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Price Elasticity of Electricity Revisited: A Meta-Analysis

Bachelor's thesis

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Abstract

Despite a plethora of empirical research on electricity demand, results regarding estimated price elasticities persist to be inconclusive. Our meta-analysis synthesizes 4521 estimates from 413 studies to explore the presence of publication and endogeneity bias. We code for over 100 variables to quantify the response of electricity consumers to price shifts. The price elasticity of electricity is inelastic and the short-run elasticity sample average of -0.231 is double (in magnitude) the short-run elasticity corrected for publication bias, which is -0.116. The long-run elasticity adjusted for publication bias is -0.303. We conclude that experimental studies, while also suffering from publication bias, report unbiased elasticities of -0.07. Our thesis also confirms a significant occurrence of p-hacking across multiple specifications. By employing Bayesian model averaging, we explore the heterogeneity among reported elasticities, finding that factors such as decreasing tariffs, demographics and fuel usage controls, daylight hours and number of citations critically influence the variability in findings. The average and marginal price of electricity and time of use tariffs play a negligible role in explaining the differences in estimated elasticities.

JEL Classification	D01, Q40, Q49, C11
Keywords	Electricity, Price elasticity, Meta-analysis, Publication bias, Endogeneity bias

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Abstrakt

Navzdory množství empirických výzkumů poptávky po elektřině jsou výsledky odhadů cenové elasticity stále nejednoznačné. Zpracovaná meta-analýza shromažďuje 4521 pozorování ze 413 studií s cílem prozkoumat přítomnost publikačního zkreslení a zkreslení endogenity. Kódujeme více než 100 proměnných, abychom kvantifikovali reakci spotřebitelů elektřiny na změny ceny. Cenová elasticita poptávky po elektřině je neelastická a průměrná krátkodobá elasticita (z našeho vzorku studií) -0,231 je dvojnásobná (co do velikosti) oproti krátkodobé elasticitě očištěné o publikační zkreslení, která je -0,116. Dlouhodobá elasticita očištěná o publikační zkreslení je -0,303. Docházíme k závěru, že experimentální studie, ačkoli také trpí publikačním zkreslením, uvádějí nižší nezkreslené elasticity v hodnotě -0,07. Naše práce také potvrzuje významný výskyt p-hackingu ve více specifikacích. Pomocí Bayesovského průměrování modelu zkoumáme heterogenitu mezi uváděnými elasticitami a zjišťujeme, že faktory, jako jsou klesající tarify, demografické údaje a kontroly spotřeby paliva, denní doba a počet citací dané studie, rozhodujícím způsobem ovlivňují variabilitu zjištění. Průměrná a mezní cena elektřiny a časové tarify spotřeby hrají při vysvětlování rozdílů v odhadovaných elasticitách zanedbatelnou roli.

Klasifikace JEL	D01, Q40, Q49, C11
Klíčová slova	Elektřina, Cenová elasticita, Meta-analýza, Publikační zkreslení, Endogenní zkreslení

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Acronyms

2SLS Two Stage Least Squares

3SLS Three Stage Least Squares

BMA Bayesian Model Averaging

BPE Best Practice Estimate

CDD Cooling Degree Days

CI Confidence Interval

EBB Effect Beyond Bias

FAT Funnel Asymmetry Test

FE Fixed-effects Model

GMM Generalized Method of Moments

HDD Heating Degree Days

I-R Intermediate-run

IV Instrumental Variable

LE Lagged-endogenous Model

L-R Long-run

MAIVE Meta-analysis Instrumental Variable Estimator

ML Maximum Likelihood

OLS Ordinary Least Squares

PE Price Elasticity

PET Precision Asymmetry Test

PIP Posterior Inclusion Probability

RE Random-effects Model

SD Standard Deviation

SEM Simultaneous Equation Technique

SE Standard Error

SUR Seemingly Unrelated Regression

S-R Short-run

TOU Time of Use

UIP Unit Information Prior

VIF Variance Inflation Factor

WAAP Weighted Average of Adequately Powered

Chapter 1

Introduction

Electricity as an energy source plays a key role in the viability of an economy. Many sectors such as manufacturing, health, construction and communications just to mention a few rely heavily on the production of power for their activities. Furthermore, Onakoya *et al.* (2013) characterize electricity as "the pillar of wealth creation" in many, especially developing, countries. According to Dahl (2011), electricity consumption increases its share among energy fuels as countries become richer. Making sure that a country has an adequate electricity generation capacity is among the most important prerequisites for economic growth (Outlook 2013). Therefore, understanding the responsiveness of a consumer can help the government and utility companies to reliably predict future energy needs and design pricing and taxation policies (Espey & Espey 2004).

On the individual level, however, matching electricity demand and supply is often difficult. Demand is subject to sudden shifts throughout the day and suppliers of electricity have to take into account this variability, usually by storing the surplus of generated electricity in times of low demand, which can be very costly (Ajanovic *et al.* 2020). The ability to store excess energy during peak generation periods and release it during times of high demand or low renewable generation emerges as a critical component for ensuring a stable, resilient, and sustainable energy future, as highlighted in the top EU Energy priorities (Commission 2017). Being able to quantify characteristics of consumer demand would help with such an endeavour. In this respect, the response of consumers can be measured using the price elasticity of electricity demand, a normalized parameter that informs policymakers how consumption changes as a response to price shifts.

It is not surprising that electricity demand has been one of the most heavily studied energy products over the past decades (Dahl 2011). While there is a theoretical foundation that the relationship between electricity demand and price is negative, the results of studies vary widely. Multiple papers argue that price elasticity is below -6 (Bernard *et al.* 1996; Narayan *et al.* 2007; Kohler 2014), while other authors estimate the price elasticity to be above 1 (Hartman & Werth 1981; Pesaran *et al.* 1999; Bildirici *et al.* 2012). Such differences arise due to various reasons, to name a few: inclusion of control variables (e.g. household characteristics, temperature), treatment of endogeneity, data granularity and the length of period for which price elasticity of electricity is measured. Hence, it is challenging to make generalizations of the electricity price and demand relationship from individual studies. Labandeira *et al.* (2017) argue that despite the increasing number of individual studies on electricity, there is still a scarcity of attempts to summarize the elasticities into a single number. Given the heterogeneity of the estimates, we can employ a method which reconciles the variety of results called meta-analysis, introduced by Glass (1976).

Admittedly, several surveys (including meta-analyses) attempt to explain the relationship between electricity price and demand and present generalized findings. Few studies briefly summarize the previous research on the topic (Taylor 1975; Aigner 1985; Dahl 1993; 2011). The first meta-analysis of the price elasticity of electricity was conducted by Espey & Espey (2004), followed by Horáček (2014), Labandeira *et al.* (2017), Zhu *et al.* (2018), Zabaloy & Viego (2022) and Fatima (2023). This thesis advances the previous studies in multiple ways. We collect 4521 estimates from 413 studies and employ novel techniques to quantify publication and endogeneity bias. Furthermore, by collecting over 100 variables, we can investigate the heterogeneity of the estimates by employing the Bayesian and Frequentist model averaging estimation techniques. The objective of this thesis is to summarize the plethora of literature on the topic and assess to what extent are the estimated elasticities affected by the potential publication and endogeneity bias.

Firstly, we hypothesize that the estimates of the price elasticity of electricity are tainted by publication selection bias. This means that researchers consciously or unconsciously manipulate the research process and decide not to publish certain results. This might be caused by various reasons, such as lack of time to undergo the peer review process (Song *et al.* 2013) or counter-intuitive results (Havranek & Irsova 2012). Some authors even argue that about 50% of completed studies remain unpublished (Scherer *et al.* 2018). We find that the sample averages considerably exaggerate unbiased elasticities due to publication bias. While the sample average reports a short-run elasticity of -0.231, we establish the corrected estimate to be -0.116. Similarly, in the long-run, we estimate the price elasticity to be -0.303, compared to the biased sample average of -0.532. The significant presence of p-hacking is also supported by various tests. Approximately 200 estimates out of 4521 (4.4%) lie right below the 5% significance threshold (-1.96), occupying less than 0.04% of our winsorized t-statistic range.

Moreover, this is the first meta-analysis to explicitly address the issue of potential endogeneity bias. While previous surveys and meta-analyses collect information on estimation techniques, the authors only conclude that various estimation techniques, such as IV, have no significant effect (even on the 10% level) on the overall price elasticity estimate. We find that there is no significant difference between studies neglecting endogeneity and those which account for it, however, experimental data designed to establish a causal relationship report much lower elasticities of -0.07 compared to non-experimental data. Quasi-experimental surveys report a relatively more elastic response of -0.110. In terms of heterogenous effects on estimates, we conclude that the use of experimental and cross-sectional data, decreasing tariff, daylight hours and number of citations significantly affect the price elasticities in the literature. On the other hand, neither average nor marginal price and also time of use tariff do not help to explain the heterogeneity of the effects.

The rest of the thesis is structured as follows: Chapter 2 introduces the reader to the topic of electricity demand and provides a literature overview. The contribution of this thesis to the existing literature is also incorporated in this section. Chapter 3 describes the selection criteria for assembling the data set and a general overview of the literature. In Chapter 4, different statistical tests are conducted to assess the publication and endogeneity bias in collected studies and their results are presented. Chapter 5 describes and employs model averaging techniques to explore heterogeneity between the estimates. Further robustness checks are included. Best practice estimates, cross-country elasticities and sensitivity analysis are the subject of Chapter 6. Chapter 7 encapsulates the final results, highlights possible limitations, discusses areas for further research, and concludes the thesis.

Chapter 2

Price Elasticity of Electricity Demand

This chapter begins by presenting the theoretical framework for electricity demand estimation. Following this, we explore the practical aspects of the effect estimation using price elasticity. The chapter then provides a concise summary of previous meta-analyses and highlights the contributions of the thesis. This approach aims to introduce the reader to the topic, covering key findings and setting the stage for subsequent discussions and results.

2.1 Modelling Electricity Demand

The growth of electricity demand is expected to accelerate in 2024, particularly in emerging economies like India and China, according to the Agency (2024). Similarly, the energy use for electricity production is continuously increasing, due to various factors, including global warming (Asadoorian *et al.* 2006). Power generation emissions are projected to escalate in Asia, in contrast to the emphasis on renewables in the European Union and the United States. The shift in the price of electricity in the last few years stimulated renewed interest in the area of electricity demand estimation. Moreover, as emphasized by Zhou & Yang (2016), the focus on mitigating the environmental impact of electricity consumption extends beyond policy and renewable energy efforts. Behavioural factors influencing individual consumer patterns play an important role, too. Understanding household-level energy consumption can aid regulators with insights to incentivize energy-saving practices and hence reduce carbon emissions (Alberini *et al.* 2019b), formulate effective electricity policies (Kwon *et al.* 2016), plan infrastructure, and assess the efficiency of environmental taxes (Benavides *et al.* 2015).

Electricity is not consumed directly, but rather through the flow of services provided by electricity-using appliances (Reiss & White 2005). In an early exploration of electricity demand, Hausman (1979) proposed an approach that centres on two fundamental components: the technological design of appliances and their utilization. This methodology offers a distinct advantage by allowing differentiation between short-term and long-term effects. In the short-run, with the capital stock held constant, electricity demand is influenced by usage patterns, such as how often consumers activate lights or utilize air conditioning. In contrast, the long-term perspective involves consumers potentially opting to replace existing appliances with those requiring less energy input. Consequently, long-term energy demand becomes a trade-off consideration between operating and capital stock costs. As pointed out by McRae & Meeks (2016), households with better knowledge of their electricity expenditure are more likely to invest in energy-efficient improvements.

The very general empirical model of electricity demand, which is denoted by Q at time t, can be thus written as a function of electricity price PE, other economic factors X and the stock of electricity using equipment K_T:

$$Q_t = F(PE_t, X_t, K_t(PE_t, X_t)) \tag{2.1}$$

The three variables might have an independent or dependent impact on the electricity demand. Silk & Joutz (1997) further break down energy appliances into two segments. The first one consists of the daily demand for energy services: refrigeration, cleaning, lighting, charging and entertainment. The second type relates to seasonal weather patterns that might influence demand for heating and cooling services. In the early days of electricity modelling, Fisher & Kaysen (1962) proposed a two-stage model, firstly focusing solely on the price of electricity and subsequently including capital stock determinants. Due to the limited availability of capital stock data, the urbanization rate was used as a proxy for the stock appliances in the study. A similar model was then developed by Taylor (1975) with the conclusion that the data on stock appliances must improve in order to conduct reliable research. The number of studies controlling for stock appliances increased around 2000 (see e.g. Garcia-Cerrutti 2000; Filippini 2011; Fell *et al.* 2014), which might indicate an increase in the availability of such data.

2.2 Estimating the Relationship

In practice, majority of researchers use various models to estimate some functional form of the following equation:

$$lnQ_{i,t} = \alpha + \beta lnPE_{i,t} + \lambda lnQ_{i,t-1} + \delta X_{i,t} + \epsilon_{i,t} \tag{2.2}$$

where Q denotes the electricity consumption, PE is the electricity price and X is a vector of other independent variables that might capture the effect of temperature, substitute fuels, household or socioeconomic characteristics. In some papers (Chang & Chern 1981a; Eltony 2006; Wang *et al.* 2020), the lag of electricity consumption Q is also included to examine long-run adjustments. This suggests that electricity demand is influenced by its own usage in the preceding period, as households cannot make capital stock adjustments immediately (see e.q. Paul *et al.* 2009). In this equation, we add subscripts for individual observation i at time t, however, the estimated model can be applied analogically to cross-sectional or time-series data. An extensive discussion of the individual variables which are usually included in the model is found in Chapter 5.

We present two brief examples of electricity demand estimation. To explore elasticity variation both across and within the United States, Bernstein & Griffin (2006) estimate model with the following fixed-effects specification:

$$lnQ_{i,t} = \lambda lnQ_{i,t-1} + \alpha lnPE_{i,t} + \beta lnX_{i,t} + \gamma lnX_{i,t-1} + s_i + y_t + \epsilon_{i,t} \tag{2.3}$$

where $Q_{i,t}$ is the electricity demand of state i at time t, $Q_{i,t-1}$ is the lagged value of electricity demand, and $PE_{i,t}$ is the price elasticity of electricity. A set of other covariates (such as other fuel prices, population, income) assumed to affect electricity demand is denoted by $X_{i,t}$ and the lagged values of such characteristics are $X_{i,t-1}$. Time-invariant variations in electricity consumption across states are denoted by s_i. Similarly, y_t captures time-variant effects, which are common to all states. The error term is represented by $\epsilon_{i,t}$ as usual. Since the double-log form is employed, the short-run price elasticity estimate is thus simply α and the long-run elasticity can be obtained by dividing the α estimate by $(1 - \lambda)$.

Another rare, but interesting model employed is the Kalman filter, which is used in selected studies (Wang & Mogi 2017; Wakashiro 2019). This approach is applied to non-stationary data for estimating regressions in which the variable (price elasticity in our case) is assumed to have a time-varying effect. For example, Hanson (2002) provides a discussion on how to test whether the parameter is indeed time-varying. The baseline model for time-series data authors employ is:

$$lnQ_t = \theta + \alpha_t lnPE_t + \beta_t lnY_t + \epsilon_t \tag{2.4}$$

$$\begin{aligned} \epsilon_t &\sim iidN(0, \sigma_e^2) \\ \alpha_t = \alpha_{t-1} + \mu_t, \mu_t &\sim iidN(0, \sigma_{\mu 1}^2) \\ \beta_t = \beta_{t-1} + \nu_t, \nu_t &\sim iidN(0, \sigma_{\nu 1}^2) \end{aligned} \tag{2.5}$$

The initial step is to determine the starting position α_0 and β_0. This can be done using the maximum likelihood approach, with a specific procedure outlined in Durbin & Koopman (2012). Equation 2.4 is the measurement function and Equation 2.5 is called the transition equation, which follows a random walk process. This procedure allows the elasticities to vary throughout periods and such changes of elasticity estimates (α_t for price and β_t for income) are independent of each other. Authors can hence provide multiple elasticities or report an individual estimate (usually the final coefficient). In-depth descriptions can be found in the papers mentioned.

In the studies collected, authors estimate the relationship using price elasticity. It is a convenient unit-free measure for the electricity demand, which is expressed as:

$$\varepsilon = \frac{\frac{\triangle Q}{Q}}{\frac{\triangle PE}{PE}} \tag{2.6}$$

Where ε is the price elasticity, Q is the consumption and P is the price, with $\triangle$ representing the respective change in the variable. The coefficient describes the relative change in the consumption of electricity due to price changes, ceteris paribus. Because it is normalized, it measures how the electricity demand varies when its price changes by one per cent. In the double log specification in (2.2), the short-run price elasticity of electricity is directly estimated by coefficient β and the long-run is prevalently estimated by dividing β by $(1 - \lambda)$ to capture appliance adjustments. Alternatively, if used in a linear model, the short-run elasticity can be obtained by multiplying the β coefficient by the ratio of mean price to mean consumption. In this thesis, we break down the price elasticity of electricity into short-run, intermediate-run and long-run as there is a theoretical foundation (not limited to energy economics) that elasticity varies for these periods (Meher 2020; Otsuka 2023).

We utilize a meta-analytical approach to consolidate findings on electricity demand. This involves conducting a regression analysis using a collection of results gathered from the literature. Additionally, we seek to identify variables that account for the heterogeneity in reported price elasticities across different studies. There is a clear consensus that the price elasticity of electricity is negative (Fan & Hyndman 2011), in other words, electricity is an ordinary and not a Giffen good. However, the quantification of the elasticity whilst controlling for specific variables is problematic. Meta-analysis was introduced by Glass (1976), who stressed that there is a need to "find knowledge in information", referring to the ability to extract generalizations from primary studies. In this respect, despite numerous attempts to explain the determinants of electricity demands, some authors still argue that the topic lacks rigorous analysis which hinders the designing of government energy policies (Al-Faris 2002; Narayan *et al.* 2007; Al Irsyad *et al.* 2018). Moreover, meta-analysis is also used to test for publication bias (Card & Krueger 1995).

Meta-analysis is a technique that has been used extensively in energy economics. For example, Espey (1998) and Havranek & Kokes (2015) applied meta-regression analysis to understand gasoline demand, while Van der Kroon *et al.* (2013) examined whether there exists a systematic fuel switching behaviour from biomass fuels to cleaner alternatives. Labandeira *et al.* (2017) further conducted a meta-analysis on energy demand, covering gasoline, heating oil, diesel and more. Chai *et al.* (2018) used techniques of meta-analysis to estimate natural gas demand. These examples highlight how meta-analysis is a valuable tool in energy economics, helping researchers make sense of diverse studies and gain a comprehensive view of the field.

2.3 Previous Research

As mentioned, there are hundreds of studies examining the price elasticity of electricity, the first one conducted by Houthakker (1951), who analysed the residential electricity demand of provincial towns in Great Britain. The 10 most-cited studies in our dataset collected over 8500 citations, highlighting the importance of the topic. Some of the studies are also published in top economic journals: Shaffer (2020) in *American Economic Journal*; Wolak (2011), Ito (2014) and Jessoe & Rapson (2014) in *American Economic Review*; Hawkins (1978) and Reiss & White (2005) in *The Review of Economic Studies*.

Based on our knowledge, there exist 6 meta-analyses on the topic: Espey &

Espey (2004), followed by Horáček (2014), Labandeira *et al.* (2017), Zhu *et al.* (2018), Zabaloy & Viego (2022) and Fatima (2023). Espey & Espey (2004) conducted the first meta-analysis of 36 studies on price and income elasticities, collecting 123 short-run and 125 long-run estimates of price elasticity, with means of -0.35 and -0.85, respectively. Interestingly, the author decided to exclude positive estimates from the analysis due to the small sample size or insignificance. The paper was the first attempt to include household characteristics, estimation technique, demand model choices and period of analysis and to inspect whether these factors influence the price elasticity of electricity. The authors do not present a single general estimate of price elasticity.

Horáček (2014) used the energy database by Professor Carol Dahl as a basis for his estimation, ultimately utilizing 834 short-run, 1325 intermediate-run and 231 long-run estimates from 247 studies spanning from 1951 to 2014. Using the OLS estimation technique and mixed-effects models, the author concludes that the true price elasticity of electricity estimate is -0.06 for short-run, between -0.16 to -0.21 for intermediate-run and -0.43 for long-run. By coding for 26 variables, the paper explores the heterogeneity of estimates based on estimation technique, type of elasticity and year of publication. Notably, the author finds that residential demand is more price elastic (in absolute value) than industrial and commercial demand and that there is no time variation with respect to price elasticity.

With the intent of facilitating a sounder economic assessment of electricity price responsiveness, another study of energy demand products such as electricity, gas, diesel, gasoline and natural oil is undertaken by Labandeira *et al.* (2017). After eliminating 5% of outliers, the authors proceed with 917 short-run and 959 long-run estimates. They conclude that the short-run estimate for electricity is -0.126 and the long-run estimate is -0.365. Other energy products are also price inelastic according to the study. Furthermore, the study finds that elasticity diminishes over time, with earlier research indicating higher levels of responsiveness. As opposed to Horáček (2014), the paper poses that commercial electricity demand is more elastic than residential and industrial.

One of the meta-analyses of recent studies published from 1990 through 2017 (but covering data from 1950 to 2014) is conducted by Zhu *et al.* (2018). The authors focus on residential electricity demand and code for the functional form, aggregation of data, estimation technique, country and sample period in order to explain the heterogeneous nature of the estimates. They collect elasticities ranging from -0.948 to 0.610 in the short-run and -4.2 to 0.6 in the long-run.

One interesting finding is that developing countries have higher elasticity in the short-run. In terms of functional form, double-log model specification yields the most price elastic estimates in both the short and long term.

Zabaloy & Viego (2022) collect 82 short-run and 131 long-run estimates exclusively for the Carribean and Latin American countries. Moreover, the study is the first one to collect variables on the type of the study, whether it is a thesis, non-indexed journal or Scopus journal. The type of variables connected is otherwise in line with previous research, focusing on the type of data, stock appliances and estimation method. There is a publication bias present, however, the evidence of bias vanishes in studies published in indexed journals according to the authors. Therefore, the publication bias is attributed to studies with econometric shortcomings. The paper concludes that due to high heterogeneity, it is impossible to offer a unique price elasticity estimate for both the short-run and long-run. Lastly, Fatima (2023) attempts to examine the income and price elasticities of electricity in Asia. The study focuses on qualitatively explaining the heterogeneity, with time-series data, long-run estimates and journal impact factors significantly influencing the estimated effect.

Table 2.1: Meta-analyses on electricity demand

Year	Observations			Estimate	
	S-R	I-R	L-R	S-R	L-R
Espey & Espey (2004)	123	-	125	-0.350	-0.850
Horacek (2014)	834	1325	231	-0.060	-0.430
Labandeira et al. (2017)	966	-	1010	-0.126	-0.365
Zhu et al. (2018)	175	-	196	-0.228	-0.577
Zabaloy & Viego (2022)	82	-	131	-0.213*	-0.351*
Fatima (2023)	113	-	57	-0.104	-0.143
This study	1866	1842	813	-0.116	-0.303

Notes: The observations are divided into short-run (S-R), intermediate-run (I-R), and long-run (L-R). Estimate column presents either preferred estimate of price elasticity or average value if single estimate is not presented. *The authors provided multiple elasticity estimates with no clear preference, results of random-effects specification were chosen. For author's elasticity estimates, we chose the best practice estimates presented in Chapter 6.

Following the efforts of the studies aforementioned, we expand the dataset published by Professor Carol Dahl to include recently published studies. As mentioned, multiple variables might affect the price elasticity of electricity. It is hence difficult to calibrate how the inclusion of a particular variable affects the price elasticity. Compared to previous studies, this study takes advantage of a much greater dataset containing over 100 variables, which allows us to illuminate the potential heterogeneity of the studies, endogeneity and aggregation bias. We are also the first to deal with model uncertainty using model averaging techniques. Lastly, multiple robustness checks are conducted to increase the credibility of our results, which are included in the Appendix A.

While previous studies of Horáček (2014) and Zabaloy & Viego (2022) controlled for publication bias, they used solely linear meta-regression methods to explore the correlation between the estimate and its standard error. The authors thus did not consider the possibility that the relationship is non-linear or that such methods were not available at the moment. Advancements in the techniques used in meta-analysis allow us to also employ non-linear techniques to quantify publication bias, if present. Consequently, we conduct tests that relax the exogeneity assumption of the standard error and explore whether there exists empirical evidence that authors manipulate their findings in order to get their studies published. Also, this thesis is the first one to address the issue of p-hacking and endogeneity bias. In conclusion, while the thesis is admittedly not delving into a novel topic, its contribution is justifiable as the dataset collected is much more complex and techniques used in previous meta-analysis are either not extensive (Zabaloy & Viego 2022) or rendered obsolete (Horáček 2014).

Chapter 3

Data

This chapter outlines the procedure employed to gather data on price elasticity estimates, including any challenges or limitations encountered during the process. Following this, we present a preliminary analysis of the collected estimates.

3.1 Data Collection

Our research builds upon the dataset initially created by Carol Dahl for her 2011 survey (Dahl 2011), which is publicly accessible. The initial action was to filter out any estimates lacking a quantifiable metric of uncertainty, such as a standard error or t-statistic, reducing the dataset from 5333 to 1839 observations across 198 distinct studies. Moreover, we conducted a thorough examination of each paper to record additional variables, such as the total number of observations, any alterations made to the elasticity calculations, the various forms of elasticity assessed, and other details. Of the studies included in the original dataset, 28 of them with initially missing information were kindly sent to us by Professor Carol Dahl.

The initial step in refining the dataset involved constructing a Google Scholar search using the term "price elasticity of electricity demand." The search query yielded approximately 210 000 results. After selecting the first 500 studies not included in the original sample, the author went over individual abstracts and excluded 243 of them. This left us with 277 studies to examine (including "snowballing"). Subsequently, some of the studies were excluded during the paper review as the estimates had no uncertainty metric or the estimates were not of own price elasticity (but rather cross price elasticity or income elasticity). During the literature review, we also used the "snowballing" technique as recommended by Irsova *et al.* (2023b). This technique refers to examining cross-references included in primary studies and therefore increasing the set of potential papers that can be included. Throughout this process, we adhered to established guidelines for conducting meta-analyses, consulting papers by Hall & Rosenthal (1995), Stanley *et al.* (2013), and Havránek *et al.* (2020).

Our final dataset consists of 4521 estimates on the price elasticity of electricity obtained from 413 studies. An overview of all studies included and the PRISMA diagram of detailed steps in the data collection procedure can be found in Appendix B. The author admits that our list of studies included is not an exhaustive, albeit considerable, set of research on price elasticity of electricity demand. The search was concluded on December 15, 2023, and no studies were added beyond this date. The number of citations for every study included (based on Google Scholar) was updated on March 3, 2024. The dataset can be provided upon request.

To be included in the dataset, the studies had to cumulatively satisfy the following criteria:

- The paper must provide an estimate of own price of elasticity of electricity. Studies focusing on cross-price elasticity of electricity with other fuels or income elasticity were excluded.
- The study must report a form of uncertainty metric, preferably standard errors or confidence intervals that can be transformed into standard errors. Studies reporting p-values were included and labelled as the transformation into standard errors is subject to approximation. Standard errors are used in meta-analysis as weights and they are needed to quantify publication bias.

The majority of studies were published in a peer-reviewed journal. There are two main reasons for this decision. The published studies are easily accessible and we expect them, on average, to be of higher quality. Adopting this approach should not affect the integrity of our publication bias analysis, as noted by Rusnák *et al.* (2013). Nevertheless, the inclusion criterion was not strictly limited to published works; select studies from government agencies and working papers from economic departments of various institutions were also considered. For instance, contributions from the University of Gothenburg (Walfridson 1987) and the American Economic Association (Garbacz 1983a) were included. Therefore, there was a strong marked preference for peer-reviewed publications, but this did not preclude the consideration of other reliable sources.

3.2 Data Adujstments

One important variable we had to include for all studies was whether an appropriate transformation of price elasticity was used. There was an ex-ante expectation that some authors might express price elasticity as a transformation of the coefficient, for instance, by setting the elasticity to be $1/\beta$ with respect to (2.2) when estimating the inverse demand equation. Fortunately, the vast majority of studies used either coefficient directly as an elasticity measure (for double-log specifications) or the elasticity was calculated as a transformation that does not manipulate the relationship between standard error and the estimate (e.q. obtaining elasticity from the linear model by multiplying the ratio of average price and consumption). Some studies did not provide an explanation as to how the specific elasticity was computed from a regression and the author was not able to deduct such transformation. Such 20 observations are hence excluded from the tests.

Selected studies did report p-value instead of standard error. While this is generally not an issue as the standard error can be derived from the p-value, the problem arises when authors reported a p-value of 0 due to rounding ((Madlener 2011)). In this case, we labelled such studies and used a low p-value of 0.001 to calculate standard errors. Furthermore, some studies (Blázquez *et al.* 2013; Saha & Bhattacharya 2018; Frondel *et al.* 2019) did not report the standard error connected to the long-run elasticity estimate, which can be calculated based on the short-run coefficient and lagged coefficient on electricity consumption. In this case, we used the delta method to derive the missing standard error from the estimates in the original regression. This method helps with the approximation of transformation of functions and their asymptotical distribution using Taylor polynomials. For instance, Oehlert (1992) presents a more thorough discussion of this method. As a last step in the data adjustments, we applied a 1% winsorization to the data to mitigate any potential skewing in our results. While trimming could be considered given our ample dataset, it is deemed less favourable due to the greater loss of information it would entail. This is also emphasized by Irsova *et al.* (2023b), as the writers argue that removing such outliers should be the tool of last resort.

3.3 Preliminary Analysis

Prior to examining publication bias, calibration bias, and heterogeneity, this section outlines the summary statistics of the dataset compiled. In total, we collected 4521 estimates from 413 studies and coded for 109 variables. The oldest and newest paper in our dataset was published in 1951 and 2023, respectively. Only two studies were published before 1971 (Houthakker 1951; Fisher & Kaysen 1962). The publication dates therefore seem to be uniformly distributed for the period (1971-2023) with the median year of 1995. In Figure Figure 3.1, we present a boxplot of price elasticity for selected studies to emphasise the great variability of price elasticity. There is a dispersion of the estimated effects, however, there seems to be no systematic trend concerning the variance of reported elasticities. We also present boxplots for all studies in Appendix A.

The average number of estimates reported per study is between 10 and 11. Some studies are providing a single estimate of price elasticity (Fuss 1977; Fouquet 1995; Delfino 1995; Chaudhary *et al.* 1999). There are studies on the other end of the spectrum, too. McRae & Meeks (2016) provide 85 estimates of price elasticity of elasticity while Cao *et al.* (2023) and Walfridson (1987) produced 120 and 192 estimates, respectively. A large number of estimates per study can be attributed to multiple factors, to name a few: estimation for multiple countries, various models employed, gradual inclusion of control variables or estimation for subsets of periods examined in the study. To address the disparities in the volume of estimates across studies, we later apply a weighting method, utilizing the inverse of the number of estimates each study provides.

The shortened summary statistics on the variables can be found in Table 3.1. The mean value of price elasticity in our dataset is -0.395, stating that on average, if the price increases by one unit, the demand is to decrease by 39.5% (or vice versa). When considering the absolute values of elasticity, findings suggest higher elasticity for studies focused on periods before 1984 and shorter time frames. Consistent with established theories, we observe that short-run elasticities are less than those of the intermediate and long-run. A discernible pattern within our data is the rise in elasticity corresponding to increased data granularity; for instance, household-level data show an average elasticity of -0.523, in contrast to -0.289 at the country level. Consumers responding to marginal prices seem to exert higher elasticity compared to consumers of electricity reacting to the average price. Notably, there is a great

Figure 3.1: Variability of the estimated effect for a subset of studies

Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

difference in price responsiveness considering tariff structures. The observation that price elasticity peaks among customers with a decreasing tariff structure indicates a sensitivity to cost savings, as lower prices for higher usage levels encourage increased electricity consumption. While time-of-use tariffs induce a more limited response, consumers nevertheless adjust their behaviour across different time blocks. Peak times, typically evenings with higher rates, see less flexibility in usage patterns, while off-peak times, offering lower rates, invite a significant uptick in consumption as consumers take advantage of cheaper electricity. It is important to keep in mind that these observations are only based on simple averages and hence should be taken with reservations. A more in-depth discussion on the effects of individual variables is presented in Chapter 5.

The summary Table 3.1 concludes that there is little difference in the effect for relatively high prestigious journals compared to low prestigious ones. It should be noted that 210 estimates do not come from peer-reviewed journals. These estimates are mostly from books (Chern 1978; Donnelly 1984) or government studies (Verleger 1973; Matsui 1979; Wijemanne 1987). The dataset lacks a substantial number of unpublished studies, rendering any statements about the potential presence of publication bias based on summary statistics premature. A more in-depth exploration of this aspect is reserved for Chapter 4.

Figure 3.2 shows the distribution of the estimated coefficient from all the studies. We can see that the elasticities are negatively skewed, with the majority of estimates lying between -1 and 0. What might be surprising is the relatively high incidence of both positive and negative extreme values, as approximately 50 elasticities are larger than 1 and a similar number is below -3.

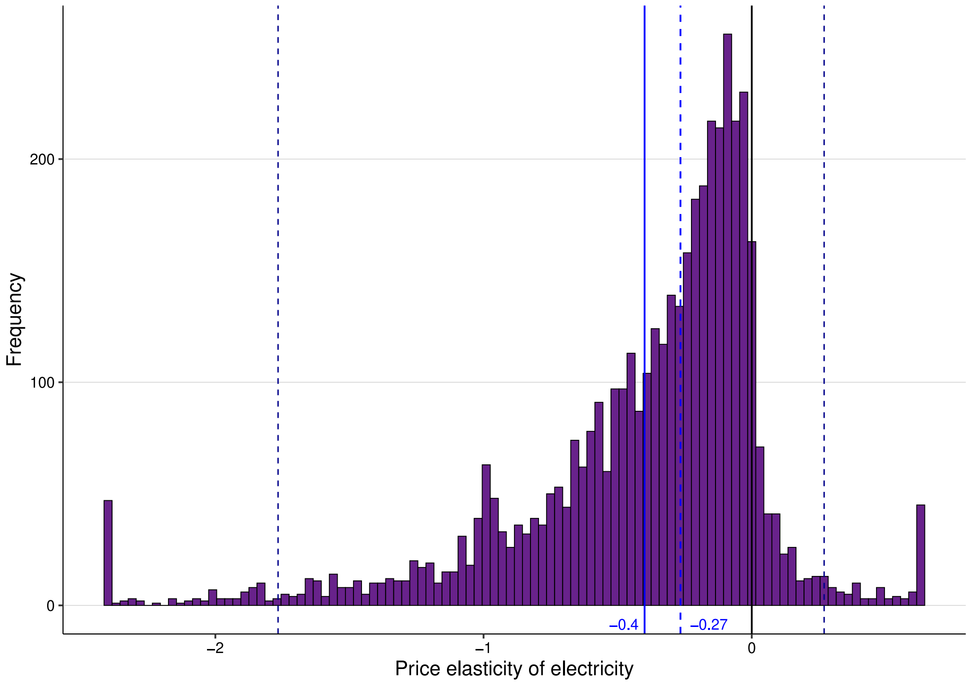
Furthermore, we inspected the nature of estimates based on various characteristics, namely elasticity period (run), type of elasticity, type of data used in the primary study and data aggregation. We present the results of four density kernels in the Appendix A in Figure A.11. We notice the difference between cross-sectional and both time-series and panel data. This intuitively makes sense, as time series and panel data both generally focus on longer periods. Cross-sectional data exert on average higher elasticity, in line with findings of Dahl (1993). We also examine the data aggregation, with the already observed result that estimates at the country level exhibit lower absolute values, while the relatively most elastic estimates emerge in disaggregated data. The effects of various models and estimation techniques are relatively similar.

Table 3.1: Statistics summary

Variable Name	Sample Mean	CI	Weighted Mean	WM CI	n
All Data	-0.395	(-1.342; 0.552)	-0.417	(-1.364; 0.530)	4521
Observations $\geq$ 608	-0.383	(-1.285; 0.519)	-0.430	(-1.332; 0.472)	2263
Observations $<$ 608	-0.408	(-1.396; 0.580)	-0.409	(-1.397; 0.579)	2258
t-statistic $\geq$ -2.68	-0.230	(-1.083; 0.623)	-0.235	(-1.088; 0.618)	2263
t-statistic$<$ -2.68	-0.561	(-1.484; 0.362)	-0.560	(-1.483; 0.363)	2258
Estimate: Short-run	-0.231	(-0.823; 0.361)	-0.247	(-0.839; 0.345)	1866
Estimate: Intermed.-run	-0.502	(-1.533; 0.529)	-0.495	(-1.526; 0.536)	1842
Estimate: Long-run	-0.532	(-1.700; 0.636)	-0.624	(-1.792; 0.544)	813
Type: Marshall	-0.398	(-1.327; 0.531)	-0.414	(-1.343; 0.515)	3326
Type: Hicks	-0.385	(-1.371; 0.601)	-0.420	(-1.406; 0.566)	1175
Type: other	-0.548	(-1.932; 0.836)	-0.465	(-1.849; 0.919)	20
Mid year $\geq$ 1984.5	-0.375	(-1.284; 0.534)	-0.429	(-1.338; 0.480)	2262
Mid year $<$ 1984.5	-0.417	(-1.393; 0.559)	-0.406	(-1.382; 0.570)	2259
Number of years $\geq$ 14.5	-0.322	(-1.186; 0.542)	-0.354	(-1.218; 0.510)	2262
Number of years $<$ 14.5	-0.470	(-1.472; 0.532)	-0.473	(-1.475; 0.529)	2259
USA	-0.395	(-1.293; 0.503)	-0.389	(-1.287; 0.509)	2151
Europe	-0.410	(-1.394; 0.574)	-0.432	(-1.416; 0.552)	833
Daylight hours $\geq$ 14.767	-0.386	(-1.333; 0.561)	-0.404	(-1.351; 0.543)	3100
Daylight hours $<$ 14.767	-0.416	(-1.355; 0.523)	-0.434	(-1.373; 0.505)	1436
Annual temp. $\geq$ 9.146	-0.356	(-1.234; 0.522)	-0.381	(-1.259; 0.497)	2305
Annual temp. $<$ 9.146	-0.437	(-1.439; 0.565)	-0.462	(-1.464; 0.540)	2231
Aggregation: Country	-0.289	(-1.116; 0.538)	-0.307	(-1.134; 0.520)	1224
Aggregation: Region	-0.397	(-1.238; 0.444)	-0.443	(-1.284; 0.398)	1082
Aggregation: City	-0.387	(-1.334; 0.560)	-0.393	(-1.340; 0.554)	654
Aggregation: Disaggr.	-0.523	(-1.597; 0.551)	-0.552	(-1.626; 0.522)	1099
Type: Residential	-0.355	(-1.337; 0.627)	-0.379	(-1.361; 0.603)	1710
Type: Commercial	-0.248	(-0.997; 0.501)	-0.309	(-1.058; 0.440)	884
Type: Industrial	-0.413	(-1.336; 0.510)	-0.417	(-1.340; 0.506)	2908
Demand: Peak	-0.256	(-1.030; 0.518)	-0.360	(-1.134; 0.414)	269
Demand: Mid-peak	-0.190	(-0.615; 0.235)	-0.186	(-0.611; 0.239)	108
Demand: Off-peak	-0.432	(-1.473; 0.609)	-0.713	(-1.754; 0.328)	83
Data: Panel	-0.389	(-1.318; 0.540)	-0.418	(-1.347; 0.511)	2290
Data: Time-series	-0.328	(-1.226; 0.570)	-0.343	(-1.241; 0.555)	1718
Data: Cross-section	-0.652	(-1.667; 0.363)	-0.649	(-1.664; 0.366)	513
Granularity: Yearly	-0.436	(-1.443; 0.571)	-0.455	(-1.462; 0.552)	3291
Granularity: Quarterly	-0.316	(-1.208; 0.576)	-0.393	(-1.285; 0.499)	126
Granularity: Monthly	-0.302	(-1.021; 0.417)	-0.289	(-1.008; 0.430)	948
Price: Average	-0.409	(-1.315; 0.497)	-0.420	(-1.326; 0.486)	2385
Price: Marginal	-0.430	(-1.435; 0.575)	-0.446	(-1.451; 0.559)	957
Price: Lump sum	-0.302	-	-0.302	-	1
Price: Shin	-0.151	(-0.408; 0.106)	-0.137	(-0.394; 0.120)	11
Tariff: Increasing	-0.350	(-1.079; 0.379)	-0.360	(-1.089; 0.369)	565
Tariff: Decreasing	-0.625	(-1.693; 0.443)	-0.474	(-1.542; 0.594)	462
Tariff: Flat	-0.495	(-1.491; 0.501)	-0.530	(-1.526; 0.466)	119
Tariff: TOU	-0.283	(-1.153; 0.587)	-0.375	(-1.245; 0.495)	559
Control: Demographics	-0.484	(-1.431; 0.463)	-0.516	(-1.463; 0.430)	1515
Control: Temperature	-0.366	(-1.219; 0.487)	-0.381	(-1.234; 0.472)	2203
Control: Stocks	-0.479	(-1.375; 0.417)	-0.465	(-1.361; 0.431)	821
Control: Fuels	-0.427	(-1.399; 0.545)	-0.458	(-1.430; 0.514)	1844
Control: Income	-0.414	(-1.372; 0.544)	-0.429	(-1.387; 0.529)	2543
Function: Linear	-0.329	(-1.160; 0.502)	-0.433	(-1.264; 0.398)	832
Function: Semi-log	-0.513	(-1.503; 0.477)	-0.344	(-1.334; 0.646)	231
Function: Double-log	-0.406	(-1.362; 0.550)	-0.412	(-1.368; 0.544)	2474
Impact Factor >= 0.061	-0.391	(-1.304; 0.522)	-0.409	(-1.322; 0.504)	2347
Impact Factor $<$ 0.061	-0.400	(-1.380; 0.580)	-0.425	(-1.405; 0.555)	2174

Notes: We present the main summary statistics table. Note that some of the variables are grouped but do not add up to 4521 as the effect of NA column was omitted. *TOU* is time-of-use tariff. Summary statistics for the whole dataset are presented in Appendix in Table A.1. All the subsequent tables and figures come from author's own computations, unless clearly stated otherwise.

Figure 3.2: Distribution of the effect



Notes: The figure depicts the distribution of elasticity effect for all estimates. Outliers are bunched to improve graphical interpretability but are included in all subsequent tests. The dashed vertical line denotes median and the solid vertical line is mean. The dark dashed lines represent 95% confidence interval.

Chapter 4

Publication and Endogeneity Bias

While part of the Chapter 3 provided a summary of published studies, its results will likely not provide an accurate overview of the body of research in an area if the literature itself reflects selection bias (De Long & Lang 1992). Such concern can be especially true when there exists a theoretical foundation presuming that the estimate of price elasticity of electricity should have a negative sign.

That is why this chapter focuses on publication bias. Firstly, we emphasize the importance of awareness regarding this topic and its implications. We consequently introduce graphical tests to examine publication bias as well as linear and non-linear tests of selection bias with multiple robustness checks. Endogeneity bias is also examined in the last section of this chapter. This chapter aims to assess the degree to which publication and endogeneity bias impact research outcomes related to the price elasticity of electricity.

4.1 Importance of Publication Bias

As already briefly introduced, publication bias (also known as selection bias) is a tendency of authors, but also editors and reviewers, to favour studies presenting a conclusion that aligns with a particular theory. The term was first used by Smith (1980a) in education research. According to Gerber *et al.* (2008), publication bias occurs when a particular study has a higher probability of being published based on the estimates produced, holding the methodology and data quality of the study constant. Moreover, in the paper on the systematic selection bias concerning minimum-wage research, Card & Krueger (1995) categorize three sources of publication selection bias in economics. First is the predilection of editors to publish papers consistent with the theory. Secondly, researchers can ex-post change model or estimation specification selection to achieve desired outcomes while renouncing controversial outcomes. This is also known as the "file drawer problem", extensively discussed by Rosenthal (1979). The main idea is that writers themselves decide to exclude particular studies with negative effects due to a lower probability of being published. Hence the studies "end up in a drawer", even if they are of high methodological quality (Gerber *et al.* 2008). The last source of publication bias is the general susceptibility to perceive statistically significant results as more favourable. The (conscious) manipulations of the estimation process, model selection or data adjustments are likely to distort the true underlying effect. This explicit and conscious manipulation of data to achieve significant outcomes is also referred to as p-hacking (Elliott *et al.* 2022). As a result, publication or selection bias can make empirical evidence of an effect seem more than 4 times larger (in absolute value) than it truly is (Stanley 2005). Without any preventive measures to account for the selection bias, the viability of empirical conclusions is likely to be questioned (Lehrer 2010).

In his work *Novum Organum*, Bacon (1620) remarked that *"it is the perpetual error of the human intellect to be more moved and excited by affirmatives than by negatives; whereas it ought properly to hold itself indifferently disposed towards both alike."* This quote on human nature is rather close to justifying the existence of publication bias, as both authors and reviewers might stance more critically towards "negatives", epitomized by positive elasticity estimates. Dickersin (1990) expressed concern about the absence of guidelines regarding when a study should be published or not. As a result, the authors may be - besides their own judgement - at the mercy of subjective assessments of editors and reviewers, who are the ones ultimately deciding which study gets published.

Trying to understand what qualities of a study make it publishable is of utmost importance, because to an extent, published and reviewed studies should represent the acme of our knowledge. While the concern regarding publication bias was initially noted by academics (Sterling 1959; Begg & Berlin 1988; Senn 2009), it became much more serious due to the prevalence of meta-analyses created for government policy setting or healthcare intervention studies (Dickersin 1997). Lately, there have been numerous meta-analyses discussing publication bias in economics (minimum-wage research by Stanley (2001); effect of democracy on economic growth by Doucouliagos & Ulubaşoğlu (2008); effect of student employment on education outcome by Kroupova *et al.* (2024)). For studies focusing on publication bias in energy economics, one might consult

Havranek & Kokes (2015), Havranek *et al.* (2018b) or Havranek *et al.* (2018a).

While publication bias might be immune to works solely summarizing previous research, meta-analysis can be employed to directly accommodate such biases (Stanley 2005). There exist methods that explore the relationship between the estimates and their standard errors and techniques exploring the distribution of t-statistics or p-values. Such methods will be discussed shortly. Two of the previous meta-analyses (Horáček 2014; Zabaloy & Viego 2022) found a statistically significant presence of publication bias, as will be further discussed later.

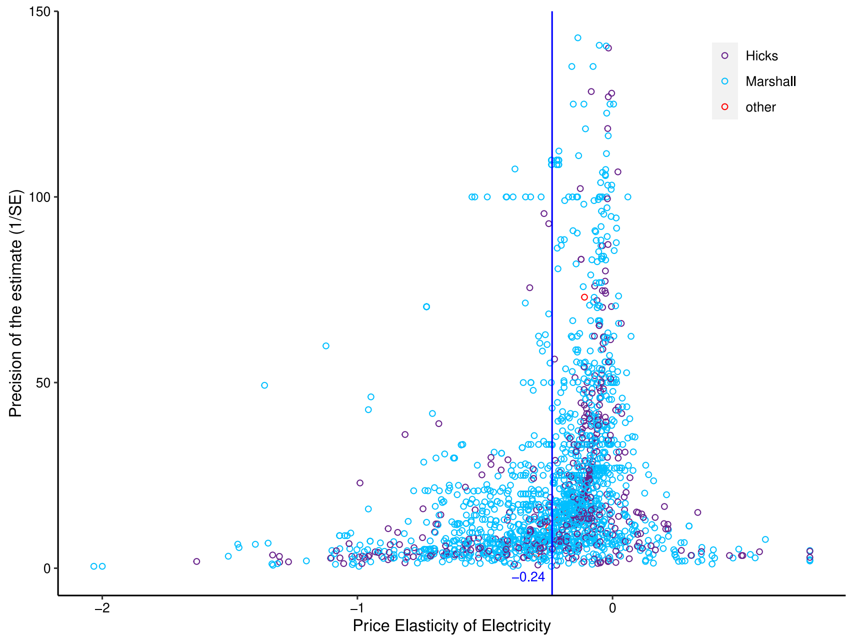
In the electricity demand setting, despite the consensus that the price elasticity is negative, some studies should produce either insignificant or positive estimates. Such results can arise due to measurement or data noise. If such studies are discarded, then on average, the true elasticity effect will be lower than the study average (in our case, -0.231 for the short-run, -0.502 and -0.532 for the intermediate-run and long-run, respectively). The exclusion of very large estimates of elasticity is unlikely, as there is hardly a framework describing what value should be the upper threshold (as seen in our study, there are estimates as low as -12, which might very well be subject to random noise exacerbating the elastic nature of the estimate). Hence, we cannot rely on the procedure that outliers from the end of both extremes are discarded with the underlying effect largely unaltered.

4.2 Testing for Publication Bias

4.2.1 Graphical Analysis

In practice, one of the most prevalent tests for publication bias is the funnel plot introduced by Egger *et al.* (1997), firstly applied to examine bias in healthcare research. The main idea is to plot the estimated effect on the horizontal axis against its precision measure (usually $1/SE$) on the vertical axis (Sterne & Harbord 2004). It is assumed that small sample studies (which are generally less precise) will be scattered close to the x-axis whilst more precise studies will create a funnel-like shape. Some very low effects combined with large standard errors might hence be positioned close to the origin. On the other hand, the most precise estimates should form a vertical line. A symmetrical funnel plot could indicate that the existence of publication bias is unlikely. While in the absence of symmetry, the graph could insinuate that publication

Figure 4.1: Funnel plot for short-run elasticity



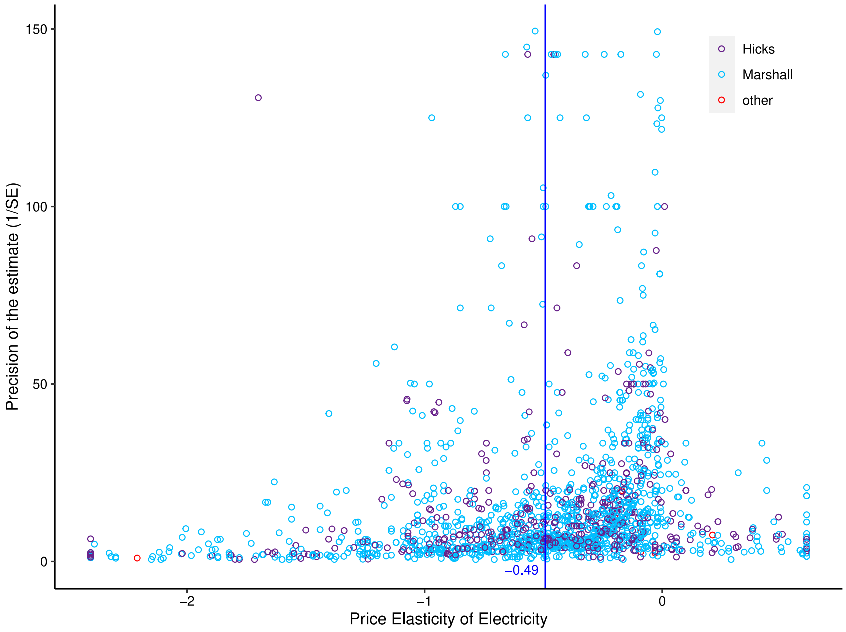
Notes: The figure displays a funnel plot for the short-run subsample. The plot should be symmetrical in the absence of publication bias. Estimates with higher precision form funnel-like pattern. A vertical blue line marks the average value of these estimates, with a total count of 1771 data points (due to the exclusion of extreme values).

bias is present. Furthermore, Egger *et al.* (1997) also provide different sources of funnel asymmetry rather than publication bias: true heterogeneity of effects, data irregularities or chance. The funnel plot asymmetry hence does not have to arise due to bias (Sterne *et al.* 2005). Specifically, the asymmetry in our data might be caused by vast heterogeneity, for instance, due to data aggregation, type of electricity tariff present etc., as we have seen in Chapter 3. Lastly, it should be kept in mind that graphical tests are assessed subjectively and each individual may provide different conclusions for a given graph.

We decide to conduct funnel plot analysis by first segmenting elasticities into short-run, intermediate-run and long-run. This is for two reasons, first, we are interested in whether there is a systematic difference in our initial publication bias analysis and second, a graph containing the full sample might be difficult to make sense of due to the high number of estimates. This approach will be used in the following tests, too. Furthermore, we trimmed outliers to improve readability. There outliers are, however, included in all subsequent tests and calculations.

Figure 4.1 presents a funnel graph concerning short-run elasticity, we can indeed see that the graph structure resembles a slightly skewed funnel. One finding is that the most precise estimates lie in the interval between a very

Figure 4.2: Funnel plot for intermediate-run elasticity



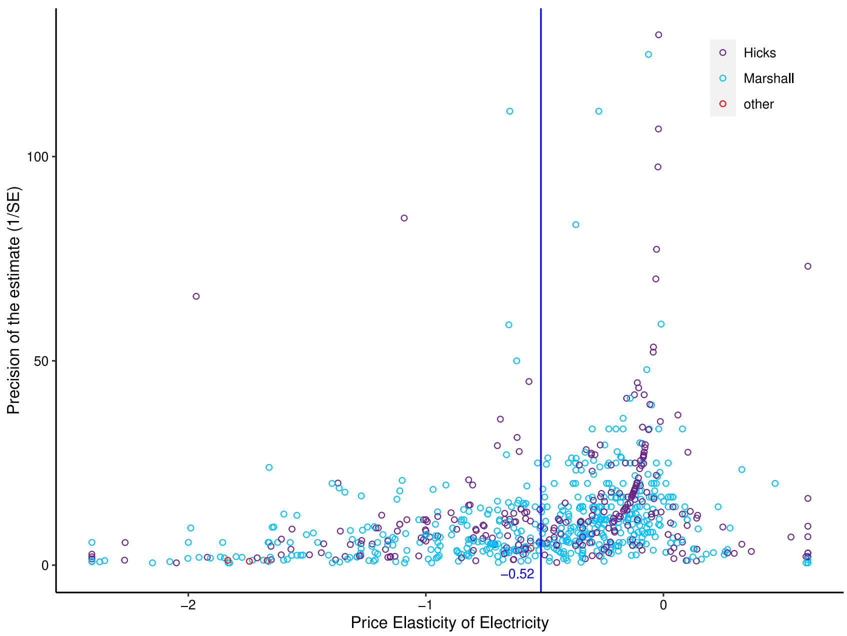
Notes: The figure displays a funnel plot for the intermediate-run subsample. The plot should be symmetrical in the absence of publication bias. Estimates with higher precision form funnel-like pattern. A vertical blue line marks the average value of these estimates, with a total count of 1671 data points (due to the exclusion of extreme values).

slight negative effect (approximately -0.1) and zero effect. Furthermore, we see relatively few precise positive estimates and negative estimates are on average more precise, with the negative side of the funnel being much more dense. As the most precise estimates should be scattered around the true effect, this implies that the true elasticity is much lower than our sample average (-0.231).

Figure 4.2 conveys a different message for the intermediate-run effects. Interestingly, the most precise estimates lie within the elasticity of -0.5 and -0.05. Initially, the plot contained highly precise estimates around the value of -1 from a study by Fisher & Kaysen (1962), who expressed strong reservations about the availability of data and hence the reliability of results. As a consequence, we decided to omit this study from the test in the following chapters. The funnel plot with the study included can be found in Appendix A in Figure A.12. The overall shape does not resemble a funnel and the lower number of positive estimates is precise, compared to the short-run funnel. There is no way to identify a possible true elasticity effect. The graph appears notably asymmetric, with the bulk of the observations positioned to the left of the most precise estimates, indicating a trend where positive estimates generally exhibit less precision. One reason why this particular funnel plot should be taken with slight reservation is the lack of general consensus on the definition of intermediate-run, as will be discussed in Chapter 5. The asymmetry is more pronounced in the long-run funnel plot. Again, assuming the most precise estimates lie around the value of -0.05, the right part of the funnel graph is basically absent. This might indicate that authors prefer to avoid presenting significant positive estimates. To conclude, while short-term and especially long-term funnel plots exhibit considerable asymmetry, it is too soon to conclude whether publication bias is present. One important point is that the most precise estimates are not similar to the underlying sample mean elasticity. Therefore, we will conduct various tests to either confirm or refute the hypothesis that publication bias is present.

Lastly, note that the dataset consists of different types of elasticities, which might not be directly comparable. In the following tests where the Marshallian and Hicksian elasticities are not estimated separately, we transformed the Hicksian elasticities into Marshallian elasticities using the income elasticity, which is accompanied by Hicksian elasticity estimates in individual studies. We used the delta method to approximate the standard errors of these newly transformed effects. Moreover, we omitted 20 observations for which we have no indication of the type of elasticity estimated.

Figure 4.3: Funnel plot for long-run elasticity



Notes: The figure displays a funnel plot for the short-run subsample. The plot should be symmetrical in the absence of publication bias. Estimates with higher precision form funnel-like pattern. A vertical blue line marks the average value of these estimates, with a total count of 786 data points (due to the exclusion of extreme values).

4.2.2 Linear Tests for Publication Bias

While funnel plot assists with understanding the nature of our data (Sterne *et al.* 2005), regression-based tests are usually employed to quantify publication bias. The basis for modelling publication selection is the Funnel Asymmetry Test (FAT) examining the relationship between the estimate (price elasticity of electricity) and its uncertainty metric (standard error of the estimate) (Card & Krueger 1995; Stanley 2005):

$$PE_{ij} = \alpha + \beta * SE(PE_{ij}) + \epsilon_{ij} \tag{4.1}$$

where PE_{ij} is the $i-th$ estimate of price elasticity collected from $j-th$ study, while $SE(PE_{ij})$ denotes the estimate's standard error. ϵ is the (heteroskedastic) error term. As pointed out by Havranek & Irsova (2017), α (*effect beyond bias*) represents the true effect independent of the standard error. On the contrary, β (*publication bias*) quantifies publication selection bias in terms of significance, direction and magnitude.

We provide multiple model specifications applied to the full data sample, which are in line with methods used in published meta-analyses (Zigraiova *et al.* 2021; Kroupova *et al.* 2024). Results for selected subsamples are presented in Appendix A. Firstly, we deal with the probable heteroskedasticity of the standard error by clustering at the study level. Therefore, we acknowledge the correlation among estimates coming from a particular study while assuming independence across individual papers. However, because the clusters are not of the same size (dependent on the number of observations per study), we also construct a 95% confidence interval of the wild bootstrap, as recommended by MacKinnon & Webb (2017). Moreover, at least at this stage, we assume that respective models are exogenous. Firstly, we use the ordinary least square (OLS) estimation technique. Secondly, we add a study-level fixed effect (FE) and then we follow by accounting for between-study variation (BE). Random-effect (RE) specification is also included to assign weights for both within-study and between-study variation, as discussed in Bom & Rachinger (2019).

Furthermore, we decide to apply two weighting schemes. Firstly, to consider considerable discrepancy in the reported estimates per study (as discussed in Chapter 3), we weigh the estimated elasticities by the inverse of the number of estimates obtained per each study (SW). This way, each study has the same impact on the estimation. Generally, a high constant (in absolute value) of the study-level weighting scheme could insinuate that publication bias is driven by only a fraction of the studies included. Secondly, we use the precision of the estimate as a weight (PW), as described in Stanley (2005). After dividing the equation by standard error, we obtain:

$$\frac{PE_{ij}}{SE(PE_{ij})} = \frac{\alpha}{SE(PE_{ij})} + \beta + \varepsilon_{ij} \tag{4.2}$$

Note that the left-hand side of Equation 4.2 is a t-statistic of the $i-th$ estimate from $j-th$ study. Moreover, β now represents the price elasticity corrected for publication bias, whereas α denotes the magnitude and direction of publication bias. The equation is also called the precision asymmetry test (PET). This procedure helps to deal with heteroskedasticity of standard error by assigning more weight to more precise estimates. The results are presented in Table 4.1.

We find significant evidence of publication bias for all specifications and throughout all elasticity periods. The random-effects specification yields the highest corrected elasticities, however, slight reservations for the RE results are in order. There are some studies that report only a single estimate of price elasticity, which can alter the within-study variation and hence the results. Overall, the short-run elasticity corrected for publication bias varies from -0.046 to -0.161. It should be not surprising that the magnitude of publication bias exceeds the underlying true effects (Stanley *et al.* 2008), as is the case in our estimation, too. While the estimates are relatively similar for most of the models, the study and especially the precision weighting technique report much lower elasticity estimates (and the highest magnitude of bias). The results of the fixed-effects model posit that on average, an increase in the price of electricity by 1% lowers the demand by 0.15% for the short-run, with the decrease of 0.38% and 0.43% for intermediate and long-run, respectively. This is a considerable decrease from the sample averages presented in Chapter 3.

The results are on average (apart from long-run elasticity and the precision specifications) of higher magnitude compared to the study by Horáček (2014), who finds the respective true effects to be -0.065, -0.210 and -0.431. For further comparison, Zabaloy & Viego (2022) reports true short-run effect of -0.087 and long-run elasticity of -0.154. However, these estimates might not be directly comparable to ours as the paper focuses solely on Latin America and the Caribbean.

Table 4.1: Elasticity results segmented by period

	OLS	**FE**	**BE**	**RE**	**SW**	**PW**
Short-Run Elasticity						
PB	-0.766^{***}	-0.751^{***}	-1.209^{***}	-0.794^{***}	-0.979^{***}	-2.854^{***}
PB SE	(0.096)	(0.035)	(0.096)	(0.033)	(0.142)	(0.212)
Boot. CI	[-0.959; -0.580]			[-0.979; -0.610]	[-1.178; -0.572]	[-3.255; -2.377]
EBB	-0.147^{***}	-0.149^{***}	-0.123^{***}	-0.161^{***}	-0.077^{***}	-0.046^{***}
EBB SE	(0.010)	(0.007)	(0.019)	(0.015)	(0.014)	(0.009)
Boot. CI	[-0.167; -0.128]			[-0.182; -0.136]	[-0.115; -0.053]	[-0.068; -0.030]
Total observations = 1846						
Intermediate-Run Elasticity						
PB	-0.734^{***}	-0.630^{***}	-0.947^{***}	-0.658^{***}	$-0,998^{***}$	-3.148^{***}
PB SE	(0.087)	(0.046)	(0.141)	(0.044)	(0.117)	(0.842)
Boot. CI	[-0.921; -0.595]			[-0.838; -0.494]	[-1.250; -0.781]	[-4.332; -1.207]
EBB	-0.359^{***}	-0.381^{***}	-0.345^{***}	-0.390^{***}	-0.386^{***}	-0.153^{***}
EBB SE	(0.017)	(0.014)	(0.038)	(0.027)	(0.030)	(0.053)
Boot. CI	[-0.386; -0.314]			[-0.423; -0.347]	[-0.446; -0.326]	[-0.264; -0.083]
Total observations = 1723						
Long-Run Elasticity						
PB	-0.715^{***}	-0.616^{***}	-0.617^{***}	-0.621^{***}	-0.117	-3.468^{***}
PB SE	(0.100)	(0.056)	(0.184)	(0.054)	(0.267)	(0.340)
Boot. CI	[-0.931; -0.540]			[-0.840; -0.345]	[-1; 0.233]	[-4.021; -2.254]
EBB	-0.400^{***}	-0.429^{***}	-0.529^{***}	-0.522^{***}	-0.255^{***}	-0.062^{*}
EBB SE	(0.026)	(0.023)	(0.085)	(0.054)	(0.033)	(0.032)
Boot. CI	[-0.448; -0.357]			[-0.601; -0.437]	[-0.310; -0.157]	[-0.180; -0.009]
Total observations = 813						

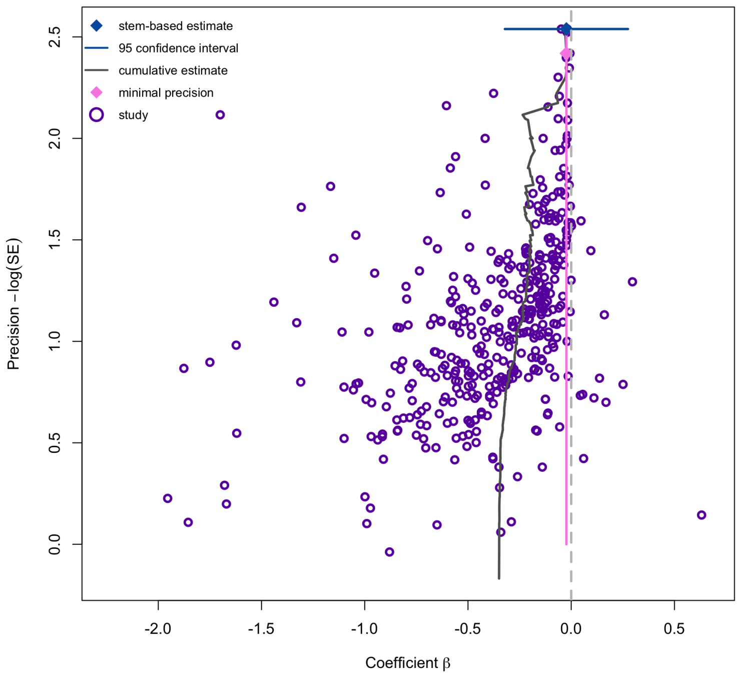
Notes: This table presents the results of publication bias and effect beyond bias across different time horizons: short-run, intermediate-run, and long-run. Standard errors are presented in parentheses. SW = Study weighted, PW = Precision weighted, PB = Publication Bias, EBB = Effect Beyond Bias, SE = Standard error, Boot. CI = Bootstrapped confidence interval (n=1000). Asterisks denote significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

4.2.3 Non-linear Tests for Publication Bias

While the previous linear methods are usually employed in meta-analyses, they rely on one crucial assumption - that the relationship between the price elasticity and its standard error is linear, which might be erroneous. Bom & Rachinger (2019) suggest that Equation 4.1 fails to account for the lower likelihood of selection bias in very precise estimates. Linear methods, while providing important initial insight into publication bias, could potentially be imprecise and ultimately lead to either upward or downward bias of the effect. The methods applied in this section assume that the most precise estimates will probably not suffer from publication selection, but they proceed in a different manner to deal with the non-linearity of the relationship between price elasticity and its standard error. The subsequent tests are also used in (among others) published meta-analyses conducted by Cazachevici *et al.* (2020) and Havranek *et al.* (2022).

The first method we employ is Top10 proposed by Stanley & Doucouliagos (2010). The idea of Stanley's method is rather simple, the authors believe that the selection bias among more precise estimates is lower than their less precise counterparts. Specifically, if there is a strong effect with simultaneous low standard error, there is no need to modify the results to achieve statistical significance. That is why the Top10 method focuses on the spike in the funnel plot. The idea that most precise estimates should exert very limited selection bias is conveyed in the stem-based method by Furukawa (2019), too. However, while Top10 sets an exogenous cutoff at 10%, the stem-based method calculates the preferred number of studies to be included by minimizing the mean squared error of the estimates. Hence, the approach tries to find the most precise studies, which are represented by the stem of the funnel plot in Figure 4.4 while filtering out those that are statistically insignificant or lack precision. For the category of intermediate elasticity specifically, the stem-based method yields a significant corrected effect of -0.446, marking the largest relative price elasticity among the non-linear methods.

Figure 4.4: Stem-based method



Notes: The illustration portrays the "true effect" estimate derived from the Stem-based method by Furukawa (2019). A blue diamond marks the estimated true effect (which is rather low and insignificant), while a blue line denotes its 95% confidence interval. Estimates across different precision levels are shown by the dark grey line, and violet circles represent individual study mean elasticities. A logarithmic scale was applied to minimize discrepancies among standard error values.

Next method used is the Weighted Average of Adequately Powered (WAAP) introduced by Ioannidis *et al.* (2017). The authors suggest using weighted least square technique, but only for limited amount of estimates which are "adequately powered". Such power is needed to detect effect if it is truly present. In practice, a price elasticity estimate is adequately powered if the estimated effect divided by 2.8 is still larger than its standard error. Hence, the criteria is more strict than the usual 5% statistical significance level (1.96). In the estimation procedure, one-third to one-half of the estimates (712 for short-run, 783 for intermediate-run and 346 for long-run) satisfy such condition.

Table 4.2: Non-linear tests for publication bias

Effect beyond bias *(Short-run)*			
WAAP	-0.127*** (0.006)	-0.123*** (0.006)	**Selection model**
Top10	-0.104*** (0.009)	-0.107*** (0.021)	**Hierarchical Bayes**
Stem-based method	-0.08 (0.011)	-0.046*** (0.003)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-1.262*** (0.142)	-2.854*** (0.227)	**Endogenous kink**
Number of observations = 1846			
Effect beyond bias *(Intermediate-run)*			
WAAP	-0.335*** (0.013)	-0.337*** (0.014)	**Selection model**
Top10	-0.245*** (0.022)	-0.341*** (0.035)	**Hierarchical Bayes**
Stem-based method	-0.446*** (0.011)	-0.153*** (0.009)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-0.940*** (0.146)	-3.148*** (0.484)	**Endogenous kink**
Number of observations = 1723			
Effect beyond bias *(Long-run)*			
WAAP	-0.320*** (0.020)	-0.345*** (0.030)	**Selection model**
Top10	-0.241*** (0.038)	-0.388*** (0.104)	**Hierarchical Bayes**
Stem-based method	0.002 (0.057)	-0.062*** (0.008)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-0.999*** (0.155)	-3.468*** (0.295)	**Endogenous kink**
Number of observations = 813			

Notes: Results of the three specifications of price elasticities using six non-linear methods. We also include the publication bias for Hierarchical Bayes and Endogenous kink methods. WAAP = Weighted Average of the Adequately Powered (n=712 for S-R, 783 for I-R, 346 for L-R). Top10 = Top10 Method (n=187 for S-R, 173 for I-R, 82 for L-R). Standard errors are included in the parentheses. Asterisks denote significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Consequently, we employ Selection Model by Andrews & Kasy (2019). This method proposes a sophisticated approach to correcting for publication bias. This model introduces the concept of 'conditional publication probability', which quantifies the likelihood of a study's findings being published given the results it reports. This approach recognizes that publication bias can stem from the inclination towards statistically significant results, impacting the visibility of certain studies.

The Endogenous Kink technique (Bom & Rachinger 2019), on the other hand, is built on the premise that more precise estimates are less influenced by publication bias. It aims to isolate these precise estimates for a clearer computation of the average effect. The method achieves this by determining a cutoff value through a piece-wise linear meta-regression of estimates against their standard errors, which effectively segments the most precise estimates from those potentially distorted by publication bias. The 'kink', or the point of intersection, marks the transition between these segments.

Lastly, we employ the Hierarchical Bayes model developed by Allenby & Rossi (2006). The method uses Bayesian inference to aggregate individual-level data with variability across studies. This model assigns weights to individual estimates by assessing variation within each study and then synthesizes these estimates at a higher, study-wide level.

Based on the results of non-linear tests presented in Table A.7, we confirm our assumption that publication bias is present and is significant in magnitude. Furthermore, the corrected effect (if we take the median values reported) is approximately -0.11, -0.34 and -0.33 for the short, intermediate and long-term, respectively. For the short-run, the sample elasticity mean (-0.231) is more than twice as large as the elasticity estimate corrected for publication bias. The results are slightly surprising, as one would expect the long-run elasticity to be the largest in absolute value. Moreover, the stem-based method provides insignificant corrected elasticities for the short-run and long-run. However, we have noticed throughout our analysis that intermediate and long-term effects are similar in magnitude, which might indicate that some authors use different guidelines in assigning the elasticity period.

4.2.4 Relaxing the Endogeneity Assumption

So far, we have assumed that the relationship between the effects and their standard error is exogenous. For this subsection, we will allow for simultaneous determination of the price elasticity estimate and its standard error. The endogeneity of standard error has multiple sources. For instance, deliberate adjustments to the standard error have been investigated by Pütz & Bruns (2021). Additionally, factors like measurement inaccuracies in the standard error or specific methodological approaches could simultaneously impact both the standard error and the unobserved error term.

Initially, we apply Instrumental Variable (IV) regression and p-uniform* method by van Aert & van Assen (2021). The results of the two tests are presented in Table 4.3. We tried various instruments to account for the endogeneity, including the logarithmic transformation of the number of observations, the inverse of the number of observations and the square root of the number of observations. The last instrument performed the best. Furthermore, this instrument diminishes the source of endogeneity Havranek *et al.* (2022) while maintaining a linear relationship with the standard error. The result of the regression suggests that publication is present even after controlling for endogeneity. The corrected estimate is -0.116 for the whole sample. We present results for short and long-run in the Appendix in Table A.8.

Table 4.3: Tests accounting for potential endogeneity

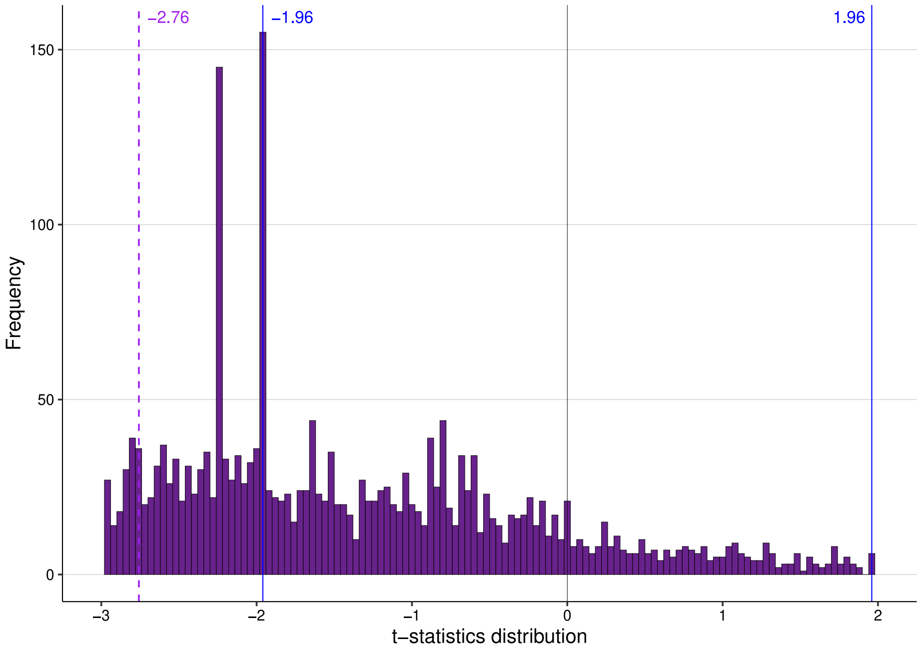
	IV Regression	**p-uniform***
Publication Bias	-1.640^{***}	L=46.03
Standard Error	(0.293)	(p<0.001)
Effect Beyond Bias	-0.116^{**}	-0.174^{***}
Standard Error	(0.054)	(0.029)

Notes: IV = Instrumental Variable, the instrument is the inverse of the square root of number of observations. F-test statistic for the instrument is 40.22. The standard errors presented in the parenthesis are clustered at the study level. P-uniform* method developed by van Aert & van Assen (2021)) uses maximum likelihood estimation. Asterisks denote significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The p-uniform* method, as discussed in detail in van Aert & van Assen (2021), is based on the premise that p-values should uniformly distribute around the true effect size when evaluating whether the estimated coefficient matches the actual effect value. Publication bias can skew this distribution by causing a scarcity of higher p-values and an excess of those just under the standard significance threshold of 0.05. The results indicate a significant presence of publication bias as well. The outcome of the tests is in line with our current analysis, showing that elasticity estimates corrected for publication bias are significantly lower than their sample counterparts.

We will now proceed with tests examining p-hacking. While there is no clear distinction between publication bias and p-hacking, the latter is usually defined by conscious or unconscious manipulation of data or methodology until statistical significance (more favourable p-values) is achieved (Irsova *et al.* 2023b). This can be done by further collection of selective data points, manipulating estimation procedure or excluding certain subsamples to enhance significance (Brodeur *et al.* 2023).

Figure 4.5: Distribution of the t-statistic for a restricted sample



Notes: The figure depicts the distribution of elasticity t-statistics for all estimates. Outliers are hidden to improve readability but are included in all tests. The dashed purple line denotes the mean value of the restricted subsample ($|t| < 10$). The blue solid lines represent critical values -1.96 and 1.96. Notice notable discontinuity right around the -1.96 critical value. The other peak occurs at a value of -2.24.

Firstly, observing the t-statistics distribution in Figure 4.5, we notice a notable spike right below the critical value of -1.96. Almost 200 estimates lie in the interval $[-1.99, -1.96]$. After reducing the width of the interval to 0.01, still over 130 observations were located right below the negative critical value. This finding is quite striking. Also, there seems to be a slight jump around the positive 1.96 critical value. This analysis was performed for the restricted sample, but the distribution for the wider sample is present in Appendix A.

We proceed with the inspection of publication bias with caliper tests proposed by Gerber *et al.* (2008). The idea of the test concerns the distribution of t-statistics of individual estimates as opposed to the relationship between the effects and their standard errors, as we have seen previously. Specifically, the focus is on critical values, such as 1.645 or 1.96, which represent the 10% and 5% significant levels, respectively. The test compares the number of observations in small intervals around critical values and measures the asymmetry. This possible discontinuity (for example, a rise in t-statistics frequency right above the 5% significance threshold) is an unambiguous sign of bias (Gerber *et al.* 2008).

Table 4.4: Caliper test for publication bias

Threshold	-1.96	-1.645	1.96
Caliper width 0.05			
Estimate	-0.114^{***}	-0.338^{***}	0.172^{**}
Standard Error	(0.015)	(0.034)	(0.084)
Observations	77	78	5
Caliper width 0.1			
Estimate	-0.155^{***}	-0.335^{***}	0.162^{**}
Standard Error	(0.014)	(0.026)	(0.067)
Observations	142	133	9
Caliper width 0.2			
Estimate	-0.206^{***}	-0.314^{***}	0.196^{***}
Standard Error	(0.012)	(0.018)	(0.049)
Observations	265	245	21
Caliper width 0.3			
Estimate	-0.281^{***}	-0.335^{***}	0.147^{***}
Standard Error	(0.009)	(0.015)	(0.035)
Observations	514	346	36
Caliper width 0.4			
Estimate	-0.280^{***}	-0.392^{***}	0.162^{***}
Standard Error	(0.008)	(0.009)	(0.031)
Observations	659	610	49
Caliper width 0.5			
Estimate	-0.285^{***}	-0.390^{***}	0.169^{***}
Standard Error	(0.007)	(0.008)	(0.028)
Observations	795	746	60

Notes: Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Caliper widths are chosen to represent varying precision levels around the thresholds.

The overall perspective is further substantiated by the caliper tests in Table 4.4. Focusing on the critical value of -1.96, approximately 61% of observations lie below the negative threshold (and hence are significant). This original coefficient is obtained by subtracting 0.5. The asymmetry becomes more even pronounced as the caliper width increases. Moreover, the asymmetry around the critical value of -1.645 is of higher magnitude, with over 80% of estimates passing the 10% significance level. Similarly, there is significant asymmetry for the positive critical value of 1.96, too. However, we should feel slightly reserved about this result as the number of observations included in the tests for positive threshold is rather low. Lastly, Gerber *et al.* (2008) state that even for distributions centred away from the critical value (as in our case, the t-statistic mean is -2.76), the statistical test is not affected.

We conclude the analysis of publication bias with two further p-hacking tests. We begin with the methodology of Elliott *et al.* (2022), which scrutinizes the distribution of p-values, known as the p-curve. Under the null hypothesis of

Table 4.5: P-hacking tests

A. P-hacking test by Elliott et al. (2022)

	Test for non-increasing of the p-curve	**Test for monotonicity and bounds**
P-value	0.000	0.000
Observations ($p \leq 0.1$)	3373	
Total observations	4521	

B. MAIVE estimator by Irsova et al. (2023)

	MAIVE coefficient	**F-test**
Coefficient	0.605*	0.687
Standard Error	(0.353)	

Notes: Panel A details the outcomes of the examination into p-hacking, implemented by Elliott *et al.* (2022), which includes assessments for the constancy of distribution tails and for monotonic and bounded p-curves. In Panel B, the results derived from the implementation of the robust spurious precision approach, utilizing the MAIVE estimator developed by Irsova *et al.* (2023a), are displayed. The F-test is indicative of the strength of the instruments used in the initial stage of the IV estimation. For the MAIVE estimations, cluster-robust standard errors have been utilized, which are denoted in parentheses. Asterisks denote significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

no true effect, we expect only 5% of p-values to be less than 0.05. A significant skew to the right in the p-curve distribution typically signifies genuine effects, while a leftward skew suggests the presence of p-hacking. Christensen (2018) provides a detailed discourse on the concept of p-curves. Elliott *et al.* (2022) propose that, in the absence of p-hacking, p-curves derived from t-statistics should demonstrate monotonicity, with their magnitude and derivative being constrained by specific exponential upper bounds — the exponential bounds are contingent on the p-test's critical value and the nature of the t-statistic (one-tailed or two-tailed). Generally, these tests require a substantial sample size to affirm the robustness and credibility, which should not be an issue with respect to our dataset. Panel A in Table 4.5 presents the results. We find that for both tests, the null hypothesis of no p-hacking is rejected at any reasonable significance level. We also conducted the test concerning observations below $p \leq 0.05$ with the same null results, as in line with visual inspection of Figure 4.5. This result is underscored by a consistently null p-value, which holds true across various data subsets characterized by granularity and elasticity type, indicating a pervasive trend of p-hacking.

Finally, we report on the results of Meta-Analysis Instrumental Variable Estimator (MAIVE) introduced by Irsova *et al.* (2023a). The starting point for this technique is the Egger regression, which estimates the relationship between the estimated effect and the quadratic standard error. The problem, according to the authors, lies in the endogeneity of standard error due to p-hacking. As a remedy, the authors propose instrumenting the standard error with the inverse of sample size, which should be correlated with the standard error but have no effect on the estimated coefficient. The advantage of using sample size is hence its independence of measurement error, estimation procedure or changing variables (Opatrny *et al.* 2023). In the first step, this instrumental variable's fitting is assessed using F-test. Unfortunately, the outcome based on the F-test indicates that the employed instrument is rather weak. Nevertheless, the MAIVE coefficient is 0.605 as presented in Table 4.5.

In summary, the various analytical tests consistently indicate a significant presence of publication bias and specifically p-hacking within the data. After adjusting for this bias, the elasticities show some variation. Typically, the short-run elasticity, once corrected, hovers around -0.1 across most methods, while the estimates for intermediate and long-run effects fluctuate from -0.33 to -0.38. These figures are generally larger (apart from the long-run estimates) than those reported by Horáček (2014), yet on average, remain lower in magnitude than the outcomes documented by Zhu *et al.* (2018).

4.3 Endogeneity Bias

One of the potential issues when estimating electricity demand is the simultaneous determination (endogeneity) of electricity consumption and price (Paul *et al.* 2009) or energy appliances system choice and price (Bernard *et al.* 1996). Notably, this issue arises under variable electricity tariffs. Another intuitive reason for endogeneity is the omitted variable bias. An illustrative example is the estimation of electricity on a cold winter day, marked by heightened reliance on heating appliances. Without controlling for such additional determinants of demand, the estimates collected will most likely be biased.

To provide one particular example, consider a customer facing the increasing block structure in Figure 4.6. Consumers may opt to use just enough energy to stay within a lower-priced tier, halting usage just before reaching the threshold that triggers a higher price, as depicted by consuming quantity $Q(P)$. This behaviour pattern is known as bunching and has been the subject of numerous studies (Shaffer 2020; Lanot & Vesterberg 2021). As a consequence, it becomes difficult to isolate the pure effect of price changes on consumption behaviour. Bunching would be expected if consumers were accurately optimizing against the marginal price, but the lack of bunching indicates potential misunderstandings or sub-optimal responses to the price structure, as described

Figure 4.6: Bunching at the kink point

in Borenstein (2009). Consumers' knowledge of such kink points is a potential source of endogeneity when trying to establish a causal effect (Rapson 2014). As a consequence, the estimates of elasticity would be biased and inconsistent.

We segment the studies in the following (mutually exclusive) subsamples: natural experiments, quasi-experiments, non-experimental studies controlling for endogeneity and studies that do not control for endogeneity. Initially, our goal was to also further divide quasi-experimental approaches into regression discontinuity design and matching technique, however, the number of observations for respective approaches was limited, precluding us from conducting more granular tests and substantiating our results.

Experiments are used to estimate causal relationships by taking advantage of a naturally occurring event or situation that closely mimics a controlled experiment. These "natural experiments" arise from external factors or policies that divide subjects into groups in a manner unrelated to the characteristics or behaviours under investigation. For example, Byrne *et al.* (2021) conducted an experiment in cooperation with an electricity retailer, during which sudden price discounts on a monthly basis to selected customers. Similarly, Matsukawa (2018) explored the impact of providing hourly electricity consumption information to households through in-home displays, assessing changes in their usage patterns. Overall, we collected the 299 estimated elasticities from 16 natural experiments, which are, together with quasi-experiments, presented in the Table B.17.

Moreover, we collected 4 quasi-experimental study designs that include matching and regression discontinuity design. The regression discontinuous procedure, as the name suggests, establishes a cutoff point based on which subjects are assigned to a control or treatment group. This cutoff can be a specific value of electricity demand in kWh and then we compare observations right below and above the cutoff point, assuming that these observations are similar in all respects except for the treatment. For instance, Zhou *et al.* (2019) compare firms in Shanghai based on the average electricity demand after the introduction of an additional (increasing) block tariff. Firms with consumption below 600 kWh were generally not subject to the increased marginal electricity prices, whereas those consuming more than 600 kWh were affected by the higher rates. On the other hand, matching involves pairing units in the treatment group with similar units in the control group based on certain observed characteristics, creating a "matched sample" that's comparable across groups. By matching treated and untreated subjects with these characteristics, researchers attempt to isolate the effect of the treatment from other factors. In the context of electricity demand, this might mean matching households or firms that are similar in terms of size, location, and income, but some receive a sale on energy-efficient appliances or directly on electricity price per kWh while others do not. Generally, these methods do not capture the causal effects as strongly as natural experiments due to usually non-randomized treatment and control samples.

To further deal with endogeneity, some researchers use instrumental variables or other estimation models and techniques (Schwarz 1984; Cebula 2012; Alberini *et al.* 2019a) to mitigate this difficulty. On the other hand, there are studies (Apte 1983; Hesse & Tarkka 1986; Fan & Hyndman 2011) which do not mention the possible endogeneity of price variables at all. Furthermore, few authors (Burke & Csereklyei 2016; Liddle & Huntington 2021) argue that instrumenting for price does not alter results significantly on a macro-level.

We hence explore the difference in results between estimates from natural experiments, quasi-experiments, and studies that control for endogeneity (obtained using for example IV, 2SLS or GMM) and those that do not (usually OLS or random effects). We define the subsamples used in the estimation as follows: one group consists of natural experiments, the second group incorporated quasi-experiments (in our case regression discontinuity design or matching), the third group consists of employment of techniques controlling for endogeneity bias (using for instance IV, 2SLS, GMM) and the last subsample consists of studies which do not control for endogeneity at all.

Table 4.6: Linear tests results for endogeneity subsamples

	OLS	**FE**	**BE**	**RE**	**SW**	**PW**
Natural Experiments						
PB	-1.112^{***}	-0.964^{***}	-2.314^{***}	-1.000^{***}	-2.060^{***}	-2.313^{***}
PB SE	(0.443)	(0.098)	(0.564)	(0.097)	(0.138)	(0.418)
Boot. CI	[-2.071; -0.448]			[-1.931; -0.331]	[-2.218; -1.513]	[-3.126; -1.518]
EBB	-0.060^{***}	-0.067^{***}	-0.027	-0.087^{***}	-0.006	-0.039^{***}
EBB SE	(0.017)	(0.007)	(0.035)	(0.027)	(0.006)	(0.008)
Boot. CI	[-0.087; -0.026]			[-0.132; -0.037]	[-0.028; 0.001]	[-0.056; -0.025]
Total observations = 299						
Quasi-Experiments						
PB	-0.412	-0.499^{***}	-0.428	-0.466^{***}	-0.333	-1.585^{***}
PB SE	(0.259)	(0.133)	(0.952)	(0.124)	(0.218)	(0.378)
Boot. CI	[-0.889; 0.250]			[-0.988; 0.297]	[-0.756; 0.213]	[-2.260; -0.747]
EBB	-0.110^{***}	-0.089^{*}	-0.247	-0.108	-0.125^{***}	-0.053^{***}
EBB SE	(0.047)	(0.051)	(0.369)	(0.081)	(0.029)	(0.016)
Boot. CI	[-0.207; -0.025]			[-0.309; 0.011]	[-0.186; -0.69]	[-0.100; -0.030]
Total observations = 63						
Studies Controlling for Endogeneity						
PB	-0.848^{***}	-0.898^{***}	-0.969^{***}	-0.897^{***}	-1.151^{***}	-2.022
PB SE	(0.100)	(0.051)	(0.222)	(0.049)	(0.091)	(1.946)
Boot. CI	[-1.054; -0.065]			[-1.084; -0.731]	[-1.346; -0.985]	[-4.534; -2.346]
EBB	-0.327^{***}	-0.317^{***}	-0.263^{***}	-0.276^{***}	-0.342^{***}	-0.167
EBB SE	(0.019)	(0.016)	(0.059)	(0.038)	(0.020)	(0.103)
Boot. CI	[-0.365; -0.295]			[-0.310; -0.235]	[-0.381; -0.303]	[-0.412; -0.036]
Total observations = 944						
Studies Not Controlling for Endogeneity						
PB	-0.781^{***}	-0.723^{***}	-1.015^{***}	-0.746^{***}	-0.425^{*}	-3.265^{***}
PB SE	(0.069)	(0.032)	(0.099)	(0.031)	(0.265)	(0.207)
Boot. CI	[-0.938; -0.644]			[-0.917; -0.591]	[-1.016; -0.060]	[-3.614; -2.783]
EBB	-0.270^{***}	-0.281^{***}	-0.284^{***}	-0.329^{***}	-0.209^{***}	-0.08
EBB SE	(0.012)	(0.010)	(0.028)	(0.020)	(0.032)	(0.013)
Boot. CI	[-0.292; -0.246]			[-0.358; -0.303]	[-0.262; -0.140]	[-0.109; -0.059]
Total observations = 3096						

Notes: This table presents the results of publication bias (PB) and effect beyond bias (EBB) for studies segmented by natural experiments, quasi-experiments, studies controlling for endogeneity, and lack of endogeneity control. Standard errors are presented in parentheses. SW = Study weighted, PW = Precision weighted, PB = Publication Bias, EBB = Effect Beyond Bias, SE = Standard Error, Boot. CI = Bootstrapped Confidence Interval. Asterisks denote significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The results of linear tests are presented in Table 4.6. Generally, there is a great difference between experimental and non-experimental data and little difference between the two endogeneity subsamples (control and no control). The average price elasticity corrected for publication bias is around -0.06 for the natural experiments, which is approximately one-fifth of the non-experimental elasticity results. Nevertheless, the publication bias is still present according to the tests and the median value of the corrected price elasticity estimate is approximately between -0.3 and -0.28 for both non-experimental subsamples. Ultimately, we decided not to segment the elasticities into short-run, intermediate run and long-run due to the relatively low number of observations and also due to the fact that experiments mostly focus on short-run elasticities.

The non-linear techniques for publication bias detection convey a similar message as the linear tests. The price elasticity corrected for publication bias is lower for studies controlling for endogeneity (median value is around -0.243) and studies that do not control for endogeneity (median value is around -0.2), compared to their sample average of around -0.4. Note that the estimate based on the stem-based method is insignificant even at 10% for experimental studies. We conclude that the tests provide significant evidence of publication bias for these subsamples as well and the underlying elasticity for natural experiments is much lower compared to non-experimental elasticities. Yet, it seems that even experimental elasticities suffer from publication bias.

Table 4.7: Non-linear tests for publication bias for endogeneity sub-samples

Effect beyond bias *(Natural Experiments)*			
WAAP	-0.078^{***} (0.007)	-0.071^{***} (0.005)	**Selection model**
Top10	-0.035^{***} (0.007)	-0.055^{***} (0.161)	**Hierarchical Bayes**
Stem-based method	-0.014 (0.027)	-0.039^{***} (0.004)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-2.458 (1.768)	-2.313^{***} (0.501)	**Endogenous kink**
Number of observations = 299			
Effect beyond bias *(Quasi Experiments)*			
WAAP	-0.140^{***} (0.017)	-0.145^{***} (0.017)	**Selection model**
Top10	-0.051^{***} (0.011)	-0.148^{**} (0.065)	**Hierarchical Bayes**
Stem-based method	-0.032 (0.047)	-0.047^{***} (0.011)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-1.109^{*} (0.868)	-2.037^{***} (0.473)	**Endogenous kink**
Number of observations = 63			
Effect beyond bias *(Endogeneity Control)*			
WAAP	-0.292^{***} (0.013)	-0.296^{***} (0.016)	**Selection model**
Top10	-0.161^{***} (0.026)	-0.201^{***} (0.045)	**Hierarchical Bayes**
Stem-based method	-0.426^{***} (0.016)	-0.167^{***} (0.014)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-1.367^{***} (0.194)	-2.022^{***} (0.779)	**Endogenous kink**
Number of observations = 944			
Effect beyond bias *(No Endogeneity Control)*			
WAAP	-0.232^{***} (0.008)	-0.212^{***} (0.008)	**Selection model**
Top10	-0.198^{***} (0.015)	-0.249^{***} (0.023)	**Hierarchical Bayes**
Stem-based method	-0.081^{***} (0.026)	-0.08^{***} (0.004)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-1.196^{***} (0.110)	-3.265^{***} (0.208)	**Endogenous kink**
Number of observations = 3096			

Notes: Results of the three specifications of price elasticities using six non-linear methods. We also include publication bias for Hierarchical Bayes and Endogenous kink methods. WAAP = Weighted Average of the Adequately Powered (n = 128 for N.E., 22 for Q.E.,500 for E.C.,1602 for N.E.C.). Top10 = Top10 Method (n = 30 for N.E., 6 for Q.E., 98 for E.C., 323 for N.E.C.). Standard errors are included in the parentheses. Asterisks denote significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Chapter 5

Heterogeneity

As presented in Chapter 3, the estimated effects vary widely according to different factors or control variables. We now present the variables included in the dataset, discuss the rationale for their inclusion and how their variation in the literature may affect the price elasticity estimate. We subsequently employ Bayesian model averaging (BMA) techniques to deal with model uncertainty and investigate heterogeneity further. This will prove helpful in the following chapter, in which we construct our best-practice estimate.

5.1 Explanatory Variables

We initially compare different characteristics of variables included within studies and subsequently, we try to find specifications that systematically influence the price elasticity. Even though we have collected 109, we omit some of them, mostly variables used for identification or a low number of positive observations for the given variable column, from the main analysis in this section. The full variable description summary is presented in Table A.1. Initially, this leaves us with 74 variables to consider for the model averaging.

For the ease of exposition, we divide the elasticity and study characteristics into variables describing the *Study Level Data Characteristics* (19), *Data Aggregation* (3), *Type of Price Elasticity* (6), *Type of Electricity Demand* (3), *Type of Electricity Price* (5), *Type of Electricity Tariff* (4), *Demand Controls* (5), *Model and Function Specification and Estimation Technique* (23), *Endogeneity Control* (2) and lastly *Publication Characteristics* (4). While this is a considerable amount of aspects that influence electricity modelling procedures, it is by no means a fully encompassing set of variables. In the following subsections, we also do not present an exhaustive list of variables, but rather discuss the most important variables and offer insights on their connection to price elasticity estimates obtained from the most prominent studies.

Study Level Data Characteristics

Naturally, we collected estimates such as the elasticity estimate with corresponding t-statistic and standard error. We also coded for the number of observations used in the primary studies, which vary widely. For instance, Auray *et al.* (2019) estimated price elasticity by using more than 16 million observations from bi-annual household meter readings, whereas some papers report a number of observations below 40 (Halvorsen & Ford 1979; Nasir *et al.* 2008), especially those estimating price elasticity on a country level and using annual data. We coded for specific countries and then created dummy variables for the USA, Europe, and the rest of the world, which allowed us to investigate the systematic differences in price elasticity that Liddle & Huntington (2021) suggest vary by country. To explore endogeneity bias, we also labelled studies employing experimental design with dummy variable *exp*. A potential distortion in elasticity measurements from study experiments is highlighted by Dahl (1993), referring to the Hawthorne effect. This phenomenon, detailed in Schwartz *et al.* (2013), suggests that consumers might alter their electricity usage simply because they know they are being observed, not necessarily as a genuine adjustment to their consumption habits. The experimental data reviewed did not address whether this effect could skew the elasticities.

We further accounted for variables relating to the temporal scope of primary studies, recording the start and end years to reflect shifts in electricity demand, which, as noted by many authors (Chaudhry 2010; Gam & Rejeb 2012; Silva *et al.* 2018), has been on an upward trajectory in numerous countries throughout the years. We also created a variable denoting the middle year of the dataset to include in the model averaging. We tried to explore the assumption that investigation periods of primary studies are one of the main significance with respect to electricity demand (Frondel *et al.* 2019). Further, we factored in the length of daylight hours and temperature, recognizing that longer daylight can reduce the necessity for artificial lighting and temperature can alter the need for heating or cooling, thereby influencing demand elasticity. Additionally, we incorporated the average annual temperature, the status of countries as electricity exporters, and the carbon intensity of their electricity production into our analysis. Sources of these data can be found in the dataset attached.

Moreover, we also collected the type of data that are used in the estimation. We differentiated between cross-sectional, time-series and panel data. This is another potential driver of heterogeneous effects in our sample. Espey & Espey (2004) find that time-series data are more price elastic compared to cross-sections, as they can capture long-term effects (stock adjustments), contrary to the findings of Dahl (1993). Lastly, we included the logarithmic transformation of population and income variables, as especially the income level might be one of the determinants of electricity demand.

Data Aggregation

In the research on the electricity demand, there are great sources of data to choose from. Some authors use nation-level data to estimate price responsiveness and as an other extreme, some papers (mainly but not limited to experiments) take advantage of data from electricity meters on a household level. Other sources include regional or utility-level data. It is important to avoid mixing these levels of data without proper acknowledgement, as this practice could introduce aggregation bias. Krishnamurthy & Kriström (2015) argue that whilst aggregation data might be easier to collect, they are generally more difficult to both interpret and apply to energy policy. Therefore, the impact of data aggregation on elasticity estimates remains a subject of debate. Dahl (1993) observed that aggregated data tend to exhibit higher elasticities compared to more granular, disaggregated data. Nonetheless, more recent analyses, such as those by Miller & Alberini (2016), indicate that there is no consensus on whether data aggregation necessarily leads to systematically higher or lower measures of elasticity.

Type of Price Elasticity

In electricity demand studies, Marshallian, Hicksian, and Morishima types of elasticities, are usually estimated to analyze consumer responses to price changes. Marshallian elasticity gauges immediate consumption shifts due to price changes, assuming constant income and potential prices of other goods. Hicksian elasticity, in contrast, assesses demand changes by holding utility constant, thus isolating the substitution effect from income effects. Morishima elasticity further expands on this by examining the substitution rate between two goods (or appliances) as their relative prices change. Admittedly, only part of the studies explicitly mentioned either utility maximization (Jones 1995; Eskeland & Mideksa 2010; Shaffer 2020) or cost minimization (Pitt 1985; Sterner 1989; Cao *et al.* 2023) estimation framework, which provides clear instruction regarding what type of elasticity is of interest. Further information from the authors of primary studies would provide more robust evidence for our findings on the heterogeneity of price elasticities with respect to the three types. In this respect, we provide model averaging results for each type of price elasticity in the Appendix 7 as a robustness check.

To capture the full scope of price elasticity estimates, we consider *short-run*, *intermediate-run* and *long-run* effects. While electricity is usually segmented into short-run and long-run, we decided to include intermediate-run elasticity in order to avoid loss of information from authors who decide to further differentiate. Short-run elasticity concerns the immediate reaction of households to price changes, which is the case for short time-series data and more specifically, time-of-use modelling. On the other hand, long-run effects capture adjustments of stock appliances and are usually examined by employing multiple year time-series. Hence, prior papers employed dynamic models with lagged dependent variables to estimate long-run elasticities (Houthakker 1980; Filippini 2011). On the other hand, Burke & Abayasekara (2018) argue that employing dynamic models might be problematic if the exact process of price shocks is not known. Intermediate-run elasticity is usually estimated using pooled cross-sectional data and is based on quarterly or annual data.

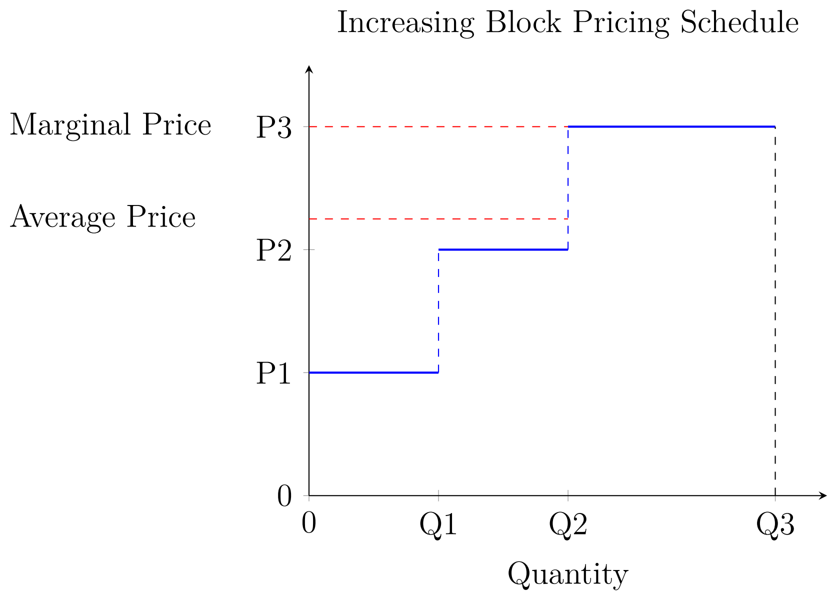
Type of Electricity Demand

Electricity demand is usually segmented into industrial, commercial, and residential types, each serving distinct uses in policy-making. Residential demand concerns the consumption of electricity in households, covering various appliances and daily living needs. Commercial electricity demand includes energy used in business operations, services, retail environments, and office buildings, facilitating activities ranging from lighting to powering electronic devices that support business functions. Industrial demand, on the other hand, is associated with more substantial energy needs such as operating heavy machinery and maintaining warehouse operations. For instance, Bildirici *et al.* (2012) explore the causal relationship between industrial electricity consumption and economic growth, emphasizing the role of industrial electricity demand as a potential driver of economic development. Residential electricity demand, as investigated by Sa'ad (2009), shows that price elasticity is influenced by income levels and efficiency improvements within the household sector. Woo *et al.* (2018) reports that in the U.S. commercial electricity demand has the largest variance in the short-run. There might be systematic differences in the reported elasticities for each consumer type (Gautam & Paudel 2018). Some authors also included other types of electricity demand such as agriculture or government demand elasticities (Khan & Abbas 2016), however, such practices were rare, therefore we included both in the industrial demand.

Type of Electricity Price

The type of electricity price employed in the estimation technique is of utmost importance and there is an ongoing debate about whether consumers respond to the average or the marginal price of electricity, which are the two most widely used metrics. For exhaustive discussion on the topic, see Borenstein (2009), Ito (2014) or Shaffer (2020). While the cost of additional electricity in terms of kWh (marginal price) might be a more pertinent tool to base decisions on, such information is often costly (Shin 1985), especially under multiple block pricing structures. A customer is hence more prone to consider the average price, which might not reflect the true cost of additional consumption. Mount *et al.* (1974) argue that while households decide with respect to marginal price, average price is of higher practical importance, especially to utility companies as the data on average price are generally more readily available (Alberini *et al.* 2011). Furthermore, Shaffer (2020) conducts an experiment concerning increasing block tariffs in British Columbia and finds that 85% of customers respond to average price, 7% respond to marginal price while the 8% of consumers left mistakenly assume that once they move to higher pricing block, their current marginal price applies to all the electricity consumed. As a simple example, in the Figure 5.1 below, we can notice the difference between the customer's average and marginal price. Suppose the customer is facing increasing block pricing denoted by solid blue horizontal lines and is consuming quantity *Q3*. While the marginal price is reflected only in the specific tariff block (that is, price *P3*), the average also incorporates previous blocks with lower prices as a weighted average. However, the consumer might assume that either the average price is the true cost of additional electricity or that the marginal price applies to the whole electricity consumption. Subsequently, consumer responsiveness can be greatly affected by using different price specifications.

Figure 5.1: Increasing block pricing with marginal and average price



Besides the average and marginal electricity, some consumers face lump sum electricity prices. Unlike average or marginal pricing models that adjust based on consumption levels, a lump sum price is a set fee paid by consumers for accessing electricity services, regardless of how much or how little electricity they consume within a billing period. This fixed pricing structure could potentially encourage the over-consumption of electricity, as it lacks the financial incentives to conserve energy. Widespread adoption of such pricing could, therefore, strain electricity supply systems and challenge sustainable energy management practices. However, the prevalence of the lump sum price is very limited, as such price is reported only in Uri (1982). Overall, it is also observed that electricity prices tend to be lower in developing countries, as highlighted by McRae & Meeks (2016).

Misunderstanding of prices as explored by Shaffer (2020) can have detrimental implications for policymakers and lead to undesired outcomes. Various experiments (Schneider & Sunstein 2017) conclude that "the most cost-reflective electricity policies might not be the most efficient ones". On the other hand, Jessoe & Rapson (2014) conduct a randomized trial in which high-frequency information is conveyed to the customers. Informed households exhibit greater responsiveness up to three standard deviations.

Type of Electricity Tariff

Electricity pricing tariffs typically fall into one of the four main categories: *increasing_tariff* where the marginal price increases either in blocks or continuously; *decreasing_tariff* which is the reverse, with marginal costs diminishing; *flat_tariff* for which the marginal price stays constant; and *time-of-use* tariff for which the price of electricity varies (usually non-monotonically) throughout the day. Specifically, TOU is becoming of high interest. By setting higher prices during evening peak hours and lower prices during off-peak times, TOU encourages more even consumption patterns and can help alleviate excess capacity issues during off-peak periods. Electricity during peak hours in the evening is more expensive compared to non-peak time and some of the customers can hence smooth their consumption patterns, which helps to address the overcapacity during peak times. Many of the studies focus specifically on the time-of-use tariff (Holland & Mansur 2008; Filippini 2011). For example, Wolak (2011) conducts an experiment to examine whether consumers actually respond to hourly prices, which requires continual monitoring of electricity prices. His research found that dynamic pricing programs can significantly reduce electricity consumption. Beyond individual behaviour, the broader effects of TOU pricing have significant implications, with Holland & Mansur (2008) suggesting that it could facilitate electricity generation from cleaner sources, thereby mitigating carbon emissions. However, this outcome depends on the nature of the energy sources in use, as reductions in carbon emissions could be offset if peak demand is already met by cleaner energy sources and base load demand is fulfilled by carbon-intensive fuels like coal.

Demand Controls

To capture further possible heterogeneity of price elasticity we also include five dummy control variables in our dataset: *demographics_control* signifying if any information about household or customers was included, such as household size or date of construction; we also code for *temperature_control*, usually in terms of cooling degree days or heating degree days. These characteristics denote the possible need for additional electricity usage. The main idea is that when the temperature outside is 65°F (or 18.3°C), consumers do not need electricity for heating or cooling. For days when the temperature exceeds 65F, cooling degree days (CDD) are computed by averaging the day's highest and lowest temperatures and then subtracting 65°F. For instance, on a day with a high of

90°F and a low of 60°F, the CDD would be 10, reflecting the increased demand for cooling and, consequently, electricity. If the average temperature is below 65°F, we use HDD in a similar manner. Climate is becoming increasingly important for electricity consumption (Auray *et al.* 2019) and is also becoming of a central interest with respect to price elasticity estimates (see e.q. Holtedahl & Joutz 2004; Lee & Chiu 2011). Specifically, Dong & Kim (2018) estimate the temperature effect on the electricity demand, concluding that not controlling for temperature leads to a downward bias in price elasticity. Thirdly, we also control for *stock_control*, indicating whether the demand equation incorporates information about the stock of appliances, which, as shown in Chapter 2, is an inherent part of electricity modelling. The fourth control employed is the *fuels_control*, which states whether the author included substitute fuels for electricity, mainly coal, oil or gas. As a result, such papers usually also present elasticities of substitution, which are, however, not the focus of the thesis. Lastly, we denote whether the author also considers information about income represented by the *income_control* dummy. Including these variables helps mitigate the risk of omitted variable bias, a concern highlighted by Lanot & Vesterberg (2021) and echoed by Miller & Alberini (2016), who point out the pitfalls of using detailed household data without considering key determinants of consumption patterns like dwelling characteristics.

Model, Estimation and Function Specification

As already mentioned in Chapter 3, various model specifications and estimation techniques provide systematically different effects, as the models and estimation techniques work with various assumptions and also assume different unobserved patterns between the price elasticity estimates. Based on the literature review, we decided to code for both 9 models and estimation methods.

Selected studies estimate the simple models, which are usually static and reduced-form models. These models simply regress the electricity consumption on the price of electricity. To capture the complexity of estimated models, we employ several dummy variables (*reduced_form, structural_form, static_model, dynamic_model*). Moreover, the *dynamic_model* variable is specifically used to indicate whether the model accounts for long-term effects, typically by incorporating lagged values of electricity consumption as independent variables. Dynamic models offer the advantage of simultaneously capturing both immediate (short-run) and eventual (long-run) responses to changes in electricity price, facilitated by their inherent lag structure. However, a potential limitation of these models is the risk of collinearity between the current and lagged consumption variables, which could complicate the analysis.

In terms of functional specification, the ones usually employed are *linear*, *semi-logarithmic* and *double-logarithmic*. We also collected information regarding the usage of Box-Cox transformations. Subsequently, we also decided to use dummy variable *endo_control* to address whether papers did correct the price elasticities for the endogeneity bias. For a further overview of different models and estimation procedures with respect to electricity modelling, one might consult Dahl (1993) or Kamerschen & Porter (2004).

Publication Characteristics

The last group of variables included in the dataset concern the publication information. We code for *impact journal* and *publication year*, which convey the information on the quality of the journal a particular study was published in and the year of publication, respectively. We also collected the number of citations (*citations*) and subsequently transformed them (*citations (t)*) to account for different publication dates.

5.2 Model Averaging Techniques

5.2.1 Introducing BMA

In this section, our main focus is to examine the heterogeneity of the price elasticities collected. Our analysis consists of 74 variables collected. One possibility would be to examine the following model:

$$PE_{ij} = \alpha + \beta * SE(PE_{ij}) + \sum_{k=1}^{74} \gamma * X_{ijk} + \epsilon_{ij} \tag{5.1}$$

where the interpretation is similar to Equation 4.1, the only difference being that X is now the set of all ($k = 1, 2, ...74$) explanatory variables. This approach, regressing the price elasticities on all explanatory variables would inflate standard errors as a lot of these variables would be insignificant and hence redundant. Furthermore, we might encounter issues with multicollinearity (Irsova *et al.* 2023b). The problem we are facing is the ex-ante uncertainty of inclusion with respect to our variables. For example, we cannot be certain

Table 5.1: Definition and summary statistics of selected variables (Part 1)

Variable	Description	Mean	SD
Data characteristics			
Observations (n)	How many observations were used	82194	558570
Experiment	= 1 if the study is an experiment,	0.066	0.249
P value	= 1 if p value instead of standard error is used	0.086	0.280
effect	Price elasticity of electricity estimate	-0.400	0.486
Standard error	Standard error of the price elasticity estimate	0.169	0.257
Start year*	Starting year of the study	1979	17.53
End year*	Ending year of the study	1993	17.48
Mid year*	Middle year of the study	1986	16.65
Number of years*	How many years does the study cover	14.28	10.53
USA	= 1 if the examined country is the USA	0.477	0.500
Europe**	= 1 if the examined country is in Europe	0.187	0.390
Other location*	= 1 if outside of USA and Europe	0.350	0.477
Daylight hours	Daily average time between sunrise and sunset for a given year	15.076	1.800
Annual temperature*	Average annual temperature	10.96	7.585
Electricity exporter*	= 1 if the country exports electricity	0.831	0.375
C. intensity of production*	Log of carbon intensity of electricity production	6.114	0.866
Population (log)	Log of population in the given country	18.315	1.541
Daylight hours	Daily average time between sunrise and sunset for a given year	15.076	1.800
Income level (log)*	Log of GDP per capita	8.932	1.375
Data aggregation			
Aggregation: Country	= 1 if data aggregation is at the country level	0.275	0.446
Aggregation: Region*	= 1 if data aggregation is at the regional level	0.243	0.429
Aggregation: City*	= 1 if data aggregation is at the city level	0.141	0.349
Aggregation: Disaggr.	= 1 if data are for smaller units than city (household level or granular firm data)	0.237	0.425
Data type			
Data: Panel**	= 1 if panel data are used	0.499	0.500
Data: Time-series	= 1 if time series data are used	0.386	0.487
Data: Cross-section	= 1 if cross section data are used	0.115	0.319
Type of elasticity			
Estimate: Short-run	= 1 if short-run effect is estimated	0.401	0.490
Estimate: Intermediate-run	= 1 if intermediate-run effect is estimated	0.417	0.493
Estimate: Long-run	= 1 if long-run effect is estimated	0.183	0.386
Type: Marshall	= 1 if the type of elasticity is Marshallian	0.736	0.498
Type: Hicks**	= 1 if the type of elasticity is Hicksian	0.260	0.480
Type: other*	= 1 if other type of elasticity is used (or type unknown)	0.004	0.090
Type of electricity demand			
Type: Residential	= 1 if data is relevant for residential demand	0.379	0.485
Type: Commercial	= 1 if data is relevant for commercial sector	0.198	0.399
Type: Industrial	= 1 if data is relevant for industry	0.640	0.480
Data period			
Granularity: Yearly	= 1 if data are of a yearly granularity	0.742	0.437
Granularity: Quarterly*	= 1 if data are of a quarterly granularity	0.028	0.166
Granularity: Monthly**	= 1 if monthly data are used	0.201	0.401
Type of electricity price			
Price: Average	= 1 if average price is used	0.537	0.499
Price: Marginal	= 1 if marginal price is used	0.200	0.400
Price: Other*	= 1 if other price than marginal or average is used	0.102	0.302
Type of electricity tariff			
Tariff: Increasing	= 1 if increasing tariff is installed	0.121	0.326
Tariff: Decreasing	= 1 if decreasing tariff is installed	0.104	0.305
Tariff: Flat*	= 1 if flat tariff is installed	0.030	0.171
Tariff: TOU	= 1 if a time-of-use tariff is installed	0.113	0.317

...continued on the next page

Table 5.2: Definition and summary statistics of selected variables (Part 2)

Variable	Description	Mean	SD
TOU demand period			
Demand: Peak	= 1 if peak demand is observed	0.054	0.225
Demand: Mid-peak	= 1 if mid-peak demand is observed	0.024	0.154
Demand: Off-peak**	= 1 if off-peak demand is observed	0.019	0.135
Demand Controls			
Control: Demographics	= 1 if the study controls for demographics	0.337	0.473
Control: Temperature	= 1 if the study controls for temperature	0.488	0.500
Control: Stocks	= 1 if the study accounts for appliance stock	0.188	0.391
Control: Fuels	= 1 if the study includes other fuels as controls	0.414	0.493
Control: Income*	= 1 if the study includes income measures	0.571	0.495
Model Form			
Form: Reduced	= 1 if a reduced form model is used	0.415	0.493
Form: Structural**	= 1 if a structural form model is used	0.526	0.499
Model: Dynamic**	= 1 if a dynamic model is used	0.687	0.464
Model: Static	= 1 if a static model is used	0.310	0.463
Model Specification			
Model: RE	= 1 if a random-effects model is used	0.011	0.103
Model: FE**	= 1 if a fixed-effects model is used	0.091	0.288
Model: VAR*	= 1 if vector autoregressive model is used	0.007	0.082
Model: ARDL	= 1 if an ARDL model is used	0.078	0.268
Model: ECM*	= 1 if an error-correction model is used	0.049	0.215
Model: VECM*	= 1 if a vector error-correction model is used	0.019	0.138
Model: DS*	= 1 if an demand system model is used	0.104	0.305
Model: DC*	= 1 if a discrete-continuous model is used	0.007	0.081
Model: LE	= 1 if a lagged-endogenous model is used	0.230	0.421
Model: Other*	= 1 if another model type is specified	0.020	0.141
Estimation Technique			
Estimation: ML*	= 1 if maximum-likelihood estimation is used	0.053	0.224
Estimation: GMM	= 1 if generalized method of moments is used	0.040	0.195
Estimation: Error comp.*	= 1 if an error component model is used	0.025	0.156
Estimation: OLS	= 1 if ordinary least squares or its variations are used	0.364	0.481
Estimation: GLS*	= 1 if generalized least squares is used	0.050	0.218
Estimation: SUR*	= 1 if seemingly unrelated regression is used	0.111	0.314
Estimation: 2SLS	= 1 if two-stage least squares is used	0.099	0.298
Estimation: 3SLS	= 1 if three-stage least squares is used	0.025	0.157
Estimation: IV	= 1 if an instrumental variable is used	0.079	0.269
Estimation: other*	= 1 if other estimation technique is specified	0.022	0.160
Function Specification			
Function: Linear	= 1 if a linear function is used	0.287	0.390
Function: Semi-log**	= 1 if a semi-log function is used	0.062	0.195
Function: Double-log	= 1 if a double-log function is used	0.562	0.497
Function: Box-Cox*	= 1 if a Box-Cox transformation is used	0.004	0.060
Endogeneity Control			
Control*	= 1 if endogeneity is controlled for	0.248	0.432
No control*	= 1 if endogeneity is not controlled for	0.752	0.432
Publication Characteristics			
Publication Year	Year of publication	1997.85	16.50
Impact Factor	Journal impact factor	0.182	0.426
Citations (t)	Log-transformed number of citations	1.254	1.001
Citations	Number of citations until 3rd March	96.31	150.85

Notes: The table provides description and summary statistics on selected variables. Note that the non-available data column was omitted from the individual groups, hence some of the dummy variable groups do not add up to 1, as they should. For price variables, we included flat and shin price due to low number of observations. Citations (t) take into account the publication year, therefore, studies published earlier are penalized relatively to those published later. Asterisks (*) denote variables subsequently excluded from the model averaging estimation. Double asterisks (**) include dummy variable used as a reference for the respective group in BMA.

whether we should employ average or marginal price, or whether the underlying model should have all 5 demand control variables. Initially, we have 2^{74} options of model specifications and even if we had an idea regarding how a particular model should look like, by restricting ourselves to a single model, we run into a higher risk of misrepresenting the economic reality (Steel 2020). As a result, we are dealing with model uncertainty. The model uncertainty can be tackled by employing Bayesian model averaging (BMA), which is the usual practice in the meta-analysis (Havranek *et al.* 2018c; Bajzik 2021). As the name suggests, BMA takes averages over all possible combinations of models, while not precisely knowing which one is the correct one. Subsequently, each model is assigned a posterior inclusion probability (PIP). Ultimately, different models might capture various aspects of electricity demand elasticity, such as time-of-use pricing effects, consumer heterogeneity, and long-term vs. short-term elasticity. BMA navigates through these models, offering a composite view that accounts for the uncertainty inherent in model choice.

We will now briefly describe the process of updating model employment probability (for a more detailed explanation, see Leamer (1978) or Raftery *et al.* (1997)). Firstly we consider a set of price elasticity models $M_1, M_2, \ldots, M_k$, where each model has associated parameters θ_j for model M_j. Bayesian inference starts with specifying a prior probability for each model, $P(M_j)$, and a prior distribution for the parameters within each model, $P(\theta_j|M_j)$. Firstly, we are interested in how the posterior distribution of price elasticity (we can for now denote it by γ) changes with respect to data D we have collected:

$$P(\gamma|D) = \sum_{j=1}^{k} P(\gamma|D, M_j)P(M_j|D) \tag{5.2}$$

Where the first probability measure is the distribution of the effects given a particular model M_j and the second term is the probability of model M_j being included in the model averaging conditional on the dataset. Subsequently, the probability of observing the data D given the model M_j and its parameters is denoted by the likelihood $P(D|\theta_j, M_j)$. Upon observing the data, the posterior probability of model M_j is updated as follows:

$$P(M_j|D) = \frac{P(D|M_j)P(M_j)}{\sum_{i=1}^{k} P(D|M_i)P(M_i)} \tag{5.3}$$

where

$$P(D|M_j) = \int P(D|\theta_j, M_j)P(\theta_j|M_j)d\theta_j \tag{5.4}$$

encapsulates the model evidence, integrating the parameters' uncertainty. For model selection, one might choose the model with the highest posterior probability. However, BMA goes further by integrating over all models, weighting parameter estimates and predictions by these posterior model probabilities, enhancing predictive performance and robustness against model misspecification. There are two main challenges in implementing BMA. Firstly, the number of terms in Equation 5.2 is enormous and secondly, the integral in Equation 5.4 is often difficult to compute.

To manage the computational complexity inherent in this process, in practice, the Metropolis-Hastings algorithm within the Markov chain Monte Carlo method is often employed. This algorithm, as detailed by Zeugner & Feldkircher (2015) and utilized in the bms package in R, focuses on evaluating the most probable models, thus streamlining the computation (Zeugner & Feldkircher 2009; Cazachevici *et al.* 2020). The core of BMA is the calculation of posterior model probabilities, which reflect the likelihood of each model given the data. The estimation of each variable's importance is expressed through the Posterior Inclusion Probability (PIP). A variable's PIP, analogous to traditional measures of statistical significance, represents the sum of the posterior model probabilities of all models that include it. High PIP values indicate strong evidence for the inclusion of a variable in the final model, following the classification criteria by Kass & Raftery (1995).

The model averaging requires the specification of prior probabilities to individual model specification and estimated coefficients. In this study, we proceed with the typical approach of employing unit information prior (UIP) for the parameters in our model, where we assign each variable an equivalent weight. Moreover, in terms of model ex-ante beliefs, we will use a procedure with a dilution prior rather than a uniform model prior to better address the issue of potential collinearity among the numerous explanatory variables in our model. The dilution prior, suggested by George (2010), uses the model probabilities and the determinant of the correlation matrix of the independent variables to allocate weights, particularly favouring variables with lower correlations. This prior is beneficial when dealing with a significant number of similar variables that could introduce collinearity concerns. We present the correlation matrix in the Appendix A in Figure A.15. This prior has also been applied in a number of former meta-analyses (Cala *et al.* 2022; Elminejad *et al.* 2023). Nevertheless, we also include various other specifications to enhance the robustness of our results in the Appendix A. As a further robustness check, we employ Frequentist model averaging (FMA). This approach does not require specifying prior beliefs about the models or parameters, making it more straightforward in settings where such prior information is limited or unavailable. FMA uses data-driven weights to average over models and does not deal with collinearity (Irsova *et al.* 2023b).

To begin the model averaging process, an examination of the correlation among model variables and their Variance Inflation Factor (VIF) is conducted. This step is crucial to identify and mitigate issues like the induced correlation between variables. As a first step in our analysis, we examined the correlation and found out that the problematic pair is *publication_year* and *mid_year* of the dataset (correlation 0.95), which is not surprising. Furthermore, these are the only variables with a VIF value of 11 or more. Therefore, we excluded the *mid_year* variable from the model averaging and this subsequently lowered VIF for publication year. Other highly correlated variables were *monthly_granularity* and *yearly_granularity* (correlation of -0.85). Since this is a considerable linear dependence, we decided to omit the monthly granularity from the procedure as well. Upon further inspection, we decided to omit a few other variables (denoted by asterisks in Table 5.1) to keep the model as parsimonious as possible. This was mainly due to high multicorrelation, elevated VIF, or negligible PIP of selected variables in preliminary model averaging tests. This led to the omission of carbon intensity and electricity export dummy, as these variables had constantly PIP below 0.04 in preliminary tests. The rest of the possible pairs' correlation is below -0.58. Naturally, we omitted one variable from each dummy group to avoid the dummy variable trap.

We thus proceed with the estimation. The estimated equation is:

$$PE_{ij} = \alpha + \beta * SE(PE_{ij}) + \sum_{p=1}^{43} \gamma_p * X_{ijp} + \epsilon_{ij} \tag{5.5}$$

This equation presents the general regression of price elasticity estimation. The individual terms are defined as follows (in line with setting from Equation 4.1): α denotes the price elasticity corrected for publication bias, and β describes the magnitude and direction of publication bias. The sum $\sum_{p=1}^{43} \gamma_p * X_{ijp}$ represents the products of the variables included in the model averaging and their coefficients. ϵ_{ij} is the error term.

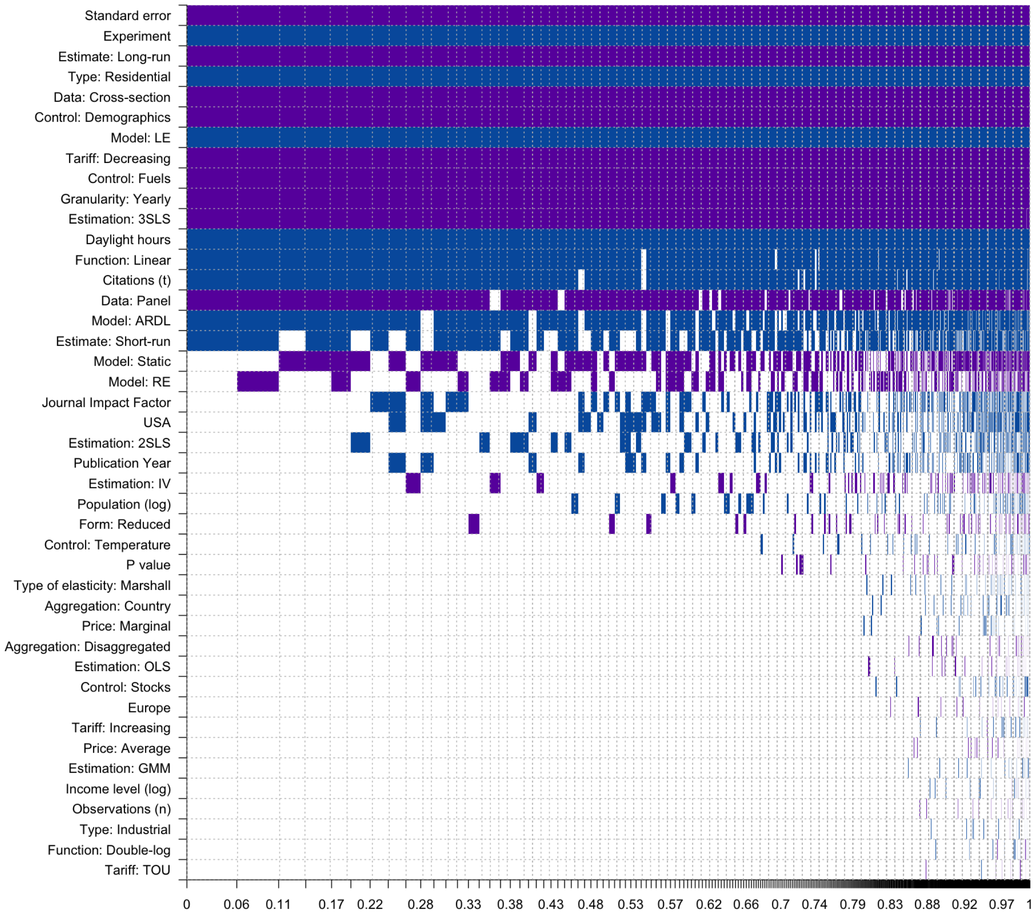
5.2.2 Results

Firstly, we provide a graphical overview of the BMA results in Figure 5.2. The figure depicts the results of BMA based on the posterior inclusion probability (PIP), where the individual variables are placed in a descending order on the vertical axis based on the frequency of inclusion in various models. Therefore, the *Standard error*, *experiment* and *long-run elasticity* are the most included characteristics during the process. Moreover, on the horizontal axis, we notice the colouring based on both the inclusion and the effect of the variable. Cells with blue colouring denote variables with a positive effect on the price elasticity estimate and purple fill signifies that variables have a negative effect on the price elasticity estimate. The white cells (uncoloured) denote that for the particular model, the variable is not included in model averaging.

The outcome indicates that approximately two-fifths of coded characteristics are of high usefulness in explaining the heterogeneity of price elasticity. Subsequently, the frequency of variables being included in various model specifications decreases rather rapidly. On the other hand, approximately one-third of variables are included in the minimum of models. With respect to Bayesian model averaging, we specify the posterior mean (Post. mean) and posterior standard deviation (Post. SD) along with the posterior inclusion probability (PIP). This is analogous to the coefficient (direction and magnitude of the effect), standard error (certainty about the estimated effect) and p-value connected to the frequentist approach. The posterior probability denotes the likeliness of a variable to be included in the final model by taking the sum of all model probabilities that include this variable. Following the framework by Jeffreys (1998), the classification of PIP is as follows: *weak effect* for variables with PIP between $[0.5, 0.75]$, *substantial effect* for variables with PIP between $[0.75, 0.95)$ and *strong* and *decisive effect* for intervals of $[0.95, 0.99)$ and $[0.99, 0.1]$, respectively.

We present the numerical results of the two model averaging approaches in Table 5.3. We note that 19 variables have PIP over 0.5, with two variables exerting weak effect (*Estimate: Short-run, Model: Static*) and one variable having a substantial effect on price elasticity determination (*Model: ARDL*). Furthermore, three characteristics signify strong effect (*Panel data, Function: Linear, Citations (t)*) and 13 variables show a decisive effect (*Constant, Standard error, Experiment, Daylight hours, Estimate: Long-run, Cross-sectional (data), Granularity: Yearly, Type: Residential, Tariff: Decreasing, Control:*

Figure 5.2: Model inclusion in Bayesian model averaging



Notes: The figure displays outcomes from Bayesian model averaging (BMA) utilizing both a uniform g-prior, as detailed by Eicher *et al.* (2011), and a dilution prior outlined by George (2010). Each vertical row represents a distinct variable, arranged according to their Posterior Inclusion Probability (PIP). The horizontal axis corresponds to the different models considered in the averaging process. Shades of blue (appearing lighter in greyscale) indicate a variable's positive influence on the effect size, whereas shades of purple (appearing darker in greyscale) indicate a negative influence. Cells left uncoloured (white) signify that the corresponding variable is not included in a particular model. The variables are described in Table 5.1, and numerical results can be referred to in Table 5.3.

Demographics, Control: Fuels, Model: LE, Estimation: 3SLS). One observation to keep in mind is that even though the constant term appears significant, too, it is unreliable to state conclusions about the effect due to the absent posterior inclusion probability. In line with evidence presented in Chapter 4, the presence of publication bias is substantiated by both BMA and FMA results and is of similar magnitude compared to the both linear and non-linear tests employed. We now highlight selected observations from the numerical results rather than covering all of the characteristics and their effect on the price elasticity estimate.

In terms of the data characteristics group, only the experimental setting and daylight hours have significant effects on the price elasticity estimates. The direction and magnitude of the experiment variable convey the evidence that price elasticities emanating from experiments are on average, less price elastic. Intuitively, this should not be surprising, as experiments are designed to identify causal relationships. The daylight hours (which are correlated with average temperature) also seem to decrease price elasticity in magnitude. Moreover, estimates coming from the US are also inclined to lower the estimated effect in magnitude but are insignificant. The aforementioned reasoning is connected to the idea that price elasticity is negative as shown previously, if the true elasticity was positive, then, of course, the effect would be of increasing magnitude. Moreover, both panel and cross-sectional data as well as the yearly granularity of data seem to significantly affect the elasticities, which is in line with findings of Zhu *et al.* (2018). However, there are discrepancies with respect to models, as we find the *3SLS* estimation technique to have a decisive effect, but Zhu *et al.* (2018) conclude that only error component models are useful. For further comparison, Horáček (2014) also finds that US data have a positive effect on the estimated elasticities, however, the divergence of results concerns the GMM estimation method (which is insignificant based on the model averaging results) and the residential type of electricity (which we found to be positively affecting the estimated elasticity). Lastly, Zabaloy & Viego (2022) find all estimation techniques included (GMM and IV) insignificant for their Carribean sample. Ultimately, this discrepancy in the results can also be caused by a higher number of variables included in our heterogeneity analysis, as previous papers focused only on a handful of characteristics.

Table 5.3: Model averaging results (Part 1)

	Bayesian model averaging			Frequentist model averaging		
Response variable:	Post. mean	Post. SD	PIP	Coef.	SE	p-value
(Intercept)	-1.603	0.000	**1.000**	-5.737	2.069	**0.006**
Standard error	-0.703	0.024	**1.000**	-0.697	0.025	**0.000**
Data Characteristics						
Observations (n)	0.000	0.000	0.003	0.000	0.000	0.000
Experiment	0.193	0.034	**1.000**	0.172	0.036	**0.000**
P value	-0.002	0.010	0.033	-0.047	0.033	0.161
USA	0.018	0.030	0.311	0.067	0.028	**0.019**
Europe	0.000	0.002	0.006	0.009	0.021	0.672
Daylight hours	0.017	0.004	**0.999**	0.020	0.005	**0.000**
Population (log)	0.001	0.004	0.097	0.003	0.006	0.670
Income level (log)	0.000	0.000	0.004	-0.009	0.009	0.302
Type of elasticity						
Estimate: Short-run	0.046	0.035	**0.695**	0.057	0.022	**0.010**
Estimate: Long-run	-0.173	0.031	**1.000**	-0.174	0.025	**0.000**
Type: Marshall	0.000	0.002	0.009	0.025	0.019	0.191
Data Aggregation						
Country level	0.000	0.003	0.012	0.016	0.019	0.387
Disaggregated	0.000	0.003	0.009	-0.021	0.023	0.296
Panel	-0.062	0.023	**0.953**	-0.076	0.019	**0.000**
Cross-section	-0.231	0.028	**1.000**	-0.239	0.028	**0.000**
Granularity: Yearly	-0.074	0.016	**1.000**	-0.055	0.021	0.010
Type of electricity demand						
Type: Residential	0.112	0.017	**1.000**	0.124	0.019	**0.000**
Type: Industrial	0.000	0.001	0.003	0.000	0.007	0.788
Type of electricity price						
Price: Average	0.000	0.001	0.005	-0.016	0.020	0.428
Price: Marginal	0.000	0.003	0.009	0.029	0.026	0.272
Type of Electricity Tariff						
Tariff: Increasing	0.000	0.002	0.005	0.001	0.020	0.969
Tariff: Decreasing	-0.140	0.028	**1.000**	-0.139	0.030	**0.000**
Tariff: TOU	0.000	0.002	0.005	0.007	0.022	0.752

....to be continued on the next page

Notes: This table presents the results of the Bayesian model averaging and Frequentist model averaging. Post. mean = Posterior Mean, Post. SD = Posterior Standard Deviation, PIP = Posterior Inclusion Probability, Coef. = Coefficient, SE = Standard Error. The variables with PIP > 0.5 are highlighted in bold.

Table 5.4: Model averaging results (Part 2)

	Bayesian model averaging			Frequentist model averaging		
Response variable:	Post. mean	Post. SD	PIP	Coef.	SE	p-value
Demand Controls						
Demographics	-0.099	0.016	**1.000**	-0.086	0.018	**0.000**
Temperature	0.001	0.005	0.028	0.016	0.017	0.359
Stocks	0.000	0.002	0.007	0.004	0.018	0.807
Fuels	-0.080	0.016	**1.000**	-0.081	0.016	**0.000**
Model Specification						
Form: Reduced	-0.003	0.011	0.072	-0.021	0.020	0.293
Model: Static	-0.038	0.034	**0.626**	-0.052	0.023	**0.021**
Model: RE	-0.070	0.089	0.423	-0.146	0.060	**0.016**
Model: ARDL	0.101	0.048	**0.887**	0.107	0.035	**0.002**
Model: LE	0.173	0.025	**1.000**	0.151	0.028	**0.000**
Estimation Technique						
Estimation: GMM	0.000	0.005	0.007	0.017	0.037	0.652
Estimation: OLS	0.000	0.003	0.011	-0.014	0.017	0.410
Estimation: 2SLS	0.015	0.030	0.233	0.045	0.028	0.103
Estimation: 3SLS	-0.201	0.048	**1.000**	-0.228	0.048	**0.000**
Estimation: IV	-0.009	0.024	0.139	-0.067	0.030	**0.024**
Function Specification						
Function: Linear	0.083	0.022	**0.989**	0.078	0.025	**0.002**
Function: Double-log	0.000	0.002	0.005	0.005	0.018	0.765
Publication Characteristics						
Year of publication	0.001	0.001	0.199	0.003	0.001	**0.007**
Impact Factor	0.018	0.028	0.351	0.056	0.020	**0.006**
Citations (t)	0.040	0.011	**0.977**	0.029	0.009	**0.001**

Notes: This table presents the results of the Bayesian model averaging and Frequentist model averaging (n=4402). Post. mean = Posterior Mean, Post. SD = Posterior Standard Deviation, PIP = Posterior Inclusion Probability, Coef. = Coefficient, SE = Standard Error. TN citations = transformed number of citations, LE = Lagged endogenous. The variables with PIP > 0.5 are highlighted in bold.

One observation to highlight is the insignificance of both average and marginal prices in determining the estimated elasticities. This might contradict many authors of the electricity literature, who argue that people respond rather to average and not marginal price (Ito 2014; Shaffer 2020). Moreover, we find that decreasing tariffs has the highest effect on price elasticity and the other tariffs are not useful in explaining the elasticities' heterogeneity. It is interesting to see that the time-of-use tariff apparently does not explain the estimated effect, especially as there is extensive literature convinced otherwise (Holland & Mansur 2008; Torriti 2020). As per demand controls, only controlling for demographics and substitute fuels seems to be useful. Stock as insignificant similarly to results of Zabaloy & Viego (2022). Contrary to the findings of Holtedahl & Joutz (2004), our analysis indicates that temperature controls, whether through Cooling Degree Days or Heating Degree Days, do not significantly alter the elasticity estimates. Lastly, we note that only *Citations (t)* from the Publication Characteristics group affect positively and significantly the estimated effects.

Generally, the results of BMA and FMA concerning the direction of the effect of individual variables coincide. We note that all variables exerting significant explanatory power in BMA are also significant in the frequentist model averaging with a p-value below 0.05 and moreover, affect price elasticity in the same direction. One slight inconsistency is that FMA includes more variables as significant, such as *USA, Model: RE, Year of publication, Estimation: IV* and *Impact Factor*. These variables are, however, significant for the model averaging Marshallian elasticity subsample. The results can be found in Appendix A.

In our analysis, we have categorized the sample into short-run, intermediate-run, and long-run segments for detailed estimation. As an additional layer of verification, we present the results of the model averaging for short-run elasticity in the Appendix in Table A.13. The goal was to explore whether the TOU tariff is helpful in explaining at least short-run effects, which is not the case. The Appendix also includes model averaging analysis focused on Marshallian and Hicksian price elasticities. Additionally, we explore various models and g-prior specifications for the full sample, with the outcomes of these assessments available in Appendix A. For a visual representation of the robustness across these categorizations, we present the figure showcasing the posterior inclusion probabilities for each model, presented below in Figure A.14. The model inclusion posterior probabilities are close to identical for the four specifications.

Chapter 6

Best Practice Estimate

In the last chapter of this thesis, we aim to create a best practice estimate as recommended by Irsova *et al.* (2023b) based on the results of Bayesian Model Averaging. Ultimately, we also present cross-country implied elasticities.

Constructing best practice estimates incorporates using the coefficients obtained from the model averaging and including the variables that epitomize the ideal scenario for the elasticity estimation. One constraint of this procedure is its subjectivity. For instance, we decided to include one variable based on the performance in BMA, which can be dependent on the prior beliefs we assigned or preliminary tests conducted. Therefore, this practice should be perceived as an additional robustness check rather than an independent and novel finding.

We initially set the value of respective variables to their sample means. There are a few specific values that should be set otherwise. To correct for publication bias, we set the value of the standard error to 0. Furthermore, we set the journal impact factor and citations characteristics to the maximum values, as they are assumed to represent the most reliable methodology and hence present credible findings. We decide to employ panel data due to greater information attainment. For short-run results, we naturally set the value of the short-run to 1 and the long-run value to 0 (and vice versa for long-run).

For the short-run, our best practice estimate (-0.116) is borderline insignificant based on the confidence interval and half the sample average (-0.231). Interestingly, the effect corresponds to the elasticity obtained using IV regression in Table 4.3. Furthermore, the long-run estimate is -0.303. The results are slightly below those reported by Labandeira *et al.* (2017) and almost half the estimates from Zhu *et al.* (2018). Horáček (2014) reports a lower short-run elasticity of -0.06 but a higher long-run elasticity of -0.430. We remind that the comparison to selected studies is also present in Table 2.1. We also estimate results for the European subsample and experimental design, with both of them being insignificant.

Cross-country implied elasticities are also estimated. Admittedly, the robustness of the results can be limited by the low number of elasticities reported for a specific country. The estimates oscillate around our best practice estimate apart from Norway, Pakistan and Japan. The results for Norway and Pakistan should be taken with slight reservation due to the low number of observations, which is below 20. For Japan, we collected 185 elasticities and many of them were elastic, which might substantiate our significant estimate of -0.200. This would imply that in Japan, if the price increases by 1%, the electricity demand decreases by 0.2%. Comparison to sample values for countries can be found in Figure A.9 in Appendix A.

Table 6.1: Best practice estimates

	Short-run		*Long-run*	
	PE	CI	PE	CI
Author	**-0.116**	(-0.243; 0.011)	**-0.303**	(-0.438; -0.168)
Europe	-0.125	(-0.260; 0.010)	-0.312	(-0.453; -0.171)
Experimental design	0.041	(-0.084; 0.166)		
Cross-country implied elasticities				
Bangladesh	-0.078	(-0.260; 0.104)	-0.276	(-0.458; -0.094)
Brazil	-0.117	(-0.321; 0.087)	-0.322	(-0.528; -0.116)
Canada	-0.084	(-0.209; -0.041)	-0.282	(-0.417; -0.147)
China	-0.104	(-0.286; 0.078)	-0.309	(-0.497; -0.121)
France	-0.080	(-0.319; 0.159)	-0.278	(-0.521; -0.035)
Germany	-0.086	(-0.243; 0.071)	-0.287	(-0.450; -0.124)
India	-0.074	(-0.237; 0.089)	-0.301	(-0.468; -0.134)
Italy	-0.085	(-0.310; 0.140)	-0.286	(-0.511; -0.061)
Japan	-0.200	(-0.341; -0.059)	-0.395	(-0.542; -0.248)
Mexico	-0.083	(-0.267; 0.101)	-0.288	(-0.476; -0.100)
Norway	-0.246	(-0.432; -0.060)	-0.473	(-0.665; -0.281)
Pakistan	-0.240	(-0.418; -0.062)	-0.448	(-0.626; -0.270)
South Korea	-0.120	(-0.294; 0.054)	-0.290	(-0.468; -0.112)
Switzerland	-0.127	(-0.309; 0.055)	-0.325	(-0.513; -0.137)
UK	-0.082	(-0.286; -0.122)	-0.280	(-0.490; -0.070)
USA	-0.106	(-0.235; -0.023)	-0.293	(-0.430; -0.156)

Notes: The table presents calculated cross-country implied elasticities. We employed our base BMA model with additional country dummies. We also construct a 95% confidence interval by employing OLS and incorporating clustered standard errors at the study level. The cross-country effect was obtained by including a specific country dummy variable in the original model averaging estimation. Then, we set specific country dummy variables to 1 or 0 and obtain a best practice estimate. Other characteristics (income level, population, EU dummy) were also modified if applicable.

Additionally, we compare the results of our subjective best practice esti-

Table 6.2: Best practice estimates from literature

Alberini & Filippini (2011a)	-0.373* (-0.459; -0.287)	0.068 (-0.055; 0.191)	**Ito (2014)**
Wolak (2011)	-0.102 (-0.216; 0.120)	0.084 (-0.034; 0.202)	**Shaffer (2020)**

Notes: The table presents calculated estimates for the best practice estimate derived from four distinct studies. We also construct a 95% confidence interval by employing OLS and incorporating clustered standard errors at the study level. Results denoted by asterisks (*) are significant on the 5% level.

mate to three other studies. Firstly, we chose a study by Ito (2014). This study is a natural experiment published in the most prestigious journal in our dataset (*American Economic Review*) and attracted one of the highest number of citations. We also present an estimate based on the paper by Wolak (2011), published in (*American Economic Review*), who examined consumers' responses to hourly pricing. Thirdly, we include another experiment conducted by Shaffer (2020) examining non-linear pricing. This study was published in one of the best journals of our dataset *(American Economic Journal)* and is among the most recent ones. Lastly, we decided to include a non-experimental study with a relatively higher number of citations and a recent methodology. Therefore, we also computed the best practice estimate for Alberini *et al.* (2011), who address the endogeneity of their model estimating aggregate elasticities in the US. Overall, we focused on primary studies published after 2000 due to advancements in methodology and data availability. As a result, we omitted the best practice estimate for the most cited paper (Hausman 1979) in our dataset.

The general pattern is clear, two of the experiments have perhaps surprisingly positive elasticities. This outcome generally aligns with the results presented in the Section 4.3, in which the elasticities corrected for publication bias of natural experiments were much lower compared to the non-experimental studies. We completed more best practice estimates and the theme persisted, non-experimental studies yielded significantly negative (but inelastic) estimates and experimental studies estimated mostly positive and seldom statistically significant effects. The calculation of the baseline model is included in the dataset attached. Further research could potentially seek to describe what experiment characteristics (hourly vs. monthly data, smart-home features controls etc.) systematically affect the best practice estimates as they vary quite a bit, but this issue is beyond the scope of this thesis.

The last table presented in this thesis concerns the sensitivity analysis of selected variables. We are interested in how a shift in one variable affects our estimate, ceteris paribus. The first column denotes change by one standard deviation and the second column presents what is the effect of a minimum to maximum change (or vice versa). The results of our sensitivity analysis show

Table 6.3: Sensitivity analysis of selected variables

	One SD change		Maximum change	
Response variable:	Effect of PE	% of BP	Effect on PE	% of BP
SE of the estimate	-0.203	175.38%	-1.375	1185.51%
Experiment	0.053	-45.65%	0.193	-166.14%
Estimate: Short-run	0.023	-19.59%	0.046	-39.63%
Estimate: Long-run	-0.067	57.88%	-0.173	149.13%
Daylight hours	0.031	-26.69%	0.361	-310.97%
Type: Residential	0.054	-46.96%	0.112	-96.5%
Data: Panel	-0.031	26.55%	-0.062	53.14%
Data: Cross-section	-0.073	63.1%	-0.231	199.35%
Granularity: Yearly	-0.033	28.45%	-0.074	63.39%
Tariff: Decreasing	-0.043	36.91%	-0.140	120.4%
Control: Demographics	-0.046	39.81%	-0.099	85.54%
Control: Fuels	-0.039	33.96%	-0.080	68.82%
Model: Static	-0.017	14.8%	-0.038	32.52%
Model: ARDL	0.027	-23.6%	0.101	-87.46%
Model: LE	0.073	-63.07%	0.173	-149.22%
Estimation: 3SLS	-0.032	27.24%	-0.201	172.94%
Function: Linear	0.032	-27.88%	0.083	-71.2%
Citations (t)	0.042	-36.2%	0.183	-158.02%

Notes: This table outlines the isolated impact of select variables on the price elasticity estimate. Included are only variables that exhibited a Posterior Inclusion Probability (PIP) greater than 0.5 within the Bayesian model averaging (BMA) framework. The term 'One SD change' refers to the variation in the PE when a specific variable is altered by one standard deviation from its mean. 'Maximum change' denotes the shift in the PE when the variable shifts from its lowest to highest value. The benchmark best practice value used for comparison is -0.116. Here, SD stands for Standard Deviation, PE for Price Elasticity, and BP for Best Practice. More information concerning the variables can be found in Table 5.1.

that there are 10 variables with a negative effect on price elasticity and 8 variables positively influencing price elasticity. Firstly, we note that the publication bias (represented by the standard error) has a large effect on the elasticity. One standard deviation change in the error could decrease our best practice estimate (or increase in magnitude) by -0.203 and going from the minimum to maximum value of the standard error collected can make price elasticity relatively highly elastic. Secondly, other variables' effect on the elasticity estimate is in line with what we have seen so far. For example, estimating long-run elasticity, collecting yearly data or focusing on decreasing tariffs decreases the elasticity. Moreover, the inclusion of demographics and fuel controls lowers the elasticity, too.

On the other hand, experimental design, and residential electricity demand characteristics have all positive effects on the elasticity. The effect on residential demand might be a bit surprising, as we have estimated residential elasticity in the most elastic based on linear and non-linear tests presented in Appendix A in Table A.5. However, to be fair, we have shown in Chapter 5 that there is no consensus among authors regarding differences in elasticity for various consumer groups. One other important point to note is that is it mostly residential demand analysis which incorporates various (demographic, fuel) controls, which subsequently negatively affect the elasticity. The result for the residential demand should be hence taken with reservation. Ultimately, one quantitative observation is that if we increase the transformed number of citations by one SD (1.001, which corresponds very roughly to 150 citations), the price elasticity increases by 0.042.

Chapter 7

Conclusion

Despite the statement that it appears that price elasticities of electricity are "like snowflakes, no two are alike" (Dahl 1993), this thesis aims to assess the presence of various biases and to provide a systematic overview of characteristics that significantly influence the effects. We collect 4521 estimates from 413 studies.

Expanding the works of Horáček (2014) and Zhu *et al.* (2018), we analysed the presence of publication bias and p-hacking in greater detail using various statistical tests. The tests conclude that short-run elasticity estimates lie generally between -0.12 and -0.07, and both intermediate-run and long-run elasticities oscillate from -0.38 to -0.34, which is well below their sample average values. Various tests also suggest a significant presence of both publication bias and p-hacking. This is the first thesis to deal with endogeneity bias. We find that elasticities from natural experiments are much lower (-0.07 to -0.05) than non-experimental studies (around -0.3). Tests of non-experimental studies with methodology controlling for endogeneity report relatively higher elasticity than studies non-addressing the endogeneity at all, however, the differences are negligible. All tests performed indicate that price elasticity corrected for publication bias is inelastic, i.e. lower than 1 in absolute value (even -0.5, for that matter).

Consequently, we define more than 100 variables and after preliminary tests include 43 in the Bayesian and Frequentist model averaging. Similarly to previous tests, we also find strong publication selection bias. Experimental design, type of data collected, type of electricity demand and study characteristics are among the most significant influences on price elasticities. Both natural and quasi-experiments, short-run elasticity, daylight hours, residential demand, auto-regressive distributed lag and lagged endogenous model and linear function specification have a positive relationship with the elasticities. In terms of publication characteristics, the number of citations positively affects the estimated elasticities. On the contrary, we found a negative relationship between elasticity and the following variables: long-run estimates, both panel and cross-sectional data, decreasing tariff, demographics and fuel controls, static model and 3SLS estimation. Given the general framework that price elasticity should be negative, the variables with positive relationships make the consumers' response less elastic. Generally, there are no grave surprises in the estimation. One slight ex-ante expectation could be that the average price would have a higher inclusion probability as has been argued by multiple papers (Borenstein 2009; Ito 2014). There seems to be no systematic difference between disaggregated data and data on the country or regional level. We also provide a number of robustness checks, be it publication bias or model averaging estimation for subsets of data (demand type, type of elasticities).

We conclude the thesis with our best practice elasticity estimate, which is -0.116 for the short-run and -0.303 for the long-run, respectively. We also present a sensitivity analysis to quantify the ceteris paribus changes in selected variables, including the effects of publication selection bias, which exaggerates the estimates and even can make the price elasticity of electricity elastic.

Ultimately, we acknowledge the possible limitations of our study and present the possibility of future research focus. It is likely that the potential of the dataset is not fulfilled by the publication and endogeneity bias analysis conducted in the thesis. The true elasticities might vary for different subsamples, such as type of electricity demand and more granular geographical areas (such as country instead of continental level). Furthermore, a more thorough examination of different elasticity types (Marshallian and Hicksian demand) could be of future interest, too.

Exploring variations in price elasticities along different delivery streams, as suggested by Dahl (1993), remains an underexplored area due to limited data segmentation in existing studies. Additionally, some research has begun to estimate price elasticities across various income quantiles and there is intuitive merit in investigating whether higher-income households exhibit more price elasticity compared to their lower-income counterparts, as reported in Gundimeda & Kohlin (2008). Moreover, Volland & Tilov (2018) focus on appliance-specific elasticities, which can help policymakers address electricity conservation policies more efficiently. In a similar vein, Chaudhry (2010) estimated price elasticities of electricity for various industrial subsectors, finding positive price elasticities for chemical and leather subsectors. It is likely that the heterogeneity of consumer responsiveness in the industrial sector will vary widely. Lastly, with the increasing shift towards renewable and nuclear energy sources, examining how electricity price elasticities differ based on the energy mix could provide critical insights for future energy policies. These areas might present fruitful avenues for further research that builds on the groundwork laid in the thesis.

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Appendix A

Data

Table A.1: Full statistics summary (Part 1)

Variable Name	Sample Mean	CI	Weighted Mean	WM CI	n
All Data	-0.395	(-1.342; 0.552)	-0.417	(-1.364; 0.530)	4521
Observations (n) $\geq$ 608	-0.383	(-1.285; 0.519)	-0.430	(-1.332; 0.472)	2263
Observations (n) $<$ 608	-0.408	(-1.396; 0.580)	-0.409	(-1.397; 0.579)	2258
If appropriate transformation is used	-0.385	(-1.310; 0.540)	-0.409	(-1.334; 0.516)	4501
t-statistic $\geq$ -2.68	-0.230	(-1.083; 0.623)	-0.235	(-1.088; 0.618)	2263
t-statistic $<$ -2.68	-0.561	(-1.484; 0.362)	-0.560	(-1.483; 0.363)	2258
Standard error $\geq$ 0.086	-0.544	(-1.679; 0.591)	-0.588	(-1.723; 0.547)	2261
Standard error $<$ 0.086	-0.247	(-0.825; 0.331)	-0.252	(-0.830; 0.326)	2260
Experiment	-0.129	(-0.527; 0.269)	-0.180	(-0.578; 0.218)	362
P value	-0.347	(-1.258; 0.564)	-0.369	(-1.280; 0.542)	381
Estimate: Short-run	-0.231	(-0.823; 0.361)	-0.247	(-0.839; 0.345)	1866
Estimate: Intermediate-run	-0.502	(-1.533; 0.529)	-0.495	(-1.526; 0.536)	1842
Estimate: Long-run	-0.532	(-1.700; 0.636)	-0.624	(-1.792; 0.544)	813
Type: Marshall	-0.398	(-1.327; 0.531)	-0.414	(-1.343; 0.515)	3326
Type: Hicks	-0.385	(-1.371; 0.601)	-0.420	(-1.406; 0.566)	1176
Type: other	-0.548	(-1.932; 0.836)	-0.465	(-1.849; 0.919)	20
Start year $\geq$ 1977	-0.390	(-1.327; 0.547)	-0.440	(-1.377; 0.497)	2315
Start year $<$ 1977	-0.401	(-1.357; 0.555)	-0.398	(-1.354; 0.558)	2206
End year $\geq$ 1991	-0.348	(-1.222; 0.526)	-0.390	(-1.264; 0.484)	2448
End year $<$ 1991	-0.452	(-1.465; 0.561)	-0.445	(-1.458; 0.568)	2073
Mid year $\geq$ 2000	-0.389	(-1.249; 0.471)	-0.410	(-1.270; 0.450)	1166
Mid year $<$ 2000	-0.398	(-1.372; 0.576)	-0.418	(-1.392; 0.556)	3355
Number of years$\geq$ 14.5	-0.322	(-1.186; 0.542)	-0.354	(-1.218; 0.510)	2262
Number of years $<$ 14.5	-0.470	(-1.472; 0.532)	-0.473	(-1.475; 0.529)	2259
Daylight hours $\geq$ 14.767	-0.386	(-1.333; 0.561)	-0.404	(-1.351; 0.543)	3100
Daylight hours $<$ 14.767	-0.415	(-1.358; 0.528)	-0.433	(-1.376; 0.510)	1421
Annual temperature $\geq$ 9.146	-0.355	(-1.235; 0.525)	-0.381	(-1.261; 0.499)	2290
Annual temperature $<$ 9.146	-0.437	(-1.441; 0.567)	-0.462	(-1.466; 0.542)	2231
Electricity exporter dummy	-0.398	(-1.345; 0.549)	-0.430	(-1.377; 0.517)	3738
Carbon intensity production $\geq$ 6.124	-0.394	(-1.327; 0.539)	-0.397	(-1.330; 0.536)	3307
Carbon intensity production $<$ 6.124	-0.399	(-1.381; 0.583)	-0.464	(-1.446; 0.518)	1214
Population (log) $\geq$ 19.096	-0.358	(-1.240; 0.524)	-0.396	(-1.278; 0.486)	2263
Population (log) $<$ 19.096	-0.433	(-1.435; 0.569)	-0.432	(-1.434; 0.570)	2258
Income level (log) $\geq$ 9.012	-0.350	(-1.236; 0.536)	-0.384	(-1.270; 0.502)	2261
Income level (log) $<$ 9.012	-0.441	(-1.437; 0.555)	-0.439	(-1.435; 0.557)	2260
Hottest months	-0.213	(-0.856; 0.430)	-0.247	(-0.890; 0.396)	68
Coolest months	-0.427	(-1.632; 0.778)	-0.367	(-1.572; 0.838)	78
USA	-0.395	(-1.293; 0.503)	-0.389	(-1.287; 0.509)	2151
Europe	-0.410	(-1.394; 0.574)	-0.432	(-1.416; 0.552)	833
Other location	-0.391	(-1.373; 0.591)	-0.427	(-1.409; 0.555)	1598
Aggregation: Country	-0.289	(-1.116; 0.538)	-0.307	(-1.134; 0.520)	1224
Aggregation: Region	-0.397	(-1.238; 0.444)	-0.443	(-1.284; 0.398)	1082
Aggregation: City	-0.387	(-1.334; 0.560)	-0.393	(-1.340; 0.554)	654
Aggregation: Disaggregated	-0.523	(-1.597; 0.551)	-0.552	(-1.626; 0.522)	1099
Type: Residential	-0.355	(-1.337; 0.627)	-0.379	(-1.361; 0.603)	1710
Type: Commercial	-0.248	(-0.997; 0.501)	-0.309	(-1.058; 0.440)	884
Type: Industrial	-0.413	(-1.338; 0.512)	-0.417	(-1.342; 0.508)	2893
Demand: Peak	-0.256	(-1.030; 0.518)	-0.360	(-1.134; 0.414)	269
Demand: Mid-peak	-0.190	(-0.615; 0.235)	-0.186	(-0.611; 0.239)	108
Demand: Off-peak	-0.432	(-1.473; 0.609)	-0.713	(-1.754; 0.328)	83

...continued on the next page

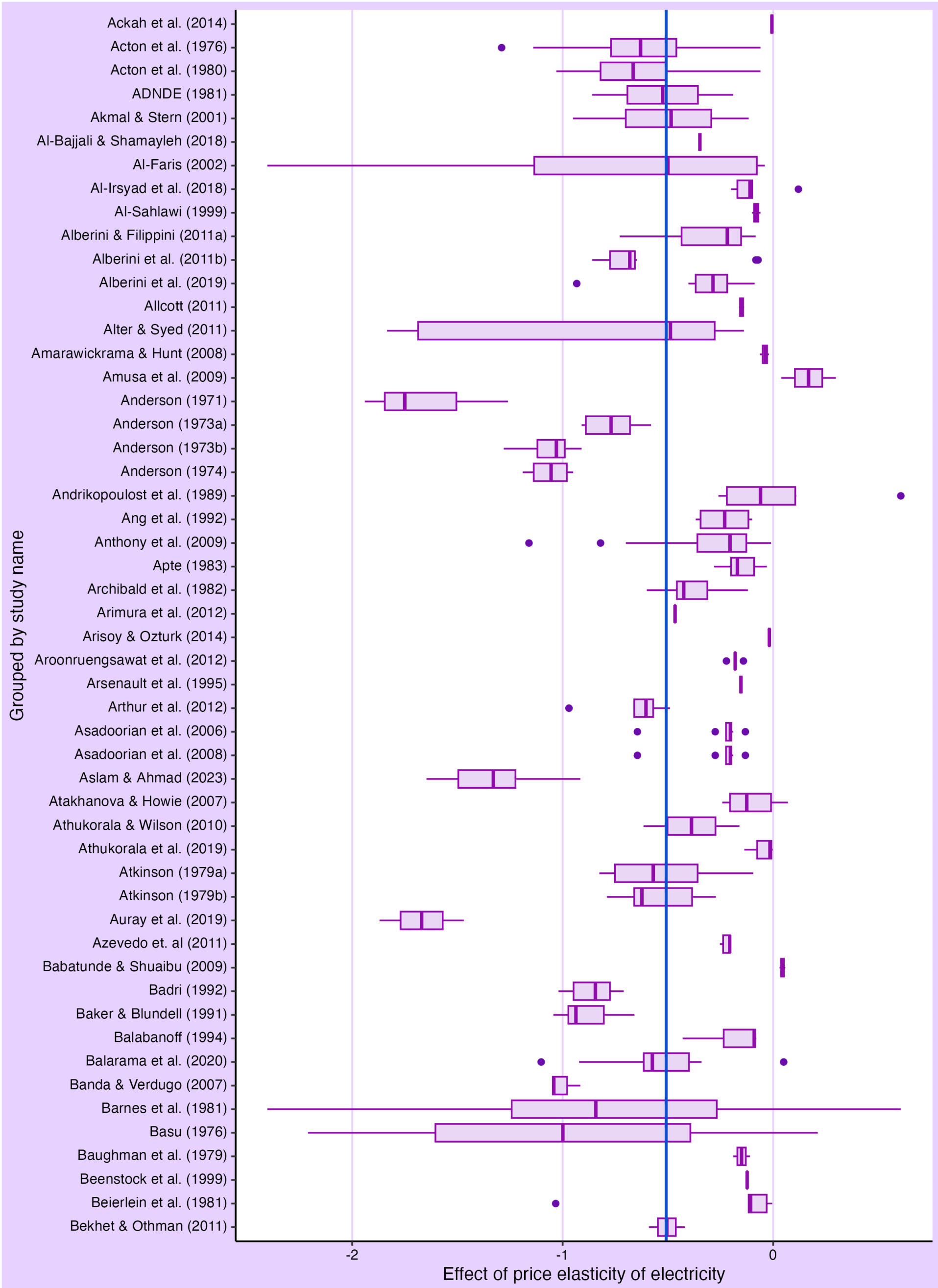
Notes: We present the full summary statistics table. Note that some of the variables are grouped but do not add up to 4521 as the effect of NA column was omitted.

Table A.2: Full statistics summary (Part 2)

Variable Name	Sample Mean	CI	Weighted Mean	WM CI	n
Data: Panel	-0.389	(-1.318; 0.540)	-0.418	(-1.347; 0.511)	2290
Data: Time-series	-0.328	(-1.226; 0.570)	-0.343	(-1.241; 0.555)	1718
Data: Cross-section	-0.652	(-1.667; 0.363)	-0.649	(-1.664; 0.366)	513
Granularity: Yearly	-0.436	(-1.443; 0.571)	-0.455	(-1.462; 0.552)	3291
Granularity: Quarterly	-0.316	(-1.208; 0.576)	-0.393	(-1.285; 0.499)	126
Granularity: Monthly	-0.302	(-1.021; 0.417)	-0.289	(-1.008; 0.430)	948
Price: Average	-0.409	(-1.315; 0.497)	-0.420	(-1.326; 0.486)	2385
Price: Marginal	-0.430	(-1.435; 0.575)	-0.446	(-1.451; 0.559)	957
Price: Lump sum	-0.302	-	-0.302	-	1
Price: Shin	-0.151	(-0.408; 0.106)	-0.137	(-0.394; 0.120)	11
Price: Other	-0.340	(-1.347; 0.667)	-0.392	(-1.399; 0.615)	453
Tariff: Increasing	-0.350	(-1.079; 0.379)	-0.359	(-1.088; 0.370)	565
Tariff: Decreasing	-0.625	(-1.693; 0.443)	-0.474	(-1.542; 0.594)	462
Tariff: Flat	-0.495	(-1.491; 0.501)	-0.530	(-1.526; 0.466)	119
Tariff: TOU	-0.283	(-1.153; 0.587)	-0.375	(-1.245; 0.495)	559
Tariff: Undefined	-0.420	(-1.398; 0.558)	-0.428	(-1.406; 0.550)	1058
Control: Demographics	-0.484	(-1.431; 0.463)	-0.516	(-1.463; 0.430)	1515
Control: Temperature	-0.366	(-1.219; 0.487)	-0.381	(-1.234; 0.472)	2203
Control: Stocks	-0.479	(-1.375; 0.417)	-0.465	(-1.361; 0.431)	821
Control: Fuels	-0.427	(-1.399; 0.545)	-0.458	(-1.430; 0.514)	1844
Control: Income	-0.414	(-1.372; 0.544)	-0.429	(-1.387; 0.529)	2543
Form: Reduced	-0.407	(-1.340; 0.526)	-0.410	(-1.343; 0.523)	1873
Form: Structural	-0.370	(-1.293; 0.553)	-0.422	(-1.345; 0.501)	2386
Model: Dynamic	-0.327	(-1.184; 0.530)	-0.377	(-1.234; 0.480)	3100
Model: Static	-0.546	(-1.589; 0.497)	-0.501	(-1.544; 0.542)	1407
Lag: Dependent	-0.299	(-1.114; 0.516)	-0.347	(-1.162; 0.468)	1831
Lag: Other	-0.279	(-1.059; 0.501)	-0.345	(-1.125; 0.435)	442
Model: RE	-0.579	(-1.690; 0.532)	-0.487	(-1.598; 0.624)	63
Model: FE	-0.406	(-1.166; 0.354)	-0.379	(-1.139; 0.381)	436
Model: VAR	-0.519	(-1.865; 0.827)	-0.498	(-1.844; 0.848)	30
Model: ARDL	-0.284	(-1.097; 0.529)	-0.350	(-1.163; 0.463)	348
Model: ECM	-0.369	(-1.463; 0.725)	-0.389	(-1.483; 0.705)	217
Model: VECM	-0.263	(-1.276; 0.750)	-0.362	(-1.375; 0.651)	85
Model: DS	-0.445	(-1.464; 0.574)	-0.527	(-1.536; 0.492)	497
Model: LE	-0.254	(-1.026; 0.518)	-0.287	(-1.059; 0.485)	1025
Model: Other model	-0.282	(-0.933; 0.369)	-0.350	(-1.001; 0.301)	112
Estimation: ML	-0.302	(-1.178; 0.574)	-0.419	(-1.295; 0.457)	265
Estimation: GMM	-0.367	(-1.071; 0.337)	-0.300	(-1.004; 0.404)	176
Estimation: Error component	-0.398	(-1.443; 0.647)	-0.252	(-1.297; 0.793)	111
Estimation: OLS	-0.412	(-1.423; 0.599)	-0.447	(-1.458; 0.564)	1641
Estimation: GLS	-0.412	(-1.151; 0.327)	-0.418	(-1.157; 0.321)	220
Estimation: SUR	-0.359	(-1.237; 0.519)	-0.459	(-1.337; 0.419)	494
Estimation: 2SLS	-0.488	(-1.550; 0.574)	-0.400	(-1.462; 0.662)	440
Estimation: 3SLS	-0.431	(-1.554; 0.692)	-0.562	(-1.685; 0.561)	112
Estimation: IV	-0.367	(-1.114; 0.380)	-0.375	(-1.122; 0.372)	377
Estimation: other	-0.253	(-0.770; 0.264)	-0.391	(-0.908; 0.126)	100
Endogeneity: Control	-0.382	(-1.289; 0.525)	-0.371	(-1.278; 0.536)	1306
Endogeneity: No control	-0.401	(-1.363; 0.561)	-0.431	(-1.393; 0.531)	3215
Function: Linear	-0.329	(-1.160; 0.502)	-0.433	(-1.264; 0.398)	832
Function: Semi-log	-0.513	(-1.503; 0.477)	-0.344	(-1.334; 0.646)	231
Function: Double-log	-0.406	(-1.362; 0.550)	-0.412	(-1.368; 0.544)	2474
Function: Box-Cox	-0.453	(-1.235; 0.329)	-0.464	(-1.246; 0.318)	16
Function: non available	-0.397	(-1.387; 0.593)	-0.435	(-1.425; 0.555)	968
Publication Year >= 2000	-0.362	(-1.242; 0.518)	-0.406	(-1.286; 0.474)	2161
Publication Year < 2000	-0.427	(-1.423; 0.569)	-0.425	(-1.421; 0.571)	2375
Journal Impact Factor >= 0.061	-0.391	(-1.304; 0.522)	-0.409	(-1.322; 0.504)	2347
Journal Impact Factor < 0.061	-0.401	(-1.379; 0.577)	-0.426	(-1.404; 0.552)	2189
Citations (t) >= 1.128	-0.385	(-1.326; 0.556)	-0.383	(-1.324; 0.558)	2286
Citations (t) < 1.128	-0.407	(-1.356; 0.542)	-0.444	(-1.393; 0.505)	2250
Number of citations >= 50	-0.368	(-1.270; 0.534)	-0.422	(-1.324; 0.480)	1094
Number of citations < 50	-0.404	(-1.360; 0.552)	-0.415	(-1.371; 0.541)	3442

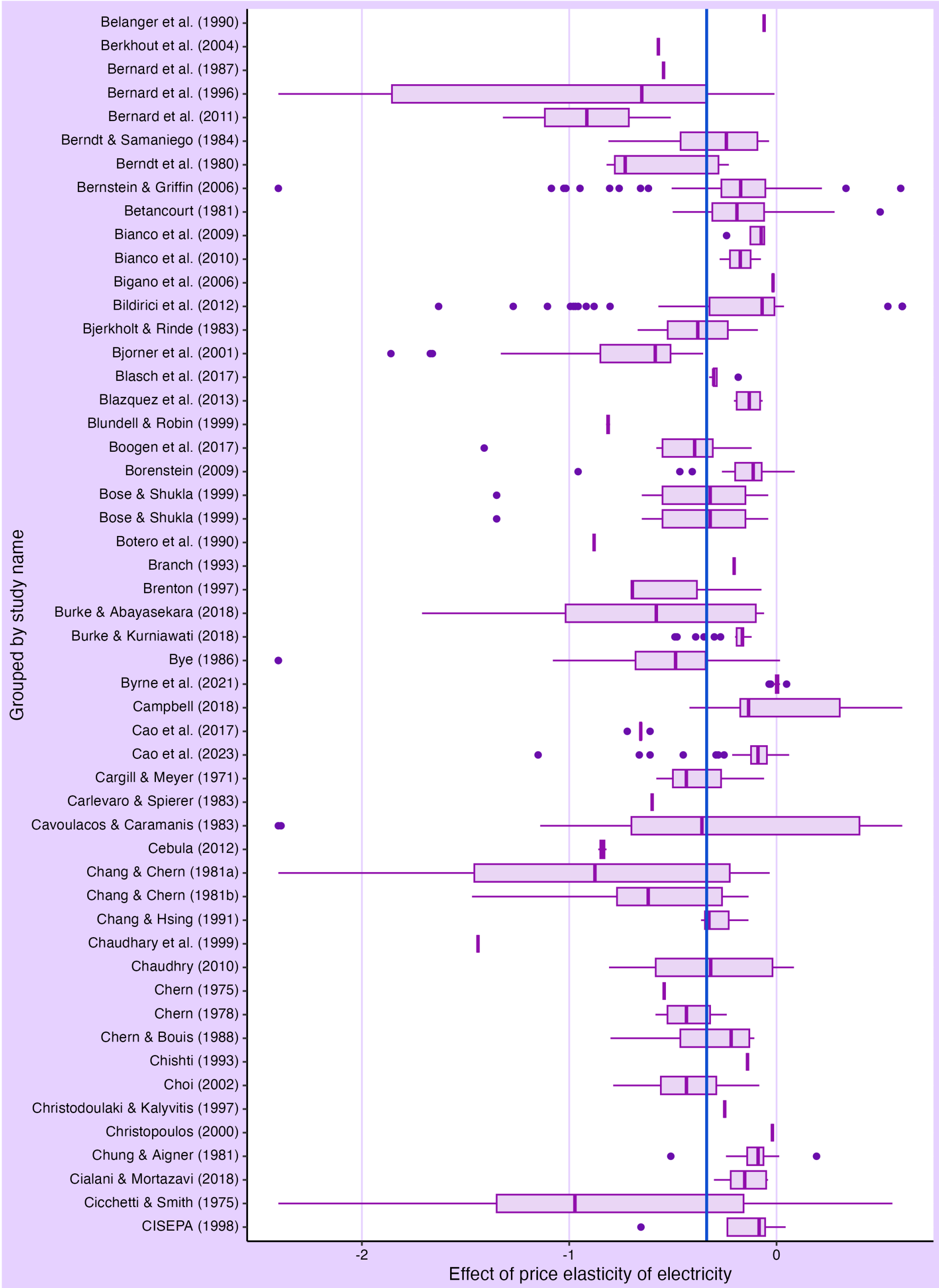
Notes: We present the full summary statistics table. Note that some of the variables are grouped but do not add up to 4521 as the effect of NA column was omitted. Endogeneity control included both experiments and studies employing estimation method accounting for endogeneity. *TOU* is time-of-use tariff.

Figure A.1: Variability of the estimated effect for all studies (Part 1)



Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

Figure A.2: Variability of the estimated effect for all studies (Part 2)

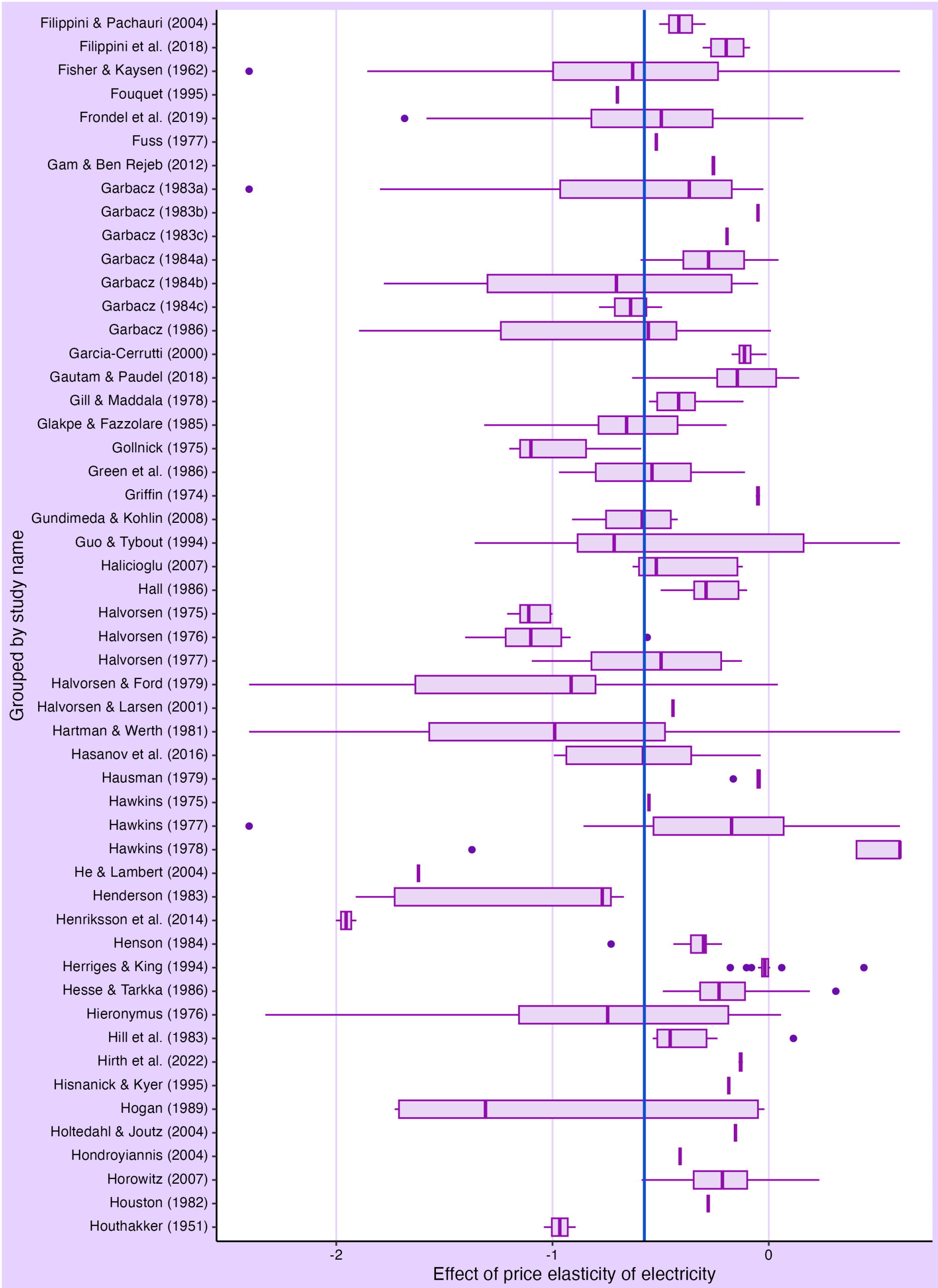


Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

Figure A.3: Variability of the estimated effect for all studies (Part 3)

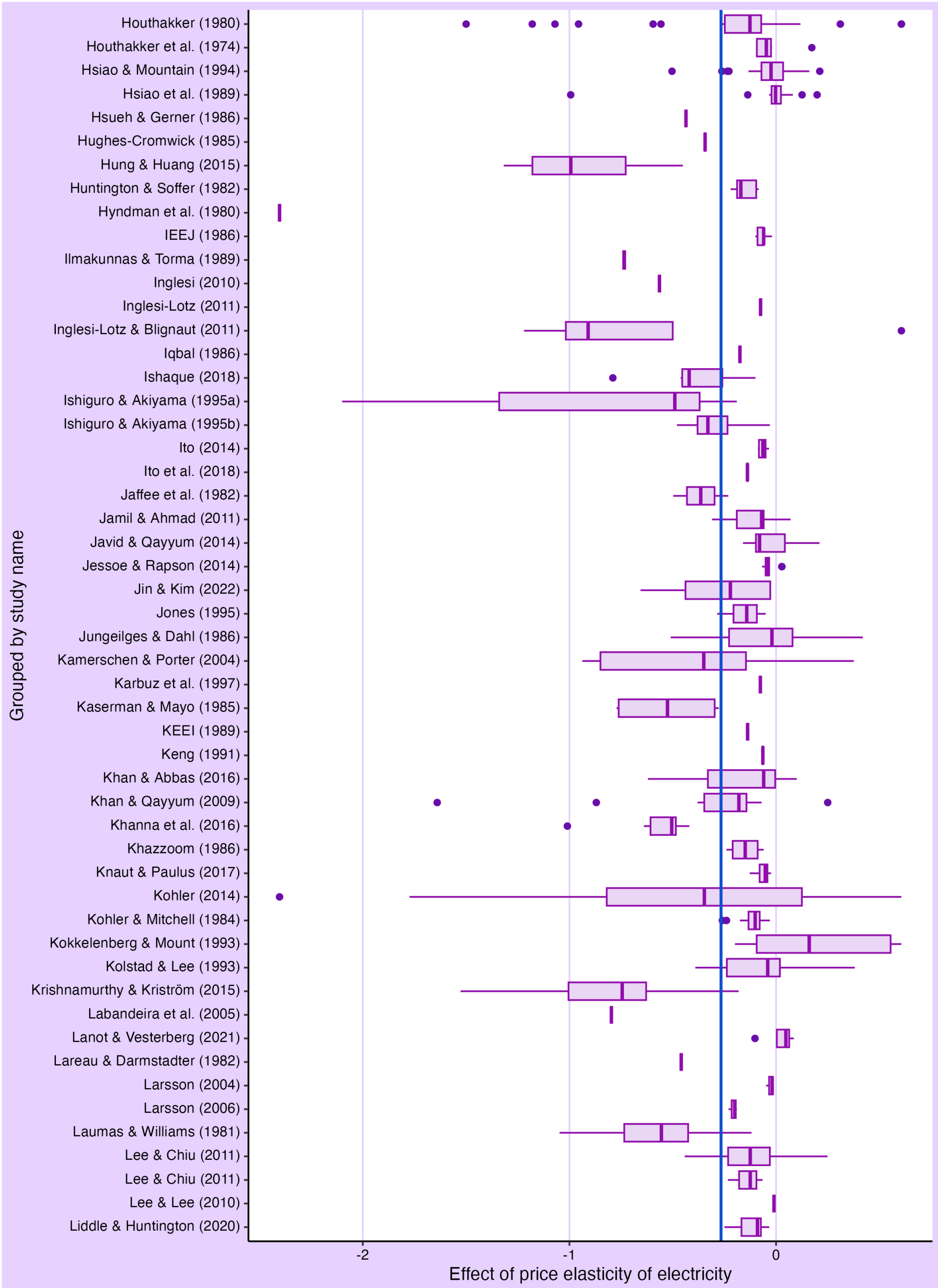
Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

Figure A.4: Variability of the estimated effect for all studies (Part 4)



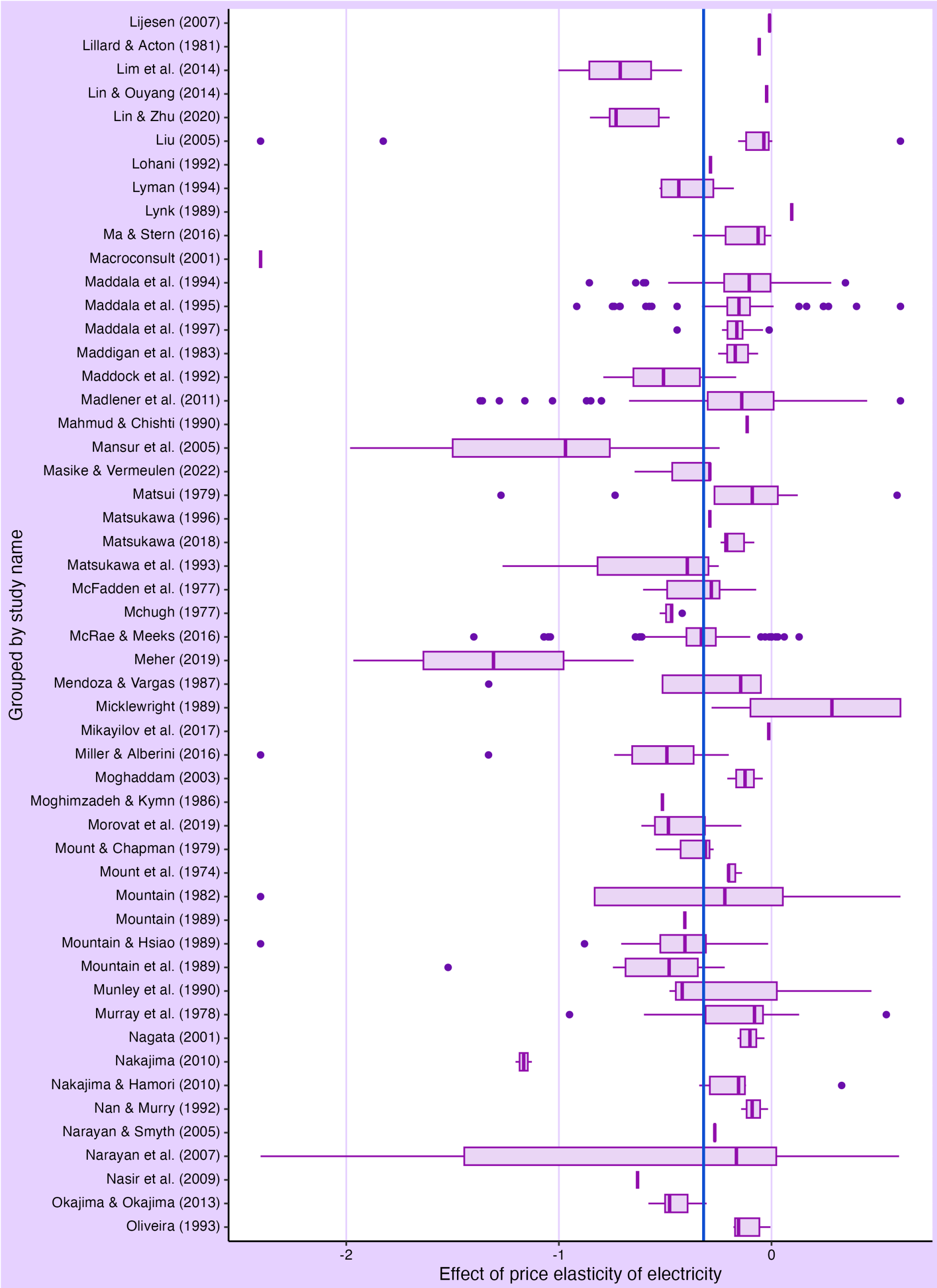
Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

Figure A.5: Variability of the estimated effect for all studies (Part 5)



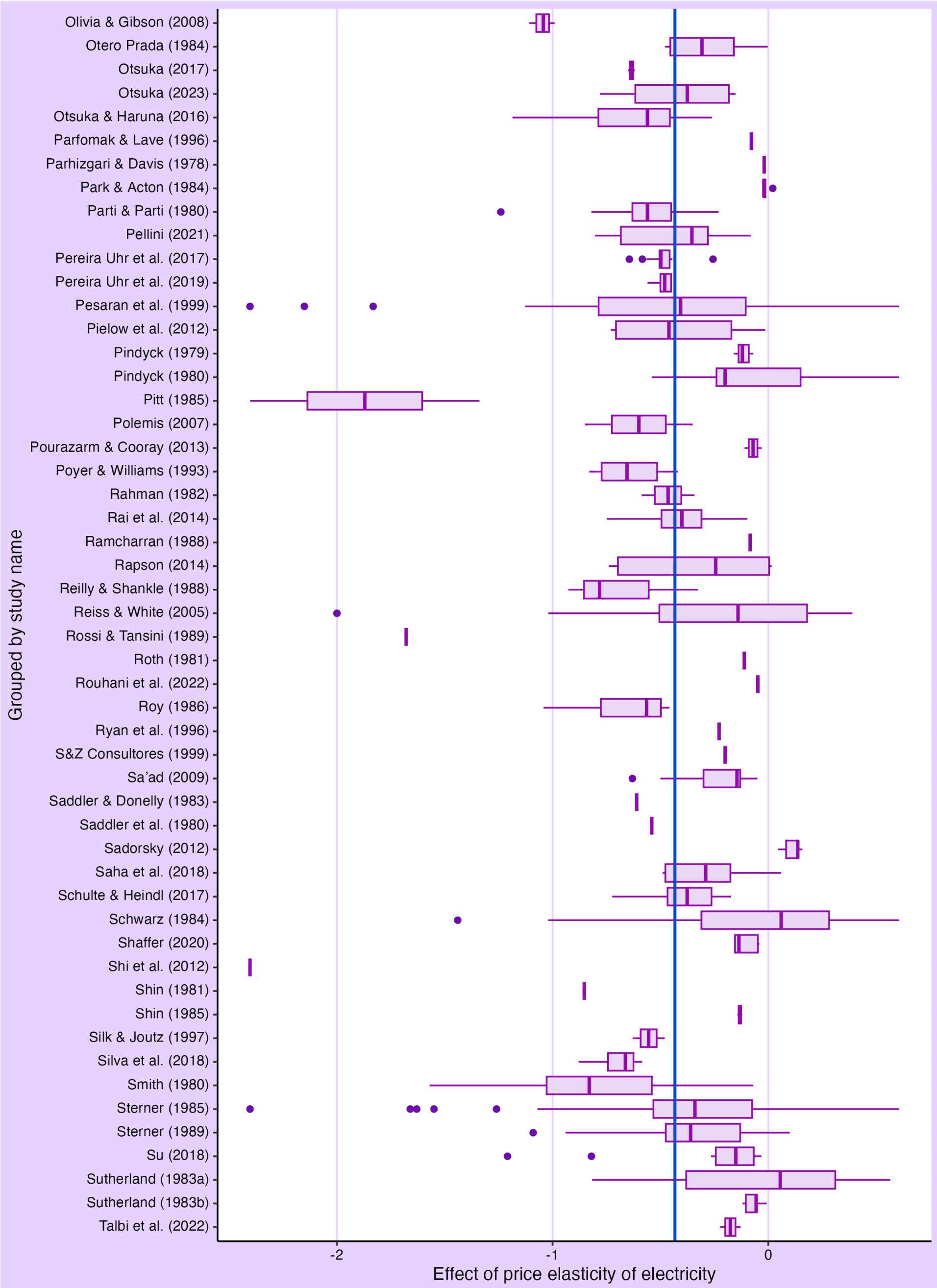
Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

Figure A.6: Variability of the estimated effect for all studies (Part 6)



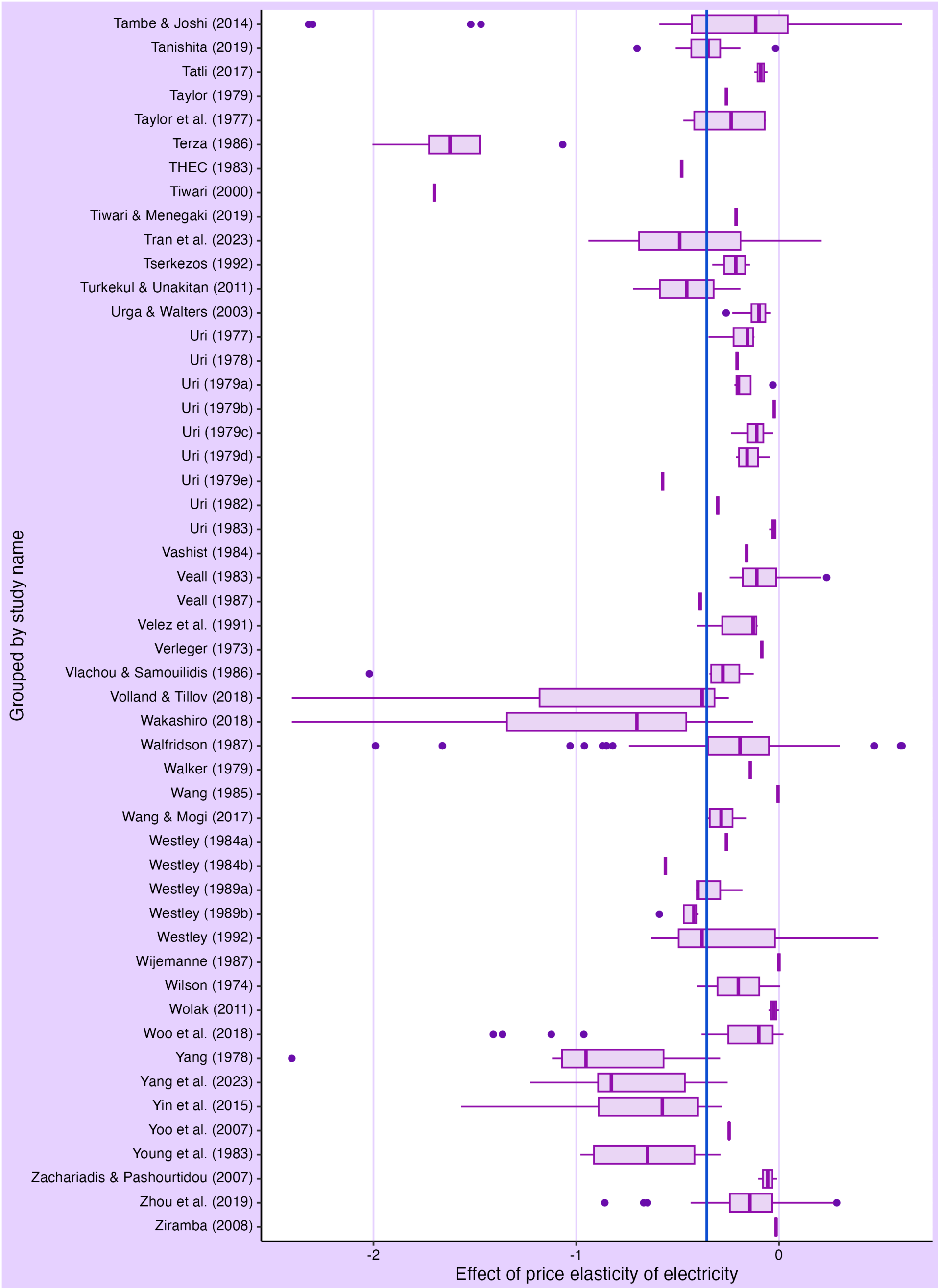
Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

Figure A.7: Variability of the estimated effect for all studies (Part 7)



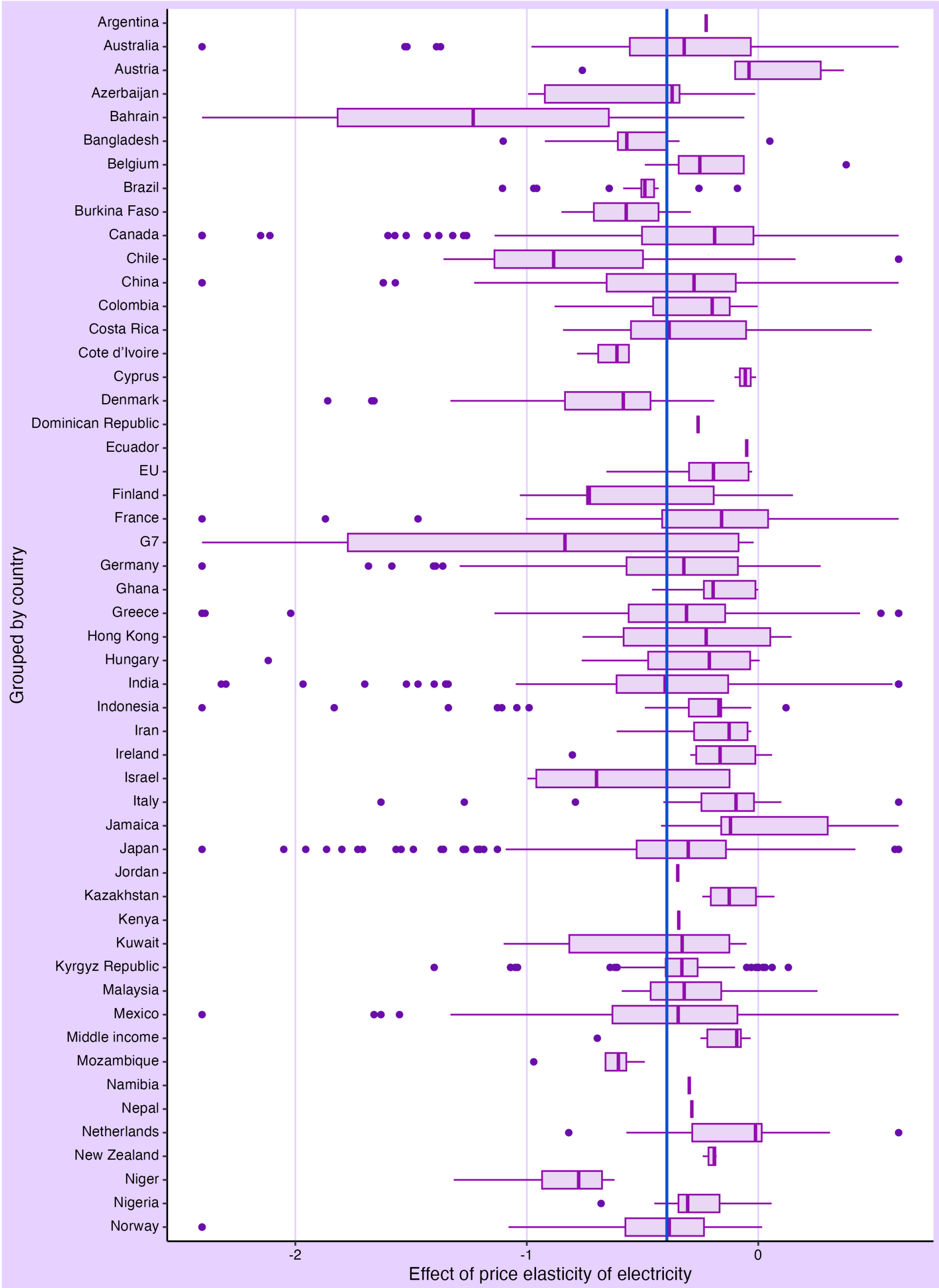
Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

Figure A.8: Variability of the estimated effect for all studies (Part 8)



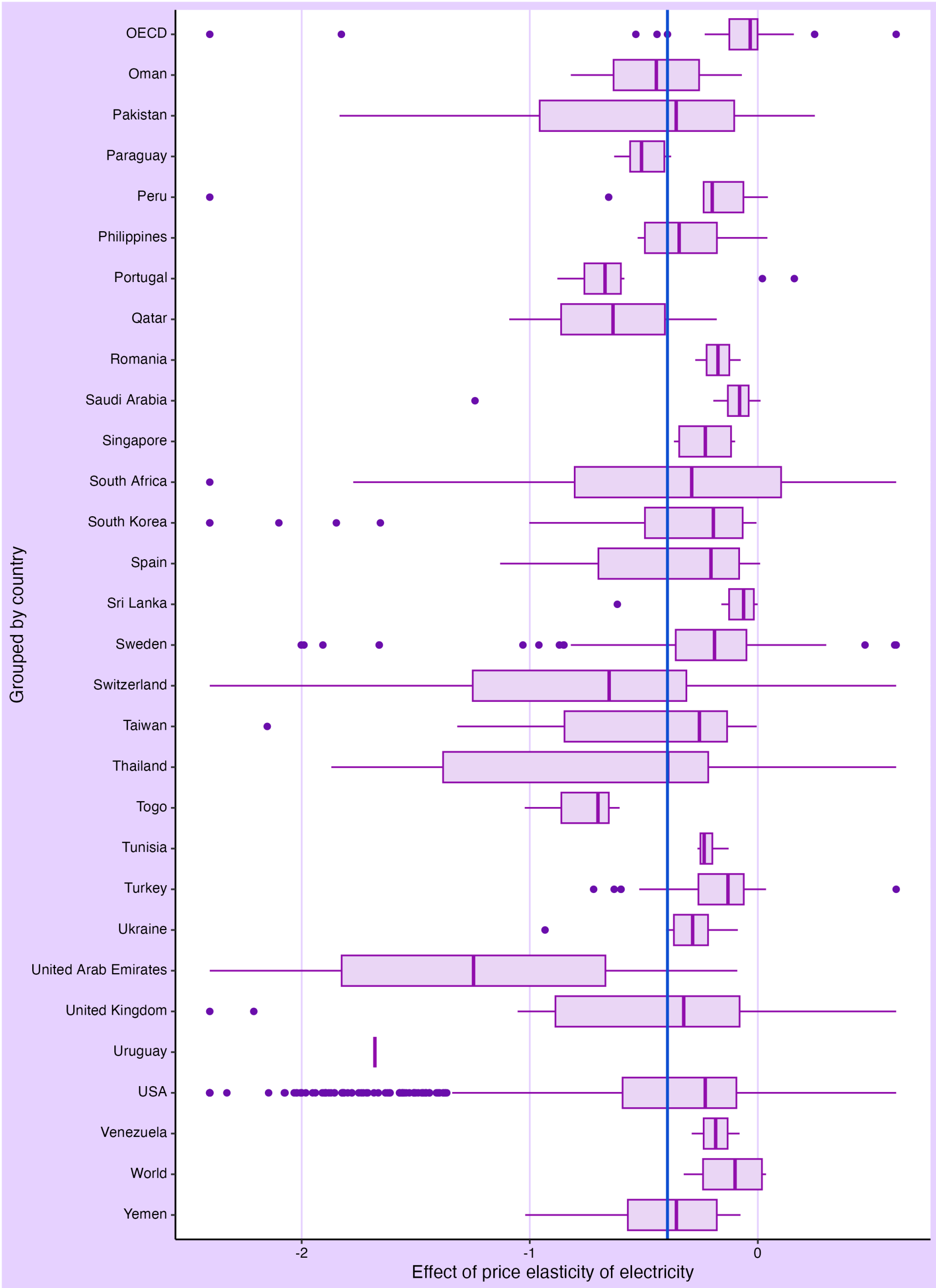
Notes: The figure shows a box plot of the price elasticity of electricity estimates for a given study. The vertical line denotes the mean value (-0.395).

Figure A.9: Variability of the estimated effect by country (Part 1)



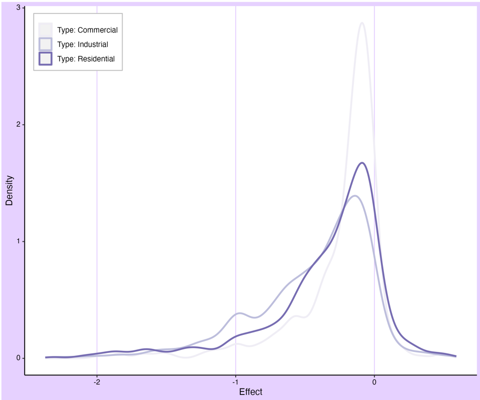
Notes: The figure shows a box plot of the price elasticity of electricity estimates segmented by countries. The vertical line denotes the mean value (-0.395).

Figure A.10: Variability of the estimated effect by country (Part 2)

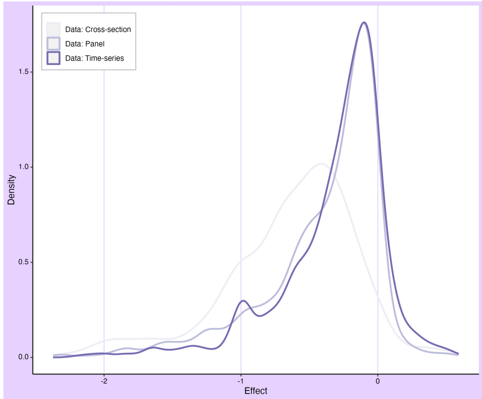


Notes: The figure shows a box plot of the price elasticity of electricity estimates segmented by countries. The vertical line denotes the mean value (-0.395).

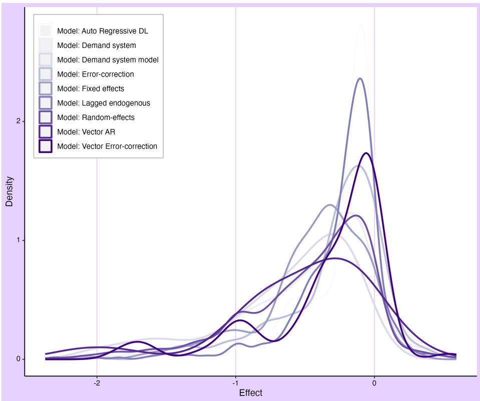
Figure A.11: Density distribution for selected subsamples



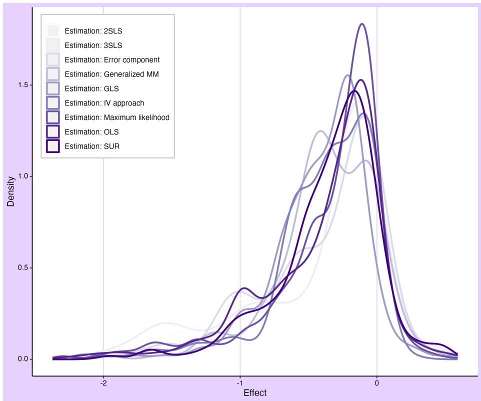
Notes: Scaled density plot of the effect for different electricity demand types.



Notes: Scaled density plot of the effect for different data types.



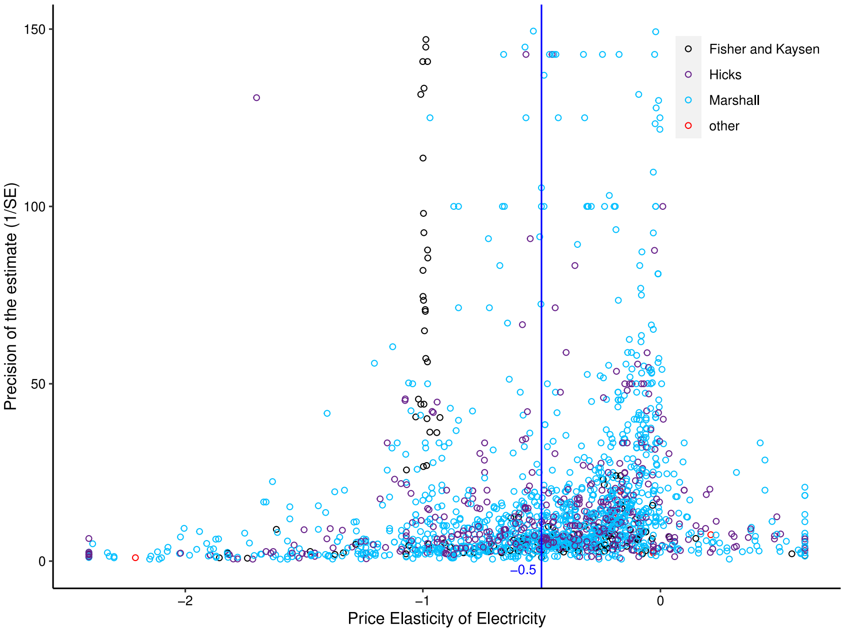
Notes: Scaled density plot of the effect for various model specifications. The model with spike is the LE model. The model with spike at the largest elasticity in magnitude is FE model.



Notes: Scaled density plot of the effect for various estimation techniques.

Publication Bias Robustness Checks

Figure A.12: Original funnel plot for intermediate-run elasticity



Notes: The figure displays funnel plot for the intermediate-run subsample including the study by Fisher & Kaysen (1962), highlighted by black scatter points. The plot should be symmetrical in the absence of publication bias. Estimates with higher precision form funnel-like pattern. A vertical blue line marks the average value of these estimates, with a total count of 1781 data points.

Table A.3: Linear tests results for Marshallian elasticity

	OLS	FE	BE	RE	SW	PW
Short-run						
PB	-0.715^{***}	-0.766^{***}	-0.963^{***}	-0.773^{***}	-0.943^{***}	-3.041^{***}
PB SE	(0.105)	(0.041)	(0.125)	(0.039)	(0.162)	(0.265)
Boot. CI	[-0.907; -0.484]			[-0.992; -0.572]	[-1.124; -0.532]	[-3.542; -2.475]
EBB	-0.161^{***}	-0.155^{***}	-0.154^{***}	-0.166^{***}	-0.089^{***}	-0.052^{***}
EBB SE	(0.011)	(0.008)	(0.021)	(0.016)	(0.018)	(0.011)
Boot. CI	[-0.185; -0.140]			[-0.193; -0.143]	[-0.140; -0.062]	[-0.073; -0.034]
Total observations = 1415						
Long-run						
PB	-0.617^{***}	-0.455^{***}	-1.285^{***}	-0.511^{***}	0.190	-1.367
PB SE	(0.137)	(0.070)	(0.197)	(0.067)	(0.182)	(0.933)
Boot. CI	[-0.906; -0.347]			[-0.795; -0.146]	[-0.734; 0.392]	[-3.145; 0.315]
EBB	-0.390^{***}	-0.431^{***}	-0.290^{***}	-0.502^{***}	-0.290^{***}	-0.262^{***}
EBB SE	(0.030)	(0.026)	(0.077)	(0.063)	(0.029)	(0.089)
Boot. CI	[-0.460; -0.334]			[-0.609; -0.416]	[-0.343; -0.193]	[-0.422; -0.089]
Total observations = 517						

Notes: This table presents the results of linear tests for publication bias for Marshallian elasticities. Standard errors are presented in parentheses. SW = Study weighted, PW = Precision weighted, PB = Publication Bias, EBB = Effect Beyond Bias, SE = Standard Error, Boot. CI = Bootstrapped Confidence Interval

Table A.4: Linear tests results for Hicksian elasticity

	OLS	FE	BE	RE	SW	PW
Short-run						
PB	-1.070^{***}	-0.781^{***}	-1.488^{***}	-0.957^{***}	-1.099^{***}	-2.191^{***}
PB SE	(0.204)	(0.085)	(0.131)	(0.074)	(0.374)	(0.392)
Boot. CI	[-1.438; -0.695]			[-1.522; -0.567]	[-1.915; -0.551]	[-2.809; -1.320]
EBB	-0.085^{***}	-0.114^{***}	-0.076^{***}	-0.125^{***}	-0.044^{**}	-0.035^{**}
EBB SE	(0.018)	(0.013)	(0.027)	(0.026)	(0.021)	(0.017)
Boot. CI	[-0.123; -0.057]			[-0.175; -0.072]	[-0.079; 0.001]	[-0.075; -0.005]
Total observations = 445						
Long-run						
PB	-1.055^{***}	-1.016^{***}	-0.345	-0.959^{***}	-1.406^{***}	-4.163^{***}
PB SE	(0.148)	(0.096)	(0.322)	(0.094)	(0.150)	(0.574)
Boot. CI	[-1.321; -0.694]			[-1.267; -0.681]	[-1.713; -1.039]	[-5.379; -2.138]
EBB	-0.295^{***}	-0.303^{***}	-0.475^{***}	-0.359^{***}	-0.053^{***}	-0.049
EBB SE	(0.033)	(0.026)	(0.105)	(0.077)	(0.016)	(0.046)
Boot. CI	[-0.370; -0.231]			[-0.438; -0.255]	[-0.088; -0.018]	[-0.230; 0.008]
Total observations = 292						

Notes: This table presents the results of linear tests for publication bias for Hicksian elasticities. Standard errors are presented in parentheses. SW = Study weighted, PW = Precision weighted, PB = Publication Bias, EBB = Effect Beyond Bias, SE = Standard Error, Boot. CI = Bootstrapped Confidence Interval

Table A.5: Linear tests results by electricity demand

	OLS	**FE**	**BE**	**RE**	**SW**	**PW**
Residential demand elasticities						
PB	−0.854***	−0.822***	−1.160***	−0.844***	−0.832***	−3.624***
PB SE	(0.066)	(0.031)	(0.104)	(0.030)	(0.096)	(0.364)
Boot. CI	[-0.979, -0.708]	-	-	[-0.973, -0.704]	[-1.012, -0.609]	[-4.351, -2.825]
EBB	−0.287***	−0.293***	−0.245***	−0.299***	−0.269***	−0.128***
EBB SE	(0.011)	(0.009)	(0.027)	(0.020)	(0.020)	(0.019)
Boot. CI	[-0.311, -0.265]	-	-	[-0.325, -0.272]	[-0.314, -0.230]	[-0.168, -0.091]
Total observations = 2790						
Industrial demand elasticities						
PB	−0.594***	−0.564***	−0.804***	−0.572***	−0.302***	−1.955***
PB SE	(0.085)	(0.042)	(0.132)	(0.040)	(0.283)	(0.776)
Boot. CI	[-0.764, -0.424]	-	-	[-0.775, -0.392]	[-1.125, 0.080]	[-2.951, 0.493]
EBB	−0.251***	−0.258***	−0.238***	−0.272***	−0.248***	−0.077
EBB SE	(0.015)	(0.014)	(0.039)	(0.026)	(0.036)	(0.048)
Boot. CI	[-0.285, -0.222]	-	-	[-0.316, -0.233]	[-0.305, -0.148]	[-0.221, -0.018]
Total observations = 1694						
Commercial demand elasticities						
PB	−0.794***	−0.751***	−1.010***	−0.776***	−1.133***	−2.400***
PB SE	(0.141)	(0.054)	(0.149)	(0.051)	(0.129)	(0.130)
Boot. CI	[-1.075, -0.545]	-	-	[-1.033, -0.505]	[-1.358, -0.801]	[-2.698, -2.139]
EBB	−0.145***	−0.153***	−0.172***	−0.200***	−0.049***	−0.007*
EBB SE	(0.019)	(0.016)	(0.041)	(0.030)	(0.013)	(0.004)
Boot. CI	[-0.184, -0.100]	-	-	[-0.257, -0.144]	[-0.081, -0.024]	[-0.016, -0.001]
Total observations = 884						

Notes: This table presents the results of linear tests for publication bias segmented by electricity demand type. Standard errors are presented in parentheses. Some studies examined aggregate electricity demand, which was noted by coding one for all three electricity type demand dummies. Therefore, the sum of individual types is greater than the total number of observations in the dataset. SW = Study weighted, PW = Precision weighted, PB = Publication Bias, EBB = Effect Beyond Bias, SE = Standard Error, Boot. CI = Bootstrapped Confidence Interval

Table A.6: Non-linear tests for publication bias by elasticity type

Effect beyond bias for Marshallian elasticity			
WAAP	-0.227*** (0.007)	-0.201*** (0.007)	**Selection model**
Top10	-0.177*** (0.013)	-0.213*** (0.023)	**Hierarchical Bayes**
Stem-based method	-0.542* (0.282)	-0.096*** (0.005)	**Endogenous kink**
Publication bias for Marshallian elasticity			
Hierarchical Bayes	-1.275*** (0.110)	-3.062*** (0.295)	**Endogenous kink**
Number of observations = 3207			
Effect beyond bias for Hicksian elasticity			
WAAP	-0.179*** (0.013)	-0.176*** (0.028)	**Selection model**
Top10	-0.107*** (0.016)	-0.261*** (0.042)	**Hierarchical Bayes**
Stem-based method	-0.090*** (0.024)	-0.036*** (0.005)	**Endogenous kink**
Publication bias for Hicksian elasticity			
Hierarchical Bayes	-1.141*** (0.181)	-3.291*** (0.269)	**Endogenous kink**
Number of observations = 1176			

Notes: Results of the three specifications of price elasticities using six non-linear methods. We also include the publication bias for Hierarchical Bayes and Endogenous kink methods. WAAP = Weighted Average of the Adequately Powered (n= 1418 for Marshall, 366 for Hicks). Top10 = Top10 Method (n= 317 for Marshall, 118 for Hicks). Standard errors included in the parentheses. Asterisks denote significance level ***p<0.01, **p<0.05, *p<0.1.

Table A.7: Non-linear tests for publication bias by electricity demand

Effect beyond bias (*Residential demand*)			
WAAP	-0.244*** (0.008)	-0.223*** (0.010)	**Selection model**
Top10	-0.172*** (0.012)	-0.223*** (0.023)	**Hierarchical Bayes**
Stem-based method	-0.080*** (0.022)	-0.079*** (0.004)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-1.367*** (0.126)	-3.900*** (0.249)	**Endogenous kink**
Number of observations = 2790			
Effect beyond bias (*Industrial demand*)			
WAAP	-0.132*** (0.010)	-0.164*** (0.010)	**Selection model**
Top10	-0.109*** (0.016)	-0.203*** (0.032)	**Hierarchical Bayes**
Stem-based method	-0.241** (0.121)	-0.077*** (0.007)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-0.960*** (0.123)	-1.944*** (0.437)	**Endogenous kink**
Number of observations = 1694			
Effect beyond bias *(Commercial demand)*			
WAAP	-0.054*** (0.008)	-0.087*** (0.007)	**Selection model**
Top10	-0.051*** (0.014)	-0.134*** (0.041)	**Hierarchical Bayes**
Stem-based method	-0.080*** (0.005)	-0.008*** (0.002)	**Endogenous kink**
Publication bias			
Hierarchical Bayes	-1.147*** (0.182)	-2.400*** (0.197)	**Endogenous kink**
Number of observations = 884			

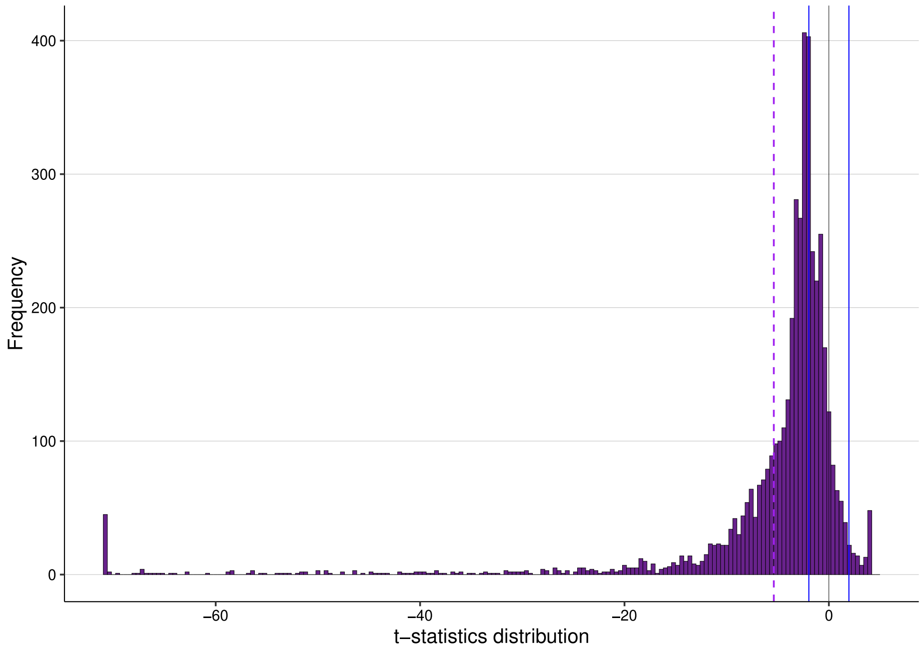
Notes: Results of the three specifications of price elasticities using six non-linear methods. We also include the publication bias for Hierarchical Bayes and Endogenous kink methods. WAAP = Weighted Average of the Adequately Powered (n= 1223 for RD, 469 for ID, 173 for CD). Top10 = Top10 Method (n= 278 for RD, 170 for ID, 88 for CD). Standard errors included in the parentheses. Asterisks denote significance level ***p<0.01, **p<0.05, *p<0.1.

Table A.8: IV Regression results for short-run and long-run periods

	Short-run	*Long-run*
Publication Bias	-0.786^{**}	-1.44^{***}
Standard Error	(0.328)	(0.382)
Effect Beyond Bias	-0.145^{***}	-0.193^{*}
Standard Error	(0.039)	(0.107)
F-test	18.22	

Notes: IV = Instrumental Variable, the instrument is the inverse of the square root of number of observations. The standard errors presented in the parenthesis are clustered at the study level. Asterisks denote significance level ***p<0.01, **p<0.05, *p<0.1.

Figure A.13: Distribution of the t-statistic for an unrestricted sample



Notes: The figure depicts the distribution of elasticity t-statistics for all estimates from the winsorized sample. The vertical lines again denote the mean and critical values, respectively. In the original sample, the lowest t-statistic was -900 and the highest one was 74.1.

Model Averaging Robustness Checks

Figure A.14: Model inclusion probabilities from Chapter 5 for various priors

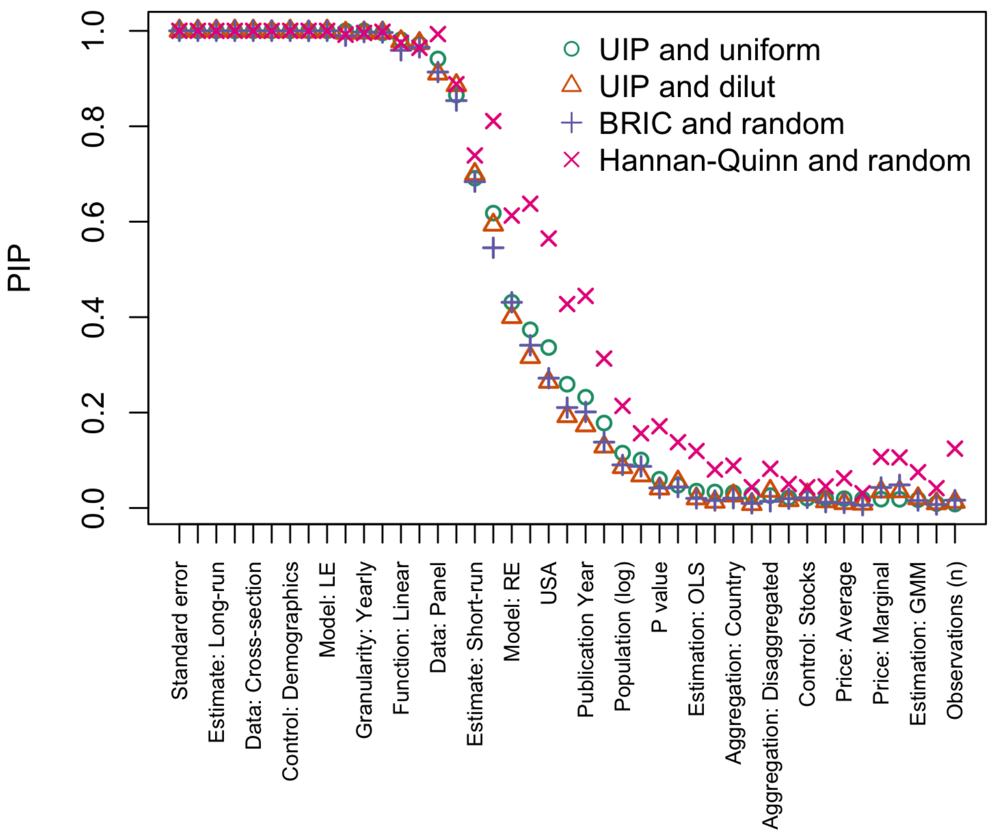


Figure A.15: Correlation among selected variables used in model averaging

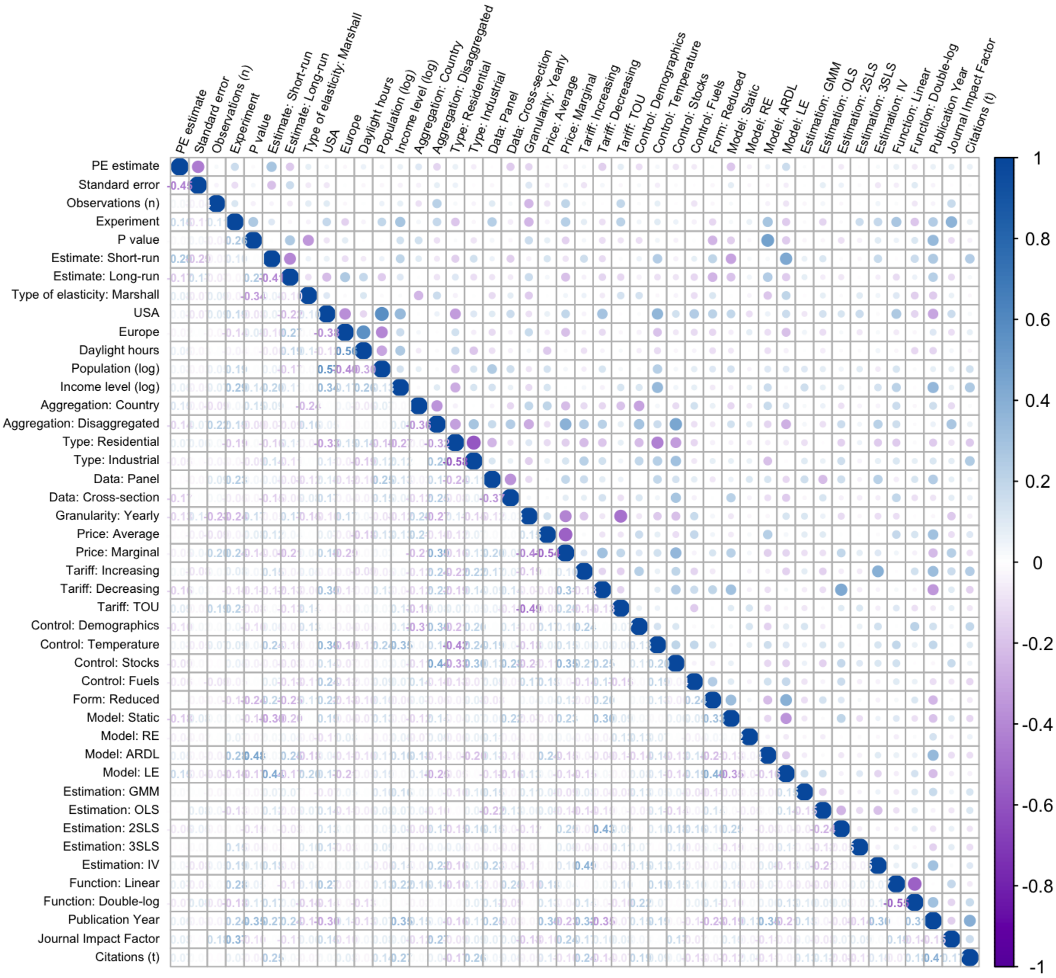


Figure A.16: Results of Bayesian model averaging using benchmark g-prior and random model prior

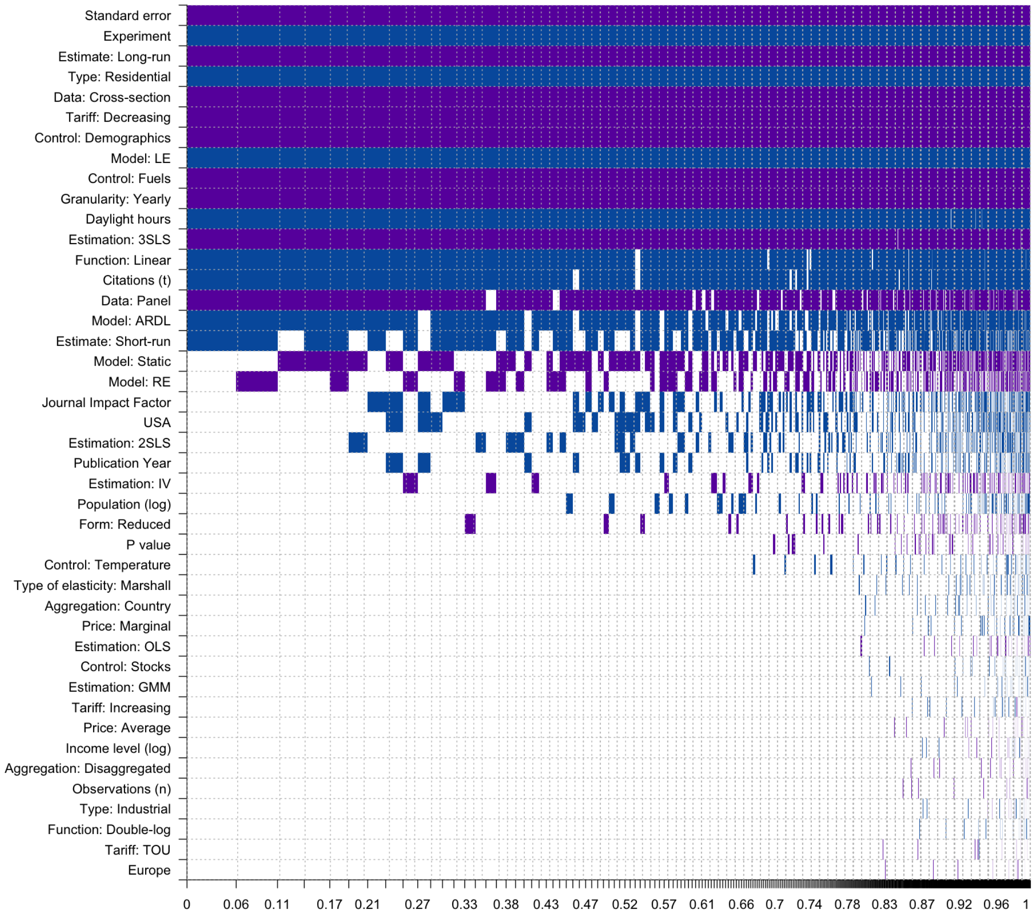


Figure A.17: Results of Bayesian model averaging using HQ g-prior and random model prior

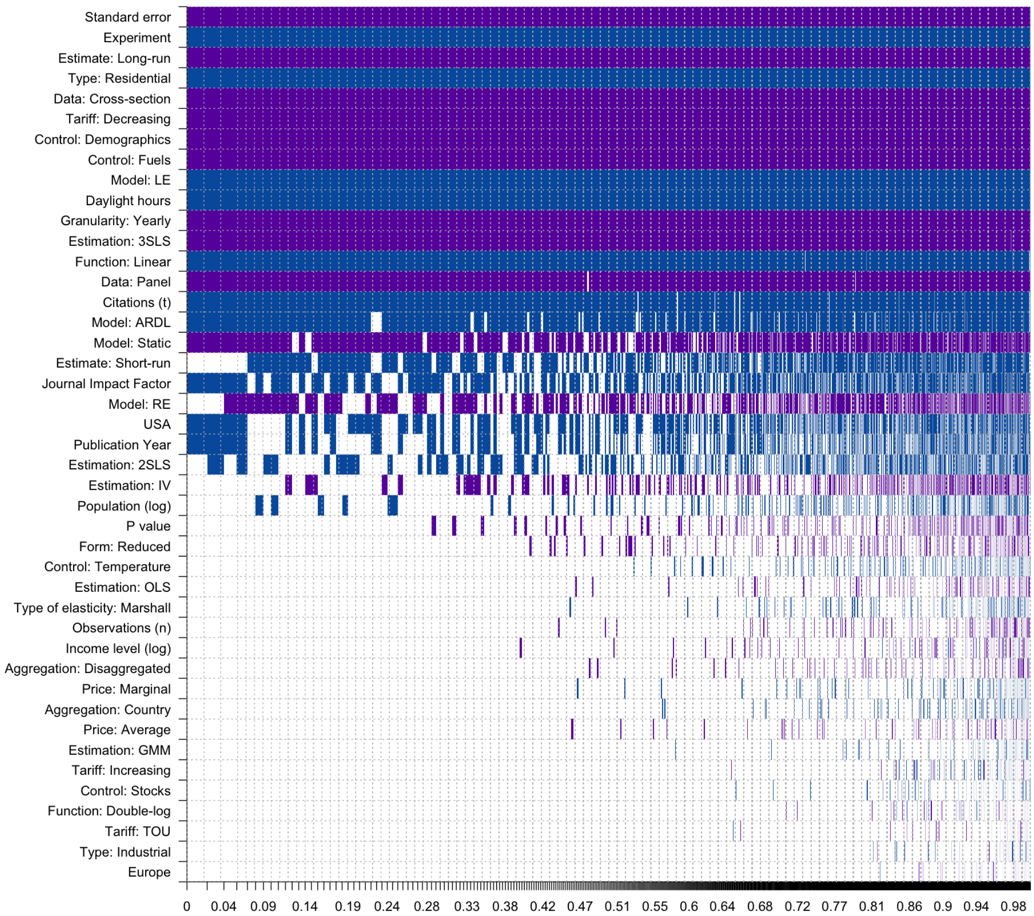
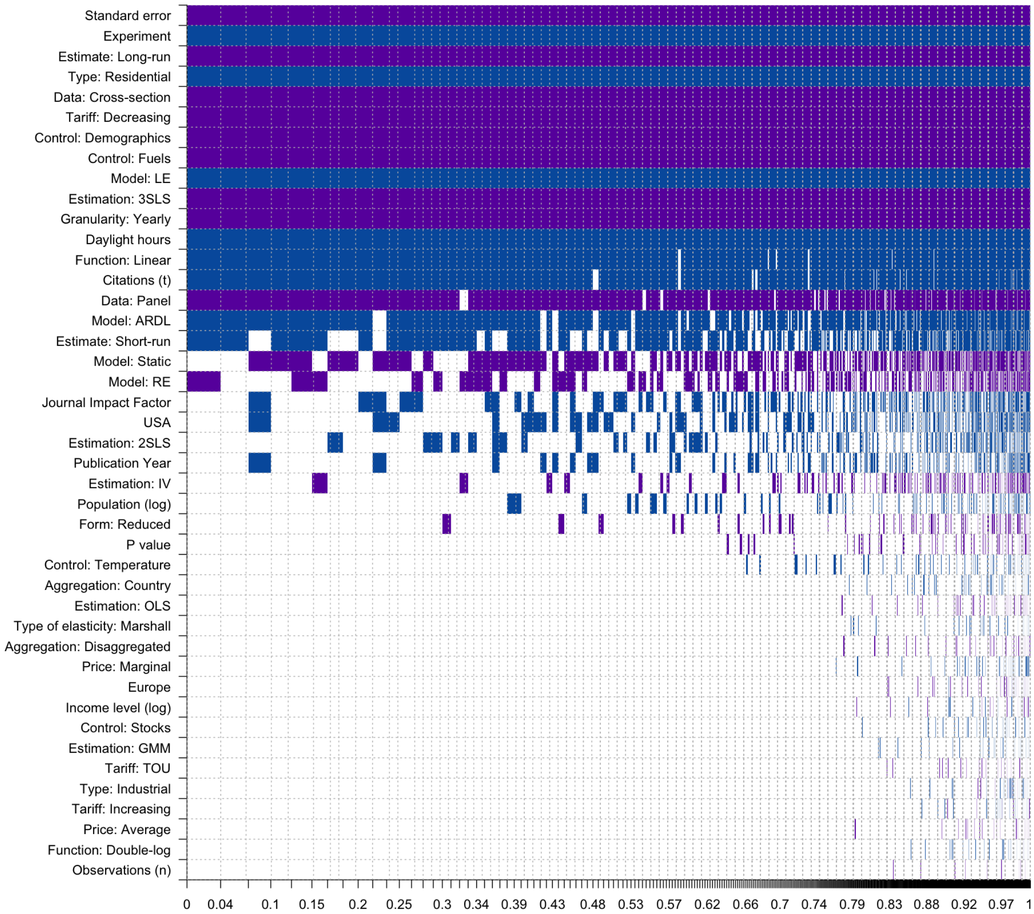


Figure A.18: Results of Bayesian model averaging using uniform g-prior and uniform model prior



Results of the BMA by elasticity types

Table A.9: Model averaging results for Marshallian elasticities (Part 1)

	Bayesian model averaging			Frequentist model averaging		
Response variable:	Post. mean	Post. SD	PIP	Coef.	SE	p-value
Constant	-4.888	NA	**1.000**	-5.277	1.720	**0.002**
Standard error	-0.615	0.027	**1.000**	-0.607	0.028	**0.000**
Data Characteristics						
Observations (n)	0.000	0.000	0.012	0.000	0.000	0.000
Experiment	0.008	0.031	0.080	0.056	0.043	0.192
P value	0.000	0.004	0.005	-0.08	0.059	0.171
USA	0.018	0.030	0.290	0.015	0.026	0.572
Europe	0.000	0.005	0.009	-0.01	0.026	0.706
Daylight hours	0.011	0.009	**0.703**	0.018	0.006	**0.005**
Population (log)	0.009	0.011	0.476	0.015	0.007	0.053
Type of elasticity						
Estimate: Short-run	0.046	0.035	**0.696**	0.058	0.022	**0.008**
Estimate: Long-run	-0.173	0.031	**1.000**	-0.174	0.025	**0.000**
Data Aggregation						
Country	-0.001	0.006	0.021	-0.022	0.023	0.322
Disaggregated	-0.001	0.007	0.024	-0.022	0.024	0.353
Time-series	0.105	0.021	**1.000**	0.123	0.021	**0.000**
Cross-section	-0.162	0.025	**1.000**	-0.162	0.027	**0.000**
Granularity: Yearly	-0.100	0.019	**0.999**	-0.057	0.026	**0.029**
Type of electricity demand						
Type: Residential	0.054	0.031	**0.821**	0.059	0.024	**0.016**
Type: Industrial	-0.008	0.021	0.143	-0.017	0.022	0.439
Type of electricity price						
Price: Average	-0.093	0.018	**1.000**	-0.078	0.023	**0.001**
Price: Marginal	0.000	0.002	0.003	0.017	0.027	0.518
Type of electricity tariff						
Tariff: Increasing	0.000	0.003	0.008	0.044	0.033	0.182
Tariff: Decreasing	-0.148	0.031	**1.000**	-0.129	0.032	**0.000**
Tariff: TOU	0.001	0.008	0.024	0.043	0.032	0.176

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Notes: This table presents the results of the Bayesian model averaging and Frequentist model averaging. Post. mean = Posterior Mean, Post. SD = Posterior Standard Deviation, PIP = Posterior Inclusion Probability, Coef. = Coefficient, SE = Standard Error. The variables with PIP > 0.5 are highlighted in bold.

Table A.10: Model averaging results for Marshallian elasticities (Part 2)

	Bayesian model averaging			Frequentist model averaging		
Response variable:	Post. mean	Post. SD	PIP	Coef.	SE	p-value
Demand Controls						
Demographics	-0.077	0.016	**1.000**	-0.077	0.019	**0.000**
Temperature	0.003	0.012	0.082	0.027	0.020	0.167
Stocks	0.002	0.010	0.035	0.041	0.027	0.127
Fuels	-0.060	0.021	**0.962**	-0.069	0.018	**0.000**
Model specification						
Form: Reduced	-0.001	0.005	0.020	-0.007	0.020	0.706
Model: Static	-0.072	0.030	**0.916**	-0.063	0.025	**0.012**
Model: RE	-0.010	0.041	0.075	-0.139	0.074	0.062
Model: ARDL	0.028	0.051	0.262	0.104	0.049	**0.033**
Model: LE	0.140	0.027	**1.000**	0.132	0.029	**0.000**
Estimation Technique						
Estimation: GMM	0.001	0.010	0.018	0.017	0.040	0.675
Estimation: OLS	0.000	0.002	0.007	-0.018	0.019	0.346
Estimation: 2SLS	0.093	0.034	**0.948**	0.073	0.030	**0.016**
Estimation: 3SLS	-0.148	0.057	**0.938**	-0.195	0.048	**0.000**
Estimation: IV	-0.052	0.051	**0.570**	-0.104	0.035	0.003
Function Specification						
Function: Linear	0.088	0.021	**1.000**	0.077	0.023	**0.001**
Function: Double-log	0.000	0.001	0.003	0.000	0.007	0.000
Publication Characteristics						
Year of publication	0.002	0.001	**0.836**	0.002	0.001	**0.015**
Impact Factor	0.069	0.022	**0.960**	0.057	0.021	**0.007**
Citations (t)	0.040	0.010	**1.000**	0.037	0.009	**0.000**

Notes: This table presents the results of the Bayesian model averaging and Frequentist model averaging for the Marshallian elasticity (n = 3326). Post. mean = Posterior Mean, Post. SD = Posterior Standard Deviation, PIP = Posterior Inclusion Probability, Coef. = Coefficient, SE = Standard Error. TN citations = transformed number of citations, LE = Lagged endogenous. The variables with PIP > 0.5 are highlighted in bold. Note that we used different dataset for separate estimation (where Hicksian elasticities are not transformed into Marshallian elasticities), few selected variables are hence labelled differently.

Figure A.19: Results of Bayesian model averaging using UIP g-prior and dilution model prior for Marshallian elasticities

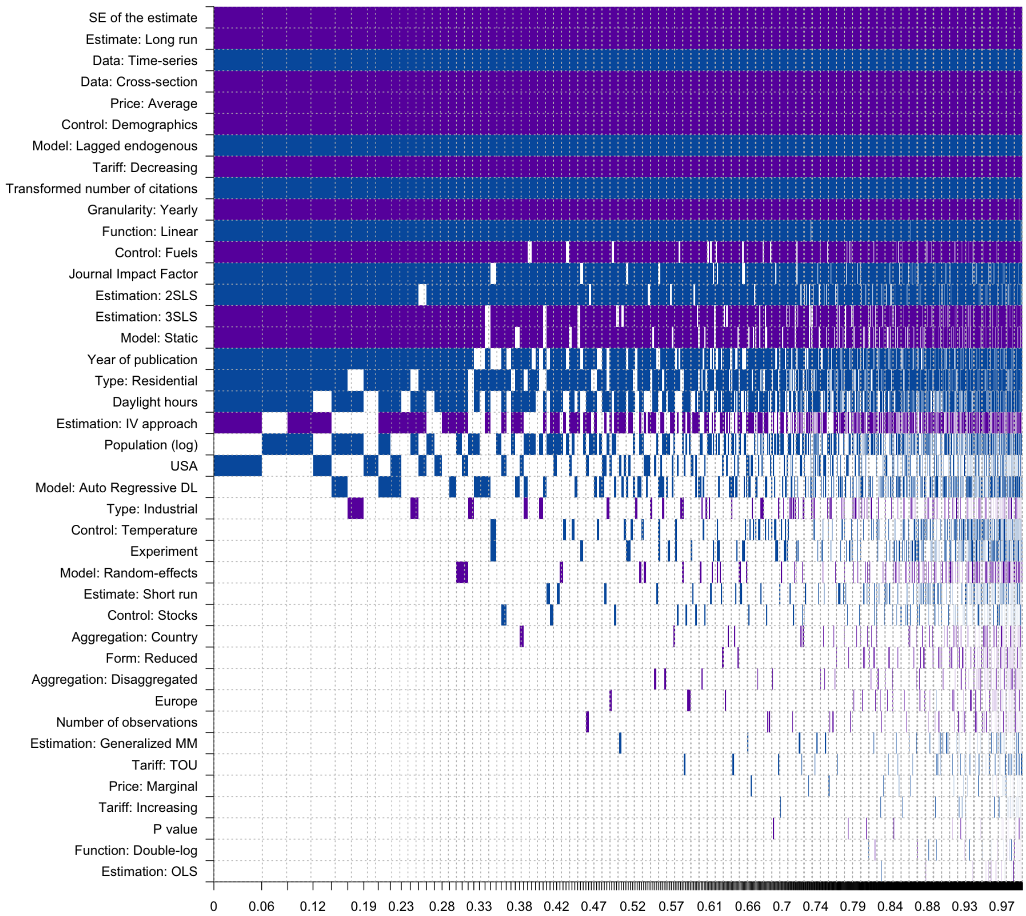

Figure A.20: Model inclusion probabilities for Marshallian elasticities

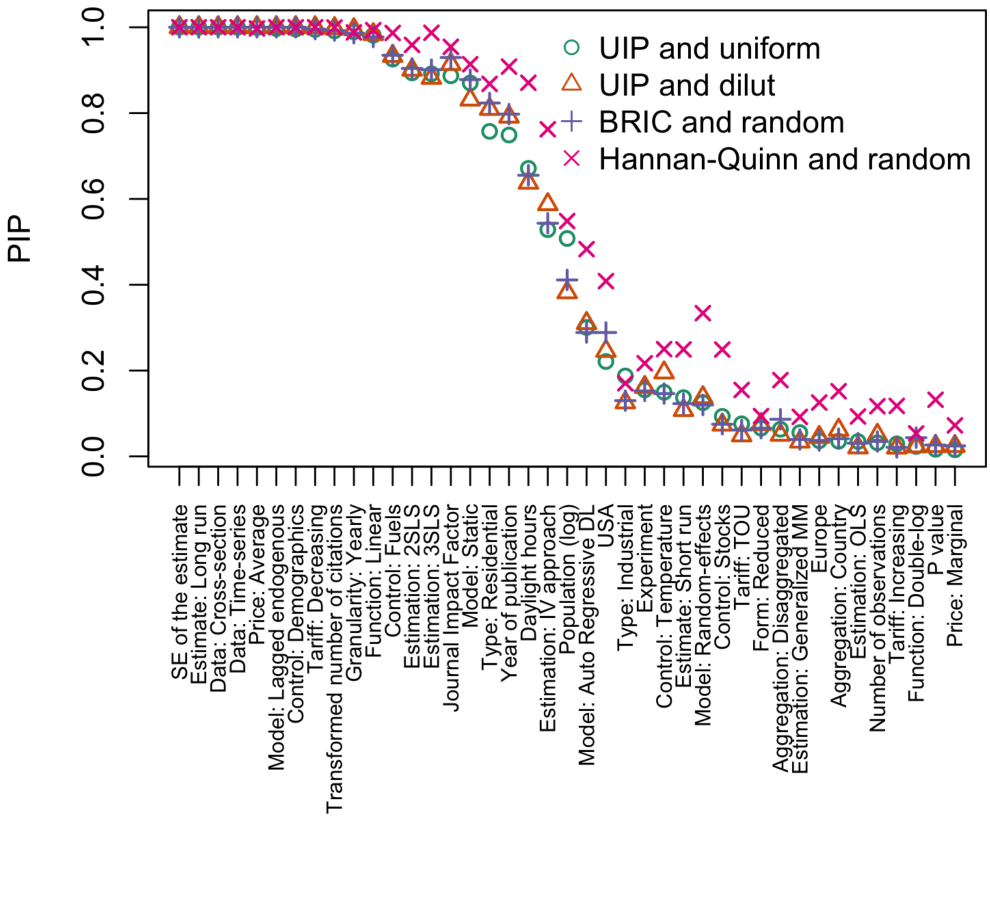

Table A.11: Model averaging results for Hicksian elasticities (Part 1)

	Bayesian model averaging			Frequentist model averaging		
Response variable:	Post. mean	Post. SD	PIP	Coef.	SE	p-value
(Intercept)	-0.544	NA	**1.000**	-8.900	5.793	0.124
SE of the estimate	-0.898	0.050	**1.000**	-0.876	0.052	**0.000**
Data Characteristics						
Number of observations	0.000	0.000	0.019	0.000	0.000	0.000
Experiment	0.306	0.101	**0.987**	0.542	0.141	**0.000**
P value	-0.010	0.033	0.104	-0.116	0.060	0.052
USA	0.001	0.009	0.015	0.051	0.062	0.408
Europe	0.013	0.037	0.138	0.118	0.058	**0.042**
Daylight hours	0.007	0.011	0.367	0.017	0.010	0.082
Population (log)	0.000	0.002	0.014	-0.010	0.014	0.468
Type of elasticity						
Estimate: Short-run	0.195	0.040	**0.993**	0.166	0.061	**0.006**
Estimate: Long-run	-0.004	0.023	0.045	-0.064	0.067	0.340
Data Aggregation						
Aggregation: Country	0.142	0.037	**0.997**	0.132	0.041	**0.001**
Aggregation: Disaggregated	-0.002	0.016	0.028	-0.029	0.070	0.679
Data: Time-series	0.005	0.020	0.076	0.067	0.050	0.177
Data: Cross-section	-0.171	0.093	**0.847**	-0.194	0.078	**0.013**
Granularity: Yearly	-0.323	0.057	**1.000**	-0.269	0.069	**0.000**
Type of electricity demand						
Type: Residential	0.253	0.039	**1.000**	0.269	0.047	**0.000**
Type: Industrial	0.000	0.004	0.011	0.003	0.022	0.897
Type of electricity price						
Price: Average	0.000	0.005	0.013	0.036	0.053	0.495
Price: Marginal	-0.209	0.069	**0.968**	-0.075	0.092	0.414
Type of electricity tariff						
Tariff: Increasing	0.006	0.026	0.060	0.040	0.067	0.549
Tariff: Decreasing	-0.005	0.027	0.052	-0.076	0.076	0.321
Tariff: TOU	-0.001	0.014	0.018	-0.202	0.120	0.091

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Notes: This table presents the results of the Bayesian model averaging and Frequentist model averaging for the Hicksian elasticity (n = 1176). Post. mean = Posterior Mean, Post. SD = Posterior Standard Deviation, PIP = Posterior Inclusion Probability, Coef. = Coefficient, SE = Standard Error. TN citations = transformed number of citations, LE = Lagged endogenous. The variables with PIP > 0.5 are highlighted in bold.

Table A.12: Model averaging results for Hicksian elasticities (Part 2)

	Bayesian model averaging			Frequentist model averaging		
Response variable:	Post. mean	Post. SD	PIP	Coef.	SE	p-value
Demand Controls						
Demographics	-0.204	0.049	**1.000**	-0.206	0.055	**0.000**
Temperature	-0.004	0.017	0.056	-0.016	0.042	0.697
Stocks	-0.003	0.018	0.035	-0.020	0.056	0.722
Fuels	-0.002	0.013	0.041	-0.087	0.055	0.113
Model specification						
Form: Reduced	0.002	0.014	0.040	0.112	0.069	0.104
Model: Static	0.000	0.006	0.013	-0.027	0.051	0.600
Model: RE	-0.002	0.020	0.017	-0.121	0.127	0.341
Model: ARDL	0.000	0.008	0.012	-0.008	0.041	0.839
Model: LE	0.268	0.058	**1.000**	0.236	0.078	**0.003**
Estimation Technique						
Estimation: GMM	0.000	0.009	0.010	0.025	0.079	0.754
Estimation: OLS	0.000	0.005	0.013	-0.054	0.047	0.254
Estimation: 2SLS	-0.002	0.016	0.024	0.051	0.088	0.564
Estimation: IV	-0.001	0.010	0.014	-0.067	0.076	0.382
Function Specification						
Function: Linear	-0.029	0.065	0.200	-0.143	0.081	0.079
Function: Double-log	0.000	0.004	0.009	-0.012	0.040	0.776
Publication Characteristics						
Year of publication	0.000	0.001	0.028	0.004	0.003	0.148
Impact Factor	0.003	0.020	0.042	0.162	0.090	0.074
Citations (t)	0.040	0.032	**0.689**	0.040	0.023	0.083

Notes: This table presents the results of the Bayesian model averaging and Frequentist model averaging for the Hicksian elasticity (n = 1176). Post. mean = Posterior Mean, Post. SD = Posterior Standard Deviation, PIP = Posterior Inclusion Probability, Coef. = Coefficient, SE = Standard Error. TN citations = transformed number of citations, LE = Lagged endogenous. The variables with PIP $>$ 0.5 are highlighted in bold. Note that we used different dataset for separate estimation (where Hicksian elasticities are not transformed into Marshallian elasticities), few selected variables are hence labelled differently.

Figure A.21: Results of Bayesian model averaging using UIP g-prior and dilution model prior for Hicksian elasticities

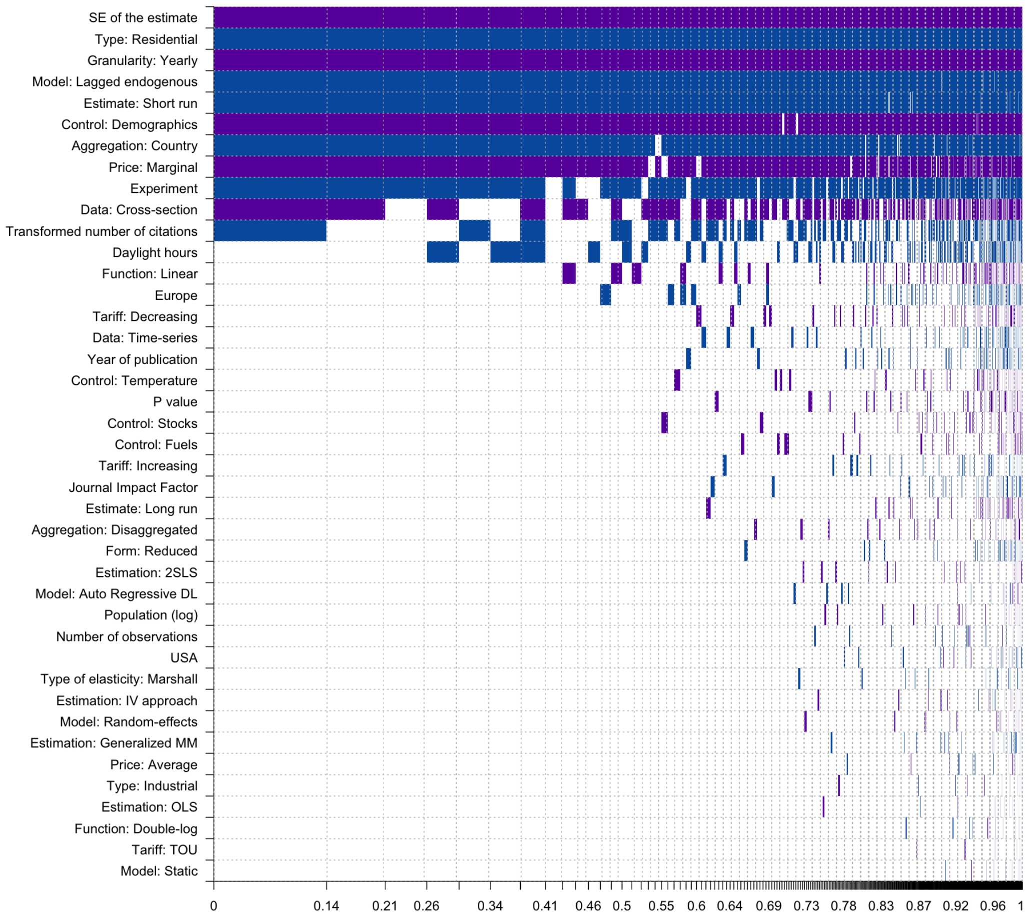


Figure A.22: Model inclusion probabilities for Hicksian elasticities

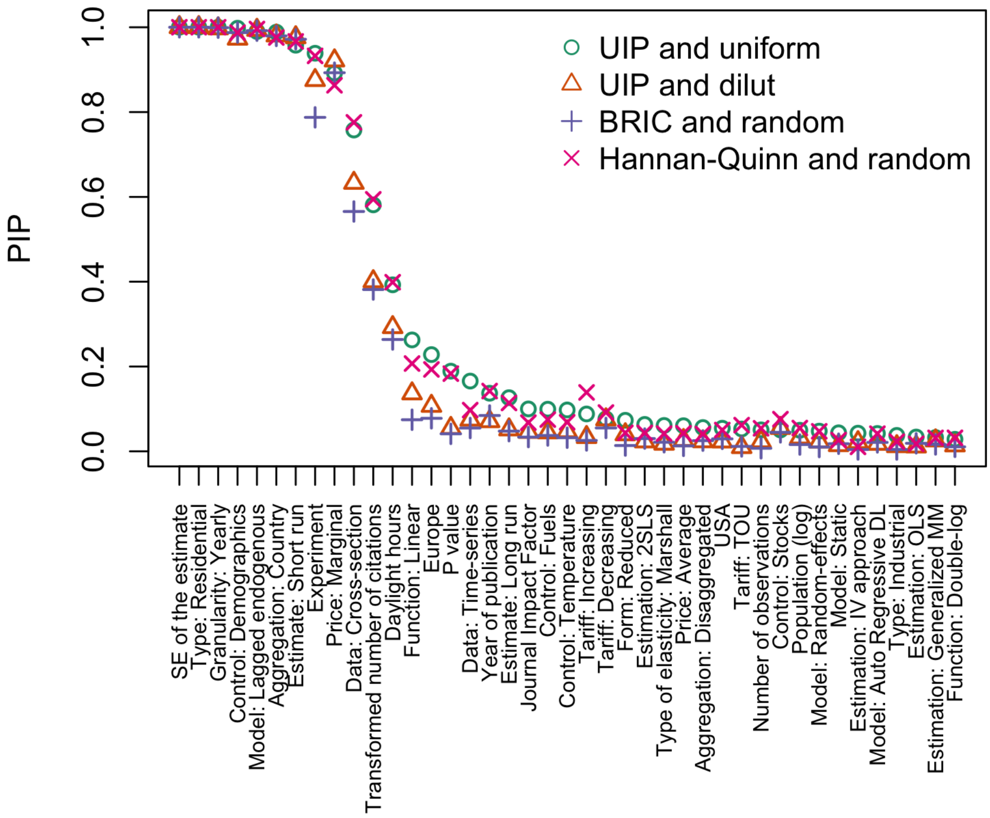


Table A.13: Model averaging results for short-run elasticities (Part 1)

	Bayesian model averaging			Frequentist model averaging		
Response variable:	Post. mean	Post. SD	PIP	Coef.	SE	p-value
Constant	-6.122	NA	**1.000**	-10.095	2.128	**0.000**
SE of the estimate	-0.784	0.032	**1.000**	-0.774	0.033	**0.000**
Data Characteristics						
Observations (n)	0.000	0.000	0.008	0.000	0.000	0.000
Experiment	0.141	0.028	**1.000**	0.139	0.035	**0.000**
P value	0.000	0.005	0.016	-0.040	0.032	0.217
USA	0.000	0.002	0.008	0.048	0.030	0.119
Europe	-0.089	0.024	**0.989**	-0.079	0.025	**0.001**
Type: Marshall	-0.043	0.025	**0.830**	-0.061	0.020	**0.002**
Daylight hours	0.025	0.005	**1.000**	0.025	0.005	**0.000**
Population (log)	0.000	0.001	0.012	-0.008	0.007	0.258
Income level (log)	0.000	0.001	0.011	-0.010	0.008	0.238
Data Aggregation						
Aggregation: Country	-0.001	0.006	0.042	-0.025	0.017	0.138
Aggregation: Disaggregated	0.000	0.004	0.015	0.017	0.023	0.476
Type: Residential	0.080	0.016	**1.000**	0.079	0.019	**0.000**
Type: Industrial	0.000	0.001	0.009	0.006	0.017	0.695
Data: Panel	-0.110	0.014	**1.000**	-0.114	0.017	**0.000**
Data: Cross-section	0.000	0.005	0.016	-0.018	0.034	0.583
Granularity: Yearly	-0.003	0.013	0.093	-0.040	0.022	0.072
Type of electricity price						
Price: Average	-0.002	0.009	0.081	-0.044	0.019	**0.023**
Price: Marginal	0.000	0.004	0.015	-0.041	0.026	0.110
Type of electricity tariff						
Tariff: Increasing	-0.002	0.011	0.054	-0.046	0.028	0.097
Tariff: Decreasing	0.001	0.006	0.016	0.011	0.031	0.714
Tariff: TOU	-0.001	0.009	0.035	-0.036	0.029	0.217

Notes:This table presents the results of the Bayesian model averaging and Frequentist model averaging for the short-run elasticity (n = 1866). Post. mean = Posterior Mean, Post. SD = Posterior Standard Deviation, PIP = Posterior Inclusion Probability, Coef. = Coefficient, SE = Standard Error. The variables with PIP > 0.5 are highlighted in bold. Numerical results are based on updated data.

Table A.14: Model averaging results for short-run elasticities (Part 2)

	Bayesian model averaging			Frequentist model averaging		
Response variable:	Post. mean	Post. SD	PIP	Coef.	SE	p-value
Demand Controls						
Control: Demographics	-0.085	0.016	**1.000**	-0.089	0.019	**0.000**
Control: Temperature	0.000	0.002	0.009	0.006	0.015	0.703
Control: Stocks	-0.061	0.026	**0.915**	-0.071	0.024	**0.003**
Control: Fuels	-0.002	0.008	0.079	-0.020	0.016	0.220
Model specification						
Form: Reduced	-0.028	0.032	0.484	-0.006	0.025	0.810
Model: Static	-0.033	0.038	0.478	-0.039	0.028	0.173
Model: RE	0.000	0.005	0.010	-0.028	0.046	0.546
Model: ARDL	0.001	0.007	0.021	0.037	0.034	0.285
Model: LE	0.130	0.030	**1.000**	0.137	0.030	**0.000**
Estimation Technique						
Estimation: GMM	0.001	0.008	0.023	0.030	0.036	0.396
Estimation: OLS	-0.001	0.007	0.051	-0.029	0.017	0.088
Estimation: 2SLS	0.133	0.023	**1.000**	0.132	0.026	**0.000**
Estimation: 3SLS	-0.250	0.048	**1.000**	-0.246	0.052	**0.000**
Estimation: IV	-0.020	0.030	0.360	-0.067	0.027	**0.013**
Function Specification						
Function: Linear	0.000	0.002	0.010	-0.002	0.020	0.904
Function: Double-log	0.000	0.001	0.008	0.000	0.000	0.000
Publication Characteristics						
Publication Year	0.003	0.001	**0.985**	0.005	0.001	**0.000**
Journal Impact Factor	0.009	0.017	0.241	0.028	0.018	0.123
Citations (t)	0.026	0.007	**0.990**	0.021	0.008	**0.006**

Notes: This table presents the results of the Bayesian model averaging and Frequentist model averaging for the short-run elasticity (n = 1866). Post. mean = Posterior Mean, Post. SD = Posterior Standard Deviation, PIP = Posterior Inclusion Probability, Coef. = Coefficient, SE = Standard Error. TN citations = transformed number of citations, LE = Lagged endogenous. The variables with PIP > 0.5 are highlighted in bold.

Figure A.23: Results of Bayesian model averaging using UIP g-prior and dilution model prior for short-run elasticities

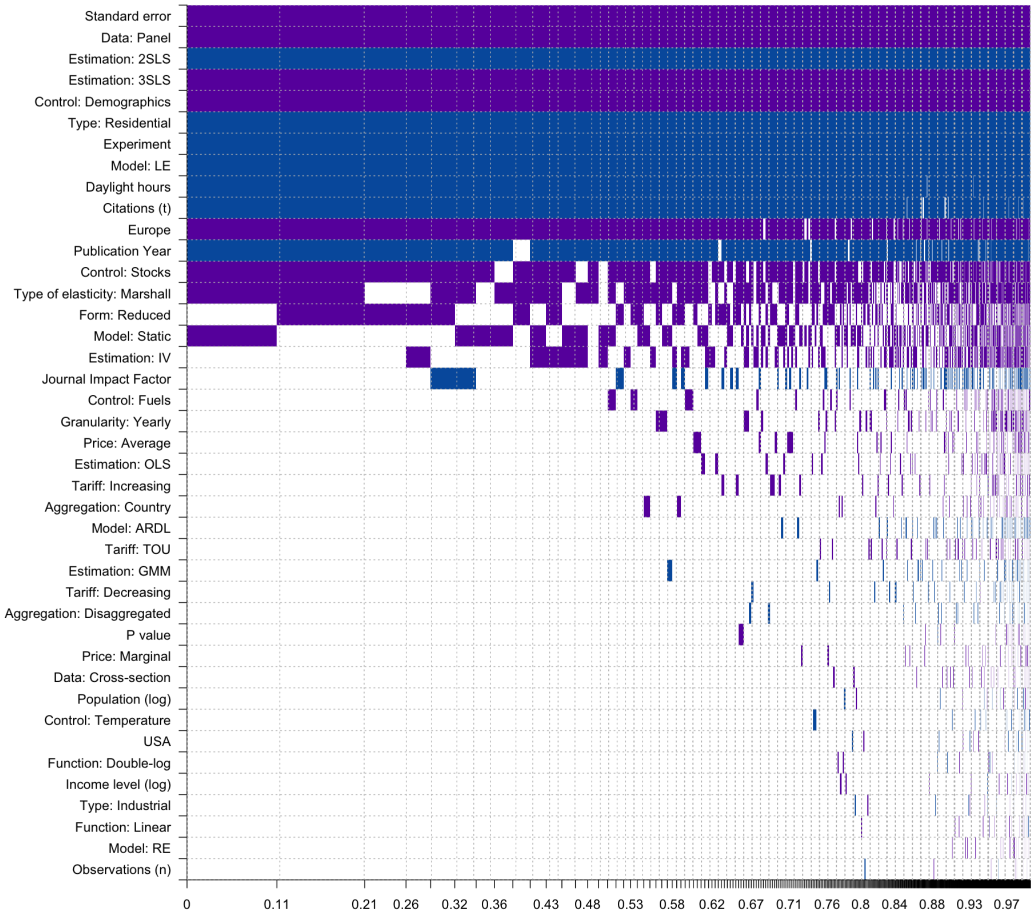
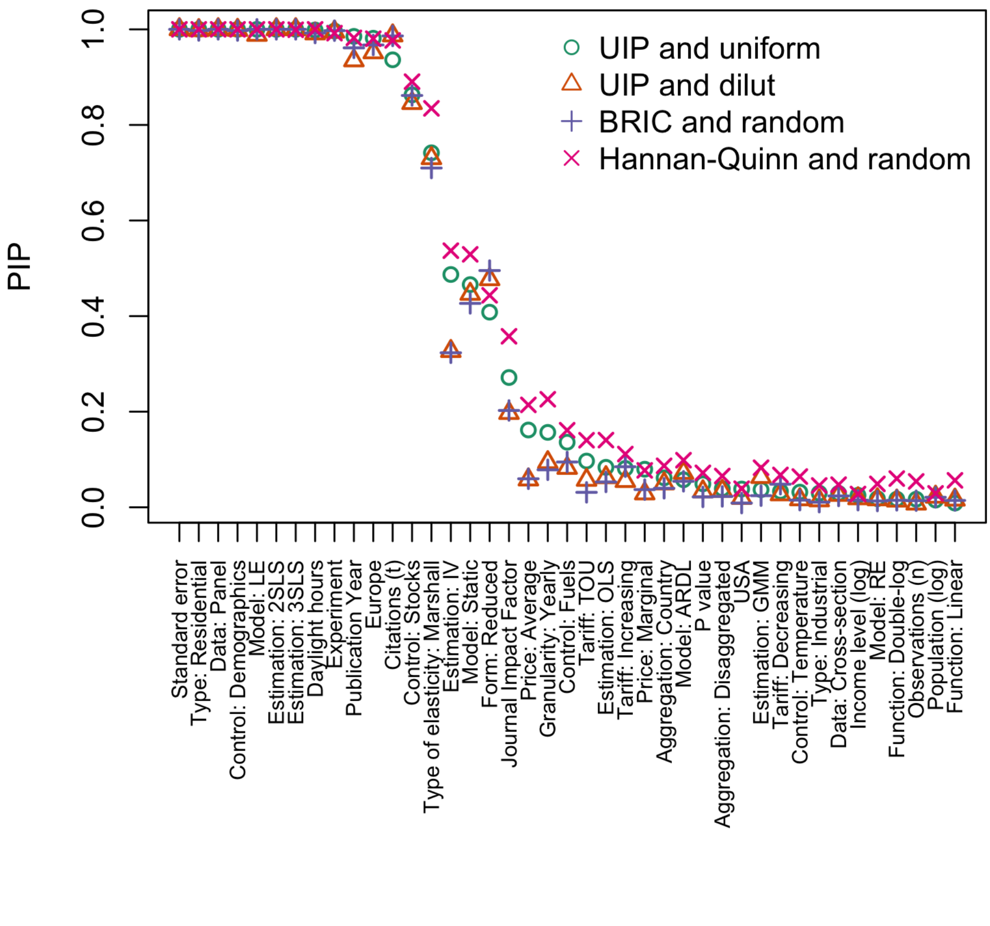


Figure A.24: Model inclusion probabilities for short-run elasticities



Appendix B

Literature Collection

Table B.15: List of studies (Part 1)

List of studies included in the Meta-analysis (Part 1)		
Ackah *et al.* (2014)	Blasch *et al.* (2017)	Douthitt (1989)
Acton *et al.* (1976)	Blázquez *et al.* (2013)	Dubin (1985)
Acton *et al.* (1980)	Blundell & Robin (1999)	Duncan & Binswanger (1976)
ADNDE (1981)	Boogen *et al.* (2017)	Dunstan & Schmidt (1988)
Akmal & Stern (2001)	Borenstein (2009)	Durmaz *et al.* (2020)
Al-Bajjali & Shamayleh (2018)	Bose & Shukla (1999)	Ekpo *et al.* (2011)
Al-Faris (2002)	Botero *et al.* (1990)	Eltony (1995)
Al Irsyad *et al.* (2018)	Branch (1993)	Eltony (2004)
Al-Sahlawi (1999)	Brenton (1997)	Eltony (2006)
Alberini & Filippini (2011)	Burke & Abayasekara (2018)	Eltony & Al-Awadhi (2007)
Alberini *et al.* (2011)	Burke & Kurniawati (2018)	Eltony & Hajeeh (1999)
Alberini *et al.* (2019a)	Bye (1986)	Eltony & Hoque (1996)
Allcott (2011)	Byrne *et al.* (2021)	Eltony & Mohammad (1993)
Alter & Syed (2011)	Campbell (2018)	Erickson *et al.* (1973)
Amarawickrama & Hunt (2008)	Cao *et al.* (2019)	Eskeland & Mideksa (2010)
Amusa *et al.* (2009)	Cao *et al.* (2023)	Eskeland *et al.* (1994)
Anderson (1971)	Cargill & Meyer (1971)	Fan & Hyndman (2011)
Anderson (1973a)	Carlevaro & Spierer (1983)	Fan *et al.* (2019)
Anderson (1973b)	Cavoulacos & Caramanis (1983)	Fatai *et al.* (2003)
Anderson (1974)	Cebula (2012)	Fell *et al.* (2014)
Andrikopoulos *et al.* (1989)	Chang & Chern (1981a)	Filippini (1995a)
Ang *et al.* (1992)	Chang & Chern (1981b)	Filippini (1995b)
Apte (1983)	Chang & Hsing (1991)	Filippini (1999)
Archibald *et al.* (1982)	Chaudhary *et al.* (1999)	Filippini & Pachauri (2004)
Arimura *et al.* (2012)	Chaudhry (2010)	Filippini (2011)
Arisoy & Ozturk (2014)	Chern (1975)	Filippini *et al.* (2018)
Aroonruengsawat *et al.* (2012)	Chern (1978)	Fisher & Kaysen (1962)
Arsenault *et al.* (1995)	Chern & Bouis (1988)	Fouquet (1995)
Asadoorian *et al.* (2006)	Chishti (1993)	Frondel *et al.* (2019)
Asadoorian *et al.* (2008)	Choi (2002)	Fuss (1977)
Aslam & Ahmad (2023)	Christodoulakis & Kalyvitis (1997)	Gam & Rejeb (2012)
Atakhanova & Howie (2005)	Christopoulos (2000)	Garbacz (1983a)
Athukorala & Wilson (2010)	Chung & Aigner (1981)	Garbacz (1983b)
Athukorala *et al.* (2019)	Cialani & Mortazavi (2018)	Garbacz (1983c)
Atkinson (1979a)	Cicchetti & Smith (1975)	Garbacz (1984a)
Atkinson (1979b)	CISEPA (1998)	Garbacz (1984b)
Auray *et al.* (2019)	Cohn (1980)	Garbacz (1984c)
Azevedo *et al.* (2011)	Considine (2000)	Garbacz (1986)
Babatunde & Shuaibu (2009)	Coughlin (1995)	Garcia-Cerrutti (2000)
Badri (1992)	Cuddington & Dagher (2015)	Gautam & Paudel (2018)
Baker & Blundell (1991)	Dahan (1996)	Gill & Maddala (1978)
Balabanoff (1994)	Davis (2008)	Glakpe & Fazzolare (1985)
Banda & Verdugo (2007)	De Cian *et al.* (2007)	Gollnick (1975)
Barnes *et al.* (1981)	De Vita *et al.* (2006)	Green *et al.* (1986)
Basu (1976)	Delfino (1995)	Griffin (1974)
Baughman *et al.* (1979)	Denton *et al.* (1999)	Gundimeda & Kohlin (2008)
Beenstock *et al.* (1999)	Denton *et al.* (2003)	Guo & Tybout (1994)
Beierlein *et al.* (1981)	Dergiades & Tsoulfidis (2008)	Halicioglu (2007)
Bekhet & Othman (2011)	Deryugina *et al.* (2020)	Hall & Rosenthal (1995)
Belanger *et al.* (1990)	Diabli (1998)	Halvorsen (1975)
Berkhout *et al.* (2004)	Dias-Bandaranaike & Munasinghe (1983)	Halvorsen (1976)
Bernard *et al.* (1987)	Dilaver & Hunt (2011a)	Halvorsen (1977)
Bernard *et al.* (1996)	Dilaver & Hunt (2011b)	Halvorsen & Ford (1979)
Bernard *et al.* (2011)	Dobozi (1988)	Halvorsen & Larsen (2001)
Berndt & Samaniego (1984)	Dodgson *et al.* (1990)	Hartman & Werth (1981)
Berndt *et al.* (1980)	Donatos & Mergos (1989)	Hasanov *et al.* (2016)
Bernstein & Griffin (2006)	Donatos & Mergos (1991)	Hausman (1979)
Betancourt (1981)	Dong & Kim (2018)	Hawkins (1975)
Bianco *et al.* (2009)	Dong *et al.* (2020)	Hawkins (1977)
Bianco *et al.* (2010)	Donnelly (1984)	Hawkins (1978)
Bigano *et al.* (2006)	Donnelly (1985)	He & Lambert (2004)
Bildirici *et al.* (2012)	Donnelly (1987)	Henderson (1983)
Bjerkholt & Rinde (1983))	Donnelly & Diesendorf (1984)	Henriksson *et al.* (2014)
Bjorner *et al.* (2001)	Donnelly & Saddler (1984)	Henson (1984)

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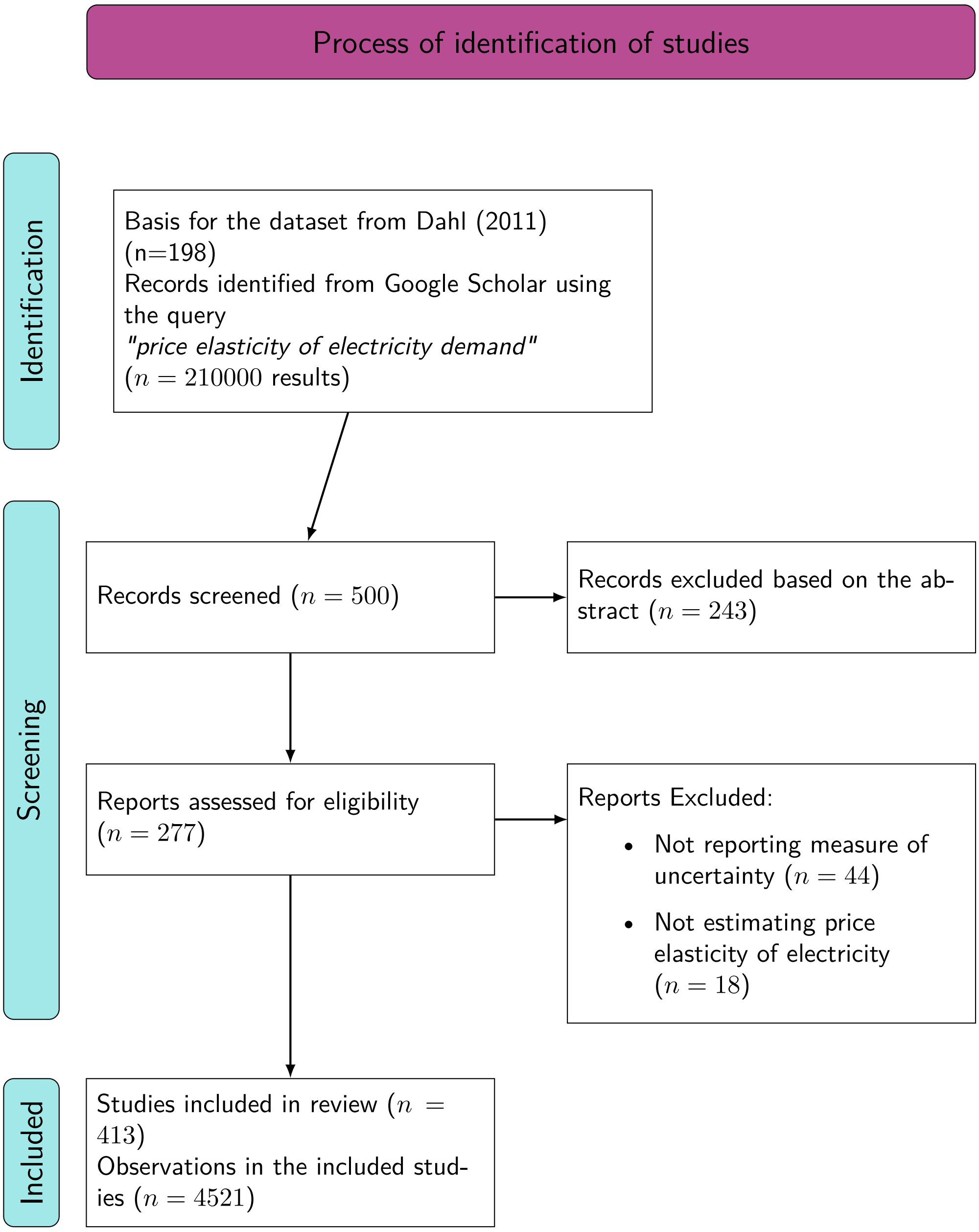
Table B.16: List of studies (Part 2)

List of studies included in the Meta-analysis (Part 2)		
Herriges & King (1994)	Lim *et al.* (2014)	Pindyck (1980)
Hesse & Tarkka (1986)	Lin & Ouyang (2014)	Pitt (1985)
Hieronymus (1976)	Lin & Zhu (2020)	Polemis (2007)
Hill *et al.* (1983)	Liu (2005)	Pourazarm & Cooray (2013)
Hirth *et al.* (2022)	Lohani (1992)	Poyer & Williams (1993)
Hisnanick & Kyer (1995)	Lyman (1994)	Rahman (1982)
Hogan (1989)	Lynk (1989)	Rai *et al.* (2014)
Holtedahl & Joutz (2004)	Ma & Stern (2016)	Ramcharran (1988)
Hondroyiannis (2004)	Macroconsult (2001)	Rapson (2014)
Horowitz (2007)	Maddala *et al.* (1994)	Reilly & Shankle (1988)
Houston (1982)	Maddala *et al.* (1995)	Reiss & White (2005)
Houthakker (1951)	Maddala *et al.* (1997)	Rossi & Tansini (1989)
Houthakker *et al.* (1974)	Maddigan *et al.* (1983)	Roth (1981)
Houthakker (1980)	Maddock *et al.* (1992)	Rouhani *et al.* (2022)
Hsiao *et al.* (1989)	Madlener (2011)	Roy (1986)
Hsiao & Mountain (1994)	Mahmud & Chishti (1990)	Ryan *et al.* (1996)
Hsueh & Gerner (1986)	Mansur *et al.* (2005)	S&Z Consultores (1999)
Hughes-Cromwick (1985)	Maria de Fátima *et al.* (2012)	Sa'ad (2009)
Hung & Huang (2015)	Masike & Vermeulen (2022)	Saddler & Donnelly (1983)
Huntington & Soffer (1982)	Matsui (1979)	Saddler *et al.* (1980)
Hyndman *et al.* (1980)	Matsukawa (1996)	Sadorsky (2012)
IEEJ (1986)	Matsukawa (2018)	Saha & Bhattacharya (2018)
Ilmakunnas & Torma (1989)	Matsukawa *et al.* (1993)	Schulte & Heindl (2017)
Inglesi (2010)	McFadden *et al.* (1977)	Schwarz (1984)
Inglesi-Lotz & Blignaut (2011)	Mchugh (1977)	Shaffer (2020)
Inglesi-Lotz (2011)	McRae & Meeks (2016)	Shi *et al.* (2012)
Iqbal (1986)	Meher (2020)	Shin (1981)
Ishaque (2018)	Mendoza & Vargas (1987)	Shin (1985)
Ishiguro & Akiyama (1995a)	Micklewright (1989)	Silk & Joutz (1997)
Ishiguro & Akiyama (1995b)	Mikayilov *et al.* (2017)	Silva *et al.* (2018)
Ito (2014)	Miller & Alberini (2016)	Smith (1980b)
Ito *et al.* (2018)	Moghaddam (2003)	Sterner (1985)
Jaffee & Olshavsky (1982)	Moghimzadeh & Kymn (1986)	Sterner (1989)
Jamil & Ahmad (2011)	Morovat *et al.* (2019)	Su (2018)
Javid & Qayyum (2014)	Mount & Chapman (1979)	Sutherland (1983a)
Jessoe & Rapson (2014)	Mount *et al.* (1974)	Sutherland (1983b)
Jin & Kim (2022)	Mountain (1982)	Talbi *et al.* (2022)
Jones (1995)	Mountain & Hsiao (1989)	Tambe & Joshi (2014)
Jungeilges & Dahl (1986)	Mountain (1989)	Tanishita (2019)
Kamerschen & Porter (2004)	Mountain *et al.* (1989)	TatlÄ (2017)
Karbuz *et al.* (1997)	Munley *et al.* (1990)	Taylor (1979)
KEEI (1989)	Murray *et al.* (1978)	Taylor *et al.* (1977)
Kaserman & Mayo (1985)	Nagata (2001)	Terza (1986)
Keng (1991)	Nakajima (2010)	THEC (1983)
Khan & Abbas (2016)	Nakajima & Hamori (2010)	Tiwari (2000)
Khan & Qayyum (2009)	Nan & Murry (1991)	Tiwari & Menegaki (2019)
Khanna *et al.* (2016)	Narayan & Smyth (2005)	Tran *et al.* (2023)
Khazzoom (1986)	Narayan *et al.* (2007)	Tserkezos (1992)
Knaut & Paulus (2016)	Nasir *et al.* (2008)	Türkekul & Unakıtan (2011)
Kohler (2014)	Okajima & Okajima (2013)	Uhr *et al.* (2019)
Kohler & Mitchell (1984)	Oliveira (1993)	Urga & Walters (2003)
Kokkelenberg & Mount (1993)	Olivia & Gibson (2008)	Uri (1977)
Kolstad & Lee (1993)	Otero Prada (1984)	Uri (1978)
Krishnamurthy & Kriström (2015)	Otsuka (2017)	Uri (1979a)
Labandeira *et al.* (2005)	Otsuka (2023)	Uri (1979b)
Lanot & Vesterberg (2021)	Otsuka & Haruna (2016)	Uri (1979c)
Lareau & Darmstadter (1982)	Parfomak & Lave (1996)	Uri (1979d)
Larsson (2004)	Parhizgari & Davis (1978)	Uri (1979e)
Larsson (2006)	Park & Acton (1984)	Uri (1982)
Laumas & Williams (1981)	Parti & Parti (1980)	Uri (1983)
Lee & Chiu (2011)	Paul *et al.* (2009)	Vashist (1984)
Lee & Lee (2010)	Pellini (2021)	Veall (1983)
Liddle & Huntington (2021)	Pesaran *et al.* (1999)	Veall (1987)
Lijesen (2007))	Pielow *et al.* (2012)	Velez *et al.* (1987)
Lillard & Acton (1981)	Pindyck (1979)	Verleger (1973)
Vlachou & Samouilidis (1986)	Westley (1984b)	Yang & Liang (2023)
Volland & Tilov (2018)	Westley (1989a)	Yin *et al.* (2016)
Wakashiro (2019)	Westley (1989b)	Yoo *et al.* (2007)
Walfridson (1987)	Westley (1992)	Young *et al.* (1983)
Walker (1979)	Wijemanne (1987)	Zachariadis & Pashourtidou (2007)
Wang (1985)	Wilson (1974)	Zhang *et al.* (2017)
Wang *et al.* (2020)	Wolak (2011)	Zhou *et al.* (2019)
Wang & Mogi (2017)	Woo *et al.* (2018)	Ziramba (2008)
Westley (1984a)	Yang (1978)	

Figure B.25: PRISMA Diagram



Notes: The diagram denotes the study collection procedure. The number of assessed papers includes snowballing - studies found after the initial screening. The final number of studies in the dataset includes both the added studies (215) and the studies updated from the Dahl dataset (198).

Table B.17: List of experiments

List of experimental studies included in the Meta-analysis		
Allcott (2011)	Hill *et al.* (1983)	Munley *et al.* (1990)
Atkinson (1979b)	Ito (2014)	Reiss & White (2005)
Byrne *et al.* (2021)	Ito *et al.* (2018)	Shaffer (2020)
Cao *et al.* (2023)	Jessoe & Rapson (2014)	Tran *et al.* (2023)
Davis (2008)	Kohler & Mitchell (1984)	Wolak (2011)
Deryugina *et al.* (2020)	Lillard & Acton (1981)	Zhou *et al.* (2019)
Herriges & King (1994)	Matsukawa (2018)	