample size, neighbourhood characteristics, time trend, mortgage rate, functional form, continuity of distance variable, publication status, significance, spatial autocorrelation control	Sample size as moderator, publication status as moderator	E.g., contaminated harbours, industrial sites, landfills, nuclear waste sites, recycling centres, waste incinerators	Hedonic pricing	OLS with heteroscedasticity-robust standard errors, WLS, random effects model
Lipscomb et al. (2013) <sup>c</sup>	273 (40); 227 (33) of observations from hedonic pricing, 42% from positive amenities	23	6.1% (11.4%) per mile for contaminated water bodies (superfund sites); 2.5% per mile for air sources	Average house price, urbanity, location, type of amenity / disamenity, litigation, information disclosure, unemployment rate, sample size, logged distance, study methodology, many observations, year of data	Logged sample size as moderator, moderator "Lots of observations"	E.g., air pollution sites, landfills, nuclear power plants, superfund sites, water pollution sites	Hedonic pricing, travel cost, contingent valuation, conjoint analysis	OLS with heteroscedasticity-robust standard errors, bootstrapping

*Notes:*

<sup>a</sup> This list only includes observations (and studies) based on the hedonic pricing method valuing waste-related disamenities if not indicated otherwise. Some studies also consider other methods or commodities that are not considered here.

<sup>b</sup> R. Simons and Saginor (2006): While the number of observations from hedonic studies is stated (164), the number of studies had to be calculated proportionally from other information in the study. On request, one of the authors confirmed that 42 is approximately the correct number, lacking exact figures.

<sup>c</sup> Lipscomb et al. (2013): On request, one author answered that they do not have access to information on the exact number of observations and studies using hedonic pricing for waste site effects any longer. The number of observations from using hedonic pricing is calculated from information in the text ("83% of observations").

**Table A.2:** Search query.

Search category	Synonyms / Wildcards
Residential property	("Real estate*" OR residential* OR propert* OR hous* OR apartment OR building OR condo* OR dwllin* OR home OR residence OR mansion* OR domicile OR flat)
Hedonic Price Model	(Hedonic* OR "repeat sale*" OR "Diff*in*Diff*")
Price	(Price* OR price* OR value* OR benfi* OR cost OR premium OR worth OR compensat* OR damag* OR pay* OR sum)
Waste site	(Waste* OR refuse OR garbage OR rubbish OR litter OR trash OR junk OR debris OR dump* OR landfill* OR plant* OR site* OR depot* OR junkyard* OR station* OR transfer* OR facilit* OR cesspool* OR incinerat* OR brownfield*) (Hazardous* OR dangerous* OR contaminat* OR odor OR smell OR pollut* OR perilous* OR toxic* OR noxious* OR poisonous* OR harmful* OR pernicious* OR nuclear* OR radioactive* OR aquatic* OR recycling* OR recover*)

Notes: Search term categories are interconnected via the Boolean operator "AND".

**Table A.3:** Excluded studies sorted by reason for exclusion (N=165).**Duplicated results (N=3)**

T. Eshet et al. (2007a). "Exploring Benefit Transfer. Disamenities of Waste Transfer Stations". In: *Environmental & Resource Economics* 37.3, pp. 521–547.

T. Eshet et al. (2007b). "Externalities from landfills and transfer stations. Applying and testing the benefit transfer method". In: *Landfill research focus*. Ed. by E. C. Lehmann. New York, NY: Nova Science, pp. 153–198. ISBN: 978-1-60021-775-3.

D. J. Phaneuf et al. (2013). "Combining Revealed and Stated Preference Data to Estimate Preferences for Residential Amenities. A GMM Approach". In: *Land Economics* 89.1, pp. 30–52.

**Distance variable is binary (n=40)**

O. A. Akinjare et al. (2011b). "Monetary Implication of Environmental Disamenities on Housing Investment in Lagos State: The Ojota Scenario". In: *Mediterranean Journal of Social Sciences* 2.

M. Bléhaut (2014). *How does a change in risk perception affect the housing market? Evidence from an industrial accident in France*. European Regional Science Association.

S. Bond (2000). "A Hedonic Housing Model for Stigma Assessment. The Case of a remediated contaminated site in Perth WA". in: *New Zealand Institute of Valuers*, pp. 4–13.

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J. I. Carruthers and B. Mundy, eds. (2006). *Environmental valuation: interregional and intraregional perspectives*. Aldershot [u.a.]: Ashgate. ISBN: 0-7546-4471-5.

R. Coulomb and Y. Zylberberg (2016). *Rare Events and Risk Perception. Evidence from Fukushima Accident*.

J. Currie et al. (2016). "Environmental Health Risks and Housing Values. Evidence from 1,600 Toxic Plant Openings and Closings". In: *American Economic Review* 105.2, pp. 678–709. doi: \url{10.1257/aer.20121656}.

**Table A.3:** (continued).

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**Study is in Korean (N=3)<sup>35</sup>**

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<sup>35</sup> In the preliminary dataset, seven studies are written in non-English languages, including the listed three Korean studies. The one study in Spanish (Mardones, 2006) has an English summary and the one study in Hebrew (EMC, 1996) is well documented in Eshet et al. (2006) rendering an assessment possible. Both had to be excluded as the former analyses a nonpoint source pollution and the latter does not apply the hedonic pricing method. The remaining three studies are in German and French; Languages in which I am sufficiently proficient for this purpose. Ultimately, one study in German (Ballmer, 2011) and one study in French (Travers et al., 2009) are in the final dataset; the second study in French (Bléhaut, 2015) does not use the hedonic pricing method. All of these studies are listed in their respective categories in Table A.3 and Table A.4.

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**Table A.5:** Basic characteristics of included studies.

Author(s)	# Observations	Type of waste site	Land	Sample size
Affuso et al. (2010)	2	hazardous incinerator	USA	4783
Ballmer (2011)	1	nuclear power plant	Switzerland	30307
Bilbao-Terol (2009)	2	hazardous industrial facility	Spain	530
Braden et al. (2008b)	4	hazardous superfund	USA	3474
Braden et al. (2004)	1	hazardous harbour	USA	594
Braden et al. (2008a)	5	hazardous river / non-hazardous landfill	USA	2168
Brasington and Hite (2005)	6	various hazardous waste sites	USA	524-1580
Cameron (2006)	20	hazardous superfund	USA	5498
Carruthers and Clark (2010)	15	hazardous superfund and others (haz and non haz)	USA	29165
Carruthers et al. (2010)	40	hazardous superfund and others (haz and non haz)	USA	7871-30682
Chattopadhyay et al. (2005)	1	hazardous superfund	USA	440
Chen (2017)	3	non-hazardous river	China	968
Clark and Allison (1999)	5	nuclear power plant	USA	758
Clauw (2007)	7	hazardous waste site	Belgium	985
Cordera et al. (2019)	4	hazardous industrial facility	Italy	473
Dale et al. (1999)	10	hazardous industrial facility	USA	18180-70328
Deaton and Hoehn (2004)	3	hazardous superfund	USA	4280
Decker et al. (2005)	4	hazardous industrial facility	USA	6096
Dickes et al. (2013)	1	nuclear power plant	USA	826
du Preez et al. (2016)	1	non-hazardous landfill	South Africa	134
Du Preez and Lottering (2009)	1	non-hazardous landfill	South Africa	496
Eshet et al. (2007b)	15	non-hazardous waste transfer station	Israel	346-9505
Eyckmans et al. (2013)	2	non-hazardous industrial facility	Belgium	1420
Ferrara et al. (2007)	2	hazardous waste site	Canada	390
Gayer (2000)	2	hazardous superfund	USA	6562
Gayer et al. (2000)	4	hazardous superfund	USA	16928
Grislain-Letrémy and Katosky (2014)	6	hazardous industrial facility	France	584-1423
Hao (2008)	1	hazardous brownfield	USA	8078
Herriges et al. (2005)	4	non-hazardous CAFO	USA	1145
Hodge (2011)	8	hazardous industrial facility	USA	885

**Table A.5:** (continued).

Author(s)	# Observations	Type of waste site	Land	Sample size
Hurd (2002)	6	hazardous superfund	USA	179-475
Hwang (2003)	1	hazardous industrial facility	USA	321
Kaufman and Cloutier (2006)	4	hazardous brownfield	USA	890
Kiel (1995)	6	hazardous superfund	USA	106-689
Kiel (2006)	8	hazardous superfund	USA	105-1081
Kiel and McClain (1996)	5	non-hazardous industrial facility / incinerator	USA	131-507
Kiel and McClain (1995a)	8	non-hazardous incinerator	USA	302-2593
Kiel and McClain (1995b)	1	non-hazardous incinerator	USA	2593
Kiel and Williams (2007)	257	hazardous superfund	USA	9-4795
Kiel and Zabel (2001)	12	hazardous superfund	USA	106-682
Kim et al. (2017)	26	hazardous superfund	USA	781
Kim (2009)	19	hazardous / non-hazardous brownfield / industrial facility	USA	5933-8458
Kinnaman (2009)	1	non-hazardous landfill	USA	533
Klaiber and Smith (2013)	1	hazardous superfund	USA	242827
Lee et al. (2008)	3	hazardous industrial facility	USA	11597-48906
Leggett and Bockstael (2000)	12	non-hazardous industrial facility	USA	1183
McCluskey and Rausser (2003)	4	hazardous industrial facility	USA	205397
McMillen and Thorsnes (2003)	6	hazardous industrial facility	USA	11365
McMillen and Thorsnes (2000)	1	hazardous industrial facility	USA	8746
Mhatre (2009)	12	hazardous superfund	USA	185-997
Michaels and Smith (1990)	5	hazardous waste site	USA	268-2182
Mihaescu and vom Hofe (2012)	3	hazardous brownfield	USA	1607
Mihaescu and vom Hofe (2013)	2	hazardous brownfield	USA	6863
Munroe (2007)	5	hazardous brownfield	USA	6438-53444
Neelawala et al. (2012)	5	hazardous industrial facility	Australia	284

**Table A.5:** (continued).

Author(s)	# Observations	Type of waste site	Land	Sample size
Nelson et al. (1997)	3	non-hazardous landfill	USA	65-436
Nelson et al. (1992)	1	non-hazardous landfill	USA	708
Neupane and Gustavson (2008)	1	hazardous waste site	Canada	2212
Park and Lah (2006)	3	non-hazardous waste-to-energy plant	South Korea	744
Parisot (2007)	1	hazardous incinerator	USA	9655
Poor et al. (2007)	2	non-hazardous landfill	USA	1231-1376
Pugh (2008)	4	hazardous brownfield	USA	916-1078
Ready (2010)	3	Non-hazardous landfills	USA	11090
Reichert et al. (1992)	2	non-hazardous landfill	USA	573-963
Schmalensee et al. (1975)	24	non-hazardous landfill	USA	61-447
Seok Lim and Missios (2007)	3	non-hazardous landfill	Canada	331-1470
Shelem et al. (2011)	4	hazardous industrial facility	Israel	250
Smolen et al. (1992)	2	nuclear waste site	USA	87
Sun et al. (2017)	6	non-hazardous incinerator	China	167-2119
Sun and Jones (2013)	1	hazardous brownfield	USA	4421
Thayer et al. (1992)	2	hazardous waste site	USA	2323
Travers et al. (2009)	1	hazardous industrial facility	France	228
van Dijk et al. (2016)	2	non-hazardous wastewater treatment	Switzerland	85684
Walsh and Mui (2017)	14	various hazardous waste sites	USA	4871-6429
Winstrand (2010)	8	non-hazardous industrial facility	Sweden	430
Yamane et al. (2013)	7	nuclear power plant	Japan	310-620
Yukutake and Sugawara (2017)	16	nuclear power plant	Japan	7529
Zegarac and Muir (1998)	8	hazardous landfill	Canada	53-196
Zhang (2010)	1	hazardous industrial facility	USA	321
Zhang et al. (2010)	2	hazardous industrial facility	USA	321
Zhao et al. (2016a)	4	non-hazardous incinerator	China	70-2258
Zhao et al. (2016b)	1	non-hazardous incinerator	China	591
Zuindeau and Letombe (2008)	8	hazardous brownfield	France	622

**Table A.6:** Calculation of effect size for different functional forms of primary studies.

Model	Simplified representation	Elasticity ( $= \frac{dP}{dX} * \frac{X}{P}$ )
Linear	$P = \beta_0 + \beta_1 * X$	$\beta_1 * (\frac{X}{P})$
Lin-inverse	$P = \beta_0 + \beta_1 * \frac{1}{X}$	$-\beta_1 * (\frac{1}{XP})$
Lin-log	$P = \beta_0 + \beta_1 * \log(X)$	$\beta_1 * (\frac{1}{P})$
Lin-log(inverse)	$P = \beta_0 + \beta_1 * \log(\frac{1}{X})$	$-\beta_1 * (\frac{1}{P})$
Log-lin	$\log(P) = \beta_0 + \beta_1 * X$	$\beta_1 * X$
Log-inverse	$\log(P) = \beta_0 + \beta_1 * \frac{1}{X}$	$-\beta_1 * (\frac{1}{X})$
Log-log	$\log(P) = \beta_0 + \beta_1 * \log(X)$	$\beta_1$
Log-log(inverse)	$\log(P) = \beta_0 + \beta_1 * \log(\frac{1}{X})$	$-\beta_1$
Log-exp	$\log(P) = \beta_0 + \beta_1 * \exp -X$	$-\beta_1 X * \exp -X$

*Notes:* Own representation based on Gujarati (2009). If the calculation of the elasticity depends on the respective values for the price  $P$  or the distance to the waste site  $X$  or both, the reported mean values were chosen. If estimation included spatial lag parameters, the effect size is corrected for the spatial spillover effect (Golgher and Voss, 2016).

This representation omits all other control variables to show the effect size calculation in a simplified way. Virtually all studies considered, used several other regressors next to the distance from a waste site. I calculated semi-elasticities as cross-validating step to check for calculation errors.

**Table A.7:** Tests for the selection of the meta-regression model.

Test	Meta-analysis		Meta-regression-analysis	
	FAT-PET	PEESE	FAT-PET	PEESE
<b>Q-test</b>				
$H_0$ : No excess heterogeneity at estimate level	$Q = 11350$ $p = 0.000$	$Q = 13448$ $p = 0.000$	$Q = 5658$ $p = 0.000$	$Q = 5799$ $p = 0.000$
<b>BPLM-test</b>				
$H_0$ : No unobserved effects at study level			$BP = 0.00$ $p = 0.478$	$BP = 0.04$ $p = 0.420$

*Notes:* The Q-test and the BPLM test are  $\chi^2$ -distributed under the null of no excess heterogeneity at the estimate level and no unobserved effects at the study level, respectively. Q and BP denote the test statistics for the Q-test and the BPLM test, respectively. P-values are given below the coefficients.

**Table A.8:** Robustness regressions - Model specification and outliers.

Variables	(1) WLS-RE FAT-PET	(2) WLS-FE FAT- PET	(3) Outlier removed	(4) Trimmed 98%	(5) Trimmed 95%	(6) Trimmed 90%	(7) Sample-reuse weight	(8) Study means
Mean effect	0.082*** (0.030)	0.080*** (0.029)	0.091*** (0.032)	0.103*** (0.032)	0.090*** (0.033)	0.067** (0.027)	0.110** (0.044)	0.033 (0.029)
SE <sup>2</sup>			0.923*** (0.294)	1.623** (0.732)	1.850** (0.749)	2.287* (1.175)	0.992*** (0.106)	
SE	0.544*** (0.116)	0.531*** (0.117)						0.976*** (0.357)
Published	0.006 (0.008)	0.005 (0.006)	0.008 (0.008)	0.007 (0.009)	0.008 (0.008)	0.007 (0.008)	0.019 (0.013)	0.026 (0.020)
Year publish	0.001* (0.001)	0.001* (0.001)	0.001 (0.001)	0.001* (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.002 (0.001)
<b>Site characteristics</b>								
Multiple sites	0.041*** (0.014)	0.040*** (0.014)	0.024* (0.012)	0.037** (0.014)	0.023* (0.012)	0.025** (0.012)	0.047** (0.020)	
<i>NPL (reference category: Not USA)</i>								
Not on NPL	0.038 (0.041)	0.039 (0.035)	0.027 (0.040)	0.035 (0.039)	0.024 (0.041)	0.019 (0.038)	0.042 (0.041)	
On NPL	0.040 (0.045)	0.049 (0.038)	0.030 (0.043)	0.034 (0.044)	0.025 (0.044)	0.018 (0.041)	0.047 (0.044)	
Unclear	0.047 (0.047)	0.042 (0.041)	0.023 (0.047)	0.044 (0.045)	0.021 (0.048)	0.017 (0.045)	0.059 (0.050)	
<i>Activity status (reference category: Active)</i>								
Inactive	-0.026 (0.029)	-0.015 (0.024)	-0.003 (0.016)	-0.024 (0.029)	-0.007 (0.015)	-0.012 (0.014)	-0.034 (0.028)	
Unclear	-0.041 (0.033)	-0.021 (0.020)	-0.014 (0.021)	-0.039 (0.033)	-0.016 (0.021)	-0.018 (0.020)	-0.037 (0.037)	

**Table A.8:** (continued).

Variables	(1) WLS-RE FAT-PET	(2) WLS-FE FAT- PET	(3) Outlier removed	(4) Trimmed 98%	(5) Trimmed 95%	(6) Trimmed 90%	(7) Sample-reuse weight	(8) Study means
<i>Employment opportunities (reference category: Employment opportunities)</i>								
No employment opportunities	-0.013 (0.024)	-0.019 (0.017)	-0.020 (0.015)	-0.014 (0.024)	-0.020 (0.014)	-0.011 (0.013)	-0.013 (0.031)	
Unclear	-0.005 (0.031)	-0.017 (0.022)	-0.025 (0.023)	-0.007 (0.031)	-0.025 (0.022)	-0.014 (0.021)	-0.009 (0.042)	
<i>Type of waste site (reference category: Hazardous)</i>								
Non-hazardous	-0.037 (0.026)	-0.036 (0.024)	-0.037* (0.020)	-0.042 (0.026)	-0.042** (0.020)	-0.034* (0.018)	-0.046 (0.036)	
Nuclear	-0.023 (0.069)	0.001 (0.061)	-0.034 (0.058)	-0.018 (0.068)	-0.044 (0.055)	-0.054 (0.045)	-0.041 (0.060)	
<i>Waste-affected element (reference category: Soil)</i>								
Air	0.043** (0.017)	0.039** (0.016)	0.038** (0.017)	0.043** (0.018)	0.039** (0.017)	0.030* (0.016)	0.035* (0.020)	
Water	-0.005 (0.017)	-0.003 (0.016)	-0.011 (0.017)	-0.007 (0.018)	-0.012 (0.017)	-0.015 (0.017)	0.008 (0.010)	
Unclear	-0.017 (0.014)	-0.021 (0.014)	-0.010 (0.012)	-0.018 (0.014)	-0.009 (0.012)	-0.011 (0.012)	-0.046** (0.021)	
<i>Continent (Reference category: Europe)</i>								
North America	-0.033 (0.044)	-0.032 (0.038)	-0.035 (0.042)	-0.033 (0.043)	-0.032 (0.043)	-0.024 (0.040)	-0.043 (0.048)	-0.027 (0.028)
Asia	-0.010 (0.026)	-0.014 (0.023)	-0.008 (0.025)	-0.014 (0.027)	-0.003 (0.026)	0.018 (0.021)	-0.008 (0.031)	0.023 (0.061)
Other	0.012 (0.023)	0.017 (0.019)	0.017 (0.023)	0.004 (0.024)	0.022 (0.023)	0.040* (0.021)	0.018 (0.035)	-0.032 (0.034)
<i>Clean-up stage (Reference category: Recognised, no clean-up plan exists)</i>								



**Table A.8:** (continued).

Variables	(1) WLS-RE FAT-PET	(2) WLS-FE FAT- PET	(3) Outlier removed	(4) Trimmed 98%	(5) Trimmed 95%	(6) Trimmed 90%	(7) Sample-reuse weight	(8) Study means
Not recognised	-0.009 (0.015)	-0.002 (0.012)	-0.004 (0.011)	-0.008 (0.014)	-0.006 (0.011)	-0.009 (0.010)	-0.002 (0.010)	
Clean-up begun, but not finished	-0.009 (0.016)	-0.017 (0.015)	-0.008 (0.011)	-0.007 (0.015)	-0.003 (0.011)	0.003 (0.010)	-0.001 (0.007)	
Cleaned-up	-0.055* (0.028)	-0.053** (0.026)	-0.025 (0.017)	-0.057** (0.028)	-0.023 (0.017)	-0.017 (0.015)	-0.024 (0.023)	
Unclear	0.011 (0.018)	0.014 (0.016)	0.002 (0.016)	0.005 (0.018)	-0.001 (0.015)	0.003 (0.014)	0.030 (0.018)	
<b>Data characteristics</b>								
Dist greater mean	-0.027* (0.014)	-0.024* (0.014)	-0.008 (0.017)	-0.023 (0.014)	-0.005 (0.018)	-0.002 (0.017)	-0.041*** (0.007)	
Miles km								0.011 (0.019)
Sample	-0.001*** (0.000)	-0.001*** (0.000)	-0.000 (0.000)	-0.001*** (0.000)	0.000 (0.000)	0.000 (0.000)	-0.001** (0.000)	
Sale ind	-0.024 (0.019)	-0.020 (0.018)	-0.014 (0.016)	-0.025 (0.018)	-0.012 (0.016)	-0.008 (0.014)	-0.034* (0.019)	
<b>Methodology: Econometric specification</b>								
Num expl	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.001)	
Oth disamen	0.006 (0.011)	0.004 (0.010)	-0.003 (0.011)	0.002 (0.011)	-0.003 (0.011)	-0.003 (0.011)	0.010 (0.016)	
Oth amen	-0.002 (0.016)	-0.003 (0.017)	-0.003 (0.015)	-0.000 (0.017)	-0.005 (0.015)	-0.016 (0.014)	-0.007 (0.020)	
Access	-0.013	-0.016	-0.002	-0.011	-0.002	0.005	-0.013	-0.027

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**Table A.8:** (continued).

Variables	(1) WLS-RE FAT-PET	(2) WLS-FE FAT- PET	(3) Outlier removed	(4) Trimmed 98%	(5) Trimmed 95%	(6) Trimmed 90%	(7) Sample-reuse weight	(8) Study means
Industry	(0.012) -0.029*	(0.010) -0.024	(0.013) -0.017	(0.012) -0.026	(0.013) -0.015	(0.012) -0.012	(0.016) -0.012	(0.021)
Demoecon	(0.017) -0.031**	(0.014) -0.025*	(0.014) -0.021*	(0.017) -0.031**	(0.014) -0.020	(0.014) -0.024**	(0.020) -0.041**	
Time control	(0.014) 0.022	(0.015) 0.017	(0.012) 0.016	(0.014) 0.022	(0.012) 0.013	(0.011) 0.017	(0.016) 0.037*	
Direction	(0.016) 0.005	(0.015) 0.012	(0.015) 0.007	(0.016) 0.010	(0.015) 0.006	(0.015) 0.011	(0.020) -0.016	
Interaction	(0.016) -0.046***	(0.015) -0.045***	(0.018) -0.041***	(0.016) -0.046***	(0.018) -0.041***	(0.018) -0.039***	(0.027) -0.025	
	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.019)	
<b>Methodology: Estimation strategy</b>								
Log log	-0.013 (0.012)	-0.014 (0.012)	-0.010 (0.011)	-0.012 (0.012)	-0.009 (0.011)	-0.006 (0.010)	0.007 (0.016)	
OLS spatial	-0.002 (0.024)	-0.008 (0.022)	-0.012 (0.023)	-0.008 (0.026)	-0.011 (0.022)	-0.004 (0.020)	-0.023 (0.031)	
<i>N</i>	727	727	721	713	691	655	727	83
<i>R</i> <sup>2</sup>	0.283	0.258	0.288	0.276	0.323	0.350	0.221	0.373

*Notes:* Regression results are based on WLS. Standard errors clustered at the study level are given in parentheses below the coefficients. \*\*\*, \*\* and \* denote coefficients significantly different from zero at the 1-, 5- and 10-percent levels. In all columns, robustness regressions are shown, using a different publication bias control or estimator, trimmed datasets or different weighting schemes relative to the baseline specification in column (1) of Table 1.4.

**Table A.9:** Robustness regressions - Subsamples and replaced moderators.

Variables	(1) Miles km	(2) HDI	(3) Hazardous waste site	(4) Significant variables	(5) No interaction	(6) GDP	(7) North Amer- ica	(8) North America – FE
Mean effect	0.110*** (0.032)	0.118** (0.045)	0.140*** (0.037)	0.122*** (0.040)	0.114*** (0.041)	0.100*** (0.035)	0.030 (0.049)	0.097 (0.073)
SE <sup>2</sup>	0.848*** (0.277)	1.038 (0.763)	0.785*** (0.234)	0.875 (0.816)	0.709** (0.309)	0.495 (0.627)		
SE							0.547*** (0.118)	0.575*** (0.125)
Published	0.013 (0.008)	0.001 (0.011)	0.007 (0.009)	0.019* (0.010)	-0.004 (0.009)	0.015 (0.010)	0.005 (0.009)	0.005 (0.010)
Year publish	0.001 (0.001)	0.006** (0.003)	-0.000 (0.001)	0.002** (0.001)	0.003*** (0.001)	0.002** (0.001)	0.001 (0.001)	0.001 (0.001)
<b>Site characteristics</b>								
Multiple sites	0.039** (0.015)	0.041** (0.018)	0.060*** (0.020)	0.014 (0.013)	0.029* (0.015)	0.029* (0.016)	0.036** (0.017)	0.035 (0.021)
HDI		0.043 (0.162)						
GDP						-0.000 (0.000)		
<i>NPL (reference category: Not USA)</i>								
Not on NPL	0.040 (0.038)	0.009 (0.023)	0.038 (0.058)	0.037 (0.035)	0.038 (0.032)	0.024 (0.023)	0.034 (0.040)	-0.049 (0.054)
On NPL	0.039 (0.043)	0.039 (0.034)	0.038 (0.061)	0.034 (0.040)	0.032 (0.037)	0.002 (0.030)	0.033 (0.044)	-0.049 (0.058)
Unclear	0.046 (0.045)	0.024 (0.038)	0.043 (0.063)	0.036 (0.043)	0.052 (0.041)	0.020 (0.031)	0.044 (0.046)	-0.033 (0.065)
<i>Activity status (reference category: Active)</i>								
Inactive	-0.023 (0.031)	0.027 (0.032)	-0.061 (0.039)	0.016 (0.023)	-0.058* (0.033)	-0.010 (0.030)	-0.033 (0.034)	-0.045 (0.038)

**Table A.9:** (continued).

Variables	(1) Miles km	(2) HDI	(3) Hazardous waste site	(4) Significant variables	(5) No interaction	(6) GDP	(7) North Amer- ica	(8) North America – FE
Unclear	-0.027 (0.031)	-0.011 (0.028)	-0.057 (0.038)	-0.014 (0.036)	-0.091** (0.042)	-0.032 (0.036)	-0.032 (0.036)	-0.036 (0.036)
<i>Employment opportunities (reference category: Employment opportunities)</i>								
No employment opportunities	-0.016 (0.024)	-0.031 (0.024)	-0.007 (0.031)	-0.031 (0.025)	0.011 (0.031)	-0.025 (0.028)	0.006 (0.029)	0.007 (0.032)
Unclear	-0.016 (0.030)	-0.041 (0.034)	0.021 (0.040)	-0.017 (0.032)	0.020 (0.038)	-0.002 (0.035)	0.003 (0.038)	0.020 (0.050)
<i>Type of waste site (reference category: Hazardous)</i>								
Non-hazardous	-0.045 (0.028)	-0.025 (0.025)		-0.032 (0.021)	-0.034 (0.029)	-0.056** (0.027)	-0.034 (0.033)	-0.035 (0.033)
Nuclear	-0.018 (0.069)	-0.008 (0.071)		-0.155 (0.101)	-0.033 (0.066)	-0.043 (0.069)	0.028 (0.174)	0.000 (0.179)
<i>Waste-affected element (reference category: Soil)</i>								
Air	0.042** (0.018)	0.036 (0.023)	0.038 (0.029)	0.018 (0.018)	0.025 (0.018)	0.027* (0.015)	0.008 (0.018)	-0.001 (0.019)
Water	-0.007 (0.017)	-0.001 (0.030)	0.002 (0.017)	-0.044** (0.021)	-0.004 (0.017)	-0.038 (0.023)	-0.007 (0.020)	-0.002 (0.018)
Unclear	-0.014 (0.014)	0.003 (0.016)	-0.047** (0.020)	0.000 (0.013)	-0.022 (0.013)	-0.017 (0.014)	-0.035** (0.017)	-0.040* (0.023)
<i>Continent (Reference category: Europe)</i>								
North America	-0.031 (0.040)		-0.028 (0.068)	-0.037 (0.038)	-0.023 (0.038)			
Asia	-0.004 (0.026)		-0.002 (0.058)	0.006 (0.027)	0.028 (0.030)			
Other	0.014 (0.024)		0.024 (0.035)	-0.013 (0.022)	0.015 (0.026)			
<i>Clean-up stage (Reference category: Recognised, no clean-up plan exists)</i>								
Not recognised	-0.008	-0.039	-0.013	-0.012	-0.012	-0.022	-0.004	-0.003

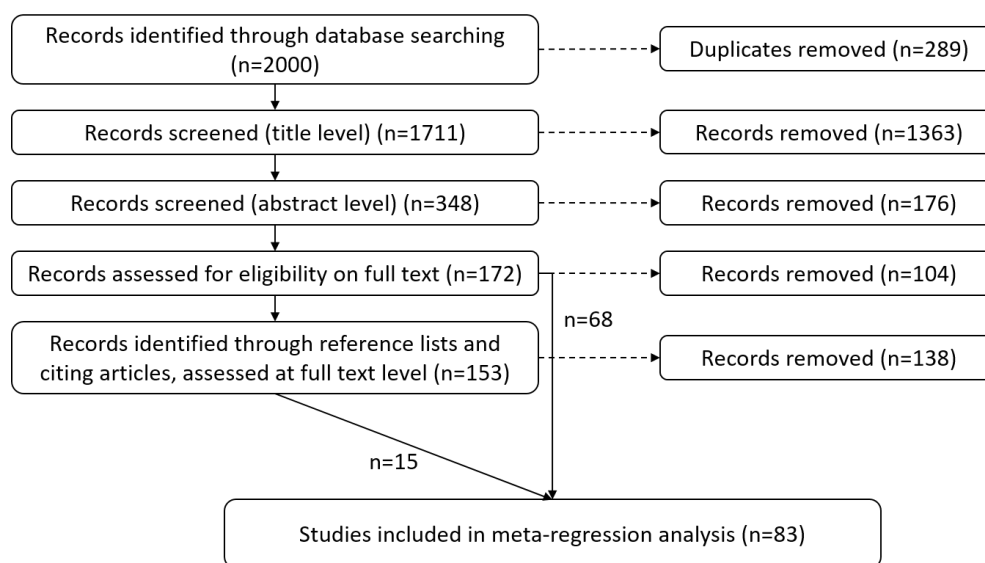
**Table A.9:** (continued).

Variables	(1) Miles km	(2) HDI	(3) Hazardous waste site	(4) Significant variables	(5) No interaction	(6) GDP	(7) North Amer- ica	(8) North America – FE
	(0.014)	(0.057)	(0.017)	(0.035)	(0.016)	(0.035)	(0.013)	(0.013)
Clean-up begun, but not finished	-0.010 (0.015)	-0.062** (0.028)	0.002 (0.013)	-0.017 (0.020)	0.001 (0.013)	-0.020 (0.024)	0.002 (0.012)	-0.003 (0.012)
Cleaned-up	-0.062** (0.027)	-0.085*** (0.026)	-0.047* (0.025)	-0.054*** (0.020)	-0.042* (0.024)	-0.088*** (0.028)	-0.049* (0.027)	-0.052* (0.027)
Unclear	-0.001 (0.017)	0.008 (0.017)	-0.000 (0.018)	-0.010 (0.015)	0.022 (0.016)	-0.021 (0.016)	0.010 (0.020)	0.004 (0.021)
<b>Data characteristics</b>								
Dist greater mean		-0.038 (0.025)	-0.033*** (0.011)	0.006 (0.017)	-0.037*** (0.011)	0.002 (0.014)	-0.034*** (0.013)	-0.030 (0.018)
Miles km	-0.016 (0.015)							
Sample	-0.001*** (0.000)	-0.001** (0.000)	-0.001** (0.000)	-0.001*** (0.000)	-0.001** (0.000)	-0.001*** (0.000)	-0.001** (0.000)	-0.001** (0.000)
Sale ind	-0.025 (0.018)	-0.035* (0.021)	-0.039* (0.023)	-0.013 (0.015)	-0.014 (0.019)	-0.024 (0.019)	-0.013 (0.020)	-0.000 (0.021)
<b>Methodology: Econometric specification</b>								
Num expl	0.000 (0.000)	-0.001 (0.001)	0.000 (0.000)		0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Num sig var				0.001 (0.001)				
Oth disamen	-0.004 (0.013)	0.016 (0.017)	0.006 (0.015)	-0.023* (0.013)	-0.013 (0.011)	-0.004 (0.013)	0.018 (0.014)	0.015 (0.016)
Oth amen	-0.002 (0.017)	0.005 (0.017)	-0.016 (0.020)	0.007 (0.015)	-0.013 (0.016)	0.005 (0.016)	-0.017 (0.015)	0.003 (0.016)
Access	-0.012 (0.012)	-0.033 (0.020)	-0.008 (0.015)	-0.011 (0.014)	0.001 (0.013)	-0.013 (0.012)	0.004 (0.012)	0.007 (0.017)
Industry	-0.020	-0.019	-0.036*	0.000	-0.018	-0.016	-0.031	-0.026

**Table A.9:** (continued).

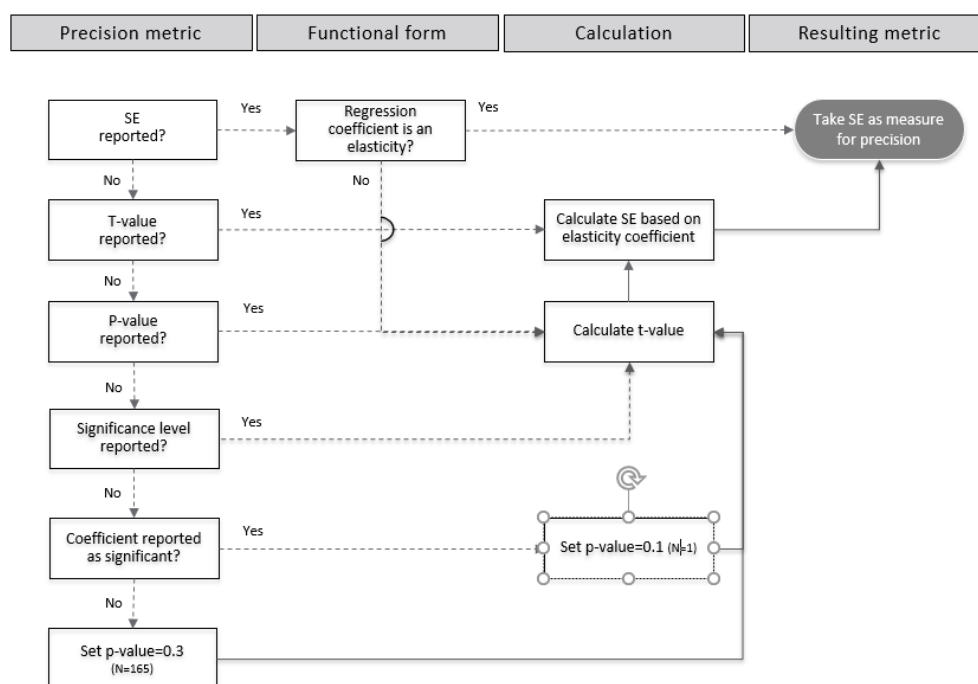
Variables	(1) Miles km	(2) HDI	(3) Hazardous waste site	(4) Significant variables	(5) No interaction	(6) GDP	(7) North Amer- ica	(8) North America – FE
	(0.016)	(0.022)	(0.020)	(0.015)	(0.019)	(0.019)	(0.020)	(0.023)
Demoecon	-0.031**	-0.057***	-0.026	-0.045**	-0.046***	-0.028*	-0.029*	-0.034*
	(0.014)	(0.021)	(0.016)	(0.018)	(0.013)	(0.016)	(0.016)	(0.018)
Time control	0.022	0.038*	0.018	0.018	0.033**	0.028	0.031*	0.031
	(0.016)	(0.022)	(0.020)	(0.015)	(0.016)	(0.018)	(0.018)	(0.025)
Direction	0.013	0.021	0.032*	0.021	0.037*	0.023	-0.018	-0.030
	(0.014)	(0.019)	(0.018)	(0.013)	(0.021)	(0.017)	(0.020)	(0.026)
Interaction	-0.051***	-0.052***	-0.034***	-0.046***		-0.045***	-0.027*	-0.016
	(0.014)	(0.019)	(0.012)	(0.013)		(0.016)	(0.014)	(0.020)
<b>Methodology: Estimation strategy</b>								
Log log	-0.013	0.007	-0.006	-0.010	0.003	-0.006	-0.007	-0.004
	(0.012)	(0.019)	(0.014)	(0.011)	(0.011)	(0.012)	(0.012)	(0.014)
OLS spatial	-0.014	-0.042	-0.026	-0.025	-0.022	-0.012	-0.005	-0.007
	(0.025)	(0.029)	(0.027)	(0.025)	(0.029)	(0.027)	(0.028)	(0.035)
<i>N</i>	727	369	571	396	655	469	620	620
Sub-national-FE	NO	NO	NO	NO	NO	NO	NO	YES
<i>R</i> <sup>2</sup>	0.255	0.467	0.263	0.406	0.299	0.335	0.226	0.255

*Notes:* Regression results are based on WLS. Standard errors clustered at the study level are given in parentheses below the coefficients. \*\*\*, \*\* and \* denote coefficients significantly different from zero at the 1-, 5- and 10-percent levels. In all columns, subsample results are depicted using moderators with missing observation or distinct subsets of observations, respectively. There are more subsample regressions available on request. These subsample regressions replace *Num expl* with subsets reflecting the type of explanatory variables (house structure; neighbourhood; environment), only include observations if *Time control* = 1 and *Num expl* ≥ 10, respectively. Additionally, p-values were changed for the cases applicable as described in the notes of Figure A.2. The results provided no additional information.

**Figure A.1:** PRISMA Diagram.

*Notes:* The PRISMA diagram (Moher et al., 2009) shows the study selection process from initial screening to full-text-level assessment.

Figure A.2: Standardization of precision measure.



Notes: Standardization based on (Stanley and Doucouliagos, 2012). If t-values were not reported, but exact p-values instead, the t-values were derived via the Excel-function "T.INV.2S"<sup>a</sup> under consideration of the degrees of freedom<sup>b</sup> of the respective regression. If only significance levels were reported, the conservative threshold was taken, e.g., for statistical significance at the 5%-level a p-value of 0.05 was assumed. If only information on statistical significance was given, not revealing its level, again the most conservative level in the range of conventional significance thresholds was taken (p=0.1). If the regression coefficient was reported as non-significant without further information, p=0.3 was assumed.<sup>c</sup> These assumptions certainly introduce some measurement error. However, the missing p-values were filled up with values on the conservative side of the spectrum, thus, resulting standard errors are rather greater in magnitude, i.e., have smaller precision. Regardless of the exact magnitude, these estimates received smaller weight in the following meta-regressions and are less influential accordingly.

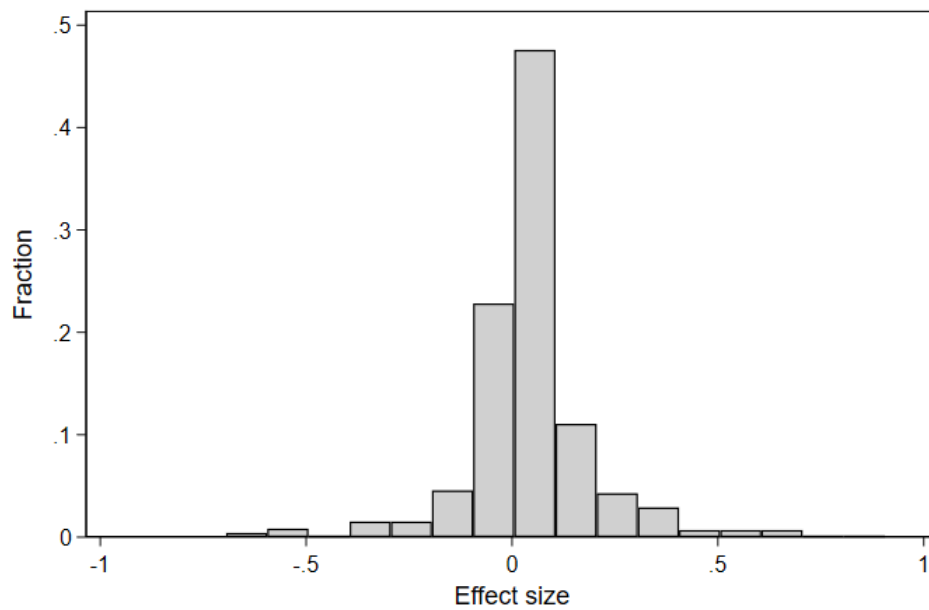
<sup>a</sup> This function only gives positive values regardless of the sign of the underlying coefficient. I corrected resulting t-values manually, if needed.

<sup>b</sup> For one paper (Kiel and Williams, 2007), the required information to calculate the degrees of freedom was not always computable as the number of regressors missed several times. In these cases, I assumed the average number of regressors in the study

<sup>c</sup> Stanley and Doucouliagos (2012) discuss several options to deal with non-significant coefficients without information on exact statistical significance. Omitting these estimates would reduce the sample size considerably (by 165 observations). Thus, I chose a p-value of 0.3 as conservative guess to conserve sample size.

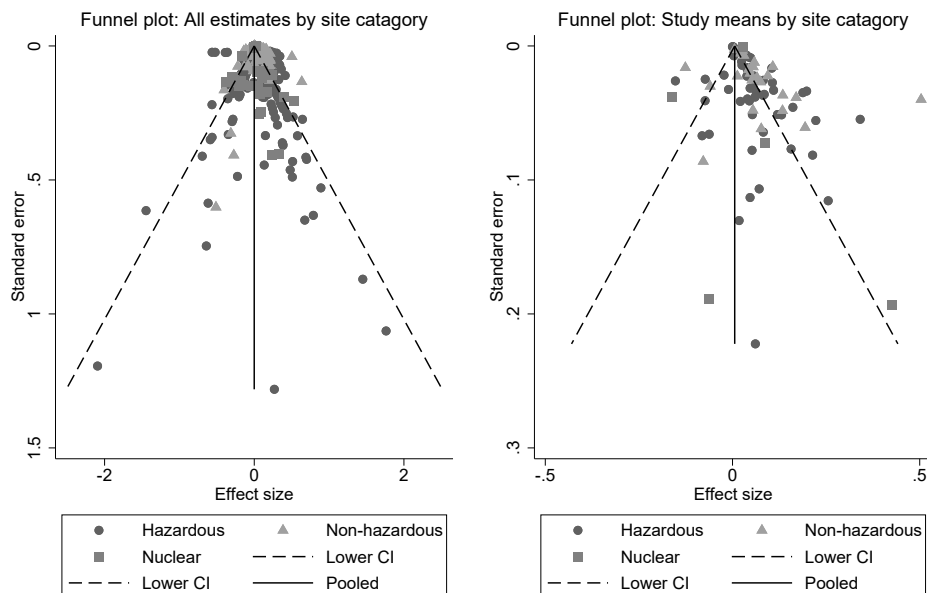


**Figure A.3:** Histogram of effect sizes.



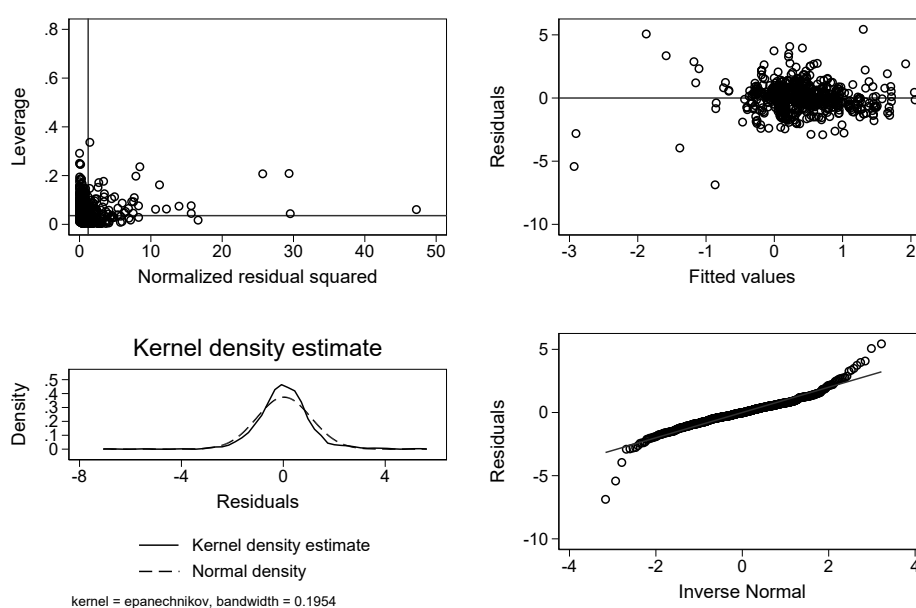
*Notes:* Four observations are outside the frame of this figure.

**Figure A.4:** Funnel plots.

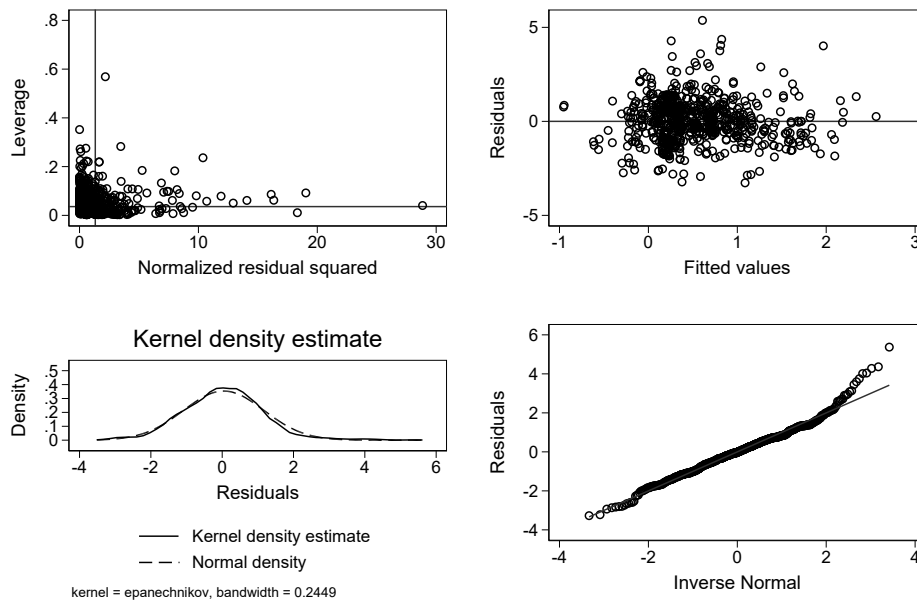


*Notes:* The funnel plots are based on all 727 observations on the left-hand side and on the respective 83 study means on the right-hand side. The solid line represents the pooled fixed effects estimate, while the dashed lines span a Pseudo 95% Confidence Interval.

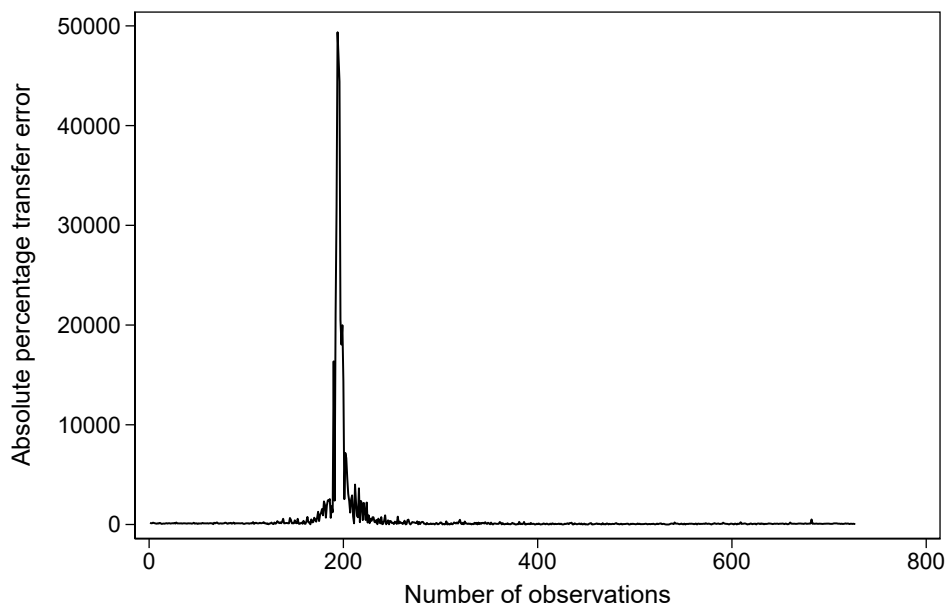
**Figure A.5:** Residual plots including outliers.



*Notes:* The upper left leverage-versus-squared-residual plot serves to detect potentially influential observations with large leverage and residuals. Here, observations rank high on one of the axis mostly. The upper right residual-versus fitted plots serves to detect heteroscedasticity. Here, there is the impression of mild heteroscedasticity due to outlying observations. The lower left kernel density plot shows some deviation from the normal distribution of residuals. The quantile-plot in the lower right points to non-normality at the tail of the residuals' distribution. Here, the regression diagnostics for the WLS-RE PEESE model are reported. The results for alternative specifications are virtually identical and are available on request.

**Figure A.6:** Residual plots excluding outliers.

*Notes:* The same type of residual plots as in Figure A.5 are displayed. Note the shrink of axes in all but one case, indicating a better overall fit. Notably, the impression of heteroscedasticity in the upper right plot vanishes as confirmed by both, the Breusch-Pagan/Cook-Weisberg and White test for heteroscedasticity. Overall, omitting six outliers resulted in moderate improvement of all residual plots.

**Figure A.7:** Absolute percentage transfer error.

*Notes:* One observation is outside the frame of this figure.

## Alternative publication selection bias methods

This section introduces alternative publication selection bias methods due to Andrews and Kasy (2019), Furukawa (2019), Ioannidis et al. (2017), Simonsohn et al. (2014a), and Stanley et al. (2010).<sup>36</sup>

### *Description of methods*

Andrews and Kasy (2019) introduced a nonlinear method that involves identifying publication probability conditional on the study's results. They reason that if publication selection distorts the true effect, jumps in the publication probability at common p-values signalling significance can be expected. They show that bias from selective publication can be corrected once the conditional publication probability is known. Their approach offers some degrees of freedom regarding the model parameters. For this study, I assume a significance level cut-off of 5% and a t-distribution of the effects (following Andrews and Kasy, 2019). Additionally, without knowing if the selection is based on significance alone or significance and sign of the effect, I test both alternatives. Furukawa (2019) offers a non-parametric, funnel-stem-based method to correct publication bias. This method chooses the optimal number of precise observations to include in calculating the average effect size, balancing bias and variance. The weighted average of the adequately powered (WAAP) proposed by Ioannidis et al. (2017) addresses publication selection bias by keeping only estimates that are sufficiently powered (>80%). By discarding inadequately powered estimates, the resulting weighted average should give a better, less-biased estimate of the true effect size. The p-curve approach by Simonsohn et al. (2014a) identifies publication selection in the form of p-hacking by the shape of the distribution of statistically significant p-values. It can also be used to assess the presence of a non-zero effect once publication selection is corrected for (Simonsohn et al., 2014b).<sup>37</sup> Lastly, Stanley et al. (2010) show that a rather simplistic estimator, the "Top 10", i.e., the average of the 10% most precise observations in the meta-dataset can

<sup>36</sup> I would like to thank one anonymous reviewer for suggesting using the Andrews and Kasy (2019) procedure and the p-curve approach by Simonsohn et al. (2014a). I apply both methods using the offered online apps, see <https://maxkasy.github.io/home/metastudy/> and <http://www.p-curve.com/app4/>, respectively. Additionally, Furukawa (2019) distributes the R-code for his method via [https://github.com/Chishio318/stem-based\\_method](https://github.com/Chishio318/stem-based_method). They are used as robustness checks for the FAT-PET-PEESE framework reported in Table 1.3

<sup>37</sup> While the p-curve method has intuitive appeal, some authors question its validity in certain situations. Bruns et al. (2016) use simulations to show that the "p-curve cannot reliably distinguish true effects and null effects with p-hacking in observational research". Moreover, Erdfelder and Heck (2019) voice a word of caution and discuss cases in which a right-skewed p-curve does not indicate true effects. In a formal reply to Bruns et al. (2016), Simonsohn et al. (2019) disagree with their conclusion, suggesting that their line of argumentation exhibits several flaws, confounding correlation and causation. In light of these conflicting positions, I decided to take caution when applying the p-curve and refrain from calculating a publication bias corrected effect size based on the p-curve method (Simonsohn et al., 2014b). Instead the results are considered as informative visual support for assessing the presence of publication selection bias through p-hacking. The p-curve is depicted in Figure A.8. Following the guidelines for valid p-curves (Simonsohn et al., 2014a), I include the P-Curve Disclosure Table that documents the selection of analysed results in Table A.11.

reduce publication selection bias and offer a more efficient estimate of the average effect size.

### *Results*

Table A.10 reports the results for all alternative publication bias correction methods that calculate a corrected mean effect size. The WAAP, Furukawa and Top10 estimates are at the lower bound of the spectrum with only the Top10 estimate being significantly different from zero (0.011). By design, however, these methods only use a subset of observations (23, eleven and 73 observations, respectively). The approaches using the full set of information generally correct the mean effect size less strongly. The Andrews and Kasy approach estimates a true average effect size of 0.019 if a symmetric selection mechanism is assumed, i.e., if selection takes place based on significance alone. Imposing instead that the selection mechanism favours significant positive estimates, i.e., expecting that waste sites reduce proximate residential property values, the mean effect is even further corrected downwards (0.008). The results for the p-curve are summarized in Figure A.8. The visual impression from the p-curve corroborates the findings from Table 1.3 and Table A.10. It indicates that there is a true empirical effect that cannot solely be attributed to selective reporting. However, the right skew of the p-curve also implies that it is not p-hacking that distorts the literature.

### *P-Curve Disclosure Table*

The following Table A.11 lists the results from every study in the meta-dataset that are used for the p-curve. This so called P-Curve Disclosure Table is a necessity for valid p-curves (Simonsohn et al., 2014a), see also the official user-guide (<http://p-curve.com/guide.pdf>). Following the guidelines, included p-values must meet three criteria: (i) They result from a test of the hypothesis of interest, (ii) have a uniform distribution under the null, and (iii) are statistically independent of other p-values in the p-curve. Evidently, the selection process of studies for this meta-analysis already ensured that the p-values included all stem from a test of the hypothesis of interest, i.e., the monetary influence from waste sites on proximate residential properties. This selection also ensured a uniform distribution under the null as studies that rely, e.g., on regression specifications with linear and squared distance as regressors are excluded, see Simonsohn et al. (2014a) for a discussion. Finally, the following steps are taken to ensure statistically independent p-values: If a study reports only one estimate, the condition is fulfilled, and the resulting p-value is considered. For studies reporting multiple estimates, the p-values are independent if the sample for each p-value is different, e.g., regression results for different waste sites or different points in time are reported. However, if multiple results stem from robustness checks in the form of, e.g., different functional forms or set of regressors, the p-values are not statistically independent. In these cases, I follow the suggestion of Simonsohn et al. (2014a) and use the first p-value in the study for a first p-curve and the last p-value in the study for a

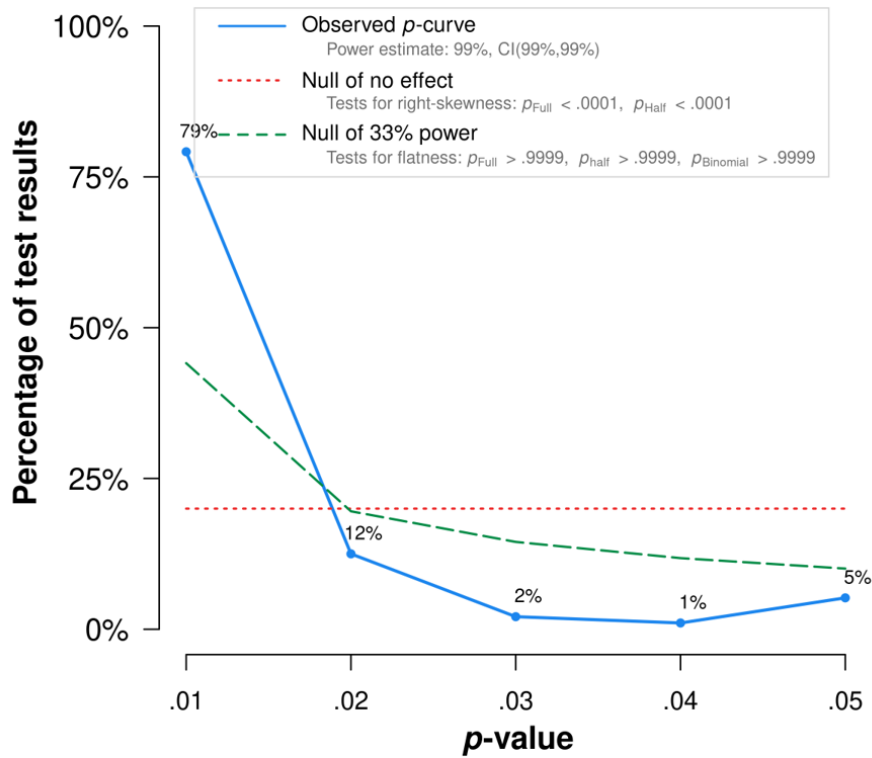
second p-curve as robustness check.

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Figure A.8: P-curve.



Note: The observed  $p$ -curve includes 96 statistically significant ( $p < .05$ ) results, of which 89 are  $p < .025$ . There were 49 additional results entered but excluded from  $p$ -curve because they were  $p > .05$ .

Notes: The  $p$ -curve was created using the  $p$ -curve online tool available at <http://p-curve.com/>. The P-Curve Disclosure Table is depicted in Table A.11.

**Table A.10:** Results for alternative tests of publication selection bias.

	(1) WLS-RE FAT-PET	(2) WLS-FE FAT-PET	(3) WLS-RE PEESE	(4) WLS-FE PEESE	(5) A&K (sym.)	(6) A&K (asym.)	(7) WAAP	(8) Furukawa	(9) Top10
Mean effect beyond bias SE	0.019*** (0.07)	0.015*** (0.05)	0.029*** (0.006)	0.023*** (0.005)	0.019*** (0.003)	0.008*** (0.003)	0.001 (0.001)	0.000 (0.000)	0.011*** (0.002)
SE <sup>2</sup>		0.382*** (0.129)	0.426*** (0.128)	0.660** (0.280)	0.857** (0.350)				
<i>N</i>	727	727	727	727	727	727	24	11	71
<i>R</i> <sup>2</sup>	0.113	0.113	0.099	0.091					

*Notes:* The table repeats the results of Table 1.3 in columns (1) to (4). In columns (5) to (9) the results for the other models controlling for publication bias are reported as described in the appendix. Standard errors are given in parentheses below the coefficients. They are clustered at the study level for columns (1) - (4). \*\*\*, \*\* and \* denote coefficients significantly different from zero at the 1-, 5- and 10-percent levels.

**Table A.11:** P-curve Disclosure Table.

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Affuso et al. (2010)	Our hypothesis is that lead pollution has a negative impact on property values, and we set out measure the size and statistical significance of the impact.	LR	RC	A location 1 km closer to the army depot resulted in a 2% decrease in the real market price. The marginal reduction in property value based on distance from the depot was approximately \$1,484 per km. (Table 3) 2LS model: ES=0.08, SE=0.003, t=5 WLS model: ES=0.02, SE=0.004, t=2.66	WLS: t(4771)=2.66, p=0.0076	2SLS: t(4770)=5 p<=0.0001



**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Ballmer (2011)	Die Nähe zu einem Schweizer Kernkraftwerk hat einen signifikant negativen Einfluss auf den Preis einer Immobilie.	LR	RC	Der Koeffizient sagt damit aus, dass sich der Preis einer Immobilie um 0.028% verändert, wenn die Distanz zum nächsten KKW um 1% variiert wird. (Table 6-1): t(30234)=25.48	t(30234)=25.48, p<=0.0001	
Bilbao-Terol (2009)	The hypothesis of this study is that housing values are reduced due to the pollution produced by the plant.	LR	RC	Similarly, the price increases by 3.4% when the distance from the plant increases by 10%, while all the other characteristics remain constant. (Table 9): t(517)=8.90	t(517)=8.908, p<=0.0001	Supplementary material was available on request: Linlog-specification: t(517)=4.78, p<=0.0001
Braden et al. (2008b)	The AOCs in Buffalo and elsewhere impair not only local ecosystems and fisheries, but they may also stigmatize nearby properties. [...] A price differential between similar properties near and far from a noxious site, controlling for other systematic factors that also affect prices, would provide a measure of the economic damage the site causes and the potential economic benefit of remediation	LR	RC	The estimated coefficient for Inaoc*south indicates that a 1% increase in distance from the AOC would result in a (6,769/100) =67.69 increase in the average price. (Table 4): t(2143)=2.61	t(3433)=2.61, p=0.0090	They provide 4 estimates of interest in one regression, 2 for a HWS and 2 for a polluted water body. Here, one estimate per waste category is taken ("south"). One as "result" one as "robustness result" t(3433) =1.89, p=0.0588
Braden et al. (2004)	The premise of our study is that the presence of contaminated sediments. like other types of noxious waste, depresses the market value of properties associated with the site. Cleanup of the site should restore the market values to levels observed with properties of similar quality and context but unaffected by the contamination.	LR	RC	The coefficient on ln(HARBMILE) provides the implicit price of a small change in harbor distance. Differentiating then computing the differential of price relative to the differential of distance for Waukegan households, the mean marginal willingness to pay for a one-mile increase in distance from the harbor is approximately 12,200( <i>median</i> 10,800). The comparable WTP estimate for households outside of Waukegan is a mean of 7,080( <i>median</i> 4,180). Table 6: t(580)=5.58	t(580)=5.58, p<=0.0001	

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Braden et al. (2008a)	This conforms to our expectation that the AOC affects nearby housing values more severely compared to properties further away.	LR	RC	According to the model estimates, distance from the AOC has a significant, positive effect on housing values for properties located nearest the upper river segment. Table 4: AOC main effect: $t(2143)=2.61$ , $p=0.0091$ ; Landfill: $t(2143)=4.76$ $p<=0.0001$	$t(2143)=2.61$ , $p=0.0091$	They estimate the effect in one major regression yielding 4 coefficients of interest. Here, only the “major” effect in front of the dam is considered, as well as the additional landfill effect as a robustness p-value: $t(2143)=4.76$ , $p<=0.0001$
Brasington and Hite (2003)	Being near a hazard should be associated with depressed house prices. Therefore, all else constant, the greater the average distance to the nearest hazard, the higher the average house price is expected to be.	LR	RC	The parameter estimates for this variable are always positive and are statistically significant in five of the six MSAs.	Not reported. They only report that five of six coefficients are significant at certain threshold levels. To avoid the pitfall of randomly assigning p-values, this study is not considered in the p-curve	
Cameron (2005)	We generally expect the effect of localized externalities to diminish with distance until any incremental effect of distance essentially disappears.	LR	RC	Allowing for directional heterogeneity reveals a statistically significant estimate of b and evidence of directional heterogeneity (in the form of statistically significant estimates for d2, and g1). The “average” distance effect, controlling for directional heterogeneity, is positive. This result makes the main point in the paper. Table 2: $t(5462)=0.95$ , $p=0.3421$ ; $t(5459)=2.26$ , $p=0.0238$	$t(5462)=0.95$ , $p=0.3421$	Again, the first and the last results are chosen: $t(5459)=2.26$ , $p=0.0238$

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**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Carruthers and Clark (2010)	Distance is the most common way of measuring the presence of noxious land uses in hedonic price analysis and it is used here because of its ability to capture both the real and perceived levels of disamenity and/or risk associated with the hazards (Expected sign summarized in Table 1)	LR	RC	Every explanatory variable carries its expected sign (if it was anticipated in advance) and all are statistically significant—most at well over a 99 percent level of confidence. Table 2, OLS: $t(29133)=5.78$ , $p<=0.0001$ ; 2SLS: $t(29133)=7.28$ , $p<=0.0001$	OLS: $t(29133)=5.78$ , $p<=0.0001$	Again, the first and last results are chosen (GWR results are omitted as information missed): 2SLS: $t(29133)=7.28$ , $p<=0.0001$
Carruthers et al. (2010)	Environmental hazards, measured as the distance from the nearest air release site, hazardous waste generator, superfund site; and toxic release site;	LR	RC	Finally, note that the parameters on distance from the three environmental hazards that are the object of this analysis (air release, superfund, and toxic release sites) are all positive and statistically significant. Table 2a: $t(22394)=4.18$ , $p<=0.0001$ ; Table 2e: $t(7836)=4.39$ , $p<=0.0001$	2SLS: $t(22394)=4.18$ , $p<=0.0001$	Again, the first and last results are chosen: 2SLS: $t(7836)=4.39$ , $p<=0.0001$
Chattopadhyay et al. (2005)	Because the locations close to Waukegan Harbor are less desirable compared to locations farther away from the harbor, we expect distance from the harbor to have a positive effect on house prices.	LR	RC	However, the insignificant main-effects coefficient on $\ln(\text{harb mile})$ suggests that, all else being equal, an increase in the distance of the homes from the harbor does not increase house price significantly for non-Waukegan homes that are already farther away from the polluted harbor. Table 5: coefficient: $-0.0812$ , S.E. $0.0670$ $t(929)=-1.21$	$t(929)=-1.21$ , $p=0.2258$	
Chen (2017)	In fact, there is little robust evidence regarding how Chinese people perceive and evaluate the environmental externalities associated with urban river water pollution and river restoration, even though sometimes the proximity to surface water bodies has become one of the environmental components that is taken into account in apartment prices by both real estate developers and homebuyers (Jim & Chen, 2006, 2007), in tandem with the commercialization of housing markets	LR	RC	However, the distance to watercourses did not influence apartment prices significantly ( $\beta = -0.017$ ; $p > 0.010$ ), and being located at a riverfront did not increase home values ( $\beta = 0.017$ ; $p > 0.10$ ). Table 2: $t(953)=-1.316$ ; $t(949)=-5.213$	$t(953)=-1.316$ , $p=0.1884$	$t(949)=-5.213$ , $p >= 0.0001$

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Clark and Allison (1999)	We are able to measure property value impacts from the plant during both the preannouncement and post-announcement periods.	LR	RC	Thus, we can conclude that a price-distance relationship does exist but that it is not static. Rather, aversion to the plant, as reflected in a rising housing price with greater distance has been declining over the five years of the sample. Table 1: $t(719)=2.59$ ; Table 2: $t(719)=2.12$	$t(719)=2.598$ , $p=0.0095$	$t(719)=2.12$ , $p=0.0343$
Clauw (2007)	Property value data is often used to value environmental attributes and thus indirectly the adverse health impacts of pollution. The perceived health risk from living near polluting industries is reflected as a characteristic in the willingness to pay for housing.	LR	RC	The distance variable does have the expected sign and is also significant. The further one is removed from the pollution source, the greater the value of the property. Table 13: $p=0.039$ ; $p=0.064$	$t(963)=2.06$ , $p=0.0390$	$t(963)=1.85$ , $p=0.064$
06 Cordera et al. (2018)	In this paper Hedonic Linear Regression models (HLR) have been estimated to verify the hypothesis that residential Dwellings exposed to bad environmental conditions will have lower market values.	LR	RC	The variables relating to environmental quality were not clearly significant in all cases in the HLR1 model. The variable of distance from the ILVA Steel plant presented the expected positive sign, i.e. there is a gradient with increasing prices from the plant, and was significant within a confidence level of 90%. Table 2: $p=0.08$	$t(458)=1.75$ , $p=0.08$	$t(458)=1.46$ , $p=0.143$
Dale et al. (1999)	As such, we would expect that, ceteris paribus, individuals would be willing to pay a premium for living away from the smelter, causing comparable property values to rise as distance from the smelter increases.	LR	RC	Looking at the first row of estimates presented in Table 4, we see that they are consistent with the description regarding price rebound provided above. Table 4: $t(18166)=7.56$ ; $t(40707)=10.65$ ; $t(26142)=4.85$ ; $t(47918)=-15.25$ ; $t(70314)=-22.1$	$t(18166)=7.56$ , $p<=0.0001$ ; $t(40707)=10.65$ , $p<=0.0001$ ; $t(26142)=4.85$ , $p<=0.0001$ ; $t(47918)=-15.25$ , $p<=0.0001$ ; $t(70314)=-22.1$ , $p<=0.0001$	

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
De Parisot (2007)	Environmental disamenities take away value from the house, thus factors such as air pollution (AIR RELEASE), PCB contamination level (PCB30M1000), and distance to the incinerator (INCINERATOR) should have a negative coefficient.	LR	RC	The estimated coefficient for the distance to the ANCADF site is positive and significant. Table 5: t(9628)=1.7	t(9628)=1.7, p=0.0891	
Deaton and Hoehn (2004)	The standard hypothesis is that reduced proximity to the hazardous waste site results in reduced perceptions of exposure and subsequently results in a marginal increase in property value. Thus, $\beta_1$ is hypothesized to be positive.	LR	RC	The results from Model 1 are consistent with standard expectations—increased proximity to hazardous waste sites reduces the environmental quality of an area and thereby, reduces housing prices. The coefficient for the hazard effect is positive so that increased distance to a site increases housing prices. Table 3: S.E.=0.012; However, the results from Model 2 that includes an industrial variable, presents a decidedly different coefficient estimate for the hazard effect. S.E.=0.013; The industrial effect coefficient shows that 10% increase in distance from an industrial area increases housing prices by 0.29%. S.E.=0.01	t(4255)=2.66, p=0.0076	t(4254)=2.9, p=0.9230
Decker et al. (2005)	Given this construction, one should find that the further away a house is from a TRI releasing zip code, ceteris paribus, the greater the home's value will be. Thus, the resulting coefficient should be positive.	LR	RC	The variable DISTANCE has a positive and statistically significant effect (as expected) on housing prices, but only in the WLS regressions. However, the DISTANCE variable is not statistically significant when data censoring is accounted for. Table 2: S.E.=0.0071; S.E.=0.0203	t(6066)=5.04, p<=0.0001	Again, first and last estimate chosen: t(6066)=-1.33, p=0.1835
Dickes et al. (2013)	One hypothesis argues there is a positive price impact on proximate properties for employees of the power plant that perceive this distance as an amenity.	LR	RC	Next, distance to the nuclear power plant variable was found to be statistically significant. Table 3: t=-4.241	t(809)=-4.241, p<=0.0001	

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Du Preez et al. (2016)	The HPM is specified to be a function of a number of control variables, $X_t$ , all of which are expected to increase the observed price or value, $V$ .	LR	RC	There is a positive relation between the value of the house and the distance from the landfill. Exhibit 4: S.E. 0.0004	t(130)=4.75; p<=0.0001	
Du Preez and Lottering (2009)	This expression can therefore be viewed as the incremental increase in the value of a house for a one-unit increase in distance from the landfill site, assuming that there is a positive relationship between distance and price (Van Kooten Bulte, 2001).	LR	RC	The results obtained above show that the Ibhayi landfill site has a negative effect on New Brighton house prices. Table 4: t(492)=4.26, p<=0.0001	t(492)=4.26, p<=0.0001	
Eshet et al. (2007b)	One would expect that properties in areas suffering high exposure to disamenities from waste transfer stations would command lower prices than similar properties in locations that do not suffer such exposure	LR	RC	The estimated elasticities are in the range of 0.04–0.07 across the cities (and 0.067 for the pooled data) with an average of 0.06, suggesting that environmental quality has a statistically significant relationship with property prices, but it is relatively small. Table 5: t(2897)=4.90, p<=0.0001; t(1961)=-0.45, p=0.6462; t(4280)=6.18, p<=0.0001; t(339)=2.762, p=0.0060; Table 8: t(2897)=2.33, p=0.0197; t(1961)=-1.33, p= 0.1817; t(4280)=5.87 p<=0.0001; t(339)=1.83, p=0.0672	Only individual sites' estimates are considered, which are independent: t(2897)=4.90, p<=0.0001; t(1961)=-0.45, p=0.6462; t(4280)=6.18, p<=0.0001; t(339)=2.762, p=0.0060	t(2897)=2.33, p=0.0197; t(1961)=-1.33, p= 0.1817; t(4280)=5.87 p<=0.0001; t(339)=1.83, p=0.0672
Eyckmans et al. (2013)	Regarding odor nuisance, different modeling strategies were implemented. The first strategy uses the straight-line distance (in km) between the house and the source of the odor.	LR	RC	The estimated coefficients are all well behaved, that is, they have the expected sign, are mostly significant at the 1% level, and are very stable across the different specifications of the model. The first approach uses distance from source, expressed in kilometers, as a proxy for odor nuisance (Model 0). This yields a negative estimate for the impact of odor, which is statistically significant at the 5% level. Table A1: S.E.= 0.009; Table A2: S.E.= 0.01	t(1399)=2.33, p=0.0197	t(1399)=2.3, p=0.0215

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Ferrara et al. (2007)	Homes closest to the site can be expected to bear the greatest externalities from the toxicity, and therefore should bear the largest negative impact on property value. [...] so the natural logarithm of the distance (LN_D_SITE) is used, with the expectation that the coefficient is positive if individuals are willing to pay to live farther away from the hazard.	LR	RC	The relationship between distance and predicted price, which is the focus of this study, is significant in both models. Table 3: S.E. =1372.23; S.E.=1390.54	t(380)=6.09, p<=0.0001	t(380)=5.03, p<=0.0001
Gayer (2000)	Given that residents are informed of the neighborhood Environmental hazards, one would expect higher prices for houses exposed to lower levels of environmental risks.	LR	RC	Table 2: OLS: t(6535)=-0.41; IV: t(6535)=2.69	t(6535)=-0.41, p=0.6818	t(6535)=2.69, p=0.0071
Gayer et al. (2000)	The environmental variables include the measure of the distance to the closest Superfund site. Although distance of the house to the closest site is also correlated with health risks from the site, we assume it is a proxy for the Superfund aesthetic disamenities.	LR	RC	The estimated coefficient for the distance to the closest Super fund site is positive and significant for each equation, suggesting that people are willing to pay to live farther away from the visual disamenities that are associated with Superfund sites. Table 2: t(16893)=7.7; Table 5: ES=627, S.E. =175	t(16893)=7.7, p<=0.0001	t(16893)=3.58, p=0.0003

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Grislain-Letrémy and Katosky (2014)	Measuring the impact of the distance from the hazardous facilities on the housing values reveals to what extent the facilities' activities are perceived as disamenities.	LR	RC	For Bordeaux, in all of the models, the proximity to the gunpowder factory is valued by the inhabitants in close vicinity (Table 4), probably because the neighborhoods around the plant are green with many trees and are very quiet. Table 4: $t(1405)=-3.03$ , $p=0.0025$ ; $t(1405)=1.90$ , $p=0.057$ . For Dunkirk, the semiparametric model reveals that the distance from highly hazardous plants does not significantly impact housing prices (Table 5). Table 5: $t(1018)=4.28$ , $p<=0.0001$ ; $t(1018)=0.3$ , $p=1$ . For Rouen, in all of the models, the distance from highly hazardous plants increases the dwellings' values (Table 6): the highly hazardous plants are perceived as disamenities. Table 6: $t(568)=6.19$ , $p<=0.0001$ ; $t(568)=4.48$ , $p<=0.0001$	$t(1405)=-3.03$ , $p=0.0025$ ; $t(1018)=4.28$ , $p<=0.0001$ ; $t(568)=6.19$ , $p<=0.0001$	$t(1405)=1.90$ , $p=0.057$ ; The following t-value is set to 0.3. as information missed $t(1018)=0.3$ , $p=1$ ; $t(568)=4.48$ , $p<=0.0001$
Hao (2008)	Therefore, it is expected that brownfields in Charlotte will not contribute to lower transaction values or less private investment, unless they are situated in neighborhoods suffering other community development obstacles.	LR	RC	In addition, although the three brownfields variables have statistical impact on house prices, the degree of the impact is meager. For instance, for every 1000 feet further away from brownfield sites, the house prices increase by only 0.01 percent; an increase in brownfield acreage of 1 acre decreases house price by only 0.001 percent. Table 6: $t(8063)=5.26$	$t(8063)=5.26$ , $p<=0.0001$	
Herriges et al. (2008)	In general, we expected that an increase in either of these distances would negatively affect a home's sale price.	LR	RC	Table 4: $t(1121)=1.04$ , $p=0.2985$ ; $t(1126)=0.52$ , $p=0.6019$ ; $t(1126)=-0.88$ , $p=0.3742$ ; $t(1128)=1.25$ , $p=0.2115$	$t(1121)=1.04$ , $p=0.2985$	$t(1128)=1.25$ , $p=0.2115$
Hodge (2011)	The coefficient for DISTpost is expected to be statistically significant and positive, indicating that the closer a house is to the plant, the lower its price.	LR	RC	As with the sugar plant in Caro, the distance to the power plant has the opposite sign of what was anticipated.	$t(580)=0.87$ , $p=0.3838$	$t(852)=0.51$ , $p=0.6090$



**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Hurd (2002)	This article compares the estimated effect of distance to the Superfund site on area home sale prices between the period when the site was added to the NPL (1984) and a more recent time period (1996)	LR	RC	The results of four of these models given in Table 1 show that after controlling for structural housing characteristics, property values are likely to be negatively correlated with proximity to the site. Table (1): t(129)=-1.54, p=0.1260; t(286)=-0.21, p=0.8338. Table (3): t(461)=-0.97, p=0.3325	t(129)=-1.54, p=0.1260; t(286)=-0.21, p=0.8338	t(461)=-0.97, p=0.3325
Hwang (2003)	If risk perceptions are accurate, then housing prices in hazardous areas are likely to be discounted below the levels that would be expected on the basis of their other structural, neighborhood, and locational characteristics.	LR	RC	However, the SEER and perceived risk of hazardous materials was statistically significant, which is consistent with the fourth and fifth hypotheses. Table 8-5: t(282)=2.35, p=0.0194	t(282)=2.35, p=0.0194	
Kaufman and Cloutier (2006)	In this study, we apply a hedonic pricing model to investigate the simultaneous impacts of brownfields and greenspaces on residential property values and obtain more accurate estimates of the benefits of brownfield remediation	LR	RC	The explanatory variables have the expected signs and all are statistically significant at the 0.05 significance level. Table 3: t(874)=-4.38; t(874)=-4.49	t(874)=-4.38, p<0.0001	t(874)=-4.49, p<0.0001
Kiel (1995)	Using regression techniques, the effect on house values of the EPA announcement of a discovered toxic site and of subsequent cleanup efforts can be measured.	LR	RC	The estimated coefficient on the distance from the nearest Superfund site is not statistically different from zero, suggesting that during the period from 1975 to 1976 the Woburn housing market did not perceive the sites as disamenities. Table 2: t(353)=1.67, t(680)=1.96, t(454)=2.83; t(174)=3.38, t(97)=1.039, t(397)=3.33	t(353)=1.67, p=0.0958; t(680)=1.96, p=0.0504; t(454)=2.83, p=0.0048; t(174)=3.38, p=0.0008; t(97)=1.039, p=0.3013; t(397)=3.33, p=0.0009	Kiel (1995) and Kiel (2006) have partially overlapping samples. Hence, this is used as robustness results.

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Kiel (2006)	We expect that the adjustment will be faster than that seen in response to an incinerator, since the federal and state governments are involved in providing information to the public in the case of a Superfund site. However, it is possible that such sites are always perceived as undesirable, even after cleaning.	LR	RC	The coefficient on the distance from the site is positive and is statistically significant in five of the eight regressions. Table 2: t(96)=0.93, t(393)=3.31, t(346)=1.83, t(667)=2.59, t(449)=3.04, t(734)=3.97, t(633)=1.73, t(1072)=5.37	t(633)=1.73, p=0.0839; t(1072)=5.37, p<=0.0001	Kiel (1995) and Kiel (2006) have partially overlapping samples. Hence, this is used as robustness results; t(96)=0.93, p=0.3521; t(393)=3.31, p=0.0010; t(346)=1.83, p=0.0669; t(667)=2.59, p=0.0096; t(449)=3.04, p=0.0025; t(734)=3.97, p<=0.0001
Kiel and McClain (1996)	Two outcomes are possible. First, local residents believe that the owners or developers of other externalities will regard the community as an undesirable prospect, since residents have successfully restricted past sitings and will prevent or raise the cost of future sitings. In this case, the location will not be a target for future sitings, and the site should not be viewed as a negative externality. In the second possibility, neighbours believe that the site, once attractive to a developer, will remain so to other developers. The location will remain a negative externality due to the stigma attached to it.	LR	RC	The coefficient on LnDIST measures the impact of the incinerator on house prices. In the pre-rumour stage, this coefficient is significant and positive, suggesting that this site was negatively impacting house values in the area even before an incinerator was proposed. t(122)=2.43, t(148)=2.84, t(391)=2.65, t(498)=3.16, t(232)=3.23	t(122)=2.43, p=0.0163; t(148)=2.84, p=0.0050, t(391)=2.65, p=0.0081, t(498)=3.16, p=0.0016, t(232)=3.23, p=0.0013	

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Kiel and McClain (1995a)	A decline in residential real estate values in the proximity of an undesirable facility is well-documented.	LR	RC	The coefficient is not significant in the pre-rumor stage [. . .]. By the time construction began, however, houses further from the site received a premium. Table 3: t(406)=0.19, t(585)=0.36, t(471)=0.82, t(292)=0.10, t(652)=1.95, t(701)=3.30, t(313)=3.03. Table 4: t(2579)=2.61, t(2579)=8.4,	t(406)=0.19, p=0.8494, t(585)=0.36, p=0.7189, t(652)=1.95, p=0.0516, t(701)=3.30, p=0.0010, t(313)=3.03, P=0.0026, t(2579)=2.61, p=0.0091,	t(471)=0.82, p=0.4126, t(292)=0.10, p=0.9204, t(652)=1.95, p=0.0516, t(701)=3.30, p=0.0010, t(313)=3.03, P=0.0026, t(2579)=8.4, p<=0.0001
97 Kiel and McClain (1995b)	Determining whether, when and by how much the location of a facility affects appreciation rates is necessary in order to correctly estimate the cost of such facilities.	LR	RC	The coefficient is positive and statistically significant. Table 2: t(2575)=2.77, p=0.0056	The sample is very similar to Kiel and McClain (1995a), thus serving as robustness result.	t(2575)=2.77, p=0.0056
Kiel and Williams (2007)	It seems an established empirical fact that Superfund sites lower local property values. [. . .] As stated above, published studies confirm that Superfund sites do indeed lower local house prices. However, it is possible that studies are only published if they find the 'expected' results.	LR	RC	We find that some sites have the expected negative impact, while other sites have either no impact or a positive impact on local property values. Table 2: t(1773)=1.47, t(313)=1.59, t(875)=4.56, t(2114)=3.3, t(2109)=0.502	t(1773)=1.47, p=0.1417; t(313)=1.59, p=0.1112; t(875)=4.56, p<=0.0001; t(2114)=3.3, p<=0.0008; t(2109)=0.502, p=0.6157	Full information only for one site available

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Kiel and Zabel (2001)	House characteristics include the physical characteristics of the house and the quality of the neighborhood. The latter is likely to be affected by proximity to an undesirable facility such as a Superfund site.	LR	RC	They can then be compared to the costs of cleanup to determine if benefits outweigh costs. Our analysis of the Superfund sites in Woburn, Massachusetts, indicates that this is the case and hence the decision to cleanup the sites is consistent with economic efficiency standards. Table 2: $t(98)=-1.28$ , $t(96)=-0.15$ , $t(396)=-2.54$ , $t(394)=-1.98$ , $t(349)=-2.05$ , $t(347)=-0.68$ ; $t(674)=-2.01$ , $t(672)=-1.32$ , $t(452)=-3.38$ ; $t(450)=-1.12$ , $t(174)=-3.321$ , $t(172)=-3.13$	$t(98)=-1.28$ , $p=0.2035$ ; $t(96)=-0.15$ , $p=0.8810$ ; $t(396)=-2.54$ , $p=0.0114$ ; $t(394)=-1.98$ , $p=0.0483$ ; $t(349)=-2.05$ , $p=0.0411$ ; $t(347)=-0.68$ , $p=0.4969$ ; $t(674)=-2.01$ , $p=0.0448$ ; $t(672)=-1.32$ , $p=0.1872$ ; $t(452)=-3.38$ , $p=0.0007$ ; $t(450)=-1.12$ , $p=0.2633$ ; $t(174)=-3.32$ , $p=0.0010$ ; $t(172)=-3.13$ , $p=0.0020$	$t(96)=-0.15$ , $p=0.8810$ ; $t(396)=-2.54$ , $p=0.0114$ ; $t(394)=-1.98$ , $p=0.0483$ ; $t(349)=-2.05$ , $p=0.0411$ ; $t(347)=-0.68$ , $p=0.4969$ ; $t(674)=-2.01$ , $p=0.0448$ ; $t(672)=-1.32$ , $p=0.1872$ ; $t(452)=-3.38$ , $p=0.0007$ ; $t(450)=-1.12$ , $p=0.2633$ ; $t(174)=-3.321$ , $p=0.0010$ ; $t(172)=-3.13$ , $p=0.0020$
Kim et al. (2017)	For the final and deleted status, housing values are likely to increase as immediate threats and health risks are addressed.	LR	RC	The key variable for the nearest distance to the final site is not statistically significant under spatial error model regardless of threshold distance framework. This result indicates that local property values in Jefferson County are not substantially affected by Superfund sites in the final stage. Table 2: $t(771)=1.33$ , Table 3: $t(770)=0.57$	$t(771)=1.33$ , $p=0.1828$	$t(770)=0.57$ , $p=0.5678$

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Kim (2009)	This study is an attempt to measure the indirect impacts of brownfield redevelopment projects in Michigan using the hedonic price method. It can be assumed that the proximity to brownfields is negatively related to the nearby property values and property values decrease as the proximity to a brownfield increases.	LR	RC	The empirical findings of the hedonic price analysis confirmed the existing literature observing that the proximity to brownfield sites or environmental hazards was negatively related to the property values in nearby neighborhoods. Tables IV-3: t(5896)=5.86, t(5896)=5.20; IV-4: t(5308)=5.64, t(5308)=5.85; IV-5: t(6506)=4.71, t(6506)=1.43; IV-6: t(8421)=11.06; IV-9: t(8422)=3.68	t(8421)=11.06, p<=0.0001; t(5896)=5.86, p<=0.0001; t(5308)=5.64, p<=0.0001; t(5308)=5.85, p<=0.0001; t(6506)=4.71, p<=0.0001	t(5896)=5.20, p<=0.0001; t(5308)=5.85, p<=0.0001; t(6506)=1.43, p<=0.1524; t(8422)=3.68, p=0.0002
Kinnaman (2009)	This article questions this assumption by estimating whether the closure of a solid waste landfill improves neighboring property values.	LR	RC	That property values are estimated here to increase by 34.0% for each mile in distance from the landfill, an estimate greater than that achieved by the previous literature, could be attributable to the fact that all data gathered for this study are within 1 mile of the landfill.	t(699)=4.41, p<=0.0001	
Klaiber and Smith (2013)	We use inverse distance to the closest site with hazardous substances as a measure of the disamenity.	LR	RC	The magnitude of the effect is not important for our purposes, because our focus is on the comparative performance of QE strategies in estimating the WTP. Table B1: t(242809)=-3.22	t(242809)=-3.22, p=0.0012	
Lee et al. (2008)		LR	RC		Not reported. Only dichotomous information available (significant at 0.01 or 0.05 level or not)	
Leggett and Bockstael (2000)	Inverse distance to the nearest sewage treatment plant is expected to have a negative coefficient in the hedonic regression if treatment plants are undesirable neighbors in their own right.	LR	RC	The coefficient associated with inverse distance to sewage treatment plants was significantly different from zero in only two specifications. Table II: t(1167)=0.5819, t(1167)=-2.7072	t(1167)=0.58, p=0.5607	t(1167)=-2.70, p=0.0068

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
McCluskey and Rausser (2003)	The resale value will most likely be lower than that of a comparable property without a history of contamination, if there is a market for the property at all.	LR	RC	The estimated coefficients have the expected signs and are statistically significant in each period, with only a few exceptions. Table 2: $t(205367)=3.5$ ; $t(205367)=10.78$ ; $t(205367)=-15.87$ ; $t(205367)=-23.93$	$t(205367)=3.5$ , $p=0.0004$ ; $t(205367)=10.78$ , $p<=0.0001$ ; $t(205367)=-15.87$ , $p<=0.0001$ ; $t(205367)=-23.93$ , $p<=0.0001$	
McMillen and Thorsnes (2003)	Superfund sites have potentially conflicting effects on property values	LR	RC	In model 1, distance from the smelter is estimated to have a positive but insignificant effect on house prices. Table 1: $t(11269)=1.567$ , $t(11260)=-4.46$	$t(11269)=1.56$ , $p=0.1171$	$t(11260)=-4.46$ , $p<=0.0001$
McMillen and Thorsnes (2000)	Are house values lower nearer the smelter?	LR	RC	The second conclusion, which differs from previous findings, is that the nearby housing market was not affected by the announcement of superfund site status or by the information released over time as the extent of soil contamination was determined. Table 2: $t(8732)=5.5$	$t(8732)=5.5$ , $p<=0.0001$	
Mhatre (2009)	Such a price effect can be temporary as the prices are expected to rebound in due course of time after pipeline repair and subsequent cleanup of affected properties.	LR	RC	For every mile away from the Superfund site, the housing sales prices increased by 2.17% or \$1,928. Table 8.1 – 8.7: $t(860)=2.58$ , $t(967)=1.94$ , $t(860)=2.08$ , $t(967)=0.29$ ,	$t(860)=2.58$ , $p=0.0100$ ; $t(967)=1.94$ , $p=0.0519$	$t(860)=2.08$ , $p=0.0370$ ; $t(967)=0.29$ , $p=0.7700$

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Michaels and Smith (1990)	Public opinion surveys (see Mitchell, 1980 and Smith and Desvousges, 1986 as examples) have consistently indicated that landfills with hazardous wastes are among the least acceptable land uses.	LR	RC	Had the analysis stopped with the conventional full-sample model, the results would have been consistent with theoretical expectations- a uniform, statistically significant effect. However, the submarket results indicate a much less “robust” association between our distance and price measures, one that is sensitive to the effects of a number of other factors, including unobservable features of the 85 towns in our sample. Table 6: t(2162)=1.28, t(270)=-0.68, t(368)=-0.68, t(1216)=-3.34, t(248)=0.36, p=0.7124	t(2162)=1.28, p=0.1978; t(270)=-0.68, p=0.4945; t(368)=-0.68, p=0.4925; t(1216)=-3.34, p=0.0008; t(248)=0.36, p=0.7124	
Mihaescu and vom Hofe (2012)	Based on the relevant brownfield literature, we expect that close proximity to potentially contaminated sites will depreciate housing prices.	LR	RC	In this paper we showed that brownfield sites have a significant influence on the price of houses when they are located within close proximity to them. Table 4: t(1593)=-2.22, t(1593)=-2.17	t(1593)=-2.22, p=0.0259	t(1593)=-2.17, p=0.0299
Mihaescu and vom Hofe (2013)	In addition, real estate properties surrounding brownfield sites often experience a decrease in value because of their close proximity to contaminated sites.	LR	RC	Increasing the distance measure DISTBR by one percent leads to an increase of housing prices by 0.1301 percent. Table 2: t(6818)=3.89, t(6818)=2.59	t(6818)=3.89, p=0.0001	t(6818)=2.59, p=0.0095
Munroe (2007)	Lastly, Euclidean distances to probable brownfields were calculated.	LR	RC	Brownfield proximity was associated with a significant reduction in sales price. Table 4: t(53425)=0.2, t(53425)=2.03. Table 5: t(12012)=-0.63, t(10025)=-0.86, t(6419)=2.41	t(53425)=0.2, p=0.8414; t(12012)=-0.63, p=0.5245; t(10025)=-0.86, p=0.3861; t(6419)=2.41, p=0.0155	t(53425)=2.03, p=0.0415; t(12012)=-0.63, p=0.5245; t(10025)=-0.86, p=0.3861; t(6419)=2.41, p=0.0155

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Neelawala et al. (2013)	A priori expectation for the Distance variable is that it should correspond positively to the property prices in such a way that when the residential properties are located away from the pollution source, the prices tend to increase with the distance.	LR	RC	The marginal willingness to pay (MWTP) for being away from the source of pollution is AUS\$ 13 947 per kilometre within the 4 km radius selected for this study. Table 4: t(268)=3.5, t(268)=3.23	t(268)=3.5, p=0.0005	t(268)=3.23, p=0.0013
Nelson et al. (1997)	In the housing market, households seek to avoid nuisances. They will pay more for sites that are not affected by nuisances and less for sites that are affected by nuisances	LR	RC	In all equations, the coefficient on distance from landfill is significant at or better than the 0.01 level. Table 1, 2 and 3: t(431)=2.51, t(138)=3.62, t(60)=2.62	t(431)=2.51, p=0.0121; t(138)=3.62, p=0.0004; t(60)=2.62, p=0.0108	
Nelson et al. (1992)	In real estate market people are willing to pay higher prices for sites that are not affected by nuisances than for sites affected by nuisances.	LR	RC	The sign on the landfill distance variable is in the expected direction, and the coefficient is significant beyond the 0.01 level of the one-tailed t-test. Table 1: t(696)=4.22	t(696)=4.22, p<=0.0001	
Neupane and Gustavson (2008)	The inverse distance specification assumes a price–distance relationship such that a negative estimated coefficient for the environmental variable indicates that price will increase with distance at a decreasing rate.	LR	RC	The hedonic model results clearly show that the presence of the Sydney Tar Ponds and Coke Ovens Sites has had a significant negative effect on the value of residential property. Table 2: t(2204)=-2.51	t(2204)=-2.51, p=0.0119	
Park and Lah (2006)	Furthermore, such facilities may decrease land and housing values of the surrounding community.	LR	RC	The sign of impact (+, -) for each variable as revealed in the statistical analysis is logically inferred, and matched with the modeled expectation as displayed in Table 3. Table 4: t(735)=7.34, t(737)=10.70	t(735)=7.34, p<=0.0001	t(737)=10.70, p<=0.0001
Poor et al. (2007)	In addition, it is expected that proximity of the residential properties to the landfill will have a negative influence on property values which would decline with distance from the landfill.	LR	RC	The signs on the coefficient estimates for the distance to the county landfill variables in both equations, were opposite of our initial expectations. Table 3: t(1217)=-8.18, t(1362)=-7.47	t(1217)=-8.18, p<=0.0001; t(1362)=-7.47, p<=0.0001	



**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Pugh (2008)	The further the property is away from the brown-field the more valuable it is. Used in all models. Expected to have a positive influence.	LR	RC	The characteristic model, however, produced a negative and insignificant value for this premium, conflicting with the appraisal results. Table 5.3 + Table 5.4: t(898)=-1.38, t(1069)=2.58	t(898)=-1.38, p=0.1652	t(1069)=2.58, p=0.0098
Ready (2010)	If a land use such as a landfill generates local disamenities to nearby residents, then the prices of nearby residences will be bid down to compensate their purchasers for putting up with the disamenity.	LR	RC	The estimated coefficient on the measure of distance to the landfill is positive and statistically significant for both Pioneer Crossing Landfill (PCL) and Rolling Hills Landfill (RHL), implying that houses nearer those landfills sold for lower prices than similar houses farther from the landfills. Exhibit 2: t(11055)=-0.76; t(11055)=7.64	t(11055)=-0.76, p=0.4423	t(11055)=7.64, p<=0.0001
103 Reichert et al. (1992)	It seems clear that homeowners have personal and financial incentives to protect their environment and the value of their real estate investment. [...] then we will expect that, ceteris paribus, the further away a house is located from a fill, the higher its value (p.379).	LR	RC	The distance-from-the-landfill variable once again carried a statistically significant negative coefficient of -\$8,813. Exhibit 4: t(948)=-5.42. Exhibit 5: t(559)=-0.15	t(948)=-5.42, p<=0.0001; t(559)=-0.15, p=0.8776	t(948)=-5.42, p<=0.0001; t(559)=-0.15, p=0.8776
		LR	RC	The general conclusion suggested by these results is that these four sanitary landfills have not had significant detrimental effects on surrounding property values (p.431). Table 6.4 -6.15: t(418)=-2.85, t(433)=-2.45, t(251)=1.69, t(262)=0.38, t(148)=1.31 t(161)=-0.69, t(35)=-0.96, t(51)=-2.89	t(418)=-2.85, p=0.0045; t(251)=1.69, p=0.0922; t(148)=1.31, p=0.1922; t(35)=-0.96, p=0.3436	t(433)=-2.45, p=0.0146; t(262)=0.38, p=0.7042; t(161)=-0.69, p=0.4911; t(51)=-2.89, p=0.00564
Seok Lim and Missios (2007)	It is unclear whether all types of landfill externalities are perceived by consumers and reflected in property values.	LR	RC	In the separate site regressions of Table 3, the coefficient of DIST_LF for houses in Keele is greater than that in Britannia by \$2.25 per meter. Table 2 and 3: t(1458)=2.85, t(1130)=2.31, t(322)=1.99	t(1458)=2.85, p=0.0043; t(1130)=2.31, p=0.0208; t(322)=1.99, p=0.0470	

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Shelem et al. (2011)	Where little to no effect on property values is expected before market acknowledgment of the contamination, and a strong significant negative effect is anticipated after the fact.	LR	RC	Regression results show that $\delta_1$ , the estimated coefficient of IDiB from Equation (2) is negative, yet not significantly different from zero. Table 2: $t(237)=-1.31$ , $t(237)=-3.02$	$t(237)=-1.31$ , $p=0.1888$	$t(237)=-3.02$ , $p=0.0027$
Smolen et al. (1992)	An accident might induce fear or concern by home owners or buyers in proximity to the landfill, and transmit abnormal price variations to the housing markets.	LR	RC	In other words, a house located immediately adjacent to the landfill, would be expected to sell for \$24,122 more. Exhibit 3: $t(187)=3.75$ . Exhibit 4: $t(84)=0.81$	$t(187)=3.75$ , $p=0.0002$ ; $t(84)=0.81$ , $p=0.4202$	
Sun et al. (2017)	In particular, the residents living close to WTE plants often complain about foul smells and even hold protests against their operations. In general, the negative emotions of NIMBY may indirectly affect the local property prices by influencing residents' MWTP.	LR	RC	Intuitively speaking, the farther away from the plants, the fewer depreciation effect the property may suffer. Results meet the expectations and all achieve 1% level of significance. Tables 4 and 5: $t(2105)=10.01$ , $t(2102)=13.23$ , $t(602)=3.29$ , $t(1316)=11.27$ , $t(150)=3.35$	$t(2105)=10.01$ , $p<=0.0001$ ; $t(602)=3.29$ , $p=0.0010$ ; $t(1316)=11.27$ , $p<=0.0001$ ; $t(150)=3.35$ , $p=0.0010$	$t(2102)=13.23$ , $p<=0.0001$ ; $t(602)=3.29$ , $p=0.0010$ ; $t(1316)=11.27$ , $p<=0.0001$ ; $t(150)=3.35$ , $p=0.0010$
Sun and Brendon (2013)	Brownfields are officially defined as "real property, the expansion, redevelopment, or reuse of which may be complicated by the presence or potential presence of a hazardous substance, pollutant, or contaminant".	LR	RC	Distance to the closest redeveloped brownfield had a positive effect, as expected. Table 1: $t(4410)=6.334$	$t(4410)=6.334$ , $p<=0.0001$	
Thayer et al. (1992)	The effect of the policy variables (air quality, water quality, distance to waste sites) are the subject of the empirical work below.	LR	RC	The waste site proximity variable is significantly different from zero and possesses the expected relationship to home sale price. Exhibit 7: $t(2305)=3.05$ , $t(2305)=5.27$	$t(2305)=3.05$ , $p=0.0023$	$t(2305)=5.27$ , $p<=0.0001$

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Travers et al. (2009)	Ces différentes pollutions atmosphériques ont non seulement des impacts sur la santé et le bien-être des habitants mais peuvent également avoir un effet négatif sur l'écosystème proche du site Port-Jérôme.	LR	RC	Il apparaît donc clairement que la présence de la zone industrielle risquée de Port-Jérôme est intégrée dans les mécanismes du marché immobilier local, résultat conforme à ceux obtenus habituellement dans les analyses hédonistes traitant de cet aspect. Tableau 5: t(213)=2.626	t(213)=2.62, p=0.0092	
van Dijk et al. (2016)	Another environmental characteristic is the distance (in m) to the nearest wastewater treatment plant (WWTP). This is included as a disamenity, because the proximity is expected to affect public perception of water quality due to the discharge of effluents. In addition, WWTP's may produce an unpleasant odor and also negatively impact house prices in that way.	LR	RC	Houses that are located 100 m further away from the nearest WWTP sell on average and all else being equal at a 0.02% higher price. Table 3 + 5: t(85632)=2, t(85632)=2	t(85632)=2, p=0.0455	
Walsh and Mui (2017)	We therefore expect that after the disclosure law is implemented, the impact of LUST sites should be amplified.	LR	RC	Starting with inverse distance to non-LUST, non-LF sites, the marginal price is negative pre-disclosure, but this effect is only significant at the 10% level when inactive sites are included. Table 5: t(4808)=-1.29, t(6391)=0.78	t(4808)=-1.29, p=0.1965	t(6391)=0.78, p=0.4297
Winstrand (2010)	In an area known for its beautiful surroundings, a direct view of the refinery is likely to have a negative effect on house prices.	LR	RC	As can be seen from the correlation coefficients in Table 2, most of the variables have the expected signs. Price and Distance to Preemraff are positively correlated which implies that prices tend to increase with distance to Preemraff. Table 4 & 11: t(418)=2.05, t(416)=4.78	t(418)=2.05, p=0.0408	t(416)=4.78, p<=0.0001

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Yamane et al. (2013)	Therefore, these events and their resultant impacts can cause property value losses.	LR	RC	However, the estimates on $\ln(\text{DIST FUKUSHIMA}\#1) \times Y_{2011}$ are not significant in both forms and do not support this hypothesis. Table V & VII: $t(582)=-0.64$ , $t(582)=1.22$ , $t(272)=1.93$ , $t(272)=1.47$ , $t(272)=0.38$ , $t(272)=0.27$	$t(582)=-0.64$ , $p=0.5184$ ; $t(272)=1.93$ , $p=0.0543$ ; $t(272)=0.38$ , $p=0.6980$ ; $t(7507)=0.38$ , $p=0.7035$	$t(582)=1.22$ , $p=0.2225$ ; $t(272)=1.47$ , $p=0.1421$ ; $t(272)=0.27$ , $p=0.7838$
Yukutake and Sugawara (2017)	In this study, we analyze the long-term effect of the Fukushima accident on land prices by estimating a longer term application of hedonic land price equations.	LR	RC	The coefficients all have the expected sign, with the exception of a few insignificant coefficients on some of the structural characteristics. Exhibit 6: $t(7507)=0.38$ , $t(7499)=-2.58$	$t(7507)=0.38$ , $p=0.7035$	$t(7499)=-2.58$ , $p=0.0096$
Zegarac and Muir (1998)	The potential impact of the Hamilton Harbour Remedial Action Plan on land values through improved environmental conditions, water quality, recreational opportunities, etc., has generally been viewed positively by those in the RAP process.	LR	RC	During this second period the coefficient for distance to parkland and the waterfront increases and becomes statistically significant. Table 2: $t(46)=-0.90$ , $t(188)=-2.48$ , $t(60)=-1.90$ , $t(62)=-2.63$ , $t(46)=-0.96$ , $t(188)=-1.85$ , $t(60)=-1.56$ , $t(62)=-2.40$	$t(46)=-0.90$ , $p=0.3691$ ; $t(188)=-2.48$ , $p=0.0138$ ; $t(60)=-1.90$ , $p=0.0613$ ; $t(62)=-2.63$ , $p=0.0105$	$t(46)=-0.96$ , $p=0.3413$ ; $t(188)=-1.85$ , $p=0.0648$ ; $t(60)=-1.56$ , $p=0.1240$ ; $t(62)=-2.40$ , $p=0.0191$
Zhang (2010)	Hypothesis 1: Environmental hazards will not affect households' housing decisions.	LR	RC	However, both hurricane hazard and toxic materials hazard are insignificant. Table 2: $t(311)=-1.03$	$t(311)=-1.03$ , $p=0.3000$	
Zhang et al. (2010)	If environmental hazards are perceived as disamenities, property values in the proximity of hazard sources would be expected to be lower than those in the less vulnerable areas.	LR	RC	The regression showed that distance to toxic chemicals hazard had a direct significant positive effect (i.e., proximity to toxic chemicals hazard had a direct negative effect) on housing value, even after controlling the mediation of risk perception (Table 3, Model VII): $t(306)=1.97$	Same sample as Zhang (2010). Hence only a robustness result	$t(306)=1.97$ , $p=0.0497$

**Table A.11:** (continued).

Original Paper	Quoted text from original paper indicating prediction of interest to researchers	Study design	Key statistical result	Quoted text from original paper with statistical results	Results	Robustness results
Zhao, Simons and Fen (2016)	Incinerators, like other municipal waste facilities, (i.e., landfills, material recovery facilities, recycling plants, etc.) are generally associated with the external cost of air pollutants such as particulates, nitrogen oxide (NO <sub>x</sub> ), dioxins, sulfur dioxide (SO <sub>2</sub> ), and other combustion byproducts (e.g., ash).	LR	RC	Consistent with expectations, we find that residential properties that are near (within three kilometers) the incinerators are more adversely affected than properties located further away from the site. Table 4 & 8: t(2240)=13.36, t(52)=1.60	t(2240)=13.36, p<=0.0001	t(52)=1.60, p=0.1139
Zhao et al. (2016)	Incinerators, like other municipal waste facilities, (i.e., landfills, material recovery facilities, recycling plants, etc.) are generally associated with the external cost of air pollutants such as particulates, nitrogen oxide (NO <sub>x</sub> ), dioxins, sulfur dioxide (SO <sub>2</sub> ), and other combustion byproducts (e.g., ash).	LR	RC	Consistent with expectations, we find that residential properties that are closer in proximity (within three kilometers) have been more adversely affected than properties located further away. Exhibit 5: t(577)=12.61	t(577)=12.61, p<=0.0001	
Zuindeau and Letombe (2008)	By indicating their agreement with a property value that is lower than that of a reference house, the buyers (or the sellers), to a certain extent, reveal the price that they agree to in order to accept (or avoid) proximity to a polluting site.	LR	RC	The effect of proximity to the plant is clearly negative, whatever the specifications. Tables 4 & 6: t(613)=-2.53, t(607)=1.86	t(613)=-2.53, p=0.0114	t(607)=1.86, p=0.0623

## Chapter 2

# Wind Turbines and Property Values: A Meta-Regression Analysis

*Marvin Schütt*

### **Abstract:**

A key concern for property owners about the set up of proximate wind turbines is the potential devaluation of their property. However, there is no consensus in the empirical hedonic literature estimating this price-distance relationship. It remains unclear if the proximity to wind turbines reduces, increases, or has no significant effect on property values. This article addresses this ambiguity, combining 720 estimates from 25 hedonic pricing studies in a first comprehensive meta-analysis on this topic. Using Bayesian model averaging techniques and novel publication bias correction methods, I calculate an average of the reported estimates that is free from misspecification and publication bias. In economic terms, I find an average reduction in property values of  $-0.68\%$  for properties 1.89 miles away, which turns to zero beyond 2.8 miles. Next to publication selection, the studies' ability to control for confounding factors such as pre-existing price differentials and spatial effects explains the variance in reported effect sizes.

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## 2.1 Introduction

Annual global electricity generation from wind farms has grown steadily over the past decade(s) and is expected to continue to grow. For example, the International Energy Agency predicts that annual wind capacity additions will reach 210GW in 2030, up from 95GW in 2021 (IEA, 2022, p. 293). Obvious reasons include the low CO<sub>2</sub> emission compared to fossil fuel-based generation, low operating costs, and reduced energy import dependencies from the countries' perspective. While these advantages can be perceived as valuable for society in general, wind farms are simultaneously sources of various negative externalities (Zerrahn, 2017; Sovacool et al., 2021). Residents in close proximity may particularly be affected by noises, shadow flicker, and visual deterioration of the landscape, which may result in negative welfare changes (Mattmann et al., 2016; Onakpoya et al., 2015; Liebich et al., 2021). In this context, many hedonic pricing studies investigate if these externalities translate to price variations in property values proximate to wind farms. Notably, the empirical evidence is ambiguous, with many studies failing to find a significant (negative) effect on property values due to the presence of wind turbines (Parsons and Heintzelman, 2022; Dorrell and K. Lee, 2020; Brinkley and Leach, 2019). Furthermore, studies that find a significant effect show a large variance in its magnitude, mostly between -10% and 10%, but also with estimates well beyond this range. Given this ambiguity, it is of great interest for researchers, decision-makers, and property owners to understand whether and under which conditions wind turbines significantly affect local property values.

I address this open question by combining 720 estimates from 25 hedonic pricing studies estimating price-distance relationships for wind turbines and residential properties in a first comprehensive meta-analysis on this topic. Bayesian Model Averaging (BMA) techniques are used to identify sources of heterogeneity attributable to the data characteristics of the primary studies and their ability to control for confounding factors. In addition, I apply a series of novel tests for publication selection bias that allow the calculation of a bias-corrected mean effect size.

Some authors contributing to this literature have already speculated about reasons for the significant differences across studies in terms of reported effect sizes. In particular, Hoen and Atkinson-Palombo (2016) argue that insignificant findings may reflect underpowered studies, having access to only small samples of truly affected properties, i.e., few observations close to wind turbines. Accordingly, for a small effect size, the respective study would be unable to identify such an effect if it exists. There is also suggestive evidence that site conditions such as local opposition (Heintzelman et al., 2017), degree of visibility (Sunak and Madlener, 2016) or the number of turbines in proximity (Dröes and Koster, 2016) can partly explain the studies' differing findings. In excellent reviews, Parsons and Heintzelman (2022) and Möllney (2022) show that the econometric specification (e.g., avoidance of omitted variable bias, treatment of

endogeneity problems) and data characteristics (e.g., distance of treated properties, type of analysed property prices) differ across studies, potentially leading to contrasting results. Similarly, Möllney (2022) acknowledges the difficulties in comparing the empirical findings due to the diversity of employed metrics to measure wind turbine impacts. Parsons and Heintzelman (2022) also conduct what they call a "mini meta-analysis" as part of their qualitative review. That is, they calculate the simple mean effect size for different wind-turbine-to-property distances using 18 observations. They document  $-4.5\%$  devaluation for houses at 1 km of distance, with the effect fading out after 4.5 km. However, this finding should be interpreted with caution since they do not (i) correct for publication bias, (ii) include other control variables, (iii) give more weight to more precise estimates, and (iv) use only a subset of the available estimates. I address all of these issues in the methodological setup of this meta-analysis, thus allowing to systematically investigate the existence of price effects of wind turbines on property values at the aggregate level. Building upon these lines of argumentation, I contribute to this literature (i) with a systematic assessment of the causes for effect size heterogeneity and (ii) by calculating an average effect size corrected for publication and misspecification bias.

The results do not confirm a substantial effect of the presence of wind turbines on residential property values. In fact, after correcting for publication and misspecification bias, the resulting effect size corresponds to a reduction of property values of  $0.68\%$  for properties 1.89 miles away, which turns zero beyond 2.8 miles of distance. The simple average of reported effect size instead would have indicated a decrease of property values by  $2.14\%$ . Selective reporting of negative findings is responsible for a  $25\%$  overestimation of the effect. Important study characteristics to avoid misspecification bias are, e.g., the accuracy of the distance calculation, usage of spatial controls, and reliance on a difference-in-difference estimation design.

The remainder of the paper is structured as follows. Section 2.2 summarizes the data collection process and gives an overview of the resulting sample. Section 2.3 assesses the severity of publication selection bias. Section 2.4 explores the heterogeneity-explaining factors relying on BMA and presents the results. Section 2.5 discusses the findings and concludes. The study selection process and coding decisions are explained in detail in the appendix. Finally, model diagnostics for BMA and results for alternative specifications are also reported in the appendix.

## 2.2 Data

The strategy to identify relevant studies followed the current guidelines for meta-analyses in economics (Havránek et al., 2020). I used a predefined search query with placeholders for "property values", "wind turbines" and "hedonic pricing" to find relevant studies. I complemented the list with a forward- and backward search using



citations and reference lists of already identified studies, respectively (see Table B.1 for a list of search terms). For consideration, studies must use the hedonic pricing method to estimate a price-distance relationship for residential properties and wind turbines. Accordingly, I refrain from combining estimates with different (i) underlying welfare measures (e.g., contingent valuation and hedonic pricing studies), (ii) wind turbine impact metrics (e.g., distance and view), or (iii) property types (e.g., residential properties and farmlands). In the appendix, I outline in more detail that these study selection criteria ensure a consistent set of effect size estimates that can reasonably be meta-analysed. There, I also summarize the study selection process (Figure B.1) and list the included studies (Table B.2) as well as excluded studies by reason for exclusion (Table B.3). The resulting final dataset consists of 720 observations from 25 studies. The search was conducted in December 2021 and documented using the reference management software Citavi.

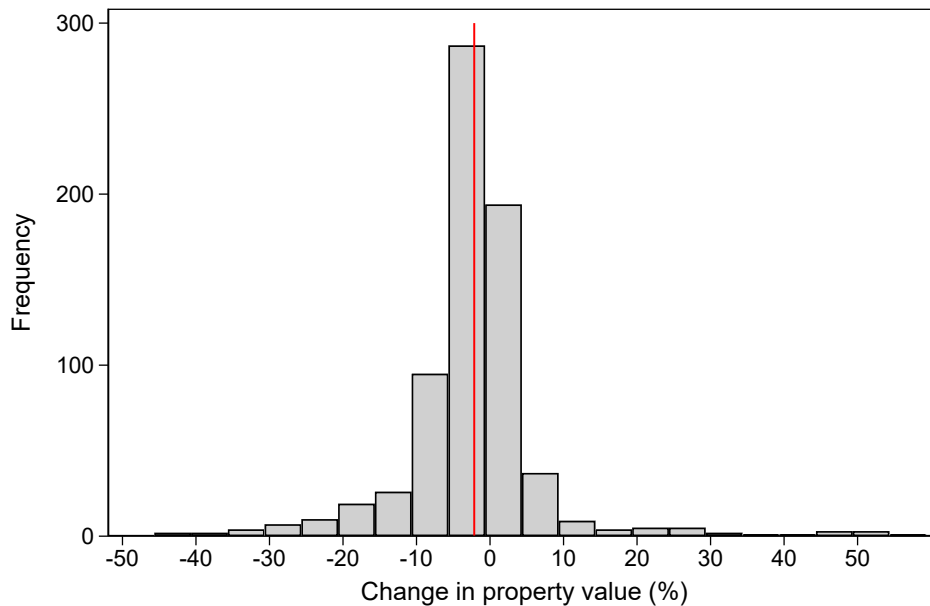
All included studies rely on a semi-logarithmic functional form to elicit the price-distance relationship such that the reported distance coefficients represent semi-elasticities. Accordingly, these semi-elasticities serve as the dependent variable. They can be interpreted as the *percentage change of residential property values at the average distance of a treated house*.<sup>1</sup>

Figure 2.1 shows the distribution of estimates. Most estimates are negative but small in magnitude, with a mean of  $-2.15\%$  and a median of  $-1.59\%$ . Still, the range of estimates is substantial, with a minimum and maximum of  $-66\%$  and  $+109\%$ , respectively. Given the large variation, I exclude three outliers exceeding  $+100\%$  and  $-50\%$ . These are at the extreme ends of the effect size distribution, well beyond all other observations.<sup>2</sup> The main results are not affected by using different outlier criteria or no outlier criterion at all, see Subsection 2.4.4. This discrepancy is also reflected in Figure 2.2, in which the effect size estimates of each study are summarized in boxplots, ordered by the studies' publication year. Clearly, the estimates vary considerably, both across and within studies. Additionally, there seems to be a trend toward more negative findings and more narrow confidence intervals over time. This might indicate, e.g., better data availability or more refined methodological choices in more recent studies (Hoen and Atkinson-Palombo, 2016; Parsons and Heintzelman, 2022).

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<sup>1</sup> Here, the average distance of a treated house is 1.89 miles. Note, however, that the definition of *treatment* varies across studies. Some studies consider the announcement of a wind turbine as treatment, while others use the construction date (or both points in time). Similarly, the distance from a wind turbine, at which a house is no longer considered as treated, differs. These aspects are reflected in the selection of moderators, introduced in Subsection 2.4.1.

<sup>2</sup> This changes the mean and median only little ( $-2.14\%$  and  $-1.58\%$ ).

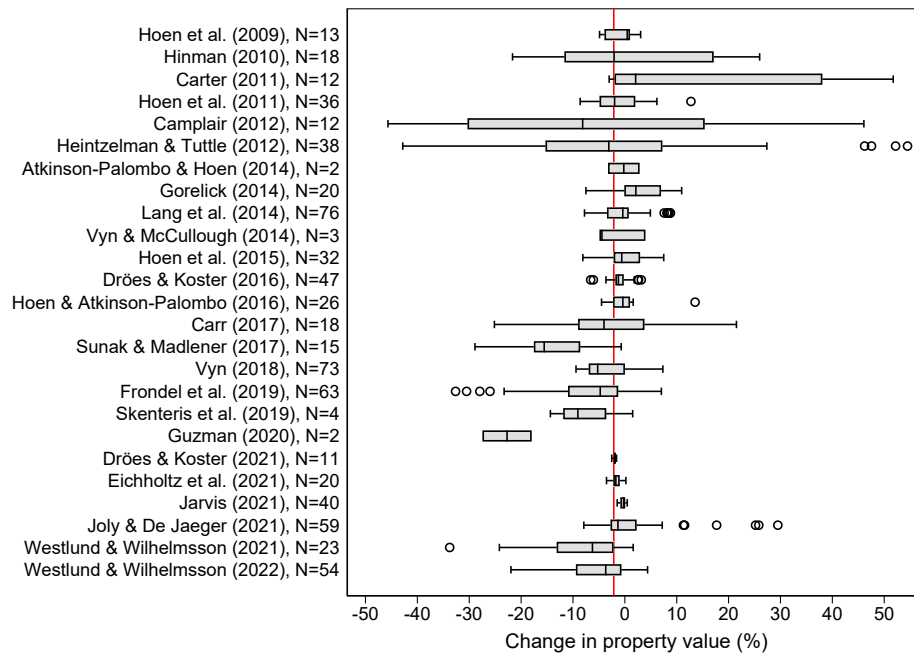
**Figure 2.1:** Histogram of effect sizes.

Notes: Histogram of effect size estimates after removing three outliers exceeding +100% and -50%, respectively. The solid vertical line depicts the sample mean.

## 2.3 Publication bias

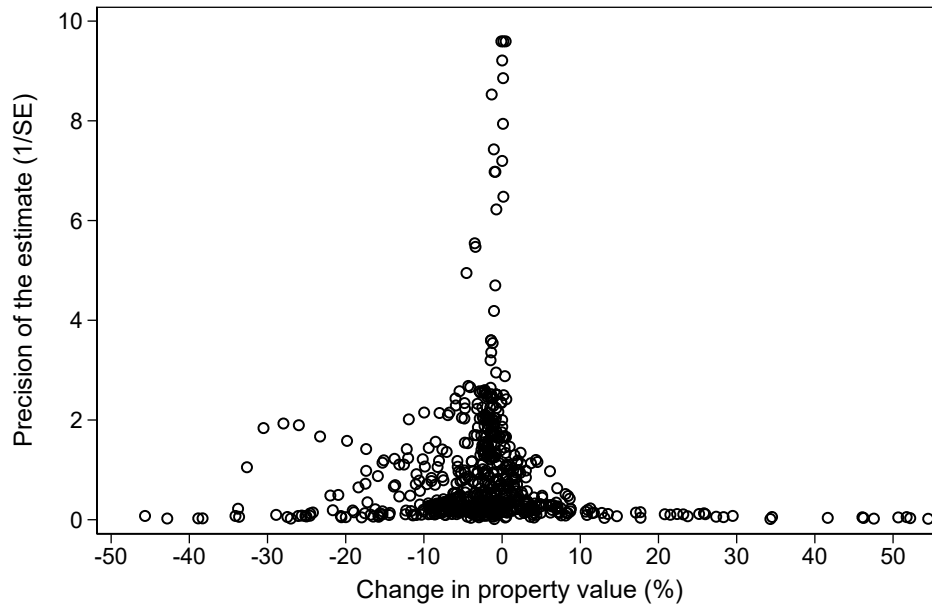
Publication selection bias commonly describes the distortion of a field of literature due to journals' selection of studies or researchers' selection of findings based on statistical precision, magnitude, direction of effect, or any combination thereof (Stanley and Doucouliagos, 2012). Accordingly, if researchers expect the presence of wind turbines to result in a reduction of property values, insignificant or significantly positive estimates may not be selected for publication, leading to a distorted picture of the literature. Figure 2.3 shows a funnel plot where the effect size (horizontal axis) is set in relation to its precision =  $\frac{1}{SE}$  (vertical axis).<sup>3</sup> With no publication bias present, the most precise estimates at the top should mirror the genuine magnitude of the effect. A symmetric dispersion around this *true* mean should result in lower precision levels. Here, the most precise observations have a corresponding effect size close to zero. A tendency for reporting negative findings becomes apparent for more imprecise estimates, i.e., moving down the inverted funnel. This is a first indication that negative findings of wind turbine effects on property values may be over-represented due to publication bias.

<sup>3</sup> There are several options for the choice of precision measure used on the vertical axis, as well as the style of the funnel plot (e.g. including pseudo 95% confidence intervals, using contour-enhanced funnel plots), see Sterne and Egger (2001) for a detailed discussion. Here,  $\frac{1}{SE}$  is used because the resulting funnel plot gives a clear visual impression of the relationship between effect size and precision. An alternative contour-enhanced funnel plot using  $SE$  as the precision measure gives a similar impression (see Figure B.4 in the appendix).

**Figure 2.2:** Boxplots of effect sizes.

*Notes:* Boxplots of effect size estimates for every primary study after removing three outliers exceeding +100% and -50%, respectively. Studies are sorted in ascending order by publication year. The boxes denote the inter-quartile range ( $P75 - P25$ ), with the mean shown as a solid vertical line. Whiskers indicate the distance up until 1.5 times the IQR starting from the  $P25$  and  $P75$ , if applicable. Dots reflect outlying observations within a study. The solid vertical line depicts the sample mean.

Next to visual tools, many more formal methods exist designed to detect and correct publication bias. Simulation studies show that there is no single best method, but instead indicate that the performance of these methods depends on the magnitude of the effect size, level of heterogeneity in the literature as well as severity and type of publication bias (Alinaghi and Reed, 2018; Hong and Reed, 2021). Here, I rely on the recently developed R package *RoBMA-PSMA* to combine the competing techniques (Bartoš et al., 2023). This method is beneficial for two main reasons. First, it aims for objectivity by testing 36 competing models simultaneously, weighting the result by the fit to the data using BMA. Second, it captures uncertainty about the publication selection mechanism by including models assuming reporting based on p-values (so-called selection models) and those assuming selection based on significance and effect magnitude. Bartoš et al. (2023) show that their approach outperforms most conventional techniques but also advise to complementing their method with other concepts. Accordingly, I also employ other techniques shown to perform well under conditions frequently met in empirical economic settings, i.e., multiple estimates per study, omitted variable bias, or a continuous dependent variable (Alinaghi and Reed, 2018; Bom and Rachinger, 2019). This encompasses the Endogenous Kink method introduced by Bom and Rachinger

**Figure 2.3:** Funnel plot.

*Notes:* The figure shows a funnel plot, relating effect size estimates to their reported precision ( $\frac{1}{SE}$ ). Without publication bias the plot should take the shape of an inverted funnel. The most precise estimates are omitted for the ease of exposition but included in all calculations.

(2019)<sup>4</sup>, the selection model advocated by Andrews and Kasy (2019) and the p-uniform\* method by van Aert and van Assen (2021). Finally, I also consider methods that only use a subset of the available observations to calculate a bias-corrected mean effect size (and are hence not covered by the RoBMA-PSMA framework). This includes the stem-based method introduced by Furukawa (2019) that is based on the funnel plot logic and the "Top Ten", which uses only the ten percent most precise observations (Stanley et al., 2010).

The results of the tests for publication bias are summarized in Table 2.1. Regardless of the chosen method, the corrected effect size is considerably smaller (in absolute terms) than the unweighted mean (OLS estimate) ( $-0.08$  to  $-1.76$  vs.  $-2.14$ ), confirming publication bias. This is also in line with the visual impression gained from the funnel plot depicted in Figure 2.3. Stanley and Doucouliagos (2015) show that with publication bias present, the unrestricted weighted least squares (WLS) estimator, i.e., using inverse-variance weights, gives a more realistic representation of the true unconditional mean and I report it for completeness. Indeed, with  $-0.38$  this simple estimator is within the range of bias-corrected means. With RoBMA-PSMA, the corrected mean effect size is  $-1.59$ , corresponding to an absolute reduction in effect size magnitude by about 25%

<sup>4</sup> The Endogenous Kink method is a non-linear extension to the popular PET-PEESE framework, which formalizes the funnel plot relationship introduced by Stanley and Doucouliagos (2012). Since it reduces to the PET-PEESE method as a special case, I do not consider the latter technique individually.

compared to the unweighted mean.<sup>5</sup> Turning to the other methods, the p-uniform\* estimate is of similar magnitude ( $-1.76$ ), while the Andrews and Kasy (2019) and the Endogenous Kink method correct more strongly ( $-0.29$  and  $-0.16$ ).<sup>6</sup> The remaining methods that only use small subsets of the most precise observations similarly induce a strong correction of the mean effect size (Top Ten:  $-1.03$ ; Furukawa:  $-0.06$ ). This is no surprise, considering that the most precise estimates are clustered around zero in this case. This peculiarity of the dataset also explains the insignificant Furukawa estimate, which is based on only 27 observations with respective effect sizes close to zero.

**Table 2.1:** Mean effect size without and with correction for publication bias..

	No correction		Correction with full sample				Correction with reduced sample	
	OLS	WLS	RoBMA	A&K	p-uniform*	End. Kink	Furukawa	Top Ten
Mean effect size	-2.14*** (0.37)	-0.38*** (0.06)	-1.59*** (0.30)	-0.29*** (0.21)	-1.76** (0.83)	-0.16** (0.06)	-0.06 (0.35)	-1.03*** (0.17)
<i>N</i>	717	717	717	717	717	717	27	72

Notes: \*\*\*, \*\* and \* denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively.

This section confirms that publication bias is present in the hedonic literature on wind turbines and proximate property values. The unweighted mean of  $-2.14\%$  is inflated, and the bias-corrected effect size is expected to be around  $-1.59\%$  using the preferred correction method RoBMA-PSMA.

In economic terms, the range of estimates translates to a slight decrease in property values on average if a wind turbine is present. None of the estimates, however, take into account the large differences in reported effect size magnitude within and across studies. This could be problematic for two main reasons. First, other sources of bias linked to measurement error or misspecification in the primary studies could systematically influence the magnitude of the reported effect. This could lead to an over- or underestimated mean effect size if only publication bias is corrected. Second, if study design choices are systematically related to the likelihood of publication or the precision of the estimates, the supposed presence of publication bias could, in fact, mirror true heterogeneity. Accordingly, I explore the drivers of the observed heterogeneity in the next section.

<sup>5</sup> The RoBMA-PSMA package does not calculate significance levels but Bayes factors in determining the likelihood of the effect being different from zero. Here, with  $BF = 30$ , there is a strong indication of the presence of an effect, following the classification of M. Lee and Wagenmakers (2014, p. 105).

<sup>6</sup> When applying the Andrews and Kasy (2019) approach, researchers have some degrees of freedom. Here, I assume a one-sided selection, i.e., under-reporting of significant positive findings that would signal increases in property values. Assuming a two-sided selection, i.e., under-reporting of insignificant findings, changes the effect size estimate to  $-1.34$ , which is close to the RoBMA-PSMA estimate.

## 2.4 Heterogeneity

The goals of this section are threefold: first, to find elements of study design that are accountable for the observed effect size heterogeneity; second, to establish if publication bias can still be confirmed even with other controls for study differences in place; and third, to calculate the expected effect of wind turbines on property values corrected for misspecification and publication bias.

### 2.4.1 Moderator selection

A rich set of 42 moderators was coded to identify relevant dimensions in which the selected studies differ. The selection of moderators is based on recommendations in the general meta-analytic literature (Stanley and Doucouliagos, 2012; Havránek et al., 2020), existing reviews (Parsons and Heintzelman, 2022; Möllney, 2022; Brinkley and Leach, 2019) and study differences becoming apparent during the data-coding process.<sup>7</sup> The moderators are grouped into four categories reflecting the primary studies' (1) data characteristics, (2) control variables, (3) specification of the wind turbine impact, and (4) publication-related information. I summarize the variables in Table 2.2.

#### *Data characteristics*

The primary studies included in the analysis examine very different samples of properties and wind turbines to estimate the wind turbine effect. Accordingly, I define ten variables controlling for data characteristics. For an accurate calculation of the distance between wind turbine(s) and properties, exact coordinates are required. Two variables control if more coarse data, e.g., using wind farm or postcode centroids lead to significantly different results in the reported effect size (*Wind coordinates*, *Property coordinates*). Similarly, I include a variable reflecting the reported mean distance of treated properties from the wind turbine site to test if the price-distance relationship experiences a distance decay (*Distance*). Turning to the sampled properties, some studies have access to actual sales transaction data, while others rely on asking- or assessed prices. Likewise, while most studies analyse effects on already built properties, some focus on residential land. The dummies *Sales* and *Res. land* reflect these differences. Next, I control for the sample size and the time span of the sampling period (*Sample size*, *Sample duration*). Finally, three moderators reflect the general context in which the data were sampled, i.e., a dummy controls if the study was conducted in the USA or elsewhere (*USA*), and two variables reflect the share of wind energy (*Share wind*) and share of renewables (*Share renewables*) in the respective country's electricity mix at the midpoint of the studies' sampling period, respectively.

<sup>7</sup> Following the guidelines referred to in Havránek et al. (2020), a second coder separately coded a substantial proportion of the final dataset, i.e., 12 studies or about 50%. Reassuringly, coding ambiguities were attributable to the varying levels of reporting detail in the primary studies and were reconciled.

*Control variables*

Although the hedonic pricing framework does not define any exact set of variables to be included in the analysis, omission of relevant control variables may lead to misspecified models and, in turn, to biases in the reported effect sizes (Wooldridge, 2010; Phaneuf and Requate, 2016). Consequently, the dummy variables *Structure var*, *Neighbourhood var*, *Access* and *Demoecon* capture if studies control for characteristics of the sampled houses (e.g., age, number of rooms), the neighbourhood (e.g., road noise, prison presence), access options (e.g., distance to central business district or highway), or socio-economic factors (e.g., income levels, crime rate, population density), respectively. Similarly, the estimated wind turbine effect may differ for studies that control for other proximate (dis-)amenities (e.g., distance to a park, beach, industrial facility or landfill). The dummies *Oth amen* and *Oth disamen* reflect this possibility. Furthermore, some studies can address the endogeneity problem using a difference-in-difference design (Kuminoff et al., 2010; Bishop et al., 2020; Greenstone and Gayer, 2009), i.e., taking advantage of the information on prices before and after treatment (temporal variation) as well as on prices of proximate and distant properties (spatial variation). This type of study can account for pre-existing price differentials and frequently interpret the estimated coefficient as a causal price change due to the presence of proximate wind turbines. Accordingly, I include the dummy *DID* to differentiate DID studies from those with a standard hedonic pricing framework.<sup>8</sup> Moreover, the hedonic pricing literature has a consensus that time-invariant unobservable spatial effects should be reflected in the econometric specification for proper identification (Parsons and Heintzelman, 2022). Accordingly, I control if the omission of spatial controls results in changes in reported effect sizes using the dummy variable *Spatial*. Finally, the moderator *OLS* distinguishes studies using ordinary least squares from those with other estimation approaches (e.g., instrumental variables or maximum likelihood).

*Specification of wind turbine impact*

The chosen econometric specification of the wind turbine impact may influence the effect size. Accordingly, I distinguish specifications with one treatment zone from those with several (*Dist binary*). Similarly, I test if the consideration of additional wind-turbine-impact variables, such as the number of turbines or the degree of visibility next to the distance variable leads to differences in the estimated effect (*Other wind*). Furthermore, three dummies test the influence of differences in the definition of the treatment period. First, I distinguish if the estimated effect size refers to the announcement or

<sup>8</sup> Parsons and Heintzelman (2022) note that concerns about biases in estimating treatment effects have been raised when treatments are staggered over time (Chaisemartin and D'Haultfœuille, 2020; Chaisemartin and D'Haultfœuille, 2022; Steigerwald et al., 2021). Since most sampled studies pool observations from different wind farms with varying corresponding construction dates, treatments are clearly staggered over time in this literature. I am aware of only one study (Möllney, 2022) that explicitly considers this methodological aspect. However, this Master's thesis could not be considered for the meta-sample as distance coefficients were not estimated. Jarvis (2021) and Dröes and Koster (2016) are the only included studies that approach this issue using an event study design. Jarvis (2021) states, however, that this is only a partial solution to the problem of staggered treatments.

the construction date of a wind turbine (*Announcement effect*). Second, some studies consider both announcement and construction date in their econometric specification, while others define treatment for only one point in time (*AE and CE*). Finally, in a few cases, authors test if the effect size adjusts over time, mirroring a habituation effect (*Adjustment control*).

#### *Publication*

Taking the presence of publication bias into account, I include three variables to test if it can still be confirmed when the controls for heterogeneity introduced above are in place. This includes the standard error of the reported coefficient (*SE*), a dummy signalling its significance (*Sig*), and a variable reflecting peer-review status in a scientific journal (*Reviewed*). Including the standard error formalizes the funnel plot logic on which the methods by Furukawa (2019) and Bom and Rachinger (2019) are based. The significance dummy mirrors selection based on p-values (Andrews and Kasy, 2019; van Aert and van Assen, 2021). The peer-review control captures two aspects. First, it should reflect some particularities of the studies linked to their quality not captured by the variables introduced above. Second, since the peer-review may have induced changes in reported results, it measures the extent of another facet of publication selection. Next, I include the publication year to account for time-trend effects (*Year publish*). Finally, in some cases, the standard error was not reported, but only information on the significance level of an estimate was given. For these estimates, I set the precision at a conservative level, see Figure B.2 for details. The dummy *Precision set* is included to assess the influence of this coding decision.

These moderators reflect the most prominent aspects of the included studies and, thus, account for key differences in data and methodology. Additional moderators, e.g., the level of urbanity in the area of the sampled properties, the degree of turbine visibility, or the presence of local opposition, were initially considered but dismissed. These variables were inconsistently defined and could, therefore, not be used in a comprehensive comparison across studies. Additionally, I tested the added value of finer-grained categorical moderators instead of dummies (e.g., using different significance levels instead of a significance dummy). If there were no changes to the results, I opted for model parsimony using a dummy specification. Finally, for another subset of moderator candidates (i.e., wind turbine and additional sample size characteristics), information was missing for several studies. I include these variables in Table 2.2 for completeness (marked with an asterisk). However, to conserve sample size, I conduct separate analyses with this type of moderator, see Subsection 2.4.4.



**Table 2.2:** Definition and summary statistics of variables.

Moderators	Definition	N	Mean	SD	Min	Max
Wind effect	Change in property value (%)	717	-2.14	9.99	-45.66	54.50
<b>Data characteristics</b>						
Wind coordinates	=1 if exact coordinates of wind turbines are used to calculate distances, 0 else	717	0.98	0.15	0	1
Property coordinates	=1 if exact coordinates of the properties are used to calculate the distance, 0 else	717	0.90	0.30	0	1
Sales	=1 if actual sales price data were used, 0 else	717	0.90	0.30	0	1
Res. land	=1 if the analysed price refers to the land parcel without the building, 0 else	717	0.02	0.14	0	1
Distance	Mean distance of treated properties in miles	717	1.90	1.93	0.13	10.56
Sample duration	Count of years the study's sample spans	717	12.72	7.37	0	34.00
Sample total	Total number of observations in the estimate's corresponding regression (in logs)	717	10.67	2.65	3.22	15.92
Sample stage*	Number of observations at the construction stage to which the estimate refers (in logs)	457	9.00	2.64	3.22	13.85
Sample treated*	Number of treated observations to which the estimate refers (in logs)	393	6.57	3.40	0.69	12.58
Sample 1 mile*	Total number of observations within 1 mile of a turbine in the study (in logs)	570	8.11	3.13	2.08	13.21
PC 1 mile*	Number of observations within 1 mile and after construction (in logs)	462	7.39	2.85	2.40	12.22
Turbines*	Number of wind turbines	596	5391.36	10439.34	1	40000
Height*	Average hub height of the turbine(s) in meters	421	80.19	14.71	59.50	104.55
Capacity*	Average installed capacity of the turbine(s) in MW	449	1.56	0.67	0.76	3.00
Farms*	Number of distinct wind farms	461	180.30	494.07	1	1775
USA	=1 if studied properties are located in the USA, 0 else	717	0.11	0.31	0	1
Share renewables	Share of renewables in national electricity generation (%) in the mid-year of the study's data	717	19.45	20.59	1.13	59.88
Share wind	Share of wind energy in national electricity generation (%) in the mid-year of the study's data	717	2.30	3.06	0.08	9.91

**Table 2.2:** (continued).

<b>Control variables</b>						
Structure var	=1 if structural property characteristics are used, 0 else	717	0.94	0.24	0	1
Neighbourhood var	=1 if neighbourhood-related property characteristics are used, 0 else	717	0.82	0.38	0	1
Access	=1 if access options (e.g., highway, central business district) are controlled for, 0 else	717	0.54	0.50	0	1
Oth disamen	=1 if other disamenities are considered in the regression (e.g., industrial facility, landfill), 0 else	717	0.33	0.47	0	1
Oth amen	=1 if other amenities are considered in the regression, (e.g., park, beach), 0 else	717	0.47	0.50	0	1
Demoecon	=1 if socio-economic factors (e.g., income, poverty, crime rate) are controlled for, 0 else	717	0.09	0.29	0	1
HPI	=1 if a housing price index is used to account for time trends, 0 else	717	0.07	0.26	0	1
Time dummy	=1 if a dummy is used to account for time trends, 0 else	717	0.53	0.50	0	1
One period	=1 if no control for time control is needed (one-period data), 0 else	717	0.01	0.12	0	1
DID	=1 if a difference-in-difference design is used, 0 else	717	0.65	0.48	0	1
Spatial	=1 if a control for unobserved spatial factors is included (e.g., spatial fixed effects, repeat sales approach), 0 else	717	0.97	0.16	0	1
OLS	=1 if OLS is used for estimation, 0 else	717	0.74	0.44	0	1
<b>Wind turbine impact</b>						
Dist binary	=1 if distance variable is binary instead of categorical, 0 else	717	0.24	0.43	0	1
Other wind	=1 if other wind turbine related variables are used (e.g., view, number of turbines), 0 else	717	0.25	0.43	0	1
Announcement effect	=1 if wind turbine announcement instead of construction is considered, 0 else	717	0.28	0.45	0	1
AE and CE	=1 if anticipation and construction effect are both controlled for in the same regression, 0 else	717	0.53	0.50	0	1
Adjustment control	=1 if price adjustment over time after construction is controlled for in the same regression, 0 else	717	0.06	0.23	0	1

**Table 2.2:** (continued).

Pooled	=1 if wind turbines sites are pooled into one impact variable, 0 else	717	0.93	0.25	0	1
One site	=1 if only one wind turbine site is analysed, 0 else	717	0.06	0.23	0	1
<b>Publication</b>						
SE	Standard error of effect size	717	4.29	6.82	0.02	76.54
Sig	=1 if 'β' is significant (up to a p-value of 0.1), 0 else	717	0.47	0.50	0	1
Reviewed	=1 if published in a peer-reviewed journal, 0 else	717	0.65	0.48	0	1
Year publish	Year in which the study was published	717	2016.84	3.73	2009	2022
Precision set	=1 if precision was set by meta-analyst, 0 else	717	0.15	0.35	0	1

Notes: SD = standard deviation. \* denotes variables with missing observations, which are only used in robustness checks.

### 2.4.2 Estimation

The choice of the correct meta-analytic model is a context-specific and data-driven issue, which involves (at least) decisions on treating publication bias as well as meta-model and moderator selection. Publication bias has clearly been confirmed, and I subsequently defined several relevant variables that can be considered to address this issue in a regression framework. Regarding model selection, I adopt a standard multivariate meta-regression model in the baseline specification. That is

$$w\text{indef}ect_i = \beta_0 + \sum_{k=1}^K \beta_k * M_{k,i} + v_i + \epsilon_i \quad i = 1, \dots, N \quad (2.1)$$

where each reported effect size  $i$  is put in relation to a set of  $k$  study characteristics used as moderators  $M_{k,i}$ , with  $v_i$  reflecting unobserved heterogeneity assumed to be  $v_i \sim N(0, \tau^2)$  and  $\epsilon_i \sim N(0, \widehat{\sigma}_i^2)$  representing the error term. Equation 2.1 is estimated using WLS with random effects weights, i.e.,  $w_i = \frac{1}{\widehat{\sigma}_i^2 + \tau^2} = \frac{1}{SE_i^2 + \tau^2}$ , where  $SE_i$  is the standard error corresponding to the reported effect size  $i$ . This gives greater weight to more precise observations in the dataset and addresses heteroscedasticity in the error term, which naturally occurs when combining estimates with different variances from several studies.<sup>9</sup> I assess the effect of choosing this baseline specification on the results using a series of robustness checks, including different assumptions on the error term, weighting schemes, and data dependency. The main results are unaffected by the model selection, as discussed in Subsection 2.4.4.

Finally, while all moderators introduced above may systematically influence the effect size, using all of them jointly in the regression may obscure true data patterns since some moderators will prove collinear in explaining wind turbine effects on property values. Accordingly, I use BMA to address model ambiguity. This technique does not require selecting one particular specification for the meta-regression.<sup>10</sup> BMA has become a frequently used tool in economics in general (Steel, 2020) and meta-analysis in particular (Havranek et al., 2015; Matousek et al., 2022) to address model uncertainty. For a recent overview, see Steel (2020). The following brief description covers the basics

<sup>9</sup> Note that there is some controversy regarding the decision for or against random effects weights in the literature. Stanley and Doucouliagos (2017) show in simulations that fixed effects estimation with inverse-variance weights is, in general, preferable to random effects estimation if publication bias is present. In this study, however, the reported standard errors differ substantially in magnitude, resulting in excessive influence of very precise observations and barely any weight for imprecise ones in the fixed effects WLS framework. Moreover, Irsova et al. (2023) show that exaggerated precision of observations entering the meta-sample leads to biases in fixed effects WLS estimation. Even excluding the most precise observations does not address the weighting problem adequately. Additionally, there are many observations with high and low precision simultaneously with no clear cut-off point. Accordingly, I decided to use the WLS-RE framework in the baseline setting, which offers more evenly distributed weights due to adding the parameter  $\tau^2$ .

<sup>10</sup> Another popular option is to follow a general-to-specific approach, which includes a step-wise exclusion of moderators that fail to fall within a certain significance threshold. While commonly applied, this technique is not statistically valid and may lead to omitting relevant moderators (Havranek et al., 2015).

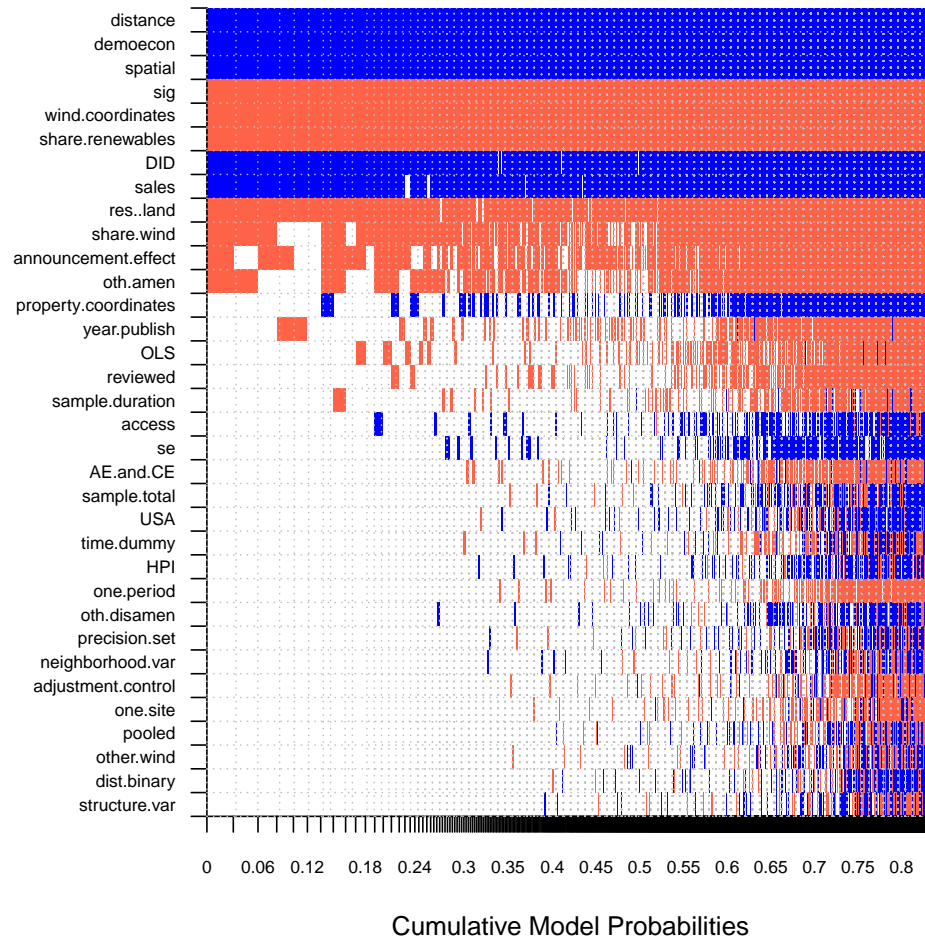
of this technique and introduces relevant terms needed for inference in the subsequent analysis.

In BMA, all possible combinations of moderators are estimated in individual regressions, and a weighted average of these models is constructed. The weights correspond to the posterior model probabilities. This measure reflects the model fit of individual specifications conditional on the data and model parsimony, analogous to adjusted  $R^2$  in a frequentist setting. The importance of each moderator is measured in terms of posterior inclusion probability (PIP), i.e., the sum of all posterior model probabilities for all regressions that include the specific variable. This corresponds to statistical significance in frequentist econometrics (Steel, 2020). Here, with 34 potential moderators with full observations  $2^{34}$  models could be estimated. To reduce computational complexity, I follow common practice and rely on the Markov Chain Monte Carlo algorithm included in the `bms` package for R (Zeugner and Feldkircher, 2015), which considers only the most promising models, i.e., those with the highest posterior model probabilities.<sup>11</sup> BMA requires the selection of priors for the model space and regression coefficients (so-called g-prior). Without sufficient knowledge about the coefficients' magnitude, I choose the popular unit information prior as g-prior in the baseline specification, which performs well in simulations (Eicher et al., 2011). This prior assumes a zero mean for all coefficients and has about the same information content as one observation. For the model space, I use the uniform model prior (Eicher et al., 2011), which gives each model the same probability. I assess the sensitivity of results using other common choices for the prior structure in robustness checks, see Subsection 2.4.4.

### 2.4.3 Results

Figure 2.4 illustrates the results of BMA. The columns represent individual regression models sorted by their posterior model probability starting with the best model on the left. The vertical axis lists the variables in descending order, sorted by posterior inclusion probability indicating importance. A blank cell indicates the variable is not included in the respective model. The red colour implies that the corresponding regression coefficient is positive, while a blue cell signals the negative sign of the coefficient. The best model in terms of posterior probabilities on the left includes twelve out of the 34 variables used in the analysis. These variables are also the only ones with a posterior inclusion probability above 0.5. This threshold signals a non-negligible effect of these variables on the effect size in the classification of Kass and Raftery (1995). According to this rule of thumb, the moderators have a weak, positive, strong, or decisive impact of the effect size if the PIP lies between 0.5 and 0.75, 0.75 and 0.95, 0.95 and 0.99, or 0.99 and 1, respectively. All other variables do not systematically influence the magnitude of the estimated effect.

<sup>11</sup> I employ 50 million iterations and 10 million burn-ins to ensure convergence.

**Figure 2.4:** Model inclusion probability of moderators.

*Notes:* The response variable is the estimated price-distance coefficient relating wind turbines and property values. The columns represent individual models sorted by posterior model probability. The variables are depicted on the vertical axis, ordered by posterior inclusion probability in a descending array. A blue (red) cell indicates the inclusion of the variable in the model and that the estimated sign is positive (negative). A blank cell indicates that the variable is not included in the model. The uniform model prior and the unit information prior (Eicher et al., 2011) are used for the model space and the coefficients, respectively. Corresponding numerical results are presented in Table 2.3.

Table 2.3 gives the corresponding numerical results of BMA. The left panel reports the posterior mean, posterior standard deviation, and posterior inclusion probability for each explanatory variable's regression coefficient. The right panel shows the results of a frequentist WLS check, including the twelve variables with a posterior inclusion probability of 0.5 and higher. The estimated coefficients in both panels have the same sign and similar magnitudes as well as the same statistical importance (posterior inclusion probability in the BMA setting and its frequentist equivalent,

p-value). Accordingly, the results of the frequentist check are consistent with the baseline BMA. In the following, I present results by moderator category.

#### *Data characteristics*

The type and quality of the analysed data are central determinants of the estimated effect size. In particular, using exact wind turbine coordinates for the distance calculation seems essential to estimate the price-distance relationship reliably. Relying instead on, e.g., wind farm centroids induces imprecise estimation of this price-distance relationship, reflected in the large coefficient for this variable.<sup>12</sup> Moreover, the type of analysed property price seems to be an important dimension of effect size variance. Using actual sales data instead of asking prices or assessed values is associated with significantly more positive findings of about 3.26 percentage points. One possible interpretation for this finding is that residents who decide to offer their property include a price discount in their asking price, mirroring the (subjectively perceived) lower value due to wind turbine presence. Similarly, assessors seem to devalue properties with a proximate wind turbine on average. Apparently, both types of property prices (asking and assessed) lead to inflated negative estimates of the effect of wind turbines compared to actual sales prices.<sup>13</sup> The type of analysed property also seems to affect the effect size. Investigating the price effect on undeveloped residential land instead of residential buildings leads to more negative estimates on average (−9.54 percentage points). This may be due to the increased visibility of wind turbines when no structure has been built yet. Note, however, that only one study (Sunak and Madlener, 2017) contributing 15 observations relies on residential land values. Accordingly, I caution against generalizing this finding even though it remains robust to the inclusion of study-level fixed effects. In line with expectations, I find that the values of properties that are located at greater distances from a wind turbine are less affected, i.e., for each additional mile of distance from a wind turbine, property values increase by 0.73 percentage points, *ceteris paribus*. Additionally, for countries with a comparatively higher share of wind power in their electricity mix (or renewable energies in general) when the respective study took place, corresponding estimates document more negative effects on property values on average. One possible explanation might be that with an increased share, wind turbines are built closer to residential areas since more remote areas have already been used. Finally, neither the sampling duration, sample size nor the USA dummy influence the reported effect size systematically.

#### *Control variables*

Using adequate control variables proves essential to disentangle the wind turbine effect from other price-influencing factors. In particular, studies lacking sufficient data to

<sup>12</sup> Note, however, that only 16 observations from two studies do not rely on exact wind turbine coordinates. Even though the finding is robust to including study-level fixed effects, see Subsection 2.4.4, I caution against generalizing this finding accordingly.

<sup>13</sup> While this finding is robust in almost all sensitivity analyses, note that including study-level fixed effects leads to a loss in significance for this variable, see Subsection 2.4.4.

**Table 2.3:** Bayesian model averaging results.

Moderators	Bayesian Model Averaging			Frequentist Check (WLS)		
	Post. Mean	Post. SD	PIP	Coef.	SE	p-value
<b>Data characteristics</b>						
Wind coordinates	-14.956	2.803	1.000	-11.605	2.603	0.000
Property coordinates	0.398	0.873	0.227			
Sales	3.262	1.153	0.955	3.543	0.817	0.000
Res. land	-9.541	3.924	0.931	-10.470	2.859	0.000
Distance	0.730	0.089	1.000	0.721	0.083	0.000
Sample duration	-0.005	0.020	0.089			
Sample total	0.006	0.051	0.050			
USA	0.020	0.233	0.048			
Share renewables	-0.059	0.013	0.997	-0.052	0.010	0.000
Share wind	-0.265	0.189	0.753	-0.301	0.100	0.003
<b>Control variables</b>						
Structure var	-0.002	0.148	0.027			
Neighbourhood var	0.007	0.119	0.033			
Access	0.060	0.258	0.081			
Oth disamen	0.013	0.133	0.038			
Oth amen	-0.728	0.756	0.550	-1.046	0.440	0.018
Demoecon	4.651	0.775	1.000	5.068	0.697	0.000
HPI	0.022	0.259	0.042			
Time dummy	-0.012	0.170	0.042			
One period	-0.296	2.196	0.040			
DID	2.429	0.826	0.963	2.327	0.538	0.000
Spatial	11.365	1.075	1.000	11.172	1.058	0.000
OLS	-0.163	0.453	0.152			
<b>Wind turbine impact</b>						
Dist binary	0.002	0.084	0.029			
Other wind	0.002	0.100	0.031			
Announcement effect	-0.761	0.657	0.649	-1.050	0.398	0.009
AE and CE	-0.058	0.301	0.068			
Adjustment control	-0.010	0.151	0.032			
Pooled	-0.002	0.369	0.032			
One site	-0.029	0.414	0.032			
<b>Publication</b>						
SE	0.001	0.004	0.076			
Sig	-2.459	0.371	1.000	-2.560	0.356	0.000
Reviewed	-0.133	0.437	0.119			
Year publish	-0.001	0.001	0.206			
Precision set	0.008	0.168	0.035			
Constant	-0.063		1.000	-4.559	2.683	0.090
$R^2$					0.669	
$N$		717			717	

*Notes:* The response variable is the estimated price-distance coefficient relating wind turbines and property values. SD = standard deviation, PIP = posterior inclusion probability, SE = standard error. On the left, the BMA results are presented relying on the uniform model prior and the unit information prior (Eicher et al., 2011). The results for a frequentist WLS regression with the most important variables (PIP > 0.5) identified by BMA are shown on the right.



control for unobserved local price differentials using, e.g., spatial fixed effects or a repeat sales approach (*Spatial*), report more negative effect sizes (about eleven percentage points). Additionally, studies unable to control for pre-existing price-differentials via a difference-in-difference design (*DID*) generally report more negative effect sizes, as expected (about  $-2.43$  percentage points).<sup>14</sup> Similarly studies accounting for socio-economic factors like income levels or population density (*Demoecon*) document less adverse wind turbine effects. Additionally, if other amenities like a park or beach are present, the estimated wind turbine effect is more negative on average (*Oth amen*). Apparently, having a living-quality enhancing element in close vicinity worsens the effect of wind turbines.<sup>15</sup> Other moderators reflecting the inclusion of control variables for house-structure characteristics, neighbourhood aspects, infrastructure access options, or the presence of other disamenities do not systematically influence the wind turbine effect. Similarly, using estimation approaches different from OLS or the choice of approaches to control for time trends are not affecting the reported wind turbine impact systematically.

#### *Specification of wind turbine impact*

Studies differ to a great extent in the way the wind turbine impact is specified. However, the reported effect size is largely unaffected by design choices in this dimension. The only exception is the dummy *Announcement effect*, which shows that choosing the announcement date of a wind turbine as treatment results in more negative estimates compared to using the construction date.<sup>16</sup> Using one treatment zone (*Dist binary*), i.e., a binary distance specification, does not lead to significantly different findings compared to categorical specifications with multiple treatment zones. In the same vein, controlling for announcement and construction effect simultaneously (*AE and CE*) or for a potential habituation effect (*Adjustment control*) does not change the effect size systematically. Additionally, the standard approach to pool observations from different sites to increase sample size (*Pooled*) does not alter the reported estimates. Surprisingly, studies using *other wind* turbine controls next to the distance variable (e.g., view, number of turbines) do not report estimates smaller in absolute terms, i.e., more positive price-distance effect sizes. This would be expected, assuming that other wind turbine controls take on some of the effects. Apparently, other moderators prove more important in explaining the effect size variance.

#### *Publication*

The combined findings for three variables show the existence and type of publication bias: First, I find that significant estimates of wind turbine effects (*Sig*) are more negative

<sup>14</sup> This finding is in line with an average location effect, i.e., a pre-existing price-differential, of  $-3.01\%$ , which could be calculated for a subset of studies included in this meta-sample that reported this value.

<sup>15</sup> Note, however, that this moderator loses importance if other prior settings are considered, see Subsection 2.4.4.

<sup>16</sup> It is worth noting, however, that this dummy loses importance using other model priors, see Subsection 2.4.4.

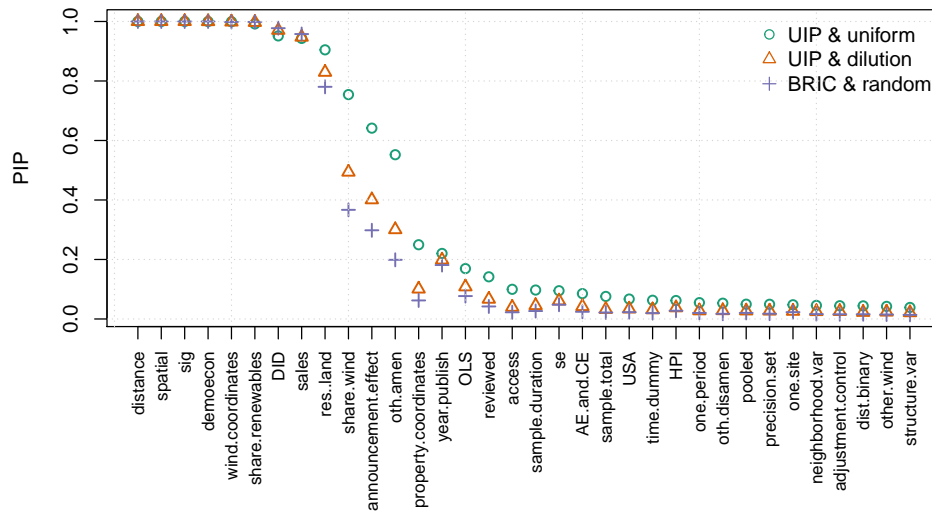
on average (-2.46 percentage points). Second, the standard error (*SE*) is not a systematic factor in explaining the heterogeneity, i.e., selection does not take place based on effect size magnitude and its significance. Finally, studies that are published in peer-reviewed journals do not differ systematically in terms of effect size magnitude compared to unpublished manuscripts. Linking these findings, I conclude that publication bias is still confirmed with heterogeneity-explaining factors in place. The type of publication bias is a one-sided selection, favouring significantly negative estimates of wind turbines' impacts on property values.<sup>17</sup> The publication year is not affecting the effect size. Reassuringly, the coding decision to set the precision level at specific values in cases when this metric is reported imprecisely (see again Figure B.2 for details) does not affect the estimated mean effect size.

#### 2.4.4 Robustness checks

The robustness of the results is assessed from several perspectives. First, I change the BMA prior settings: This includes (i) substituting the uniform model prior with the dilution prior, which allows for collinear moderators in each particular model (George, 2010), (ii) combining the benchmark g-prior with the beta-binominal model prior (Fernandez et al., 2001; Ley and Steel, 2009), which implies equal prior probability for each model size. I compare the results in terms of variable importance in Figure 2.5. I conclude that the results are largely insensitive to the selection of priors with the exceptions mentioned above for the dummies *Announcement effect* and *Oth amen*, which lose importance using the alternative priors. The corresponding numerical results are summarized in Table B.4. They confirm the main findings.

Next, I run a series of frequentist robustness checks based on the set of moderators identified by the baseline BMA summarized in Table 2.3 with a  $PIP > 0.5$ . I document the findings in Table B.5, Table B.6 and Table B.7 in the appendix. First, several moderators with missing observations that were not considered in the main specification are added in separate regressions, respectively. These moderators reflect wind turbine characteristics (average turbine height, installed capacity, number of turbines, and number of wind farms) as well as additional aspects related to the property sample used in the respective primary study (number of treated properties, number of properties at the construction stage of the wind turbine(s) to which the coefficient refers, number of properties within one mile of a turbine, number of properties within one mile of a turbine after turbine construction). Both types of variables are suspected to influence the effect of wind turbines on property values, e.g., multiple turbines are presumably related to more negative effects due to the increased likelihood of visual impacts or noises (Jensen et al., 2014; Jensen et al., 2018). At the aggregate level, however, these factors do not translate to economically significant changes in the reported effect size, see

<sup>17</sup> This interpretation is supported by a histogram of t-values shown in Figure B.3 showing a sharp drop in the number of reported observations in the significant positive domain.

**Figure 2.5:** Posterior inclusion probabilities with changed priors.

Notes: UIP and uniform = Baseline setting used in Table 2.3 with priors following Eicher et al. (2011). UIP and Dilution = Uniform model prior exchanged for dilution prior (George, 2010). BRIC and Random = Benchmark g-prior for coefficients (Fernandez et al., 2001) combined with the beta-binominal model prior (Ley and Steel, 2009).

Table B.5. Only one of the additionally included moderators is statistically significant, i.e., a larger number of treated properties analysed by the primary studies is associated with more negative findings. Most of the other moderators are robust, with only minor changes that can be attributed to the reduced number of observations considered for these subsample regressions (717 in the baseline specification vs. 393 if the number of treated properties is included as moderator).

In a second set of specifications, I investigate the effect of different definitions of outliers see Table B.6. These cover the range from *no outlier* to the omission of 89 observations using the inter-quartile range criterion following Tukey (1977). The results are robust to changes in this dimension. Finally, in Table B.7, I document the sensitivity of results to modifications of the meta-analytic model. This includes (i) changes to the estimation (using heteroscedasticity-robust standard errors and standard errors clustered at the study-level), (ii) adding study-level fixed effects and, (iii) changing the weighting scheme to inverse-variance weights (WLS-FE), or using no weights (OLS) (reported for completeness). While the main findings are confirmed, some changes are worth noting. Altering the assumptions for calculating the error term does not change the results. Using OLS reduces the explanatory power as expected ( $R^2$  drops from 0.669 to 0.218) and several variables lose significance. This again demonstrates the need to rely on WLS estimation in meta-regression analysis. Instead, the choice of weights (fixed or

random effects) is less critical, with only minor changes documented in the WLS-FE framework due to the unbalanced weighting scheme.

Finally, when study-level fixed effects are added, the dummies *Sales* and *Oth amen* lose significance. This is probably due to the fact that only a few studies do not use sales data and *Oth amen* already found to be less robust in other BMA settings. The change in sign and significance of *Share renewables* is due to the high share of renewables in Sweden of about 60% in the study period of Westlund and Wilhelmsson (2022) and Westlund and Wilhelmsson (2021). This is significantly higher than the average of 19.45% in this meta-dataset. Omitting these observations results in coefficients similar to those in the baseline specification, but with a loss of significance for *Share renewables*. I therefore caution against generalising the findings related to this variable.

#### 2.4.5 Implied effect size

As the final step of the analysis, I compute the wind turbine impact on residential property values conditional on the absence of publication and misspecification bias. To this end, I calculate an average effect size that can be expected for a hypothetical study that follows "best practice" regarding methodology and data quality. Specifically, I use the results from the baseline BMA analysis and compute fitted values of the effect size when specific values for the variables with  $PIP > 0.5$  are used. While arguably certain aspects of study design are preferable to others, any best-practice specification remains subjective by design. In order to increase plausibility, I follow a conservative calculation approach: When there is good reason to prefer a particular type of study design, I use the preferred value (e.g., I use 1 for the dummy variable corresponding to DID design); otherwise, I use the respective study mean to reflect my indifference. I summarize the assumptions on each moderator and their respective effects on the implied effect size in Table 2.4.

To permit an accurate distance calculation, I prefer exact wind turbine coordinates. Similarly, I consider estimates from actual sales prices to reflect wind turbine impacts more realistically. For the *distance* of treated properties as well as *Share wind* and *Share renewables*, I consider the respective study means (1.89 miles, 2.30% and 19.40%). Similarly, I multiply the *Res. land* coefficient by 0.5 to reflect my indifference. In terms of control variables, I prefer rich data sets allowing to control for socioeconomic factors (*Demoecon*) and unobserved spatial factors (*Spatial*) and the application of a DID design (*DID*), which - all - may lead to misspecification bias if not accounted for. Instead, the presence of other amenities is not a study quality dimension, so I remain agnostic regarding this moderator. Finally, to correct for publication bias, I set the significance dummy to 0.

The calculated conditional effect size for this best practice specification is  $-0.68\%$ . This is considerably smaller than the unconditional and only publication bias corrected mean effect size of  $-1.59\%$  identified with RoBMA-PSMA in Table 2.1. This underlines the importance of correcting for misspecification bias to obtain a realistic effect size estimate. This conditional effect size is by definition sensitive to changes in the best practice specification. For example, setting the distance to a hypothetical value of 0 changes the effect size to  $-2.04\%$ . The effect becomes zero if a distance of 2.8 miles (i.e. 4.5 kilometres) is specified. This corresponds to a cut-off point often used in primary studies beyond which no effect of wind turbines is suspected, and is identical to the result of the so-called "mini meta-analysis" by Parsons and Heintzelman (2022).

**Table 2.4:** Implied effect size.

Moderator	Assumption	Factor	Coef.	Effect
Wind coordinates	Exact coordinates preferred	1	-11.605	-11.605
Sales	Sales prices preferred	1	3.543	3.543
Res. land	Indifference	0.5	-10.470	-5.235
Distance	Sample mean	1.896	0.721	1.363
Share renewables	Sample mean	19.454	-0.052	-1.011
Share wind	Sample mean	2.301	-0.301	-0.693
Oth. amen.	Indifference	0.5	-1.046	-0.523
Demoecon	Controls for socioeconomic factors	1	5.068	5.068
DID	DID design	1	2.327	2.327
Spatial	Controls for unobserved spatial factors	1	11.172	11.172
Announcement effect	Indifference	0.5	-1.050	-0.525
Sig	Publication bias control	0	-2.560	0
Constant		1	-4.559	-4.559
<b>Implied effect size</b>				<b>-0.678</b>

## 2.5 Discussion and conclusion

This meta-study is the first to systematically assess the hedonic literature on the price-distance relationship between wind turbines and property values. It addresses the considerable ambiguity in empirical findings investigating its existence and magnitude. Combining 720 observations from 25 studies using BMA and novel publication bias correction methods, I identify the most essential moderators explaining the observed heterogeneity and calculate an average effect size for this relationship.

Three main conclusions emerge from this study. First, selective reporting of significant negative findings is responsible for overestimating the effect size by about 25% (correcting the unconditional mean effect size from  $-2.14\%$  to  $-1.59\%$ ).<sup>18</sup> This is in line with ubiquitous publication bias in large parts of the economic literature (Bartoš et al., 2022). Second, next to selective reporting, various data characteristics (e.g., accuracy of the

<sup>18</sup> Conversion of the effect size into monetary values would be informative, but is not feasible. This requires information on the average prices of the properties analysed in the primary studies and the base year to which these prices were converted for comparability. This type of information was only available for a small subset of observations, so calculating the effect size in monetary terms is not meaningful.

distance calculation, the distance of treated properties, type of property price data) as well as the ability to control for confounding factors, i.e., using a DID framework for accounting for pre-existing price differentials and inclusion of appropriate controls for socio-economic and unobservable spatial factors explain the considerable variation in empirical findings. Third, conditional on the absence of misspecification and publication bias, the effect size is  $-0.68\%$  for properties 1.89 miles away when calculating a (subjective) best-practice average. For distances greater than 2.8 miles, there is no evidence of a wind turbine effect. These results are robust to changes in the BMA setup, meta-model specification, and outlier treatment.

These findings can inform future research and policymakers in at least three ways. First, future hedonic pricing studies on this subject should rely on a proper identification strategy using a DID design and a rich dataset with sufficient control variables. This ensures that pre-existing price differentials and other confounding factors are not wrongly attributed to the presence of wind turbines. Since the effect is small, studies should also rely on many observations, especially in close vicinity to wind turbines, to have enough statistical power to detect an effect if it exists in the respective setting. In addition, recent methodological advances that reflect the staggered nature of the treatment, i.e., the fact that observations from different wind farms with different corresponding construction dates are pooled, should be adopted (Chaisemartin and D'Haultfœuille, 2022; Chaisemartin and D'Haultfœuille, 2020; Steigerwald et al., 2021).

Second, future meta-analyses on this topic could construct alternative effect size variables from other impact measures used in this literature (e.g., view, continuous distance, or number of turbines within a certain distance) that could not be used in this study to ensure comparability. This would help to better understand what drives the occasionally documented negative effects of wind turbines on property values. In addition, future meta-analyses focusing on other energy generation facilities (e.g. nuclear power plants or solar farms) could help to place the results of this study in the general context of the effects of disamenities on property values. Compared to the few existing meta-analyses that consider other types of disamenity and focus on price-distance relationships, the effects of wind turbines are in the lower range of estimates. Schütt (2021), for example, documents that property values increase on average between 1.5% and 2.9% per mile of increased distance from waste sites. Lipscomb et al. (2013) report an increase in value of 6.1% per mile for properties close to contaminated water bodies.

Lastly, for policymakers the aggregated evidence of the literature could indicate the appropriateness of monetary compensation for homeowners with properties very close to wind turbines. While the effect size is small on average, payments could be apt to acknowledge the "local cost, global benefit" (Frondel et al., 2019) situation of localized externalities from wind turbines.

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## Appendix

### Study selection

For inclusion, studies had to fulfil the following requirements: First, I only considered studies that use the hedonic pricing method to estimate the relationship between property values and wind turbines. This excludes studies that investigate simple sales-date and sales-price relationships (e.g., Sterzinger et al., 2003; Baker, 2021; Dupont and Etherington, 2009) as well as surveys (e.g., Khatri, 2004; Hoen et al., 2019). Second, I restricted the meta-sample to studies using the price of residential properties as the dependent variable. This thus excluded studies investigating the impact on farmland values (e.g., Sampson et al., 2020; Pates et al., 2020), which presumably benefit from the presence of wind turbines due to land lease payments. Third, the wind turbine impact must be measured in terms of distance from the respective properties. Distance is a reasonable proxy for wind turbine impact (Parsons and Heintzelman, 2022) and is the most frequently used metric. Accordingly, this excludes studies using other impact measures, e.g., view, number of turbines, installed capacity, or turbine density (Dong and Lang, 2022; Gibbons, 2015; Jensen et al., 2018; Sunak and Madlener, 2016)<sup>19</sup> Fourth, the distance of properties from a wind turbine has to be defined in discrete terms, i.e., using one or more distance zones for segmentation. I thus omit studies using continuous distance-variable definitions.<sup>20</sup> Fifth, studies are required to report the respective regression coefficient and its measure of precision. This excludes cases for which this essential information was unavailable despite contact (efforts) with the respective author(s) (e.g., Hebllich et al., 2016; Sims and Dent, 2007). Using these study selection criteria ensures a consistent set of effect size estimates that can reasonably be meta-analysed.<sup>21</sup> More formally, the study selection criteria ensure that studies included in the meta-sample report results from a variant of the following stylized hedonic pricing specification (omitting indices for simplicity):

$$P = \alpha + \beta * DIST + \gamma * X + u \quad (2.2)$$

with  $P$  reflecting the price of residential properties,  $DIST$  the distance to a wind turbine,  $X$  a set of control variables and  $u$  a common error term. The coefficient of interest is  $\beta$ , which serves as the dependent variable in the subsequent analysis. To arrive at the

<sup>19</sup> Different impact metrics cannot be standardized unless one uses a non-economic, dimensionless effect size such as the partial correlation coefficient (for applications see Doucouliagos and Ulubaşoğlu (2008) and Linde Leonard et al. (2014)). Here, I choose economic interpretability over sample size and include only those studies that report a price-distance relationship. This excludes twelve studies.

<sup>20</sup> Studies with continuous distance measures cannot be combined with studies using a discrete distance measure without the loss of information or the introduction of measurement error in the meta-analysis (see the discussions in Guignet et al. (2022), Schütt (2021), Ready (2010), and Debrezion et al. (2007)). Here, most studies use a discrete distance measure leading to its selection as the required metric. Accordingly, nine studies with only continuous distance definitions are dropped.

<sup>21</sup> See also the discussion on consistency in meta-analyses in Vedogbeton and Johnston (2020) and Schütt (2021).

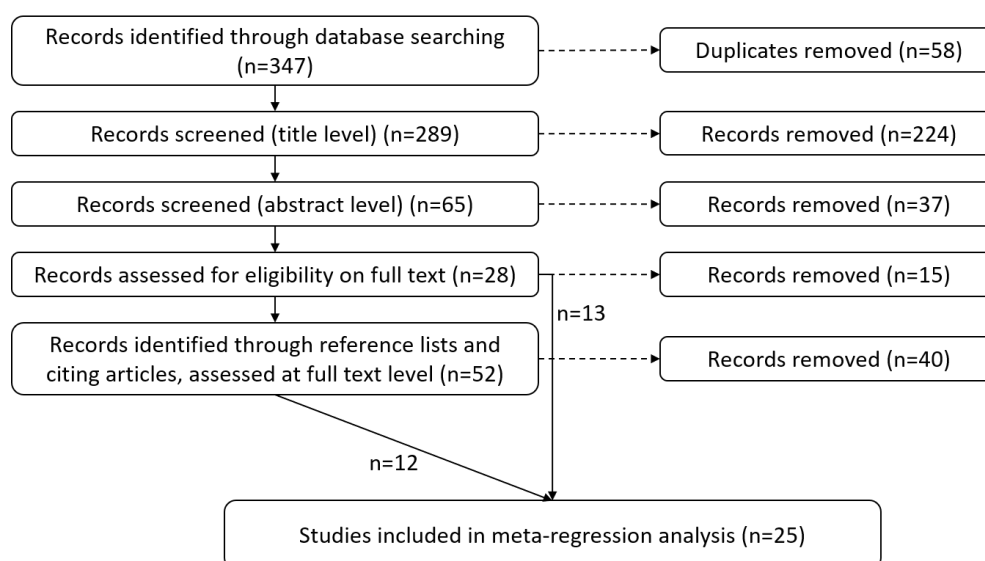
selection of studies, I use a predefined search query, which I document in Table B.1 below, followed by the PRISMA statement (Figure B.1), the list of included studies (Table B.2) as well as the list of excluded studies, ordered by reason for exclusion (Table B.3).

**Table B.1:** Search query.

Search category	Synonyms / Wildcards
Residential Property	("Real estate*" OR residential* OR propert* OR hous* OR apartment OR building OR condo* OR dwllin* OR home OR residence OR mansion* OR domicile OR flat)
Hedonic Price Model	(Hedonic* OR "repeat sale*" OR "Diff*in*Diff*")
Price	(Price* OR price* OR value* OR benfi* OR cost OR premium OR worth OR compensat* OR damag* OR pay* OR sum)
Wind Turbine	(Wind* OR turbine)

*Notes:* Search term categories are interconnected via the Boolean operator "AND".

**Figure B.1:** PRISMA diagram.



*Notes:* The PRISMA diagram (Moher et al., 2009) shows the study selection process from initial screening to full-text-level assessment.

**Table B.2:** Included studies (N=25).

- C. Atkinson-Palombo and B. Hoen (2014). *Relationship between wind turbines and residential property values in Massachusetts*. University of Connecticut.
- N. Camplair (2012). "Does Proximity to Wind Farms Affect the Value of Nearby Residential Properties? Evidence from Washington and New York States". Macalester College. URL: %5Curl%7Bhttps://digitalcommons.macalester.edu/econaward/13%7D.
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- P. Eichholtz et al. (2021). "Clean electricity, dirty electricity: the effect on local house prices". In: *The Journal of Real Estate Finance and Economics*, pp. 1–35. doi: \url{10.1007/s11146-021-09878-6}.
- M. Frondel et al. (2019). "Local cost for global benefit: The case of wind turbines". In: *Ruhr Economic Papers* 791. doi: \url{10.4419/86788919}.
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- M. Joly and S. de Jaeger (2021). "Not in my backyard. A hedonic approach to the construction timeline of wind turbines in Flanders, Belgium". In: *Land Use Policy* 108. doi: \url{10.1016/j.landusepol.2021.105527}.<sup>22</sup>

<sup>22</sup> Nine observations were dropped from this study since the sign of the reported effect size and its corresponding t-statistic did not fit. Moreover, 15 estimates of effects from pre-existing turbines were not considered to ensure comparability across studies.

**Table B.2:** (continued).

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- H. Westlund and M. Wilhelmsson (2022). *Valuating the negative externality of wind turbines: traditional hedonic and difference-in-difference approaches*. KTH Royal Institute of Technology.

**Table B.3:** Excluded studies sorted by reason for exclusion (N=58).**Not hedonic pricing method (N=15)**

- M. Alem et al. (2020). "Qualitative meta-analysis of the socioeconomic impacts of offshore wind farms". In: *Sustinere: Journal of Environment and Sustainability* 4.3, pp. 155–171. doi: \url{10.22515/sustinere.jes.v4i3.121}.
- C. E. Baker (2021). "The Impact of Wind Turbines on Agricultural Land Values in West Texas". Master's Thesis. Texas Tech University. URL: %5Curl%7Bhttps://hdl.handle.net/2346/87469%7D.
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- R. R. Dupont and J. Etherington (2009). *Preliminary assessment of the impact of wind farms on surrounding land values in Australia*. Preston Rowe Paterson Newcastle and Central Coast. URL: %5Curl%7Bhttps://www.valuergeneral.nsw.gov.au/\_\_data/assets/pdf\_file/0006/195315/Preliminary\_assessment\_impact\_of\_wind\_farms\_on\_surrounding\_land\_values\_in\_Australia.pdf%7D.
- P. Haan and M. Simmler (2018). "Wind electricity subsidies—A windfall for landowners? Evidence from a feed-in tariff in Germany". In: *Journal of Public Economics* 159, pp. 16–32. doi: \url{10.1016/j.jpubeco.2018.01.011}.
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- J. Rand and B. Hoen (2017). "Thirty years of North American wind energy acceptance research: What have we learned?" In: *Energy Research and Social Science* 29, pp. 135–148. doi: \url{10.1016/j.erss.2017.05.019}.
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- G. Sterzinger et al. (2003). *The Effect of Wind Development on Local Property Values (Washington, DC: Renewable Energy Policy Project)*. URL: %5Curl%7Bhttp://www.repp.org/articles/static/1/binaries/wind\_online\_final.pdf%7D.
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- F. Wen et al. (2018). "Exploring the rebound effect from the perspective of household. An analysis of China's provincial level". In: *Energy Economics* 75, pp. 345–356. doi: \url{10.1016/j.eneco.2018.08.018}.

**No wind turbine impact measure (N=5)**

- P. M. Anglin and R. Gencay (1996). "Semiparametric Estimation of a Hedonic Price Function". In: *Journal of Applied Econometrics* 11.6, pp. 633–648. doi: \url{10.1002/(SICI)1099-1255(199611)11:6<633::AID-JAE414>3.0.CO;2-T}.
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**Table B.3:** (continued).

J. M. Ross et al. (2011). "Inconsistency in Welfare Inferences from Distance Variables in Hedonic Regressions". In: *Journal of Real Estate Finance and Economics* 43.3, pp. 385–400. doi: \url{10.1007/s11146-009-9221-z}.

**Impact measure is not distance (N=13)**

G. Canning and L. J. Simmons (2010). *Wind Energy Study-Effect on Real Estate Values in the Municipality of Chatham-Kent, Ontario*.

Daams, Michiel, Sijtsma, Frans (2019). *Windparken en Woningprijzen in Groningen en Drenthe. Rapportage in Opdracht van RTV Noord en RTV Drenthe*. University of Groningen.

L. Dong and C. Lang (2022). "Do views of offshore wind energy detract? A hedonic price analysis of the Block Island wind farm in Rhode Island". In: *Energy Policy* 167, p. 113060. doi: \url{10.1016/j.enpol.2022.113060}.

W. Feilmayr et al. (2018). *Windkraftnutzung und der Einfluss auf Grundstückspreise in Österreich. Preisentwicklung von Grundstückspreisen in Österreichs Windkraftgemeinden (1999–2017)*. Department für Raumplanung, TU Wien.

S. Gibbons (2015). "Gone with the wind. Valuing the visual impacts of wind turbines through house prices". In: *Journal of Environmental Economics and Management* 72.4, pp. 177–196. doi: \url{10.1016/j.jeem.2015.04.006}.<sup>23</sup>

J. Grib (2019). "Wind Turbines and Residential Property Values". Undergraduate Thesis. UC Santa Barbara. URL: %5Curl%7Bhttps://escholarship.org/uc/item/0z84b44m%7D.

C. U. Jensen et al. (2018). "The impact of on-shore and off-shore wind turbine farms on property prices". In: *Energy Policy* 116, pp. 50–59. doi: \url{10.1016/j.enpol.2018.01.046}.

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I. McCarthy and H. O. Balli (2014). "Windfarms and residential property values". In: *International Journal of Strategic Property Management* 18.2, pp. 116–124. doi: \url{10.3846/1648715X.2014.889770}.

T. Möllney (2022). "Investigating the Causal Impact of Wind Turbines on Housing Prices in Germany". Master's Thesis. Kiel University.

O. Myrna et al. (2019). "The Influence of Wind Energy and Biogas on Farmland Prices". In: *Land* 8.1, p. 19. doi: \url{10.3390/land8010019}.

M. Ritter et al. (2015). "Der Einfluss von Windkraftanlagen auf landwirtschaftliche Bodenpreise". In: *Berichte über Landwirtschaft-Zeitschrift für Agrarpolitik und Landwirtschaft* 93.3. doi: \url{10.12767/buel.v93i3.83}.

Y. Sunak and R. Madlener (2016). "The impact of wind farm visibility on property values. A spatial difference-in-differences analysis". In: *Energy Economics* 55, pp. 79–91. doi: \url{10.1016/j.eneco.2015.12.025}.

**Distance is continuous (N=10)**

C. Krekel and A. Zerrahn (2017). "Does the presence of wind turbines have negative externalities for people in their surroundings? Evidence from well-being data". In: *Journal of Environmental Economics and Management* 82, pp. 221–238.

B. Castleberry and J. S. Greene (2018). "Wind power and real estate prices in Oklahoma". In: *International Journal of Housing Markets and Analysis* 11.5, pp. 808–827. doi: \url{10.1108/IJHMA-02-2018-0010}.

A. Fønnesbech-Wulf et al. (2011). *Welfare impacts of landscape dis-amenities. Comparative hedonic approaches*.

<sup>23</sup> For this study, distance and view variables are exclusively used jointly in interactions with no distinct distance variable. This precludes a meaningful comparison with the included studies.



**Table B.3:** (continued).

- M. D. Heintzelman et al. (2017). "Understanding the Amenity Impacts of Wind Development on an International Border". In: *Ecological Economics* 137.C, pp. 195–206. doi: \url{10.1016/j.ecolecon.2017.03.008}.
- B. Hoen (2006). "Impacts of windmill visibility on property values in Madison County, New York". Master's Thesis. Bard College. URL: %5Curl%7Bhttps://www.bcsd.org/site/handlers/filedownload.ashx?moduleinstanceid=1829&dataid=8961&FileName=prop\_value1.pdf%7D.
- C. U. Jensen et al. (2014). "The Vindication of Don Quixote. The Impact of Noise and Visual Pollution from Wind Turbines". In: *Land Economics* 90.4, pp. 668–682. doi: \url{10.3368/le.90.4.668}.
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- O. Joalland and T. Rambonilaza (2017b). "Valeur touristique des aménités environnementales et nuisances associées aux infrastructures d'énergie renouvelable. Une approche hédonique spatiale. (Tourist Value of Environmental Amenities and Nuisances Due to Renewable Energy Infrastructure: A Spatial Hedonic Approach. With English summary.)" In: *Region et Développement* 46, pp. 93–115. URL: %5Curl%7Bhttps://regionetdeveloppement.univ-tln.fr/wp-content/uploads/7\_Joalland.pdf%7D.
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- S. Sims et al. (2008). "Modelling the impact of wind farms on house prices in the UK". in: *International Journal of Strategic Property Management* 12.4, pp. 251–269. doi: \url{10.3846/1648-715X.2008.12.251-269}.

**Unpublished version of already assessed studies (N=7)**

- B. Castleberry (2017). "Community-Level Impacts of Wind Power in Oklahoma". Master's thesis. University of Oklahoma. URL: %5Curl%7Bhttps://hdl.handle.net/11244/50905%7D.
- P. M. A. Eichholtz et al. (2019). *Clean Electricity, Dirty Electricity. The Effect on Local House Prices*. doi: \url{10.2139/ssrn.2826508}.
- B. Hoen (2013). "Wind Farms in North America". In: *Towers, turbines and transmission lines. Impacts on property value*. Chichester: Wiley, pp. 253–287. ISBN: 978-1-4443-3007-6.
- B. Hoen et al. (2013). *A Spatial Hedonic Analysis of the Effects of Wind Energy Facilities on Surrounding Property Values in the United States*. Office of Scientific and Technical Information. doi: \url{10.2172/1165267}.
- S. Jarvis (2020). "Studies in Empirical Policy Evaluation. New Methods and Applications to the Energy Transition". Dissertation. UC Berkeley.
- C. U. Jensen et al. (2013). *The Vindication of Don Quijote. The impact of noise and visual pollution from wind turbines on local residents in Denmark*. University of Copenhagen, Department of Food and Resource Economics.
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**Missing information (N=6)<sup>24</sup>**

- C. Andersen and T. Hener (2022). *Wind Turbines and the Housing Market*.
- S. Heblich et al. (2016). *Impact of wind turbines on house prices in Scotland*. University of Sheffield.

<sup>24</sup> These studies could not be included since essential information were not available despite contact (efforts) with the respective author(s). Andersen and Hener (2022) is a working paper for which citation is currently not allowed. Heblich et al. (2016) displays results in figures for which corresponding exact point estimates and precision measures could not be obtained. For Rakitan (2017), Sims and Dent (2007) and Svensk (2010) descriptive statistics miss to calculate the effect size used in this meta-study, while for Yu and Levy (2016) information on regression results could not be accessed.

**Table B.3:** (continued).

T. J. Rakitan (2017). *Essays in the economics of energy development and disamenities*. Iowa State University. doi: \url{10.31274/etd-180810-5829}.

S. Sims and P. Dent (2007). "Property stigma. Wind farms are just the latest fashion". In: *Journal of Property Investment & Finance* 25.6, pp. 626–651. doi: \url{10.1108/14635780710829315}.

V. Svensk (2010). "Vindkraft i Sikte: Hur Påverkas Fastighetspriserna vid Etablering av Vindkraft". In: *Svensk Vindenergi: Stockholm, Sweden*.

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**Farmland values as dependent variable (N=2)<sup>25</sup>**

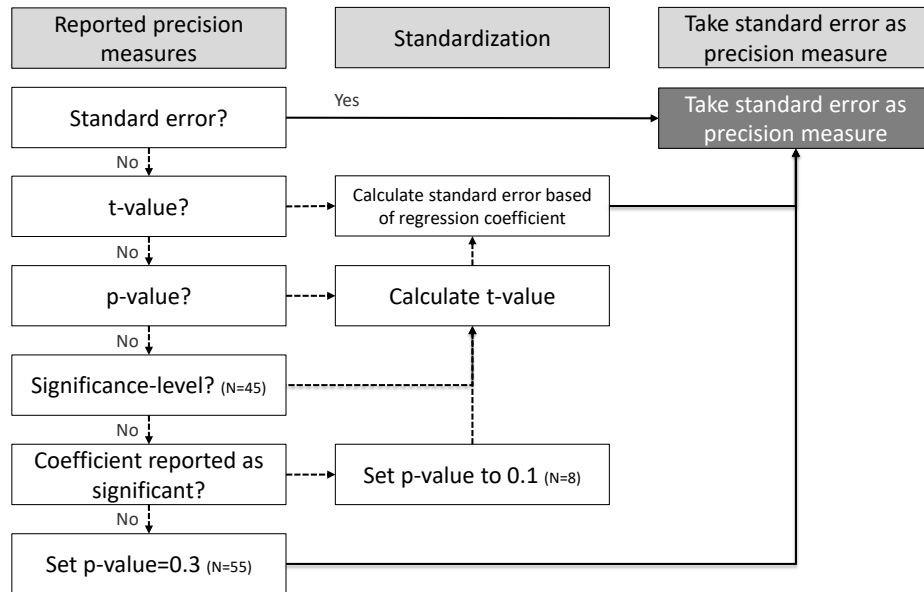
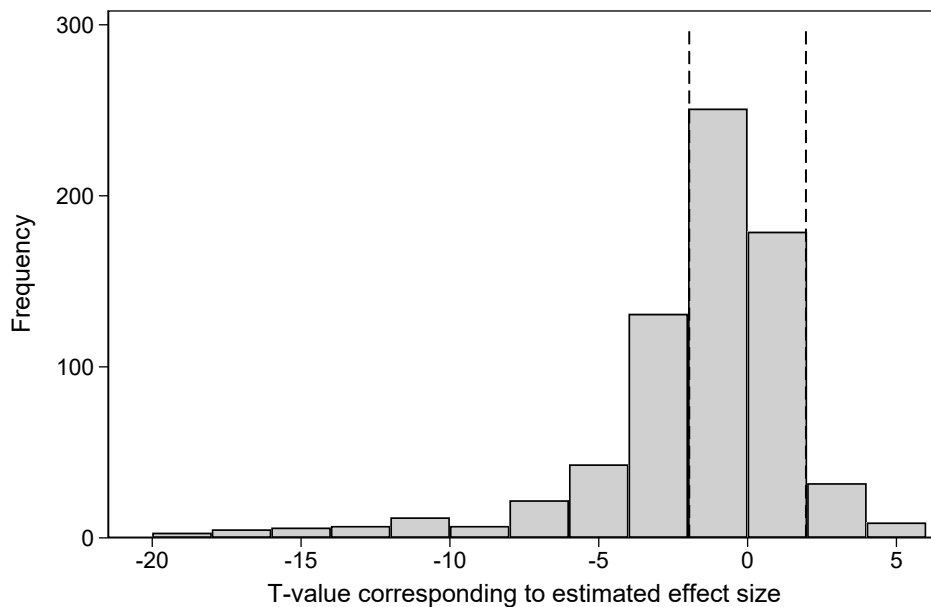
N. J. Pates et al. (2020). *Windfalls or wind falls? The Local Effects of Turbine Development on US Agricultural Land Values*.

G. S. Sampson et al. (2020). "The On-Farm and Near-Farm Effects of Wind Turbines on Agricultural Land Values". In: *Journal of Agricultural and Resource Economics* 45.3, pp. 410–427. doi: \url{10.22004/ag.econ.302463}.

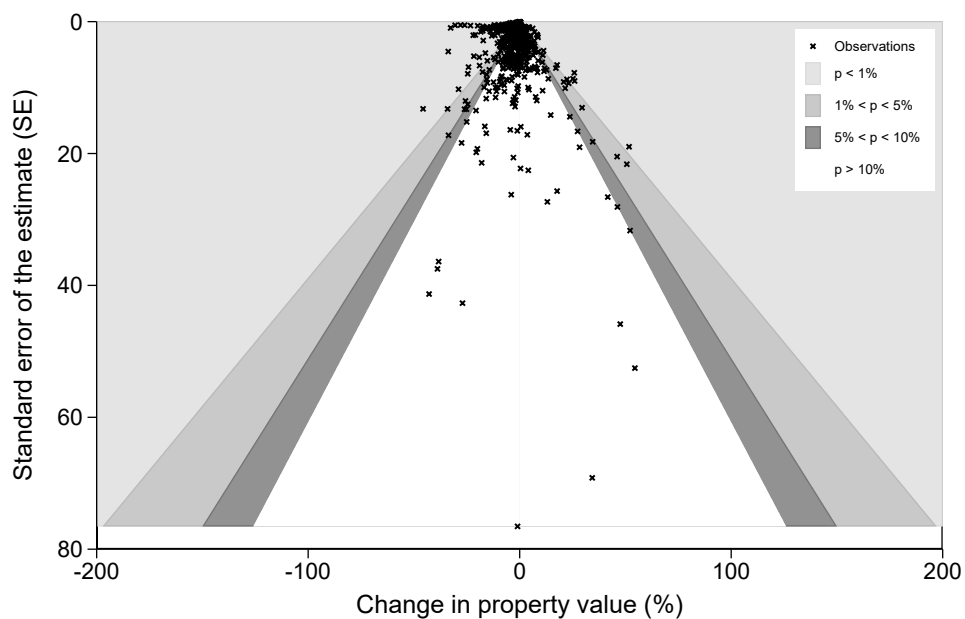
**Standardization**

The precision measures for the included effect size estimates differ in metric (e.g., standard errors, t-values, or p-values) and in accuracy, i.e., exact numbers versus reporting of significance levels. The corresponding standardization process to standard errors is summarized in Figure B.2 below following Schütt (2021). If standard errors are reported, they are considered at face value as a precision measure without further standardization being needed. T-values are converted to standard errors using the relation  $SE = \frac{\beta}{t}$ , with  $SE$  representing the standard error of the reported regression coefficient  $\beta$ . To convert p-values, I use the *t.inv2s* function from Microsoft Excel in conjunction with the reported sample size. If only significance *levels* are reported, I use the lowest precision as a conservative threshold (e.g., setting  $p = 0.05$  or a five percent significance level). If only its insignificance is known for a coefficient, I set  $p = 0.3$ . In rare cases, the p-value is reported to be 0.000, in which case I set the value to 0.0001, allowing further calculations. This procedure enables the application of common meta-analytic methods that require the standard error (e.g., correcting for publication bias and using inverse-variance weights), as outlined in Subsection 2.4.2.

<sup>25</sup> Farmland values tend to increase if wind turbines are placed in the vicinity due to expected payments for land rents. This is the opposite direction of effect expected for residential properties, such that an exclusion seems warranted.

**Figure B.2:** Standardization of precision measure.**Robustness: Figures****Figure B.3:** T-value distribution.

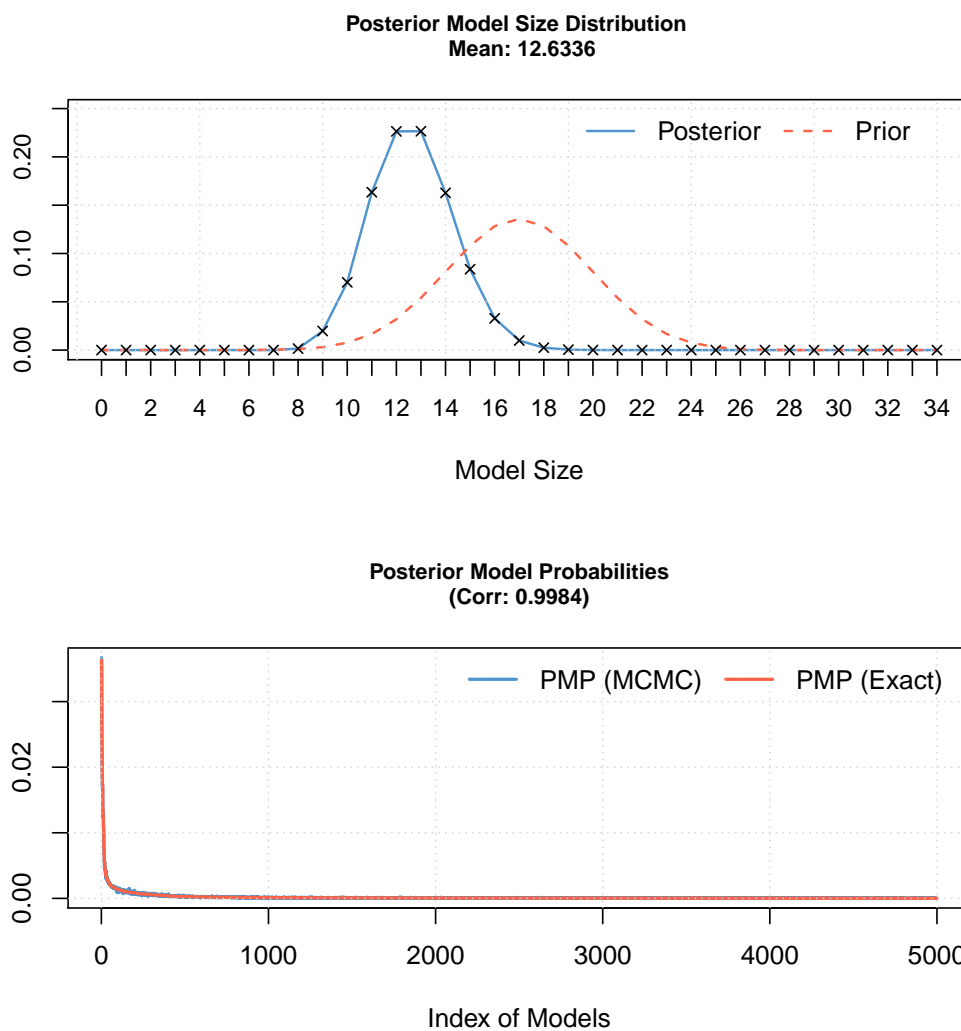
*Notes:* The histogram shows the distribution of t-values corresponding to the effect size estimates from the primary studies. The dashed lines indicate the threshold for statistical significance at the 5%-level.

**Figure B.4:** Contour-enhanced funnel plot.

*Notes:* The figure shows a contour-enhanced funnel plot, relating effect size estimates to their reported standard error (*SE*). Without publication bias the plot should take the shape of an inverted funnel. If an observation is within one of the shaded areas, the hypothesis of a null effect can be rejected at the corresponding significance level.

## Robustness: Bayesian Model Averaging

**Figure B.5:** Model size and convergence for the baseline BMA specification.



*Notes:* The figure shows the posterior model size distribution and the posterior model probabilities of the baseline BMA specification reported in Table 2.3.

**Table B.4:** Bayesian model averaging - Changing the priors.

Moderators	UIP and dilution			BRIC and random)		
	Post Mean	Post. SD	PIP	Post. Mean	Post SD	PIP
<b>Data characteristics</b>						
Wind coordinates	-15.301	2.899	0.998	-15.597	2.874	0.998
Property coordinates	0.154	0.576	0.095	0.094	0.451	0.063
Sales	3.356	1.186	0.945	3.429	1.129	0.955
Res. land	-8.518	4.734	0.831	-8.039	4.980	0.786
Distance	0.718	0.087	1.000	0.714	0.087	1.000
Sample duration	-0.002	0.012	0.038	-0.001	0.009	0.027
Sample total	0.003	0.041	0.037	0.002	0.034	0.027
USA	0.025	0.225	0.037	0.011	0.137	0.021
Share renewables	-0.063	0.013	0.997	-0.065	0.013	0.997
Share wind	-0.160	0.189	0.485	-0.119	0.174	0.377
<b>Control variables</b>						
Structure var	-0.000	0.144	0.024	-0.000	0.123	0.018
Neighbourhood var	0.001	0.090	0.022	-0.001	0.069	0.016
Access	0.019	0.154	0.037	0.012	0.123	0.026
Oth disamen	0.005	0.102	0.026	0.003	0.081	0.018
Oth amen	-0.367	0.637	0.290	-0.259	0.555	0.207
Demoecon	4.514	0.759	0.999	4.474	0.749	0.999
HPI	0.023	0.245	0.034	0.023	0.221	0.028
Time dummy	-0.004	0.137	0.031	-0.003	0.117	0.020
One period	-0.214	1.892	0.030	-0.138	1.530	0.020
DID	2.545	0.789	0.967	2.620	0.716	0.980
Spatial	11.421	1.079	1.000	11.435	1.078	1.000
OLS	-0.127	0.419	0.110	-0.094	0.367	0.080
<b>Wind turbine impact</b>						
Dist binary	0.003	0.077	0.023	0.003	0.060	0.016
Other wind	0.004	0.091	0.024	0.003	0.076	0.017
Announcement effect	-0.435	0.607	0.389	-0.324	0.550	0.295
AE and CE	-0.035	0.252	0.044	-0.012	0.148	0.023
Adjustment control	-0.008	0.127	0.023	-0.004	0.096	0.016
Pooled	-0.001	0.380	0.026	-0.002	0.325	0.019
One site	-0.030	0.422	0.027	-0.025	0.374	0.020
<b>Publication</b>						
SE	0.001	0.004	0.057	0.001	0.004	0.047
Sig	-2.462	0.368	1.000	-2.472	0.365	1.000
Reviewed	-0.062	0.302	0.063	-0.042	0.247	0.043
Year publish	-0.001	0.002	0.197	-0.001	0.001	0.179
Precision set	0.003	0.135	0.024	0.002	0.124	0.020
Constant	-0.090		1.000	-0.101		1.000
<i>N</i>		717			717	

*Notes:* The response variable is the estimated price-distance coefficient relating wind turbines and property values. SD = standard deviation, PIP = posterior inclusion probability, UIP and dilution = Unit information prior and dilution model prior (Eicher et al., 2011; George, 2010). BRIC and Random = Benchmark g-prior for coefficients (Fernandez et al., 2001) combined with the beta-binomial model prior (Ley and Steel, 2009).

## Robustness: Frequentist meta-regression results

Table B.5: Robustness regressions - Moderators with missing observations.

	Turbine characteristics					Sample sizes			
	(1) Baseline	(2) # turbines	(3) # farms	(4) Height	(5) Capacity	(6) Stage	(7) Treated	(8) 1 mile	(9) 1 mile PC
<b>Data characteristics</b>									
Wind coordinates	-11.61*** (2.60)	-7.21 (4.84)	-10.19** (4.80)	-8.29 (5.42)	-8.34 (5.32)	-12.12*** (2.96)	-11.23*** (2.86)	-12.88*** (2.58)	-15.82*** (3.04)
Sales	3.54*** (0.82)	2.44 (1.55)	11.10** (4.30)	12.58*** (4.53)	12.65*** (4.44)	4.02*** (1.19)	0.30 (1.47)	0.99 (0.98)	0.27 (1.08)
Res land	-10.47*** (2.86)	-9.54*** (2.95)	-11.79*** (2.96)	-12.30*** (3.17)	-12.28*** (3.09)	-9.56*** (2.97)	-11.01*** (2.92)	-10.46*** (2.80)	-10.21*** (2.72)
Distance	0.72*** (0.08)	1.02*** (0.12)	0.28*** (0.06)	0.15 (0.14)	0.17 (0.13)	0.92*** (0.12)	0.94*** (0.16)	0.65*** (0.09)	0.80*** (0.10)
Share renewables	-0.05*** (0.01)	-0.03 (0.03)	-0.04*** (0.01)	0.09* (0.05)	0.09** (0.04)	-0.06*** (0.01)	-0.09*** (0.02)	-0.06*** (0.01)	-0.06*** (0.01)
Share wind	-0.30*** (0.10)	-0.53*** (0.18)	-0.68** (0.34)	-0.47*** (0.13)	-0.44*** (0.13)	-0.56*** (0.17)	-0.36** (0.16)	-0.37*** (0.13)	-0.46*** (0.14)
<b>Control variables</b>									
Demoecon	5.07*** (0.70)	4.87*** (0.76)	3.56*** (0.60)	2.29*** (0.54)	2.27*** (0.53)	5.08*** (0.96)	4.49*** (0.90)	4.48*** (0.69)	4.43*** (0.70)
Oth amen	-1.05** (0.44)	-1.27** (0.53)	0.04 (0.45)	-0.11 (0.26)	-0.19 (0.26)	-3.05*** (0.85)	-1.56** (0.68)	-1.02* (0.59)	-2.08*** (0.77)
DID	2.33*** (0.54)	3.48*** (0.73)	0.82* (0.47)	0.62 (0.52)	0.65 (0.49)	3.40*** (0.79)	4.48*** (0.80)	3.17*** (0.90)	3.44*** (0.90)
Spatial	11.17*** (1.06)	11.61*** (1.18)	6.31 (5.36)	5.02*** (1.89)	5.06*** (1.85)	12.03*** (1.41)	12.94*** (1.30)	12.96*** (1.09)	13.24*** (1.13)
<b>Wind turbine impact</b>									
Announcement effect	-1.05*** (0.40)	-1.43*** (0.49)	-0.98*** (0.31)	-0.61* (0.32)	-0.63** (0.28)	-2.04*** (0.63)	-2.44*** (0.65)	-1.56*** (0.47)	-1.56*** (0.51)
<b>Publication</b>									

**Table B.5:** (continued).

	Turbine characteristics					Sample sizes			
	(1) Baseline	(2) # turbines	(3) # farms	(4) Height	(5) Capacity	(6) Stage	(7) Treated	(8) 1 mile	(9) 1 mile PC
Sig	-2.56*** (0.36)	-2.04*** (0.46)	-1.87*** (0.25)	-1.19*** (0.25)	-1.18*** (0.24)	-2.61*** (0.59)	-1.16* (0.69)	-2.54*** (0.40)	-2.62*** (0.46)
<b>Missing observations</b>									
Turbines		0.00 (0.00)							
Farms			0.00 (0.00)						
Height				-0.00 (0.01)					
Capacity					-0.12 (0.20)				
Sample stage						0.14 (0.20)			
Sample treated							-0.46*** (0.14)		
Sample 1mile								-0.15 (0.13)	
PC 1mile									-0.26 (0.16)
Constant	-4.56* (2.68)	-9.46* (5.02)	-7.48 (5.33)	-10.17 (7.33)	-10.13 (7.20)	-4.93 (3.44)	-1.06 (3.24)	-1.60 (2.78)	2.89 (3.50)
$R^2$	0.669	0.672	0.585	0.811	0.805	0.663	0.693	0.708	0.705
$N$	717	596	461	421	449	457	393	570	462

*Notes:* The table displays the sensitivity of results to the inclusion of moderators with missing observations, with the baseline WLS specification from Table 2.3 displaying on the left for ease of comparison. The added variables are defined in Table 2.2. \*\*\*, \*\* and \* denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively.



**Table B.6:** Robustness regressions - Different outlier specifications.

	Baseline		Outlier criterion			
	(1)	(2) No outlier	(3) Win. 2.5%	(4) P 1%	(5) P 2.5%	(6) IQR
<b>Data characteristics</b>						
Wind coordinates	-11.61*** (2.60)	-11.61*** (2.61)	-9.65*** (2.47)	-10.39*** (2.61)	-6.43** (2.52)	-4.02* (2.31)
Sales	3.54*** (0.82)	3.54*** (0.82)	3.42*** (0.76)	3.52*** (0.81)	3.12*** (0.74)	2.71*** (0.60)
Res. land	-10.47*** (2.86)	-10.47*** (2.87)	-10.36*** (2.77)	-10.57*** (2.82)	-9.28*** (2.87)	-5.98 (4.02)
Distance	0.72*** (0.08)	0.72*** (0.08)	0.70*** (0.08)	0.72*** (0.08)	0.65*** (0.07)	0.52*** (0.06)
Share renewables	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)
Share wind	-0.30*** (0.10)	-0.30*** (0.10)	-0.29*** (0.09)	-0.30*** (0.10)	-0.25*** (0.09)	-0.24*** (0.07)
<b>Control variables</b>						
Demoecon	5.07*** (0.70)	5.07*** (0.70)	4.90*** (0.65)	5.01*** (0.69)	4.57*** (0.63)	4.08*** (0.51)
Oth amen	-1.05** (0.44)	-1.06** (0.44)	-0.96** (0.41)	-0.97** (0.43)	-0.84** (0.40)	-0.70** (0.33)
DID	2.33*** (0.54)	2.32*** (0.54)	2.25*** (0.51)	2.28*** (0.53)	2.23*** (0.49)	1.27*** (0.41)
Spatial	11.17*** (1.06)	11.17*** (1.06)	9.93*** (0.99)	11.11*** (1.05)	7.04*** (1.09)	2.90** (1.18)
<b>Wind turbine impact</b>						
Announcement effect	-1.05*** (0.40)	-1.05*** (0.40)	-1.04*** (0.37)	-1.05*** (0.39)	-1.01*** (0.36)	-0.76** (0.30)
<b>Publication</b>						
Sig	-2.56*** (0.36)	-2.56*** (0.36)	-2.57*** (0.33)	-2.55*** (0.35)	-2.65*** (0.32)	-2.40*** (0.26)
Constant	-4.56* (2.68)	-4.55* (2.69)	-5.10** (2.55)	-5.66** (2.68)	-5.14** (2.54)	-2.24 (2.23)
<i>N</i>	717	720	720	706	684	631
<i>R</i> <sup>2</sup>	0.669	0.669	0.669	0.668	0.598	0.565

*Notes:* The table displays the sensitivity of results to changes in the outlier specification, with the baseline WLS specification from Table 2.3 displaying on the left for the ease of comparison. "No outlier" = No observation excluded, "Win 2.5%" = Observations winsorized at the 2.5% level, "P 1%" = Observations excluded based on the 1% percentile, "P 2.5%" = Observations excluded based on the 2.5% percentile, "IQR" = Observations excluded based on the interquartile range (Tukey, 1977). \*\*\*, \*\* and \* denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively.

**Table B.7:** Robustness regressions - Alternative model specifications.

	RE weights			(4) Study FE	FE weights	No weight
	(1) Baseline	(2) Robust SE	(3) Cluster SE		(5) WLS-FE	(6) OLS
<b>Data characteristics</b>						
Wind coordinates	-11.61*** (2.60)	-11.61*** (3.40)	-11.61*** (2.80)	-16.61*** (3.75)	-12.06*** (4.43)	-14.72*** (5.21)
Sales	3.54*** (0.82)	3.54*** (1.09)	3.54*** (1.24)	-10.22 (27.56)	4.49*** (0.59)	1.79 (2.71)
Res. land	-10.47*** (2.86)	-10.47*** (1.58)	-10.47*** (0.61)	-7.57** (3.36)	-12.48 (8.39)	-10.95*** (2.37)
Distance	0.72*** (0.08)	0.72*** (0.09)	0.72*** (0.20)	0.83*** (0.09)	0.25*** (0.03)	0.59*** (0.16)
Share renewables	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	0.59** (0.25)	0.00 (0.00)	-0.06*** (0.02)
Share wind	-0.30*** (0.10)	-0.30*** (0.10)	-0.30* (0.16)	-1.48*** (0.50)	-0.75*** (0.09)	-0.40* (0.22)
<b>Control variables</b>						
Semoecon	5.07*** (0.70)	5.07*** (0.96)	5.07*** (0.73)	5.01*** (0.99)	6.24*** (0.49)	3.46* (1.79)
Oth amen	-1.05** (0.44)	-1.05*** (0.37)	-1.05* (0.52)	-0.93 (1.01)	-0.99*** (0.30)	-1.23 (1.05)
DID	2.33*** (0.54)	2.33*** (0.54)	2.33*** (0.81)	3.40*** (1.30)	-0.39 (0.31)	2.50 (1.62)
Spatial	11.17*** (1.06)	11.17*** (2.27)	11.17*** (3.15)	11.85*** (1.18)	8.13*** (0.55)	14.91*** (2.66)
<b>Wind turbine impact</b>						
Announcement effect	-1.05*** (0.40)	-1.05*** (0.29)	-1.05* (0.52)	-1.17** (0.47)	-0.45 (0.29)	-0.81 (0.89)
<b>Publication</b>						
Sig	-2.56*** (0.36)	-2.56*** (0.28)	-2.56*** (0.60)	-2.43*** (0.38)	-0.61*** (0.10)	-1.28 (0.97)
Constant	-4.56* (2.68)	-4.56 (3.21)	-4.56** (2.06)	8.12 (27.73)	0.20 (4.50)	-3.23 (5.12)
$R^2$	0.669	0.598	0.598	0.689	0.684	0.218
$N$	717	717	717	717	717	717

*Notes:* The table displays the sensitivity of results to changes in the model specification, with the baseline WLS specification from Table 2.3 displaying on the left for ease of comparison. "Robust SE" = Standard errors are robust to heteroscedasticity, "Cluster SE" = Standard errors are clustered at the study level, "Study FE" = Study-level fixed effects are included, "WLS-FE" = Inverse-variance weights are used, "OLS" = Ordinary least squares. \*\*\*, \*\* and \* denote statistical significance at the 0.01, 0.05 and 0.1 level, respectively.

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## Chapter 3

# How to Explain the Huge Differences in Rebound Estimates: A Meta-Regression Analysis of the Literature

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### **Abstract:**

Rebound effects are commonly defined as the relative gap between the potential and realised savings in resource use following efficiency improvements or sufficiency changes. While a considerable number of studies quantify rebound effects, empirical estimates vary widely. Governments hence still lack reliable information on the magnitude of rebound effects in order to adjust their policies for the sustainable use of natural resources accordingly. Here, we present the first meta-analysis of microeconomic rebound effects at the household level, using 43 studies with 1043 estimates to determine average rebound effects and to explain heterogeneous empirical findings. We find that the total microeconomic rebound is, on average, about 36-43%. The variance can be explained by differences in the type of data used, the scenario setup and the specifics of the rebound estimation in the primary studies. Furthermore, we also find only small absolute transfer errors, indicating a good predictability of rebound effects using our meta-regression model.

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### 3.1 Introduction

Governments around the world have set ambitious targets for a more sustainable use of natural resources, including for example the reduction of primary and final energy consumption as part of their decarbonisation targets.<sup>1</sup> To achieve these targets, they are using a wide range of policies. Some of these policies have unintended consequences including so-called rebound effects. First described by Jevons (1865) and reintroduced by Brookes (1979) and Khazzoom (1980), rebound effects are commonly defined as the relative gap between the potential and realized savings in resource use following an efficiency improvement. Taking energy as an example: if energy prices remain constant, an energy efficiency improvement lowers the cost of the respective energy service. Economic agents can thus consume more of the energy service itself (*direct rebound effect*) or spend the money saved on other goods and services, which in turn require energy to be produced and provided (*indirect rebound effect*). Consequently, direct and indirect rebound effects result in energy savings that are lower than those expected from a pure engineering perspective that does not account for these behavioural adjustments (Azevedo, 2014; Sorrell et al., 2009; York et al., 2022). Governments hence need information on the magnitude of rebound effects in order to adjust their energy and climate policies accordingly.

The basic rebound concept is applicable in various contexts. That is, one can consider direct and indirect rebound effects on the level of individual economic agents such as households or firms (*microeconomic rebound effects*) or extend the analysis to the economy-wide market adjustments following from these effects (*economy-wide or macroeconomic rebound effects*). Furthermore, rebound effects can be assessed not only in terms of energy consumption, but also in terms of other impact measures such as greenhouse gas (GHG) emissions or material requirements. They also occur not only after energy efficiency improvements (*efficiency rebound*), but also following other actions such as sufficiency-related behavioural changes of households (*sufficiency rebound*). For example, when households voluntarily forego the consumption of meat and dairy products to reduce their GHG footprints, but spend the money saved on other goods and services, the production and provision of which in turn cause GHG emissions (Reimers et al., 2021; Sorrell, 2007).

The broad applicability of the concept and its high relevance for energy and climate policies has led to a diverse literature quantifying direct, indirect, and economy-wide rebound effects in different contexts (for recent reviews of the literature see Biewendt et al., 2020; Brockway et al., 2021; Mashhadi Rajabi, 2022; Reimers et al., 2021; Sorrell et al.,

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<sup>1</sup> The European Union's (EU's) Energy Efficiency Directive (European Parliament, Council of the European Union, 2018), for example, sets an energy efficiency target for 2030 of at least 32.5% compared to projections of the expected energy use in 2030.

2020; York et al., 2022) and discussing theoretical underpinnings and methodological challenges (e.g., Turner, 2013; Borenstein, 2015; Chan and Gillingham, 2015).

In this paper we focus on the subset of studies that quantify total microeconomic rebound effects, i.e., the sum of indirect and, where applicable, direct rebound effects, following efficiency improvements and sufficiency-related behavioural changes at the household level.<sup>2</sup> For policy making, the total rebound effect is of more relevance, as quantification of direct rebound effects alone is likely to overestimate the outcome of a policy at the household level.

To date, a considerable number of studies quantifying microeconomic rebound effects at the household level exist. Their effect sizes vary widely, mostly ranging between zero and 100%, but also providing estimates well below and above this range (see Reimers et al., 2021). This variance in estimates, which has so far been poorly explained, is not only puzzling from a research perspective, but also limits the usefulness of the literature for policy-making. Accordingly, a better understanding of the causes of heterogeneity in empirical results is needed. In this paper, we therefore use meta-analytic techniques (Stanley and Doucouliagos, 2012) to estimate the expected magnitude of microeconomic rebound effects and to explain the heterogeneity of the estimates in terms of study characteristics and scenarios considered.

To the best of our knowledge, this is the first meta-regression analysis of this literature. Existing reviews aimed to (1) explain microeconomic rebound effects from a theoretical perspective, (2) provide methodological guidance, (3) qualitatively discuss the range of empirical estimates, or (4) identify research gaps. In doing so, they focus on different aspects such as efficiency rebounds (Azevedo, 2014; Gillingham et al., 2016), sufficiency rebounds (Sorrell, 2012; Sorrell et al., 2018; Sorrell et al., 2020), and economic and psychological mechanisms driving microeconomic rebound effects (Dütschke et al., 2018; Reimers et al., 2021; Sorrell et al., 2020). Expanding the perspective beyond the literature mainly focused on microeconomic rebound effects on the household level, a number of other reviews exist that address, e.g., direct rebound effects (Sorrell et al., 2009), economy-wide rebound effects (Brockway et al., 2021; Sorrell, 2007) and the rebound literature as a whole (Biewendt et al., 2020; Hertwich, 2005; Mashhadi Rajabi, 2022; Santarius et al., 2018; York et al., 2022). The only meta-regression analysis within the entire rebound literature is presented by Dimitropoulos et al. (2018) who conducted an analysis on direct rebound effects in road transport, including 74 primary studies. They find that the average direct rebound effect is 10-12% in the short run and 26-29% in the long run, and identify several moderators that affect the size of the estimated effects.

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<sup>2</sup> See Figure C.1 and the associated text in the appendix for a detailed explanation of the total microeconomic rebound effect and its components.

With such quantitative insights missing in other fields of the rebound literature, we are the first to provide an estimate of the average microeconomic rebound effect at the household level. Based on a set of 43 primary studies with 1043 estimates, we disentangle the effects of different settings (such as efficiency and sufficiency actions in different areas of consumption preceding the rebound effect) and of methodological choices (such as the use of different types of data and different assumptions on households' re-spending behaviour) on the size of the estimated rebound effects. Furthermore, we assess the reliability of our results by evaluating transfer errors. We thus provide guidance to policy makers, who require reliable estimates of the effectiveness of resource-saving policies after accounting for both direct and indirect rebound effects. We also provide guidance to researchers, who may find our results regarding the implications of different methodological choices helpful in making informed decisions when designing future studies.

The paper is structured as follows. Section 3.2 gives an overview of the methodology. In Section 3.3, we describe the selection of studies and moderators for the meta-regression analysis. The results are presented in Section 3.4, including summary statistics and robustness checks. In Section 3.5, we assess the usefulness of the results of our analysis for policy making by calculating transfer errors. In the final Section 3.6 we discuss our findings and draw conclusions.

## 3.2 Methodology

The literature on microeconomic rebound effects we analyse is characterized by heterogeneous study designs, e.g., employing different estimation methods, relying on varying types of household expenditure data or using differing rebound impact measures (Reimers et al., 2021). This motivates the choice of a standard multivariate meta-analytical model as a starting point for our analysis to explain the presence of heterogeneity of effect sizes, i.e.,

$$Rebound_i = \alpha_0 + \sum_{k=1}^K \alpha_k * C_{k,i} + \epsilon_i \quad i = 1, 2, \dots, M, \quad (3.1)$$

with *Rebound* representing the observed effect sizes,  $C_{k,i}$  depicting the  $k^{th}$  study characteristic assigned to effect size  $i$  and  $\epsilon_i$  a random error term with  $\epsilon_i \sim N(0, \sigma_i^2)$ . Here,  $\alpha_0$  can be interpreted as an estimate of the magnitude of the total microeconomic rebound effect conditional on the set of moderating variables  $C_{k,i}$ . By assumption, any deviation from the mean that is not addressed by the set of moderators is due to random sampling error (Ringquist, 2013).

However, it is common in the relevant rebound literature to report multiple estimates, e.g., due to different re-spending scenarios, alternative rebound impact measures or

income levels. This leads to potential dependence of estimates from the same study due to the common data source, estimation procedures or other methodological choices (Stanley and Doucouliagos, 2012; Penn and Hu, 2019). Consequently, we explicitly account for potentially non-independent observations by introducing a second study layer to equation (1) that reflects the nested structure of the estimates, i.e.,

$$Rebound_{ij} = \alpha_0 + \sum_{k=1}^K \alpha_k * C_{k,ij} + \lambda_j + \epsilon_{ij} \quad j = 1, 2, \dots, S, \quad (3.2)$$

with  $j$  indexing the study level.

Following the meta-model decision pathways detailed by Feld and Heckemeyer (2011) and Stanley and Doucouliagos (2012), we employ the Breusch and Pagan Lagrange multiplier (BPLM) test to decide whether the study-level effect  $\lambda$  is a relevant addition to the model. If so, it can either be estimated explicitly using study dummies resulting in a fixed effects multilevel model (FEML), or as a study-level addition to the error term, commonly referred to as random effects multilevel model (REML). Alternatively, if a study-level layer is not relevant, clustered standard errors can be used to adjust correlated error terms at the study level.

In the literature we meta-analyse, no precision measures of the respective rebound estimates are calculated (using standard errors, t-values or alike). This limitation precludes the use of additional options that are regularly used in meta-analyses. First, we cannot estimate a multivariate random effects model, i.e., adding an additional error term to equation (1), which would capture unobserved heterogeneity at the observation-level.<sup>3</sup> Instead, we rely on a wide range of moderator variables to explain much of the observed heterogeneity. Second, we cannot weight each observation by its respective precision, which would allow us to give more weight to more precise and thus more reliable estimates.<sup>4</sup> Alternatively, we assign equal weights to each study in one of our robustness checks to limit the influence of studies reporting many estimates compared to studies with only few (Penn and Hu, 2019).

Finally, we cannot examine the literature for publication selection bias. However, we do not expect this literature to be biased by publication selection for two main reasons. First, publication bias by definition reflects the phenomenon of researchers selecting results or journals accepting studies for publication based on statistical significance or theoretical expectations (Stanley and Doucouliagos, 2012). Here, no measure of precision is calculated such that selection based on such a measure cannot occur. Second, as we will discuss later in Subsection 3.4.1, the magnitude and sign of rebound

<sup>3</sup> The estimation of the population variability in effect sizes requires knowledge of the standard error (Card, 2015).

<sup>4</sup> Using optimal inverse-variance weights would also ensure efficient estimation. Importantly, however, estimating equation (1) by OLS still ensures unbiased estimation even though efficiency may not be achieved (Wooldridge, 2010).



estimates varies considerably between and within studies. If selection were based on the magnitude or sign of the rebound, one would expect to see distinct cut-offs at certain points of the distribution, which is not the case. Nevertheless, we included a control for peer-review status in one of our robustness checks to account for the possibility of significant differences between published and unpublished studies. Despite these data-related limitations, we are confident that this setup adequately addresses the issues and lends confidence to our results.

### 3.3 Construction of the dataset

#### 3.3.1 Study selection and effect size

Our study search strategy followed the MEAR-Net guidelines for conducting and reporting meta-analyses (Havránek et al., 2020) and consisted of two steps. First, four search engines, namely Ebscohost, Web of Science, WISO, and World Wide Science, were employed. The search query included the term “indirect rebound” combined with either “household” or “residential” and “efficiency” or “sufficiency”. Excluding duplicates, the search resulted in 97 initial studies, reflecting the state of the literature on total microeconomic rebound effects at the household level as a still emerging field. We then screened the studies, discarding studies that did not fit the selection criteria outlined below (see Figure C.2 in the appendix for the full PRISMA statement (Moher et al., 2009) and Table C.1 in the appendix for a list of studies that were discarded after full-text assessment ordered by reason for exclusion). Second, we assessed the studies cited by and citing the eligible studies identified in the database search for additional eligible results using Google Scholar. This procedure was repeated until no new eligible studies were found and extended the sample from 15 studies identified via the database search to a total of 43 studies included in the meta sample (see Table C.2 in the appendix for a full list of included studies). The search was conducted in February 2021 and documented using the reference management software *Citavi*.

We adopted the study selection criteria developed by Reimers et al. (2021) in their review of the literature on microeconomic quantification of indirect rebound effects. This resulted in the following four criteria: First, we focus on studies that quantify indirect (and, optionally, direct) rebound effects. We thus exclude studies that only quantify direct rebound effects (e.g., Palmer, 2012; Stapleton et al., 2016) as well as studies that only partially capture indirect rebound effects, i.e., consider rebounds between two different appliances or energy services but neglect all other goods and services (e.g., Ghosh and Blackhurst, 2014; Santos et al., 2018a; Santos et al., 2018b). Second, the indirect rebound effects under consideration must follow efficiency improvements or sufficiency related behavioural changes on the household level. We thus exclude studies that quantify rebound effects following changes in, e.g., time use or household size, or production-side measures (e.g., Buhl and Acosta, 2016; Underwood and Fremstad, 2018;

Weidema et al., 2008). Third, we restrict our sample to microeconomic or, more precisely, partial equilibrium studies, thus excluding studies that take account of macro-level or general equilibrium effects of the relevant changes at the household level (e.g., Brockway et al., 2017; Kulmer and Seebauer, 2019). Fourth, we include only studies that estimate rebound effects based on data on actual household spending instead of employing stated preference or (quasi-)experimental approaches (e.g., Bardsley et al., 2019; Raynaud et al., 2016). As Reimers et al. (2021) show, the application of these criteria results in a sample of studies that share common features and form a homogeneous set at a superordinate level, while the underlying variation in aspects such as data sources and specific methodology applied remains considerable. Furthermore, the studies included in the sample yield widely varying estimates of rebound effect sizes, which cannot be explained by a qualitative review, thus requiring a meta-regression analysis (Reimers et al., 2021).

Accordingly, the dependent variable for our analysis is the total microeconomic rebound effect in percent, i.e., the estimated indirect rebound effect plus the direct rebound effect (if the latter is considered in the respective case). We had to opt for this combined effect because some estimation methods applied in the literature do not allow for the separation of direct and indirect rebound effects (e.g., Alfredsson, 2004; Chitnis et al., 2013; Chitnis et al., 2014; Salemdeeb et al., 2017). Therefore, and due to the interrelatedness of direct and indirect rebound effects, we use a dummy variable to control whether the respective total microeconomic rebound effect is equal to the indirect rebound effect – as is the case, e.g., in most sufficiency scenarios where direct rebound effects do not occur by definition – or whether the total rebound includes a direct effect.

### 3.3.2 Moderator selection

To explain the variation in the data, a rich set of moderators was coded to identify relevant dimensions in which the selected studies differ. Two of the authors separately coded all these moderators to ensure inter-coder reliability. Reassuringly, differences in coding could be attributed to ambiguous or incomplete documentation in the primary studies and were reconciled. The moderator dimensions consist of: (1) type of data sources, (2) setup of the rebound scenario (e.g., type and area of action, impact measure) and (3) type of rebound estimation (e.g., re-spending scenario, level of assumed direct rebound).

Regarding the first dimension, the selected studies draw on a wide variety of sources for both household consumption and impact measure data. As the type of data sources chosen could have a major influence on the outcome, we distinguish whether household consumption data come from an expenditure survey or from aggregated national

statistics, and whether the data for impact measurement come from input-output tables or from other sources such as life-cycle assessment (LCA) inventories.

Turning to the second dimension, in terms of the setup of the rebound scenario, the moderators control for the *Type of action* (efficiency increase vs. more sufficient behaviour), the *Area of action* (e.g., *Residential energy, Food, Transport / travel*), the *Rebound impact measure* (e.g., *Energy, CO<sub>2</sub> emissions*) and the differentiation by *Income groups*. To reconcile a variety of specifications for income, ranging from income quintiles (e.g., Chitnis et al., 2014) to rebound as a continuous function of income (e.g., Buhl and Acosta, 2016), a categorical income variable was defined with levels *Low, Average* and *High* (below, at or above median income recorded in the study, respectively).

Finally, rebound effects can be decomposed in a variety of ways. Dummy variables capture whether direct (in addition to indirect) rebound and substitution (in addition to income) effects are reported. Other key elements in the estimation of rebound effects accounted for by moderators in the meta-regression analysis are the choice of *Re-spending scenario* (i.e., how the money saved by the efficiency / sufficiency action is assumed to be spent) and the size of an a priori assumed fixed direct rebound effect, if applicable (see, e.g., Freire-González and Vivanco, 2017; Briceno et al., 2005). Some additional moderators beyond this core set of variables are used in robustness checks: Occasionally, rebound estimates are not reported numerically, but only in plots that can only be read off with some degree of imprecision (e.g., Cellura et al., 2013; Chitnis and Sorrell, 2015). Analogous to the control for peer-review status, a dummy is used in robustness checks to ensure that this does not result in any systematic bias. Similarly, the categorical variable *Authors group* is used to check if publications with overlapping authors exhibit structurally different findings.

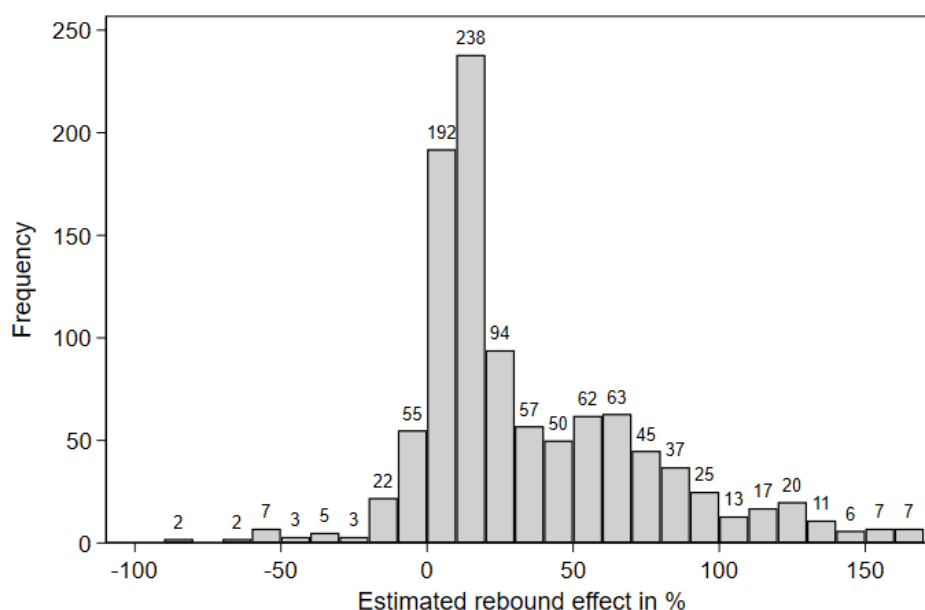
Our moderators focus on the most prominent study characteristics. We consider this focus to be appropriate as it accounts for key differences in the studies. Additional and more detailed moderators including considerations of the type and functional form of estimated demand systems or whether the expenditure category *Savings* is considered neutral with respect to the impact measure (e.g., CO<sub>2</sub> emissions), were initially explored but dismissed. Such information was inconsistently reported and therefore of little use for a broad comparison across studies. Similarly, finer-grained categorical levels were reduced if they did not exhibit significant differences in impact. Finally, moderators that showed little to no explanatory power beyond the core set of moderators were excluded in the baseline specification in favour of model parsimony but considered in robustness checks. We list all used moderators with their corresponding definitions in Table C.3 in the appendix.

## 3.4 Results

### 3.4.1 Summary statistics

Table C.4 in the appendix provides an overview of the rebound estimates reported by each of the 43 included studies. This reveals considerable heterogeneity in both the number of rebound estimates reported in each study (2 to 163 estimates) and their magnitude (−82% to 165%), even after aggregation<sup>5</sup> and removal of outliers. Our definition of outliers follows John Tukey's (Tukey, 1977) nonparametric criterion; outliers are those estimates that are further than 1.5· IQR from the lower and upper quartiles in the sample distribution. In our sample, 120 observations (10%) are discarded according to this criterion with the clear majority being exceptionally large values, in contrast to merely six negative outliers. Apparently, a few studies contain predominantly positive extreme outliers (e.g., Freire-González, 2017, (ID 29); Font Vivanco et al., 2015, (ID 23)), which far exceed all other estimates (more than 1,000% estimated rebound effect). To check robustness, we examined different outlier specifications without qualitatively changing the regression results (see Table C.6 in the appendix). The final data set for the analysis contains 1043 estimates with a mean rebound of 34.30%.

**Figure 3.1:** Histogram of rebound estimates (excluding outliers).



<sup>5</sup> Originally, 2257 separate estimates were coded (cf. column Orig. N in Table C.4 in the appendix). However, for two studies which reported values at the sub-national level (i.e., Chinese provinces) next to the national average, only the latter was ultimately kept (Chen et al., 2019, and Wen et al., 2018). For consistency, for two additional studies which only reported values at the sub-national level, we aggregated the reported values to a GDP-weighted national average (Thomas et al., 2014; Zhang et al., 2017).

Figure 3.1 shows that the distribution of rebound estimates is clearly skewed to the right, with estimates predominantly between zero and 100%, tapering off rapidly beyond these limits. 40% of the rebound estimates fall in the narrow range of zero to 20%. In summary, there is considerable heterogeneity in effect sizes within and across studies (cf. Figure C.3 in the appendix), which requires explanation.

The summary statistics (Table 3.1) confirm the large variation in rebound estimates both across and within moderator categories.<sup>6</sup> For the moderator *Area*, for example, we document mean rebound estimates ranging from 17% (*CO<sub>2</sub>*) to 97% (*Social indicator*). Similarly, there are differences in rebound estimates related to the assumed direct rebound, i.e., studies assuming 0-20% direct rebound report a total rebound of 25% on average, while studies assuming 60-90% direct rebound report 110% total rebound on average.

As specific subsamples may have otherwise unobserved characteristics, we inspect observations from three different categorical moderator levels more closely. The chosen subsamples reflect important dimensions linked to the general setup of the rebound scenario, i.e., *Efficiency improvement (Action type)*, *Residential energy (Area)* and *Energy (Impact)*, and provide a sufficient number of observations to allow for subsequent analyses.

### 3.4.2 Regression results

The results of the meta-regression are reported in Table 3.2 containing results for the whole sample and the three subsamples. In line with our methodological setup, we use the standard multivariate model as our baseline regression in column (1) and report results of alternative specifications in columns (2) to (4).<sup>7</sup> In columns (5) to (7), we additionally present results for the three relevant subsamples identified in Subsection 3.4.1. In all cases, we use the previously defined set of core moderators and exclude outliers based on the IQR, as explained above. Our description of the regression results focuses on the results of the baseline regression (column (1)) and points to the alternative specifications only in case of distinct differences. A series of robustness checks including additional moderators, different definitions of outliers and trimmed datasets is discussed in Subsection 3.4.3. Turning to the results, we tested the baseline model for normality of residuals, influential observations, heteroscedasticity and multicollinearity

<sup>6</sup> See Table C.3 in the appendix for summary statistics of all moderators including those used for robustness checks.

<sup>7</sup> The BPLM test indicated presence of additional study-level heterogeneity not accounted for by the standard multivariate model. However, both panel-model solutions have serious drawbacks: Using a REML has strong assumptions on the error term that unlikely hold in our case (Feld and Heckemeyer, 2011) and using the FEML automatically omits study-invariant moderators from the regression that are of interest. Reassuringly, the results are largely unaffected by the model choice. Apparently, the moderators included already explain study-level heterogeneity to a high degree. In this setting, we prefer a parsimonious model and hence use the multivariate model presented in Equation 3.1 as a baseline for robustness checks and subsample analyses.

**Table 3.1:** Summary statistics - Rebound estimates by selected categorical moderators.

Categorical moderators	N	Mean	SD	Min	Max
<b>Total</b>	<b>1043</b>	<b>34.33</b>	<b>39.63</b>	<b>-82.40</b>	<b>165.21</b>
<b>Rebound scenario setup</b>					
<i>Action type</i>					
Behav. change/sufficiency	206	45.18	38.39	-6.70	152.90
Efficiency improvement	808	31.06	39.01	-82.40	165.21
Both	29	47.05	49.38	-51.00	147.00
<i>Area</i>					
Residential energy	599	30.89	39.13	-82.40	165.21
Food	103	46.01	40.70	-6.70	152.90
Transport / travel	215	33.50	36.84	-26.23	135.00
Other	40	72.82	51.68	-51.00	153.00
Multiple	86	28.04	29.28	1.70	154.12
<i>Impact</i>					
CO <sub>2</sub>	259	16.90	35.36	-82.40	144.61
CO <sub>2</sub> -eq	405	26.10	27.30	-51.00	147.00
Energy	242	57.47	43.33	-56.40	165.21
Social indicator	16	97.26	15.34	68.00	128.00
Total material requirement	29	49.70	38.83	5.00	153.00
Other env. indicator	92	42.61	47.13	-6.70	154.12
<i>Income groups</i>					
Average income	878	34.74	41.24	-82.40	165.21
High income	82	27.11	25.01	4.70	111.84
Low income	83	36.71	32.92	-5.50	131.20
<b>Rebound estimation</b>					
<i>Re-spending scenario</i>					
Proportional	298	52.38	43.61	-39.00	165.21
Marginal	621	25.57	33.47	-82.40	165.00
Hypothetical: Green case	33	50.09	45.25	-35.00	162.00
Hypothetical: Worst case	7	99.96	43.85	55.29	155.00
Other	84	22.93	35.68	-56.40	128.00
<i>Direct rebound assumption</i>					
Not assumed	836	28.12	36.52	-82.40	154.12
0-20%	56	24.84	17.81	3.48	77.53
30-50%	124	63.80	38.03	1.67	165.21
60-90%	27	109.59	33.65	60.00	165.00

(see also Figure C.4 in the appendix). The findings support inference validity but indicate heteroscedasticity, which we address using standard errors clustered at the study level. Multicollinearity did not influence the regression results in any relevant way.<sup>8</sup> As the core moderators are exclusively binary or categorical variables, their corresponding coefficients represent the expected change in mean total rebound induced by a deviation from the reference case (*ceteris paribus*). For all categorical moderators, the reference case is the omitted category indicated in parentheses. The reference case for all binary regressors is the zero case.<sup>9</sup> Accordingly, the constant mirrors the mean total microeconomic rebound for a reference study. We chose our reference cases for the respective variables such that they form a consistent baseline by avoiding incongruous combinations like, e.g., a sufficiency related action and the estimation of substitution effects (cf. the explanations to Figure C.1 in the appendix). The reference study in the meta-regressions displayed in Table 3.2 employs, e.g., aggregate household expenditure data, impact data from input-output tables and a marginal re-spending scenario to estimate a total microeconomic rebound effect in terms of energy following an efficiency improvement in residential energy use. The estimated mean total rebound is rather stable across specifications with little variance, ranging between 36% and 43% for our specifications presented in Table 3.2, columns (1) to (4). Similarly, the adjusted  $R^2$  varies between 0.259 and 0.461. In the following, we present results by moderator category starting with the type of data a study used.

#### *Data*

The source of household consumption data does not seem to affect the size of the rebound effect. Apparently, using data from household expenditure surveys instead of aggregated household data does not lead to differences in the estimated size of the rebound effect. Furthermore, the estimated rebound effect is significantly smaller (42 percentage points in the baseline specification) if the information for impact measures was not derived from Input-Output tables but rather carbon intensities or direct energy use and efficiency databases. This is due to only 75 observations in that category originating from three studies with particular characteristics, which we address in the discussion in Section 3.6.

#### *Rebound scenario setup*

Behavioural changes generally result in higher rebound effects compared to efficiency related actions. Additionally, the *Area of action* considered by a study partly affects the size of the rebound effect. While the rebound effect for some areas of action (*Residential energy, Food, Multiple*) do not differ significantly, the rebound effect for transport- or travel related actions tend to be larger than actions related to *Residential*

<sup>8</sup> The mean variance inflation factor is 1.91. This is well below the usual threshold of 10, hence eliminating multicollinearity concerns.

<sup>9</sup> In some of the robustness checks, we add continuous moderators to the model. These are centred to ease their coefficients' interpretations as the effect of deviations from the mean.

*energy*.<sup>10</sup> Moreover, the choice of the *Rebound impact measure* has a sizeable impact on the estimated rebound effect. Using  $CO_2$  or  $CO_2\text{-eq}$  instead of *Energy* reduces the mean effect size by about half, albeit not significantly so in some specifications. If a *Social indicator* such as employment hours is used instead, the rebound effect approximately doubles compared to rebound effects with *Energy* as impact measure. The corresponding coefficient however, is based on only 16 observations, implying limited informative value. Turning to the differences between *Income groups*, we find that lower income levels are associated with higher rebound effects, which is consistent with the findings of individual studies investigating this matter (e.g., Chitnis et al., 2014; Hagedorn and Wilts, 2019; Murray, 2013). Rebound effects for higher income levels, on the other hand, are not significantly different from the average income levels in most specifications. This may be due to the few observations from high income groups and the coefficients' relatively small magnitude.

#### *Rebound estimation*

Studies that consider substitution effects in addition to income effects report larger rebound effects, and to a significant extent in almost all specifications. As expected, studies that only estimate an indirect rebound effect, show smaller effect sizes on average. The mean rebound effect is reduced by half in the baseline specification. Furthermore, the *Re-spending scenario* seems to have a large impact on the size of the rebound. While there is no significant difference between the two types of data-based re-spending scenarios (*Proportional* and *Marginal*), hypothetical re-spending scenarios result in significantly different rebound estimates in most cases. This is, at least partly, by design. Assuming a *Worst case* scenario, i.e., entire re-spending on a good or sector(s) with the highest impact, significantly increases the estimated rebound by a factor of two. In contrast, assuming a *Green case* scenario, i.e., re-spending with the least adverse impact, does not significantly affect the effect size in the baseline regression, but tends to reduce the estimated rebound in some of the alternative specifications. This occasional insignificance may be due to the fact that there are only 33 observations with a *Green case* scenario. Finally, another important methodological choice of the studies is the assumption of a direct rebound effect. As expected, studies that assume a high direct rebound effect also report a significantly higher total rebound effect. Reassuringly, the higher the assumed direct rebound, the larger the effect, and it turns insignificant at the smallest level (0-20% assumed direct rebound effect).

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<sup>10</sup> Note that the category *Other* is also significant. However, in this category we bundle 40 observations with very different action areas, e.g., *Durable appliances*, *No chemicals & plastics*, *Clothing*, or *Paper*. While aggregation in one category was necessary due to very few observations per action area, we refrain from drawing any conclusions accordingly.



**Table 3.2:** Regression results.

	Baseline	Alternative specifications			Subsamples		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Baseline weighted	Random effects	Fixed effects	Efficiency	Res. energy	Impact=Energy
<b>Data</b>							
HH consumption data is from expenditure survey	-0.90 (5.89)	10.14 (7.99)	-5.41 (9.34)	0.00 (.)	-4.65 (6.16)	-5.26 (4.58)	-33.14*** (7.41)
Impact measure data not from Input-Output tables	-42.43*** (12.65)	-71.27*** (14.56)	-43.32*** (10.34)	0.00 (.)	-41.13*** (11.49)	-25.51*** (6.12)	-59.44*** (8.00)
<b>Rebound scenario setup</b>							
<i>Type of action (ref. Efficiency)</i>							
Behav. change/sufficiency	23.82*** (6.75)	10.73 (6.80)	25.55*** (6.40)	28.31*** (6.83)		21.53*** (7.26)	21.61*** (3.68)
Both	7.26 (10.79)	-15.33 (11.83)	34.78** (13.77)	46.52*** (14.22)		-10.72** (5.22)	-6.60 (4.00)
<i>Area of action (ref. Residential energy)</i>							
Food	-0.63 (14.91)	-7.52 (11.11)	10.25 (14.71)	13.78 (16.00)			57.92*** (8.60)
Transport / travel	19.21* (9.59)	18.44* (9.17)	16.03* (8.27)	15.44* (8.59)	19.96* (10.02)		20.75 (14.96)
Other	44.41*** (11.04)	51.53*** (13.50)	39.35*** (9.76)	40.07*** (10.02)	19.61 (34.56)		95.63*** (6.90)
Multiple	7.09 (5.36)	2.03 (10.00)	5.85 (3.62)	5.39 (3.31)	6.70 (6.31)		19.71** (7.48)
<i>Rebound impact measure (ref. Energy)</i>							
CO <sub>2</sub>	-18.47** (8.54)	-19.44* (10.04)	-13.89 (9.94)	-20.17 (12.14)	-14.73** (6.91)	-11.84** (4.65)	

**Table 3.2:** (continued).

	Baseline	Alternative specifications			Subsamples		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Baseline weighted	Random effects	Fixed effects	Efficiency	Res. energy	Impact=Energy
CO <sub>2</sub> -eq	-24.34*** (8.25)	-22.95*** (7.93)	-19.14 (11.93)	-17.76 (14.61)	-20.16*** (6.93)	-19.06*** (6.16)	
Social indicator	47.27*** (7.73)	34.61*** (9.56)	31.69*** (9.53)	30.85*** (10.60)	58.90*** (11.74)	71.68*** (4.10)	
Total material requirement	0.38 (9.94)	2.22 (9.69)	-7.25 (8.81)	-8.49 (9.31)	14.29*** (4.91)	9.76 (6.35)	
Other env. indicator	-17.31 (12.08)	-10.96 (13.63)	-8.45 (10.63)	-7.79 (12.41)	7.58 (7.19)	11.81** (5.74)	
<i>Rebound by income group (ref. Average income)</i>							
High income	2.43 (3.43)	5.01 (6.80)	-2.36*** (0.59)	-2.87*** (0.39)	0.20 (2.76)	1.52 (3.25)	-3.15*** (0.86)
Low income	11.91* (6.06)	20.40** (8.48)	6.90** (3.29)	6.37** (2.98)	5.06* (2.95)	6.36 (4.39)	16.73*** (2.13)
<b>Rebound estimation</b>							
Substitution effect considered in addition to income effect	22.86 (14.35)	53.74*** (16.85)	16.36** (7.82)	12.25** (5.82)	21.13 (12.91)	3.33 (5.20)	41.76*** (6.83)
Rebound is indirect only	-18.50*** (5.86)	-19.71*** (4.15)	-19.59*** (6.22)	-20.41*** (6.36)	-22.70*** (6.01)	-26.13*** (7.06)	-32.11*** (3.33)
<i>Responding scenario type (ref. Marginal)</i>							
Proportional	-1.19 (6.38)	9.98 (6.89)	-3.49 (3.20)	-3.47 (2.85)	3.93 (5.04)	0.71 (3.87)	-5.70 (3.95)
Hypothetical: Green case	-9.08 (6.07)	1.19 (5.81)	-17.44*** (4.64)	-17.02*** (4.88)	0.15 (7.65)	-2.86 (6.73)	-15.41*** (3.75)

**Table 3.2:** (continued).

	Baseline	Alternative specifications			Subsamples		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Baseline weighted	Random effects	Fixed effects	Efficiency	Res. energy	Impact=Energy
Hypothetical: Worst case	46.95*** (13.60)	61.54*** (18.99)	36.68*** (8.46)	37.55*** (7.48)	63.69*** (20.12)	63.99*** (20.39)	44.86*** (7.65)
Other	3.86 (6.50)	6.45 (6.00)	-0.39 (4.57)	0.36 (4.69)	6.86 (6.85)	4.32 (4.30)	-4.77 (6.45)
<i>Level of direct rebound assumed (ref. not assumed)</i>							
0-20%	3.45 (6.85)	-8.34 (9.81)	-3.64 (4.65)	-4.67 (5.33)	2.12 (7.33)	7.94 (5.50)	8.52 (5.35)
30-50%	36.52*** (6.99)	32.22*** (9.84)	32.44*** (4.85)	29.57*** (5.91)	33.80*** (4.83)	36.76*** (5.16)	33.16*** (7.05)
60-90%	82.04*** (5.48)	67.46*** (11.15)	65.00*** (9.00)	62.30*** (10.73)	77.29*** (9.47)	79.13*** (10.25)	81.48*** (6.64)
Constant	38.42*** (8.10)	38.60*** (8.47)	42.85*** (10.25)	36.32*** (11.24)	36.87*** (7.10)	38.40*** (6.16)	51.06*** (6.71)
<i>N</i>	1043	1043	1043	1043	808	599	242
<i>R</i> <sup>2</sup>	0.444	0.473	0.402	0.275	0.533	0.641	0.608
Adj. <i>R</i> <sup>2</sup>	0.431	0.461		0.259	0.520	0.628	0.575

*Notes:* Clustered standard errors in parentheses. The *R*<sup>2</sup> for the random effects model is not directly comparable to the other specifications due to computational particularities (StataCorp, 2021). By definition, study-invariant moderators are omitted in the fixed effects model in column (4).

\* *p* < 0.1, \*\* *p* < 0.05, \*\*\* *p* < 0.01

### 3.4.3 Robustness checks

We assess the robustness of our results in several ways. First, we split the sample in three above identified subsamples, depicted in columns (5) to (7) of Table 3.2. Second, we add hitherto unused moderators to the baseline specification, see Table C.5 in the appendix. Finally, we investigate the sensitivity of results to our definition of outliers, treatment of heteroscedasticity and the effect of coding decisions as summarized in Table C.6 in the appendix.

In general, the subsample analyses depicted in Table 3.2 confirm the findings from the regression results based on the full sample. However, there are minor differences. Noticeably, the adjusted  $R^2$  increases moderately for all subsamples, now ranging between 0.520 and 0.628. The subsamples on efficiency related actions (5) and rebounds in the residential energy area (6) lead to virtually identical results. In contrast, for the subsample of rebound effects measured in terms of energy (7), we document a slightly higher mean rebound effect (51% compared to 39%) and two previously insignificant moderators become significant (*HH consumption data is from expenditure survey, Area of Action: Food*). We attribute these differences to the smaller sample size (242 compared to 1043 observations) with correspondingly fewer observations per level (40 (18) observations for *HH expenditure survey data (Area of Action: Food)*).

Similarly, adding moderators does not alter results considerably (see Table C.5). First, both the explanatory power in terms of  $R^2$  and the coefficients of the core moderators remain stable (except for two cases discussed below). Second, most of the added dummy moderators remain insignificant, e.g., studies using a *Detailed efficiency or sufficiency change scenario* (2), *Life-cycle assessment data* (3) or an *Almost Ideal Demand System* (4) do not report significantly different rebound estimates compared to studies that do not rely on these features. Adding information on whether *Only direct emissions are considered* (5), the *Year of publication* (6) or whether the study was *Published in a peer-reviewed journal* (7) also has no significant effect. In an additional specification (8) we include a moderator that controls if an *Efficiency increase is considered costly* or not (thereby affecting the amount of redistributable savings). While there is no significant difference across efficiency measures, the category-level *Not applicable* is significant. This effect, however, is attributable to high collinearity with sufficiency measures, which is also reflected in the change in the size of the respective coefficient of the categorical variable *Type of action*. Finally, when controlling for *Author group* effects (9), some minor differences emerge that accordingly result in an altered mean rebound effect due to the change in the baseline.

Finally, we investigate if our outlier specification or coding decisions have an effect on the results (Table C.6 in the appendix). We first assess if our decision to cluster standard errors at the study level is too restrictive, thus resulting in overly narrow confidence intervals. Reassuringly, the use of common heteroscedasticity-robust standard errors

(2) instead confirms the insights gained from the baseline specification.<sup>11</sup> Similarly, our coding decisions, i.e., reading numbers off figures (7) and using intra-study averages if necessary (8), did not affect the results noticeably. Finally, assessing the effect of outliers supports our decision to omit extreme rebound effect estimates using the IQR in our main specifications. First, using all available observations clearly distorts the regression results (column (3)), while regressions with fewer extreme rebound estimates (using different percentile ranges) improve in explanatory power and have more reasonable coefficient magnitudes (columns (4) and (5)). However, omitting even more observations based on the regression diagnostics (6) does not alter the baseline results such that the IQR appears to be a justified outlier criterion.

### 3.5 Transfer reliability

This meta-analysis on microeconomic rebounds primarily serves to explain heterogeneous empirical findings and calculate mean effect sizes relative to our consistent baseline scenario.<sup>12</sup> As a valuable addition, we assess the usefulness of the results of our meta-regression analysis in new policy contexts by calculating transfer errors (Chaikumbung et al., 2016). This is particularly important in the context of rebound effects, where estimation is data and time intensive and thus costly, posing a burden on primary studies (Johnston et al., 2015). We follow common practice and calculate absolute percentage errors (APE) using leave-one-out-cross-validation, i.e., we omit one observation at a time, re-estimate the model and calculate the estimated APE for the observation omitted (Johnston et al., 2018; Nelson, 2015; Chaikumbung et al., 2016). Additionally, we show the corresponding absolute errors (AE), which presumably better reflect the expected predictive accuracy of our meta-model as explained below.

In Table 3.3, we present the APE and AE for the baseline specification (Full sample) as well as for the subsamples of rebound effect sizes related to *Efficiency improvements (Action type)*, *Residential energy (Area)* and *Energy (Impact)*, respectively. We base the calculation of these error matrices on a meta-functional transfer using the respective regression results from Table 3.2, which is common practice (Johnston et al., 2015; Kaul et al., 2013).<sup>13</sup>

The general findings from Table 3.3 are consistent with previous discussions in the literature. First, transfer errors are smaller for more homogeneous sets of observations (i.e., our subsamples). Second, the mean transfer error is larger than the median transfer error regardless of the metric due to a few outlying observations. More specifically, the mean APE ranges between 102% and 368% compared to 33% and 52% for the median

<sup>11</sup> This robustness check also addresses remaining concerns regarding the appropriateness of clustering the standard errors due to the moderate number of 43 studies serving as clusters (Nelson, 2015).

<sup>12</sup> See our description of our baseline scenario in footnote 15.

<sup>13</sup> For completeness, we also calculated simple mean-value transfer errors based on an univariate analogue of Equation 3.1. Results follow the same pattern (albeit with greater transfer errors as expected).

**Table 3.3:** Transfer error.

	N	Mean (SD)	Median	Range
<i>Absolute percentage error (APE)</i>				
Full sample	1023	367.52 (2092.63)	52.03	0.05-43531
Efficiency	800	217.29 (1614.61)	49.92	0.14-44019
Residential energy	592	189.45 (1350.78)	48.57	0.11-30659
Energy	242	102.02 (259.68)	33.37	0.00-2716
<i>Absolute error (in percentage points)</i>				
Full sample	1043	21.98 (21.00)	14.85	0.04-134
Efficiency	808	19.19 (19.87)	12.19	0.05-111
Residential energy	599	16.87 (17.50)	10.99	0.04-113
Energy	242	21.73 (19.08)	16.57	0.00-116

*Notes:* The number of observations for the APE is smaller compared to the AE in some cases since 19 observations have a reported rebound of 0% which prohibits the calculation of a percentage transfer error (APE). Additionally, one observation with an extreme APE (3.20<sup>15</sup>%) is omitted which reduces the Mean APE from 3.13<sup>12</sup>% to 367% for the full sample, while the Median APE barely changes.

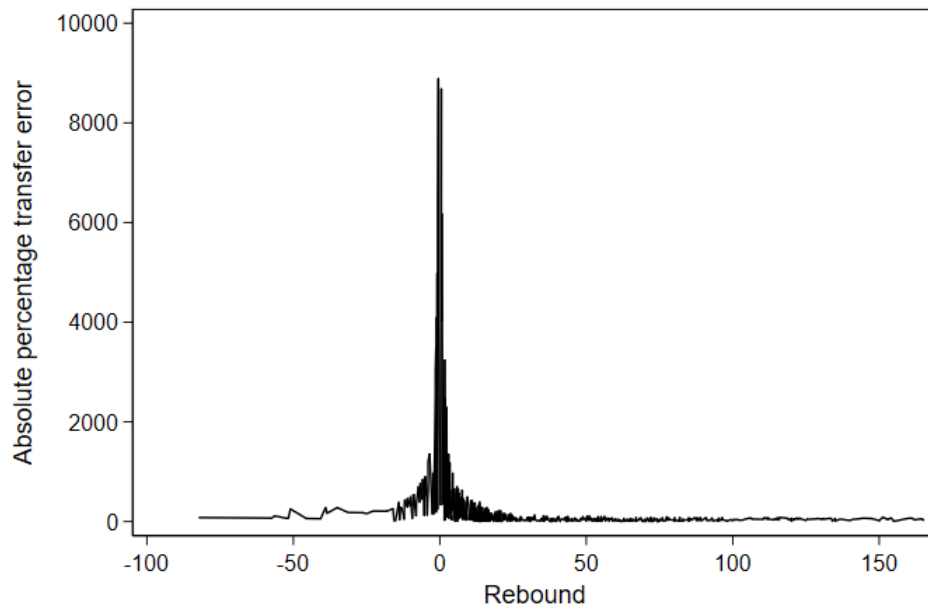
APE. In particular, extremely high APE values correspond exclusively to rebound estimates that are close to zero. In these cases, relatively small absolute errors lead to high percentage errors. This pattern is also documented in Figure 3.2, which shows the distribution of APEs.<sup>14</sup> Arguably, the median APEs in this case better capture the expected APE.

Hence, for policy makers, the AE is presumably a more informative metric to assess transfer accuracy, as it conveys a direct indication of the expected rebound magnitude. In general, the AE is relatively small with a mean of 22 percentage points and a corresponding median of 15 for the full sample. For the subsample of rebound estimates related to *Residential energy*, the mean and median AE even reduce to approximately 17 and 11 percentage points, respectively. Therefore, it appears that our meta-analytic model reliably forecasts the expected rebound on average, notwithstanding a few extreme cases. We are confident that these relatively small absolute error margins allow to form a reasonable basis for policy decisions in new contexts even though transfer error tolerance is admittedly context-dependent (Rosenberger, 2015; Brander et al., 2006).

### 3.6 Discussion and Conclusion

Information on rebound effects is essential to account for unintended consequences of policies for the sustainable use of natural resources and to achieve governments'

<sup>14</sup> Excluding rebound estimates between -1% and +1% (42 observations) reduces the mean APE from 368% to 137%, for example.

**Figure 3.2:** Absolute percentage transfer error (%) by rebound.

*Notes:* Nine observations with an APE larger than 10,000% are excluded for the ease of exposition.

ambitious targets such as decarbonisation targets. Due to the importance of adequately judging how effective a particular policy measure might be, a relatively large literature exists on estimating rebound effects. Unfortunately, the effect sizes of quantified rebound effects vary widely, ranging from negative values to above 100%, requiring a systematic assessment of the causes for the heterogeneity of empirical results. In this paper, we conducted a meta-analysis of the literature, focussing on studies that estimated total microeconomic rebound effects on the household level. We identified 43 studies published between 2002 and 2020 that provided us with 1043 observations with rebound estimates ranging from -82% to 165% (after outlier exclusion).

In our meta-regression analysis, we identified a rich set of moderators that represent relevant dimensions in which studies differ. These consist of: (1) type of data sources, (2) setup of the rebound scenario (e.g., *Type and area of action*, *Impact measure*) and (3) type of rebound estimation (e.g., *Re-spending scenario*, *Level of assumed direct rebound*). In the following, we highlight a few results, relevant for decision makers when assessing the effectiveness of future decarbonisation policies based on existing literature and for researchers when designing future rebound studies.

For our reference case<sup>15</sup>, we find a mean total rebound effect between 36% and 43%, i.e., a total microeconomic rebound effect in terms of energy following an efficiency improvement in residential energy use. Of the two variables in our first moderator dimension, i.e., type of data sources, only the dummy variable indicating that the impact measure data is not derived from IO tables exhibits a statistically and economically significant negative effect. It is worth noting here that there are only three studies (with a total of 75 observations) that do not employ IO data: Chitnis et al. (2020) and Kratena and Wüger (2010) employ direct CO<sub>2</sub> emissions and direct energy databases, respectively, whereas Zhang et al. (2017) use a database of both direct and embedded CO<sub>2</sub> emissions. As we do not find a statistically significant effect of the variable *Only direct emissions considered* (cf. Table C.5 in the appendix), we can rule out this common feature of two out of the three studies as the driver of the effect. Apart from that, we have to be somewhat cautious when interpreting the effect. It may be due to the data sources for the impact measure as such, but also to finer details in study design that our moderators cannot capture as they are rare in our dataset. For example, Kratena and Wüger (2010) focus on modelling interdependencies between the energy efficiency of the stock of household appliances and rebound effects and Chitnis et al. (2013) provide the only estimates of rebound effects between energy services (e.g., cooking) rather than energy commodities (e.g., electricity).

Turning to the second moderator dimension, we find that the setup of the rebound scenario significantly impacts the size of the estimated rebound effect across several moderators. Rebound effects preceded by sufficiency-related behavioural changes are on average 24 percentage points higher than rebound effects preceded by efficiency improvements. Again, one of our robustness checks allows us to rule out one potential driver of the effect – the costs of efficiency improvements could reduce monetary savings and thus the budget available for re-spending and, consequently, the rebound effects. However, when we control for whether such costs were explicitly accounted for via the moderator *Efficiency costs*, we find no statistically significant effect (cf. Table C.5 in the appendix). In terms of the consumption area in which the efficiency or sufficiency actions take place, actions in the transport area lead to rebound effects that are on average 19 percentage points higher than actions in the area of residential energy. This is of crucial importance for policy makers who should account for higher rebound effects when preparing and evaluating future policies in the transport sector as compared to the residential energy sector. Policy makers should also be aware that the size of the expected rebound effect depends on the *Impact measure*. Despite the close link between energy use and CO<sub>2</sub> emissions, rebound effects in terms of CO<sub>2</sub> (*CO<sub>2</sub>-eq*) are

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<sup>15</sup> The reference study in the meta-regressions displayed in Table 3.2 employs aggregate household expenditure data, impact data from input-output tables and a marginal re-spending scenario to estimate a total microeconomic rebound effect (direct and indirect) in terms of energy following an efficiency improvement in residential energy use for an average income household. Substitution effects are not considered and the direct rebound effect is calculated rather than assumed.



on average 18 (25) percentage points lower than rebound effects related to *Energy*. In terms of rebound effects by *Income groups*, our analysis confirms the consensus in the literature that rebound effects are significantly higher for low income households (12 percentage points higher than in the average income group). The effect for high income groups is small and statistically significantly negative in only three of our seven main models, suggesting that the difference between high- and average-income households in terms of rebound effects is less pronounced than the difference between low- and average-income households.

Our final group of moderators includes those that describe the rebound estimation as such, i.e., which components of the total microeconomic rebound effect are estimated, how households' re-spending is modelled and whether direct rebound effects are assumed. Unsurprisingly, most of these factors have a strong influence on the estimated rebound effects with the effects having the expected signs. The only exception is that we do not find a significant difference between the two data-based re-spending types, i.e., *Marginal* and *Proportional* re-spending. Hence, whether or not changes in households' expenditure shares due to changes in income are accounted for has no direct impact on the size of the estimated rebound effect. However, this is no reason to not apply the methodologically more challenging approach of marginal spending. This is because marginal spending and, more specifically, the estimation of own- and cross-price elasticities are necessary prerequisites for the estimation of substitution effects. The inclusion of these effects as opposed to considering income effects alone in turn leads to significantly higher rebound estimates and should thus not be neglected when estimating efficiency rebounds.<sup>16</sup> The case of sufficiency rebounds is, of course, different. There are no price changes and, consequently, no substitution effects to consider. Furthermore, sufficiency-related behavioural changes are somewhat removed from classical economic theory, which raises the question of how to realistically describe the re-spending of households that have made such changes. Few studies explore *Green case* and *Worst case* re-spending scenarios and the microeconomic literature hardly considers theories and empirical results concerning spill-over effects and moral licensing from the psychological literature (cf. Dütschke et al., 2018; Reimers et al., 2022; Sorrell et al., 2020).

In conclusion, considerations of internal consistency of the setup of the rebound scenario and the estimation approach are important both in interpreting our results and in designing future studies quantifying rebounds. Specifically our results are most informative and reliable where they concern commonly studied rebound scenarios. For non-standard data sources, areas of action and impact measure that have so far only been studied in very few cases, we have to rely on catch-all categories with few observations. Further differentiation and thereby more meaningful interpretations for these categories

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<sup>16</sup> The significant positive effect is not present in our baseline regression but in all alternative specifications.

will only become possible as the number of primary studies investigation them increases. Nevertheless, our meta-analysis already allows for reliable conclusions. In terms of predictive accuracy, we find that our meta-regression model results in relatively small absolute error margins (about 20 percentage points). This enables policy makers to form reasonable expectations about rebound magnitudes in new contexts.

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- Wen, F. et al. (2018). “Exploring the rebound effect from the perspective of household. An analysis of China’s provincial level”. In: *Energy Economics* 75, pp. 345–356. doi: \url{10.1016/j.eneco.2018.08.018}.
- Wooldridge, J. M. (2010). *Econometric analysis of cross section and panel data*. MIT Press. ISBN: 0262296799.
- York, R., L. Adua, and B. Clark (2022). “The rebound effect and the challenge of moving beyond fossil fuels: A review of empirical and theoretical research”. In: *WIREs Climate Change* 13.4. doi: \url{10.1002/wcc.782}.
- Zhang, Y.-J. et al. (2017). “The direct and indirect CO<sub>2</sub> rebound effect for private cars in China”. In: *Energy Policy* 100.2, pp. 149–161. doi: \url{10.1016/j.enpol.2016.10.010}.



## Appendix

### Rebound effect decomposition

Figure C.1: Decomposing an energy efficiency rebound effect.

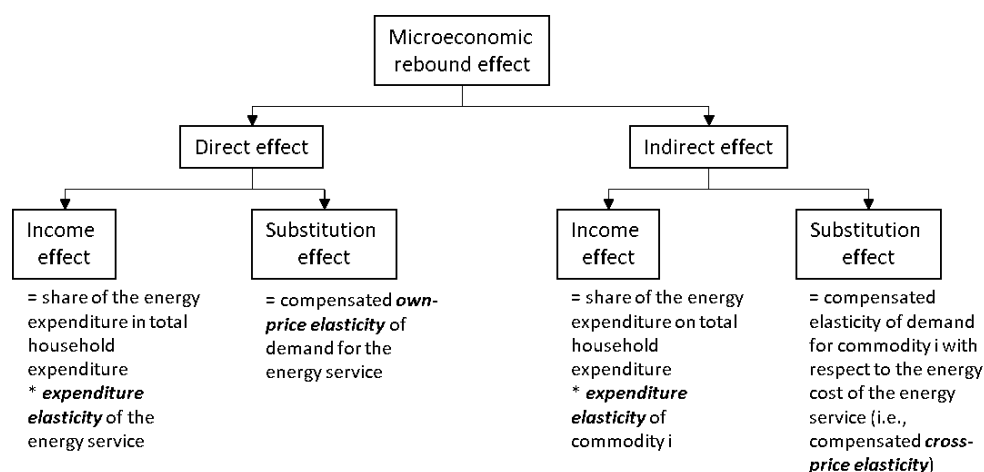
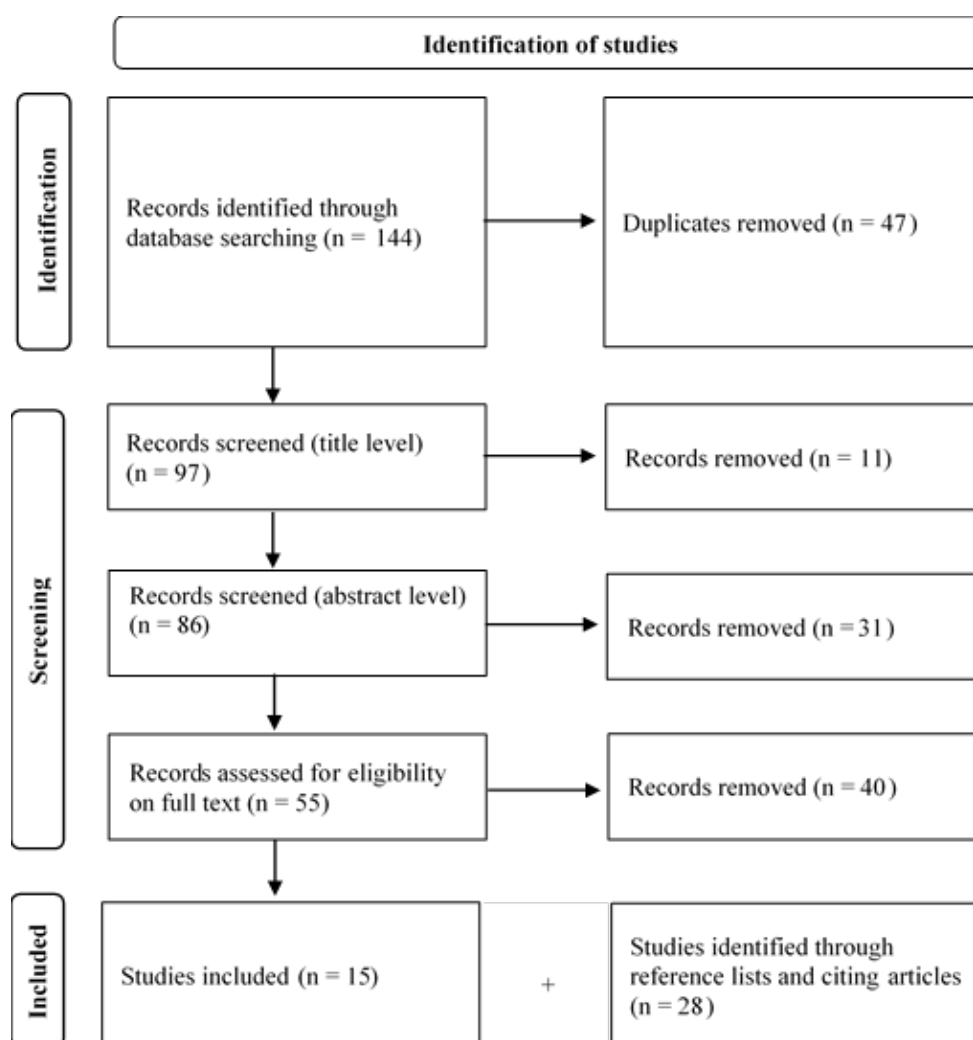


Figure C.1 visualizes rebound effect decomposition for the example of an energy efficiency increase (based on Chitnis and Sorrell, 2015). This logic transfers analogously to efficiency increases in other dimensions than energy. Sufficiency increases, on the other hand, preclude the existence of some of the effects as will be discussed below. The total microeconomic rebound effect is decomposed into direct and indirect effects which can be further decomposed into income and substitution effects and estimated based on the respective expenditure and price elasticities (Borenstein, 2015; Chitnis and Sorrell, 2015). Which effects occur depends on the scenario considered. Efficiency improvements can potentially trigger all of the effects shown above. For example, a household who installs a new, more energy efficient boiler frees up income previously bound to maintain a certain room temperature. The freed-up income can then be spent on achieving a higher room temperature (*direct income effect*) or on other goods and services (*indirect income effect*). Additionally, the energy service “heating” becomes cheaper compared to the situation before the efficiency improvement took place and relative to other goods and services, giving rise to direct and indirect substitution effects, respectively. In contrast, sufficiency-related behavioural changes do only trigger some of these effects. For example, a household who decides to maintain a lower room temperature than before also frees up income and said income can then be spent on other goods and services (*indirect income effect*). There is, however, no price change and hence, neither direct nor indirect substitution effects occur. Whether or not a direct income effect occurs depends on the classification of the consumption categories. In our example, if “heating” constitutes its own consumption category, then any spending in this category would constitute a contradiction of the sufficient household behaviour and would thus be ruled out from the outset. If “heating” was part of a broader consumption category such as “residential energy consumption”, direct income effects

would conceptually be possible as the household could spend the income saved on other energy services included in that category (Chitnis and Sorrell, 2015; Reimers et al., 2021). Not all rebound estimation techniques are able to capture all types of effects. While income effects can be estimated based on expenditure elasticities or approximated by proportional spending and under hypothetical spending scenarios, substitution effects can only be captured by the estimation of own- and cross-price elasticities. Since the estimation of the latter requires the estimation of full demand systems, only a minority of studies considers indirect substitution effects (cf. Table C.3 in the appendix).

## Tables and Figures

Figure C.2: PRISMA diagram.



Notes: The PRISMA diagram (Moher et al., 2009) shows the study selection process from initial screening to full-text-level assessment.

Figure C.3: Rebound by study (excluding outliers).

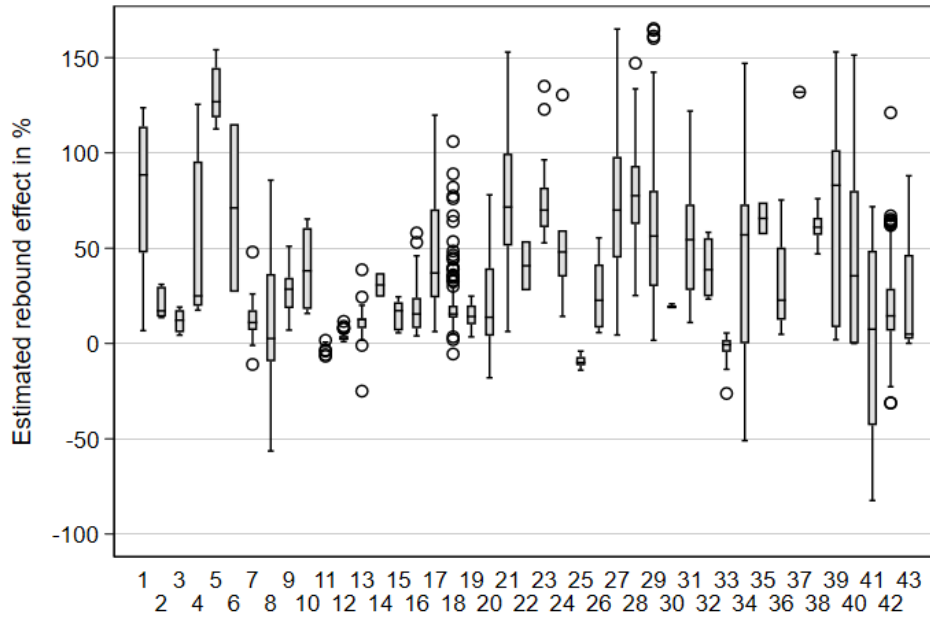
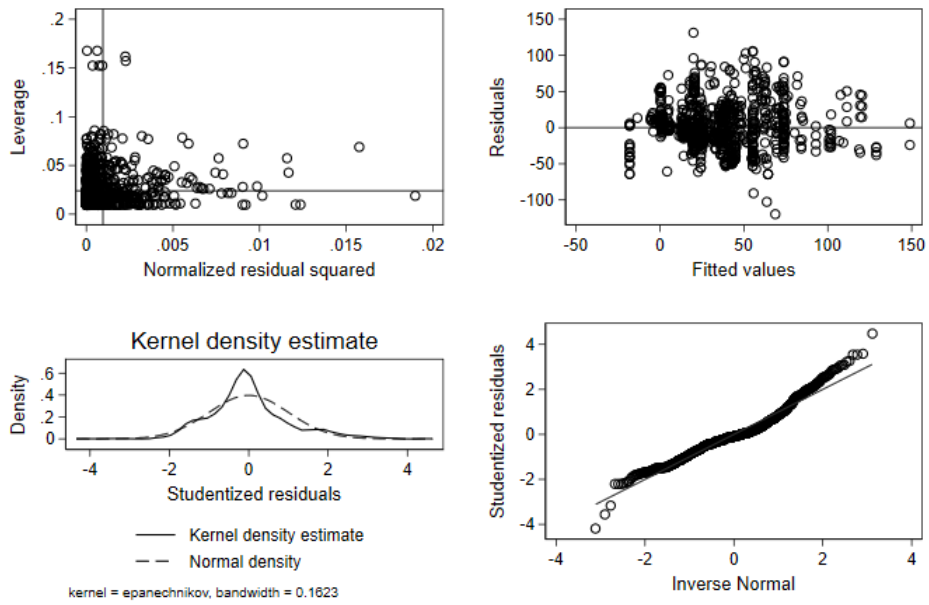


Figure C.4: Residual plots.



**Table C.1:** Excluded studies sorted by reason for exclusion (N=41).**No rebound quantification (N=19)**

- F. Belaid et al. (2020). "Investigating the factors shaping residential energy consumption patterns in France: evidence from quantile regression". In: *The European Journal of Comparative Economics* 17.1, pp. 127–151. doi: \url{10.25428/1824-2979/202001-127-151}.
- J. D. Bergesen et al. (2016). "Potential Long-Term Global Environmental Implications of Efficient Light-Source Technologies". In: *Journal of Industrial Ecology* 20.2, pp. 263–275. doi: \url{10.1111/jiec.12342}.
- S. Beucker et al. (2016). "Building Energy Management Systems: Global Potentials and Environmental Implications of Deployment". In: *Journal of Industrial Ecology* 20.2, pp. 223–233. doi: \url{10.1111/jiec.12378}.
- J. K. Breadsell et al. (2019). "Household Energy and Water Practices Change Post-Occupancy in an Australian Low-Carbon Development". In: *Sustainability* 11.20, p. 5559. doi: \url{10.3390/su11205559}.
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- A. R. Gopal et al. (2014). "Self-financed efficiency incentives: case study of Mexico". In: *Energy Efficiency* 7.5, pp. 865–877. doi: \url{10.1007/s12053-014-9263-9}.
- S. I. Isley et al. (2016). "Online purchasing creates opportunities to lower the life cycle carbon footprints of consumer products". In: *Proceedings of the National Academy of Sciences USA* 113.35, pp. 9780–9785. doi: \url{10.1073/pnas.1522211113}.
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**Table C.1:** (continued).

Q. Wang et al. (2015). “Structural Evolution of Household Energy Consumption: A China Study”. In: *Sustainability* 7.4, pp. 3919–3932. doi: \url{10.3390/su7043919}.

**Direct rebound only (N=2)**

G. Palmer (2012). “Does Energy Efficiency Reduce Emissions and Peak Demand? A Case Study of 50 Years of Space Heating in Melbourne”. In: *Sustainability* 4.7, pp. 1525–1560. doi: \url{10.3390/su4071525}.

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B. Yu et al. (2013b). “Rebound effects caused by the improvement of vehicle energy efficiency. An analysis based on a SP-off-RP survey”. In: *Transportation Research: Part D* 24, pp. 62–68. doi: \url{10.1016/j.trd.2013.06.001}.

**General equilibrium / economy wide / macro effects (N=5)**

P. E. Brockway et al. (2017). “Energy Rebound as a Potential Threat to a Low-Carbon Future. Findings from a New Exergy-Based National-Level Rebound Approach”. In: *Energies* 10.1, pp. 1–24. doi: \url{10.3390/en10010051}.

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**(Quasi-)experimental studies (N=3)**

S. Davis and I. Durbach (2010). “Modelling household responses to energy efficiency interventions”. In: *African Journals Online (AJOL)* 26.2, pp. 79–96. URL: %5Curl%7Bhttps://www.ajol.info/index.php/orion/article/view/62606/50538%7D.

M. Raynaud et al. (2016). “Evidence of an indirect rebound effect with reversible heat pumps. Having air conditioning but not using it?” In: *Energy Efficiency* 9.4, pp. 847–860. doi: \url{10.1007/s12053-015-9419-2}.

S. Seebauer (2018). “The psychology of rebound effects. Explaining energy efficiency rebound behaviours with electric vehicles and building insulation in Austria”. In: *Energy Research and Social Science* 46, pp. 311–320. doi: \url{10.1016/j.erss.2018.08.006}.

**Rebound mitigation / policy (no quantification) (N=1)**

S. Seebauer et al. (2019). “Promoting adoption while avoiding rebound: integrating disciplinary perspectives on market diffusion and carbon impacts of electric cars and building renovations in Austria”. In: *Energy, Sustainability and Society* 9.1, p. 26. doi: \url{10.1186/s13705-019-0212-5}.

**Theory / Concepts (N=4)**

E. Dütschke et al. (2018). “Moral Licensing—Another Source of Rebound?” In: *Frontiers in Energy Research* 6, pp. 1–10. doi: \url{10.3389/fenrg.2018.00038}.

J. C. J. M. van den Bergh (2011). “Energy Conservation More Effective With Rebound Policy”. In: *Environmental & Resource Economics* 48.1, pp. 43–58. doi: \url{10.1007/s10640-010-9396-z}.

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Q. Zhu and B. D. Leibowicz (2020). "Vehicle efficiency improvements, urban form, and energy use impacts". In: *Cities* 97, p. 102486. doi: \url{10.1016/j.cities.2019.102486}.

**Review (N=1)**

H. Walnum et al. (2014). "Can Rebound Effects Explain Why Sustainable Mobility Has Not Been Achieved?" In: *Sustainability* 6.12, pp. 9510–9537. doi: \url{10.3390/su6129510}.

**Rebound between different appliances / energy services only (N=4)**

N. K. Ghosh and M. F. Blackhurst (2014). "Energy Savings and the Rebound Effect with Multiple Energy Services and Efficiency Correlation". In: *Ecological Economics* 105, pp. 55–66. doi: \url{10.1016/j.ecolecon.2014.05.002}.

R. S. Santos et al. (2018a). "Energy Efficiency in Buildings by Using Evolutionary Algorithms. An Approach to Provide Efficiency Choices to the Consumer, Considering the Rebound Effect". In: *IFIP Advances in Information and Communication Technology* 521, pp. 120–129. doi: \url{10.1007/978-3-319-78574-5\_12}.

R. S. Santos et al. (2018b). "Evolutionary algorithms on reducing energy consumption in buildings. An approach to provide smart and efficiency choices, considering the rebound effect". In: *Computers & Industrial Engineering* 126, pp. 729–755. doi: \url{10.1016/j.cie.2018.09.050}.

B. Yu et al. (2013a). "Evaluating the direct and indirect rebound effects in household energy consumption behavior. A case study of Beijing". In: *Energy Policy* 57, pp. 441–453. doi: \url{10.1016/j.enpol.2013.02.024}.

**Rebound effects inseparable from other effects (N=1)**

J. Scheer et al. (2013). "Quantification of energy savings from Ireland's Home Energy Saving scheme. An ex post billing analysis". In: *Energy Efficiency* 6.1, pp. 35–48. doi: \url{10.1007/s12053-012-9164-8}.

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- E. C. Alfredsson (2004). ““Green” consumption—no solution for climate change”. In: *Energy* 29.4, pp. 513–524. doi: \url{10.1016/j.energy.2003.10.013}.
- R. Brännlund et al. (2007). “Increased energy efficiency and the rebound effect. Effects on consumption and emissions”. In: *Energy Economics* 29.1, pp. 1–17. doi: \url{10.1016/j.eneco.2005.09.003}.
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- J. Buhl (2014). “Revisiting Rebound Effects from Material Resource Use. Indications for Germany Considering Social Heterogeneity”. In: *Resources* 3.1, pp. 106–122. doi: \url{10.3390/resources3010106}.
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- M. Chitnis et al. (2020). “Rebound Effects for Household Energy Services in the UK”. in: *Energy Journal* 41.4, pp. 31–59. doi: \url{10.5547/01956574.41.4.mchi}.
- M. Chitnis and S. Sorrell (2015). “Living up to expectations. Estimating direct and indirect rebound effects for UK households”. In: *Energy Economics* 52, S100–S116. doi: \url{10.1016/j.eneco.2015.08.026}.
- M. Chitnis et al. (2014). “Who rebounds most? Estimating direct and indirect rebound effects for different UK socioeconomic groups”. In: *Ecological Economics* 106, pp. 12–32. doi: \url{10.1016/j.ecolecon.2014.07.003}.
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- D. Font Vivanco et al. (2015). “The relativity of eco-innovation. Environmental rebound effects from past transport innovations in Europe”. In: *Journal of Cleaner Production* 101, pp. 71–85. doi: \url{10.1016/j.jclepro.2015.04.019}.
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- W. Hagedorn and H. Wilts (2019). "Who should waste less? Food waste prevention and rebound effects in the context of the Sustainable Development Goals". In: *GAIA - Ecological Perspectives for Science and Society* 28.2, pp. 119–125. doi: \url{10.14512/gaia.28.2.10}.
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- P. J. Joyce et al. (2019). "A multi-impact analysis of changing ICT consumption patterns for Sweden and the EU. Indirect rebound effects and evidence of decoupling". In: *Journal of Cleaner Production* 211, pp. 1154–1161. doi: \url{10.1016/j.jclepro.2018.11.207}.
- K. Kratena and M. Wüger (2010). *The Full Impact of Energy Efficiency on Households' Energy Demand*. Ed. by Austrian Institute of Economic Research. Vienna. URL: %5Curl%7Bhttps://www.econstor.eu/handle/10419/128888%7D.
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- M. Lu and Z. Wang (2017). "Rebound effects for residential electricity use in urban China. An aggregation analysis based E-I-O and scenario simulation". In: *Annals of Operations Research* 255.1/2, pp. 525–546. doi: \url{10.1007/s10479-016-2153-0}.
- T. Makov and D. Font Vivanco (2018). "Does the Circular Economy Grow the Pie? The Case of Rebound Effects From Smartphone Reuse". In: *Frontiers in Energy Research* 6, p. 100. doi: \url{10.3389/fenrg.2018.00039}.
- K. Mizobuchi (2008). "An empirical study on the rebound effect considering capital costs". In: *Energy Economics* 30.5, pp. 2486–2516. doi: \url{10.1016/j.eneco.2008.01.001}.
- C. K. Murray (2013). "What if consumers decided to all 'go green'? Environmental rebound effects from consumption decisions". In: *Energy Policy* 54, pp. 240–256. doi: \url{10.1016/j.enpol.2012.11.025}.
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- R. Saleemdeen et al. (2017). "A holistic approach to the environmental evaluation of food waste prevention". In: *Waste Management* 59, pp. 442–450. doi: \url{10.1016/j.wasman.2016.09.042}. eprint: 27712945.
- H. Schmitz and R. Madlener (2020). "Direct and Indirect Energy Rebound Effects in German Households. A Linearized Almost Ideal Demand System Approach". In: *Energy Journal* 41.5, pp. 89–118. doi: \url{10.5547/01956574.41.5.hschr}.
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- K. Takase et al. (2005). "An Analysis of Sustainable Consumption by the Waste Input-Output Model". In: *Journal of Industrial Ecology* 9.1-2, pp. 201–219. doi: \url{10.1162/1088198054084653}.



**Table C.2:** (continued).

- B. A. Thomas and I. M. L. Azevedo (2013). "Estimating Direct and Indirect Rebound Effects for U.S. Households with Input-Output Analysis. Part 1: Theoretical Framework". In: *Ecological Economics* 86, pp. 199–210. URL: %5Curl%7Bhttp://search.ebscohost.com/login.aspx?direct=true&db=ecn&AN=1353756&site=ehost-live%7D.
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- J. Walzberg et al. (2020). "Should we fear the rebound effect in smart homes?" In: *Renewable & Sustainable Energy Reviews* 125, p. 109798. doi: \url{10.1016/j.rser.2020.109798}.
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- R. Wood et al. (2018). "Prioritizing Consumption-Based Carbon Policy Based on the Evaluation of Mitigation Potential Using Input-Output Methods". In: *Journal of Industrial Ecology* 22.3, pp. 540–552. doi: \url{10.1111/jiec.12702}.
- Y.-J. Zhang et al. (2017). "The direct and indirect CO<sub>2</sub> rebound effect for private cars in China". In: *Energy Policy* 100.2, pp. 149–161. doi: \url{10.1016/j.enpol.2016.10.010}.

**Table C.3:** Summary statistics - Moderators.

Moderator	Definition	N	Share in %	Mean	Min	Max
<b>Data</b>						
HH exp. survey data	=1 if household consumption data is from expenditure survey, 0 else	1043	100	0.35	0	1
Impact measure not IO	=1 if impact measure data is not from Input-Output tables, 0 else	1043	100	0.07	0	1
<b>Rebound scenario setup</b>						
<i>Action type</i>	Type of action	1043	100			
Behav. change / sufficiency		206	19.75			
Efficiency improvement		808	77.47			
Both		29	2.78			
<i>Area</i>	Area of action	1043	100			
Residential energy		599	57.43			
Food		103	9.88			
Transport / travel		215	20.61			
Other		40	3.84			
Multiple		86	8.25			
<i>Impact</i>	Type of rebound impact measure	1043	100			
CO <sub>2</sub>		259	24.83			
CO <sub>2</sub> -eq		405	38.83			
Energy		242	23.20			
Social indicator		16	1.53			
Total material requirement		29	2.78			
Other env. indicator		92	8.82			
<i>Income groups</i>	Rebound differentiated by income group	1043	100			
Average income		878	84.18			
High income		82	7.86			

**Table C.3:** (continued).

Moderator	Definition	N	Share in %	Mean	Min	Max
Low income		83	7.96			
<b>Rebound estimation</b>						
Substitution effect considered	=1 if substitution effect is considered in addition to income effect, 0 else	1043	100	0.13	0	1
Rebound indirect only	=1 if rebound is indirect only, 0 if indirect + direct rebound	1043	100	0.42	0	1
AIDS*	=1 if study estimates an Almost Ideal Demand System, 0 else	1043	100	0.29	0	1
Direct*	=1 if only direct emissions are considered, 0 else	1043	100	0.08	0	1
<i>Re-spending scenario</i>	Type of re-spending scenario	1043	100			
Proportional		298	28.57			
Marginal		621	59.54			
Hypothetical: Green case		33	3.16			
Hypothetical: Worst case		7	0.67			
Other		84	8.05			
<i>Direct rebound assumption</i>	Level of direct rebound assumed	1043	100			
Not assumed		836	80.15			
0-20%		56	5.37			
30-50%		124	11.89			
60-90%		27	2.59			
<i>Efficiency costs*</i>	Efficiency increase considered costly	1043	100			
Efficiency costless		597	57.24			
Efficiency costly		241	23.11			
Not applicable		205	19.65			
<b>Other</b>						
Publication year*	Year of publication	1043	100	2015.39	2002	2020
Reviewed*	=1 if study is published in a peer-reviewed journal, 0 else	1043	100	0.98	0	1
Exact numbers*	=1 if exact numbers are given in the study, 0 if numbers read off graphs	1043	100	0.70	0	1

**Table C.3:** (continued).

Moderator	Definition	N	Share in %	Mean	Min	Max
Average calculated*	=1 if averages were calculated for subsequent analysis, 0 else	1043	100	0.03	0	1
<i>Authors group*</i>	Group of authors with several papers in this dataset	1043	100			
Acosta and Buhl		24	2.30			
Chitnis and Sorrell		282	27.04			
Font Vivanco /		243	23.30			
Freire-González et al.						
Håkansson / Finnveden et al.		30	2.88			
Lu / Wang et al.		18	1.73			
None		303	29.05			
Thomas / Azevedo et al.		58	5.56			
Wood et al. / NTNU		85	8.15			

Notes: Moderators with an \* are not part of the baseline specification.

**Table C.4:** Summary statistics - Rebound estimates by study.

Study ID	Publication	Orig. N	N	Mean	SD	Min	Max
1	Lenzen and Dey (2002)	12	12	75.19	41.72	6.74	123.68
2	Alfredsson (2004) <sup>o</sup>	8	6	20.45	7.94	13.60	31.03
3	Briceno et al. (2005)	5	5	11.83	6.66	4.39	19.11
4	Takase et al. (2005) <sup>o</sup>	8	6	51.37	46.98	17.49	125.48
5	Brännlund et al. (2007) <sup>o</sup>	9	6	130.60	16.32	112.57	154.12
6	Mizobuchi (2008)	2	2	71.11	62.38	27.00	115.22
7	Nässén and Holmberg (2009)	12	12	13.00	14.52	-11.00	48.00
8	Kratena and Wüger (2010)	9	9	12.49	46.84	-56.40	85.70
9	Druckmann et al. (2011) <sup>o</sup>	9	8	27.63	13.79	7.00	51.00
10	Freire-González (2011)	8	8	39.34	23.02	15.77	65.31
11	Tukker et al. (2011)	24	24	-1.19	1.96	-6.70	1.70
12	Cellura et al. (2013)	48	48	3.43	2.37	1.00	11.60
13	Chitnis et al. (2013)	45	45	10.30	8.78	-25.00	38.68
14	Lin et al. (2013)	2	2	30.73	8.90	24.44	37.02
15	Murray (2013)	11	11	14.90	6.89	5.60	24.50
16	Thomas and Azevedo (2013)	52	52	18.43	13.84	4.00	58.00
17	Buhl (2014)	18	18	47.56	33.57	6.25	119.83
18	Chitnis et al. (2014)	163	163	20.10	15.58	-5.50	106.00
19	Thomas et al. (2014) <sup>a</sup>	300	6	14.45	7.49	3.48	24.86
20	Chitnis and Sorrell (2015)	24	24	21.63	23.28	-18.00	78.00
21	Grabs (2015)	28	28	74.55	34.53	6.30	152.90
22	Håkansson and Finnveden (2015) <sup>o</sup>	5	2	40.80	18.38	27.80	53.79
23	Font Vivanco et al. (2015) <sup>o</sup>	75	33	74.32	18.06	52.78	135.00
24	Buhl and Acosta (2016)	6	6	55.86	39.71	14.22	130.43
25	Font Vivanco et al. (2016)	24	24	-9.42	3.08	-14.00	-4.00
26	Wang et al. (2016)	12	12	24.89	17.44	5.74	55.43
27	Freire-González et al. (2017) <sup>o</sup>	80	59	76.90	41.83	4.46	165.00
28	Freire-González and Font Vivanco (2017) <sup>o</sup>	20	15	82.08	32.50	25.18	147.08
29	Freire-González (2017) <sup>o</sup>	108	98	60.27	39.60	1.67	165.21
30	Lu and Wang (2017)	6	6	19.42	0.93	18.40	20.80
31	Ottelin et al. (2017)	12	12	57.75	36.36	11.00	122.00
32	Salemdeeb et al. (2017)	6	6	39.85	14.76	23.30	58.35
33	Zhang et al. (2017) <sup>a</sup>	744	24	-2.20	7.14	-26.23	5.49
34	Steen-Olsen and Wood (2018) <sup>o</sup>	24	23	43.13	51.24	-51.00	147.00
35	Wen et al. (2018) <sup>a</sup>	52	2	65.64	11.92	57.21	74.07
36	Wood et al. (2018)	4	4	31.38	30.55	4.80	75.30
37	Chen et al. (2019) <sup>a</sup>	31	1	131.90	.	131.90	131.90
38	Hagedorn and Wilts (2019)	9	9	61.32	8.70	47.00	76.00
39	Joyce et al. (2019) <sup>o</sup>	30	28	70.54	49.28	2.00	153.00
40	Vita et al. (2019) <sup>o</sup>	84	58	46.74	42.82	-0.00	151.34
41	Chitnis et al. (2020) <sup>o</sup>	44	42	2.74	46.27	-82.40	71.80
42	Schmitz and Madlener (2020)	80	80	19.94	25.30	-31.30	121.10
43	Walzberg et al. (2020)	4	4	24.48	42.41	0.10	88.00
<b>Total</b>		<b>2257</b>	<b>1043</b>	<b>34.30</b>	<b>39.63</b>	<b>-82.40</b>	<b>165.21</b>

Notes: Orig. N refers to the initial number of rebound estimates identified by study. N is the final number of rebound estimates by study after aggregation to the national level and exclusion of outliers, if applicable. N is used as basis for the summary statistics. We use superscripts <sup>a</sup> and <sup>o</sup> to signal a change from the original number of estimates due to aggregation and outlier exclusion, respectively.

**Table C.5: Robustness results - Adding moderators.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	Change detailed scenario	LCA	AIDS	Direct emissions	Publication year	Reviewed	Efficiency costly	Authors group
<b>Data</b>									
HH consumption data is from expenditure survey	-0.90 (5.89)	0.04 (6.90)	1.01 (6.74)	-0.37 (6.08)	-0.95 (6.12)	0.45 (6.26)	-0.73 (5.95)	-0.08 (6.35)	3.98 (5.06)
Impact measure data not from Input-Output tables	-42.43*** (12.65)	-42.56*** (12.70)	-39.66*** (12.14)	-41.92*** (12.67)	-42.28*** (13.83)	-42.79*** (13.29)	-41.03*** (13.11)	-42.07*** (12.87)	-38.27*** (11.39)
<b>Rebound scenario setup</b>									
<i>Type of action (ref. Efficiency)</i>									
Behav. change/sufficiency	23.82*** (6.75)	24.74*** (7.22)	21.43*** (6.96)	21.60*** (5.98)	23.82*** (6.76)	23.49*** (6.40)	24.05*** (6.60)	43.28*** (11.54)	25.66*** (6.54)
Both	7.26 (10.79)	7.45 (10.66)	1.72 (12.14)	5.87 (11.47)	7.27 (10.88)	7.44 (9.99)	6.85 (10.84)	8.60 (11.15)	6.24 (13.45)
<i>Area of action (ref. Residential energy)</i>									
Food	-0.63 (14.91)	-0.61 (14.90)	2.76 (12.90)	1.53 (14.75)	-0.65 (14.92)	0.32 (14.04)	-0.91 (14.90)	-0.19 (14.67)	-2.83 (14.44)
Transport / travel	19.21* (9.59)	19.29* (9.71)	21.29** (9.44)	21.26** (8.57)	19.18* (9.83)	19.80** (9.57)	19.51** (9.57)	19.64** (9.54)	14.11* (7.80)
Other	44.41*** (11.04)	44.26*** (10.90)	43.25*** (10.84)	45.22*** (10.60)	44.39*** (11.19)	42.35*** (10.85)	44.37*** (11.05)	44.12*** (11.23)	36.88*** (10.49)
Multiple	7.09 (5.36)	7.88 (5.36)	7.70 (5.46)	6.39 (5.30)	7.09 (5.37)	8.18 (5.41)	7.11 (5.35)	8.03 (5.34)	6.80 (5.28)
<i>Rebound impact measure (ref. Energy)</i>									
CO <sub>2</sub>	-18.47** (8.54)	-17.11** (7.79)	-15.09 (9.07)	-14.74* (8.39)	-18.49** (8.61)	-20.48** (8.57)	-19.34** (9.09)	-16.88** (7.74)	-16.61 (10.33)
CO <sub>2</sub> -eq	-24.34*** (8.25)	-23.47*** (8.67)	-20.32** (9.47)	-23.24*** (8.38)	-24.32*** (8.34)	-25.53*** (7.88)	-24.81*** (8.37)	-24.50*** (8.07)	-19.50* (11.15)
Social indicator	47.27*** (7.73)	46.15*** (7.69)	45.63*** (8.29)	49.15*** (7.94)	47.27*** (7.72)	46.75*** (6.65)	46.45*** (8.01)	45.75*** (7.81)	37.23*** (8.50)
Total material requirement	0.38 (9.94)	-1.32 (9.65)	-3.24 (10.61)	10.99 (10.34)	0.38 (9.97)	-1.80 (9.33)	-0.10 (9.96)	-1.47 (9.84)	-6.70 (7.17)

**Table C.5:** (continued).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	Change detailed scenario	LCA	AIDS	Direct emissions	Publication year	Reviewed	Efficiency costly	Authors group
Other env. indicator	-17.31 (12.08)	-16.26 (12.21)	-13.89 (10.84)	-14.94 (12.16)	-17.31 (12.08)	-17.77 (11.95)	-17.88 (12.24)	-17.16 (12.02)	-12.50 (14.91)
<i>Rebound by income group (ref. Average income)</i>									
High income	2.43 (3.43)	2.30 (3.26)	3.00 (3.61)	0.85 (3.65)	2.42 (3.39)	2.20 (3.24)	2.36 (3.41)	2.54 (3.21)	2.97 (4.15)
Low income	11.91* (6.06)	11.77* (5.93)	12.38** (6.11)	10.35* (6.06)	11.90* (6.05)	11.62* (5.85)	11.84* (6.03)	12.01** (5.85)	12.44* (6.82)
<b>Rebound estimation</b>									
Substitution effect considered in addition to income effect	22.86 (14.35)	20.41* (11.84)	16.22 (12.34)	30.81* (15.73)	22.86 (14.31)	21.87 (15.08)	22.94 (14.28)	19.89 (12.37)	27.62** (13.47)
Rebound is indirect only	-18.50*** (5.86)	-18.89*** (5.88)	-18.53*** (5.85)	-17.41*** (6.04)	-18.51*** (5.88)	-18.72*** (5.82)	-18.73*** (5.90)	-18.77*** (5.86)	-18.15*** (5.97)
<i>Respending scenario type (ref. Marginal)</i>									
Proportional	-1.19 (6.38)	-0.83 (6.83)	-3.68 (6.19)	-5.56 (7.36)	-1.21 (6.26)	-1.35 (6.30)	-1.02 (6.42)	-0.95 (6.64)	-4.21 (6.78)
Hypothetical: Green case	-9.08 (6.07)	-8.94 (6.19)	-11.14* (5.68)	-11.93* (6.58)	-9.10 (5.96)	-10.45* (5.97)	-9.14 (6.09)	-8.50 (6.35)	-14.16** (6.90)
Hypothetical: Worst case	46.95*** (13.60)	45.95*** (13.37)	48.40*** (13.98)	47.83*** (12.01)	46.92*** (13.63)	45.32*** (13.04)	47.01*** (13.62)	46.99*** (13.39)	41.73*** (15.35)
Other	3.86 (6.50)	4.64 (7.46)	4.96 (6.91)	-1.49 (7.97)	3.85 (6.45)	5.05 (6.30)	5.47 (6.98)	5.63 (8.03)	2.98 (5.34)
<i>Level of direct rebound assumed (ref. not assumed)</i>									
0-20%	3.45 (6.85)	1.37 (7.03)	4.21 (6.82)	6.99 (6.72)	3.44 (6.87)	3.46 (6.21)	4.07 (6.37)	1.48 (6.67)	0.90 (10.08)
0-50 %	36.52*** (6.99)	34.96*** (7.50)	35.93*** (6.85)	37.64*** (7.13)	36.51*** (7.05)	34.69*** (6.91)	36.18*** (7.09)	34.85*** (7.32)	28.34* (14.69)
60-90%	82.04*** (5.48)	80.64*** (5.76)	80.32*** (5.97)	81.33*** (5.96)	82.02*** (5.54)	80.56*** (5.04)	81.83*** (5.45)	80.36*** (5.43)	73.50*** (14.67)

**Table C.5:** (continued).

	(1) Baseline	(2) Change detailed scenario	(3) LCA	(4) AIDS	(5) Direct emis- sions	(6) Publication year	(7) Reviewed	(8) Efficiency costly	(9) Authors group
Detailed efficiency / sufficiency change scenario		-3.94 (6.77)							
Life-cycle assessment data used			-10.04 (6.60)						
Estimation of an Almost Ideal Demand System				-14.80 (9.38)					
Only direct emissions considered					-0.23 (8.15)				
<b>Other</b>									
Year of publication (centred)						0.75 (0.83)			
Published in a peer- reviewed journal							10.01 (13.72)		
<i>Efficiency increase considered costly (ref. Not costly)</i>									
Efficiency costly								-5.69 (6.71)	
Not applicable								-21.91** (8.32)	
<i>Authors group with multiple papers in this dataset (ref. Chitnis and Sorrell)</i>									
Acosta and Buhl									11.05 (8.13)
Font Vivanco, Freire-González, et al.									20.43 (19.93)
Håkansson, Finnveden, et al.									29.16*** (7.75)
Lu, Wang, et al.									6.82 (11.32)
None									3.61 (7.70)



**Table C.5:** (continued).

	(1) Baseline	(2) Change detailed scenario	(3) LCA	(4) AIDS	(5) Direct emis- sions	(6) Publication year	(7) Reviewed	(8) Efficiency costly	(9) Authors group
Thomas, Azevedo, et al.									1.98 (5.81)
Wood et al./NTNU									11.91 (13.49)
Constant	38.42*** (8.10)	39.80*** (7.47)	41.22*** (8.36)	40.76*** (8.56)	38.47*** (8.08)	39.45*** (7.63)	28.74* (14.72)	39.98*** (7.57)	28.75** (11.56)
<i>N</i>	1043	1043	1043	1043	1043	1043	1043	1043	1043
<i>R</i> <sup>2</sup>	0.444	0.445	0.451	0.456	0.444	0.447	0.445	0.446	0.459
Adj. <i>R</i> <sup>2</sup>	0.431	0.431	0.437	0.442	0.430	0.433	0.431	0.432	0.442

Notes: Clustered standard errors in parentheses.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table C.6:** Robustness results - Outliers and coding decisions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Robust SE	Full observa- tions	PC 1 99	PC 2.5 97.5	Reg. diag- nostics	Exact num- bers	No average
<b>Data</b>								
HH consumption data is from expenditure survey	-0.90 (5.89)	-0.90 (2.85)	-255.15* (146.26)	-67.52 (45.97)	-30.91 (25.32)	-1.16 (5.86)	-4.96 (6.53)	-0.50 (5.99)
Impact measure data not from Input-Output tables	-42.43*** (12.65)	-42.43*** (5.91)	-326.26 (287.36)	-99.29 (70.66)	-60.68* (35.53)	-39.81*** (10.66)	-40.36*** (11.72)	-36.71*** (11.49)
<b>Rebound scenario setup</b>								
<i>Type of action (ref. Efficiency)</i>								
Behav. change/sufficiency	23.82*** (6.75)	23.82*** (4.49)	-192.41 (186.59)	1.34 (39.98)	49.18*** (18.15)	24.84*** (6.83)	19.46** (8.16)	23.41*** (6.78)
Both	7.26 (10.79)	7.26 (8.66)	-207.06 (253.28)	-21.97 (50.10)	15.37 (24.41)	15.67 (12.28)	7.56 (11.96)	6.08 (10.79)
<i>Area of action (ref. Residential energy)</i>								
Food	-0.63 (14.91)	-0.63 (5.99)	362.17 (272.83)	36.48 (64.27)	-27.70 (32.99)	1.16 (14.45)	10.46 (11.52)	0.06 (14.88)
Transport / travel	19.21* (9.59)	19.21*** (2.74)	748.03* (436.20)	165.65 (104.24)	62.87 (42.13)	19.15* (9.55)	18.37* (10.35)	21.33** (10.39)
Other	44.41*** (11.04)	44.41*** (8.97)	446.70* (262.44)	142.54** (56.64)	107.22*** (20.09)	48.47*** (10.32)	36.95*** (9.63)	45.27*** (11.07)
Multiple	7.09 (5.36)	7.09** (3.10)	209.22 (161.35)	41.65 (32.19)	10.79 (10.32)	4.49 (3.81)	14.35 (9.82)	7.77 (5.59)
<i>Rebound impact measure (ref. Energy)</i>								
CO <sub>2</sub>	-18.47** (8.54)	-18.47*** (4.00)	648.25 (482.65)	122.80 (103.15)	14.99 (34.87)	-17.74** (8.25)	-15.70 (10.66)	-18.07** (8.72)
CO <sub>2</sub> -eq	-24.34*** (8.25)	-24.34*** (3.70)	-112.25 (104.72)	-25.97 (33.85)	-29.83* (16.25)	-23.15*** (7.77)	-18.02** (7.65)	-24.44*** (8.64)

**Table C.6:** (continued).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Robust SE	Full observa- tions	PC 1 99	PC 2.5 97.5	Reg. diag- nostics	Exact num- bers	No average
Social indicator	47.27*** (7.73)	47.27*** (8.46)	73.43 (110.50)	61.53* (36.16)	43.07** (18.79)	47.71*** (7.53)	45.05*** (7.69)	47.30*** (8.05)
Total material requirement	0.38 (9.94)	0.38 (7.11)	-75.01 (103.95)	-3.60 (37.29)	-11.10 (17.95)	-3.64 (9.13)	2.11 (9.40)	0.66 (10.16)
Other env. indicator	-17.31 (12.08)	-17.31*** (5.93)	-48.75 (142.78)	53.10 (70.37)	29.00 (31.48)	-20.65* (11.48)	0.81 (8.66)	-17.33 (12.12)
<i>Rebound by income group (ref. Average income)</i>								
High income	2.43 (3.43)	2.43 (2.55)	-10.11 (42.13)	13.04 (10.85)	12.11 (9.67)	2.42 (3.39)	1.52 (3.18)	2.42 (3.41)
Low income	11.91* (6.06)	11.91*** (3.18)	17.35 (36.71)	23.88* (13.05)	20.35* (11.50)	11.89* (6.00)	16.92** (6.29)	11.89* (6.03)
<b>Rebound estimation</b>								
Substitution effect considered in addition to income effect	22.86 (14.35)	22.86*** (5.18)	-790.20 (492.95)	-143.36 (112.74)	-22.56 (44.25)	20.11 (12.52)	19.24 (14.74)	23.24 (14.33)
Rebound is indirect only	-18.50*** (5.86)	-18.50*** (2.41)	-111.36 (68.82)	-23.82** (11.04)	-18.08* (8.98)	-17.97*** (5.94)	-21.69*** (5.96)	-19.27*** (6.06)
<i>Responding scenario type (ref. Marginal)</i>								
Proportional	-1.19 (6.38)	-1.19 (3.21)	-256.75 (179.24)	-50.24 (50.32)	-23.01 (25.23)	-2.94 (6.15)	2.54 (6.32)	-0.70 (6.39)
Hypothetical: Green case	-9.08 (6.07)	-9.08* (5.35)	-159.08 (113.96)	-52.26 (31.58)	-28.91* (16.78)	-10.53* (5.92)	-16.07* (9.08)	-8.37 (6.05)
Hypothetical: Worst case	46.95*** (13.60)	46.95*** (12.39)	-6.20 (133.26)	130.88*** (45.61)	161.89*** (33.25)		41.81** (17.83)	47.52*** (13.75)
Other	3.86 (6.50)	3.86 (3.71)	-362.91 (311.10)	-82.05 (75.70)	-31.62 (32.74)	4.47 (6.52)	8.09 (7.04)	4.18 (6.57)

**Table C.6:** (continued).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Robust SE	Full observa- tions	PC 1 99	PC 2.5 97.5	Reg. diag- nostics	Exact num- bers	No average
<i>Level of direct rebound assumed (ref. not assumed)</i>								
0-20%	3.45 (6.85)	3.45 (3.90)	20.27 (84.56)	35.73 (49.91)	3.53 (19.78)	5.00 (7.03)	-5.32 (6.70)	2.83 (7.96)
30-50%	36.52*** (6.99)	36.52*** (4.96)	169.55 (101.40)	115.55*** (39.76)	68.16*** (21.03)	34.88*** (6.85)	31.06*** (4.74)	37.07*** (7.09)
60-90%	82.04*** (5.48)	82.04*** (6.08)	77.15 (89.34)	86.05** (35.35)	77.83** (30.01)	84.96*** (5.17)	51.86*** (7.38)	82.67*** (5.60)
Constant	38.42*** (8.10)	38.42*** (3.49)	254.79 (170.19)	71.72 (47.94)	58.75*** (21.02)	37.53*** (7.86)	38.94*** (7.88)	37.77*** (8.18)
<i>N</i>	1043	1043	1163	1142	1105	1022	729	1013
<i>R</i> <sup>2</sup>	0.444	0.444	0.042	0.127	0.165	0.463	0.388	0.438
Adj. <i>R</i> <sup>2</sup>	0.431	0.431	0.022	0.108	0.147	0.451	0.368	0.424

*Notes:* Standard errors in parentheses are clustered at the study level except for column (2), for which standard errors are heteroscedasticity-robust. In column (3) all observations are considered (including outliers), while for columns (4) and (5) outliers are defined by effect size percentiles. Columns (6), (7) and (8) start from the baseline regression depicted in column (1) and additionally exclude observations based on findings from regression diagnostics (6), observations where the numbers had to be read-off figures (7) or which are an intra-study average (8), respectively.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

# Appendix

## **Erklärung zum selbständigen Verfassen der Arbeit**

Ich erkläre hiermit, dass ich meine Doktorarbeit „Meta-analyses on Environmental and Energy Economics“ selbstständig und ohne fremde Hilfe angefertigt habe und dass ich als Koautor maßgeblich zu den weiteren Fachartikeln beigetragen habe. Alle von anderen Autoren wörtlich übernommenen Stellen, wie auch die sich an die Gedanken anderer Autoren eng anlehnenden Ausführungen der aufgeführten Beiträge wurden besonders gekennzeichnet und die Quellen nach den mir angegebenen Richtlinien zitiert.

Kiel,

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