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PRICE ELASTICITY OF ELECTRICITY REVISITED: A META-ANALYSIS

Vojtech Sikl
Zuzana Irsova
Peter Kudela
Anna Kudelova

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Institute of Economic Studies,
Faculty of Social Sciences,
Charles University in Prague

[UK FSV – IES]

Opletalova 26
CZ-110 00, Prague
E-mail : ies@fsv.cuni.cz
http://ies.fsv.cuni.cz

Institut ekonomických studií
Fakulta sociálních věd
Univerzita Karlova v Praze

Opletalova 26
110 00 Praha 1

E-mail : ies@fsv.cuni.cz
http://ies.fsv.cuni.cz

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Price Elasticity of Electricity Revisited: A Meta-Analysis

Vojtech Sikl
Zuzana Irsova
Peter Kudela*
Anna Kudelova

Institute of Economic Studies, Faculty of Social Sciences, Charles University, Prague
*Corresponding author: peter.kudela@fsv.cuni.cz

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Abstract:
This meta-analysis synthesizes 4,521 elasticity estimates drawn from 413 studies to examine the presence of publication and endogeneity bias in the literature. We coded over 100 study-level variables to assess how electricity consumers respond to price changes. Our results show that electricity demand is price inelastic, with an average short-run elasticity of -0.231 and average long-run elasticity of -0.532. However, after correcting for publication bias, the short-run elasticity declines in magnitude to -0.116, while the long-run elasticity adjusts to -0.303. Using Bayesian model averaging, we explore substantial heterogeneity in elasticity estimates. Factors such as declining tariff structures, demographic characteristics, fuel usage controls, daylight hours, and citation frequency significantly affect reported elasticities. In contrast, variables related to average and marginal electricity prices and time-of-use tariffs contribute minimally to the observed variation.

JEL: D01, Q40, Q49, C11
Keywords: meta-analysis, elasticity, price elasticity, electricity, heterogeneity, publication bias, consumer sensitivity

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1 Introduction

Electricity is a critical energy source for economic viability, which supports sectors such as manufacturing, healthcare, construction, and communication. Onakoya et al. (2013) describe electricity as "the pillar of wealth creation" in many developing economies, while Dahl (2011) highlight its increasing share among energy fuels as countries grow wealthier. Ensuring sufficient electricity generation is a fundamental prerequisite for economic growth (Outlook 2013). Understanding consumer responsiveness is vital for governments and utilities to forecast energy needs and devise effective pricing and taxation policies (Espey and Espey 2004).

On an individual level, aligning electricity supply with demand poses challenges. Demand fluctuates throughout the day, requiring costly energy storage during low-demand periods to meet peak needs (Ajanovic et al. 2020). Efficient energy storage is critical for a resilient and sustainable energy system, as emphasized in the EU Energy priorities (Commission 2017). Quantifying consumer demand elasticity, particularly through price elasticity metrics, aids in addressing this challenge.

Electricity demand has been extensively studied, with a consensus on a negative relationship between demand and price. However, estimates of price elasticity vary significantly. While some studies report elasticity below -0.6 (Bernard et al. 1996; Narayan et al. 2007; Kohler 2014), others estimate values above 1 (Hartman and Werth 1981; Pesaran et al. 1999; Bildirici et al. 2012). Several meta-analyses have sought to generalize the relationship between electricity price and demand. Early efforts include Taylor (1975), Aigner (1985), and Dahl (1993, 2011). Subsequent works include Espey and Espey (2004), Horáček (2014), Labandeira et al. (2017), Zhu et al. (2018), Zabaloy and Viego (2022), and Fatima (2023). This meta-analysis advances previous research by analyzing 4521 estimates from 413 studies and applying novel techniques to assess publication and endogeneity biases. Utilizing over 100 variables, it explores estimate heterogeneity using Bayesian and Frequentist model averaging.

We hypothesize that publication bias affects price elasticity estimates, as researchers may selectively publish certain findings due to time constraints or counterintuitive results (Song et al. 2013; Havranek and Irsova 2012). Some authors even argue that about 50% of completed studies remain unpublished (Scherer et al. 2018). Correcting for publication bias, we find the short-run elasticity to be -0.116, compared to a biased average of -0.231. Long-run elasticity

is revised from -0.532 to -0.303. Evidence of p-hacking is observed, with 4.4% of estimates clustered just below the 5% significance threshold.

This is also the first meta-analysis to explicitly address endogeneity bias. While prior studies suggest no significant effect of methods like IV on elasticity estimates, we find experimental data yields lower elasticities (-0.07) compared to non-experimental (-0.33) and quasi-experimental data (-0.11). Heterogeneity is influenced by variables such as data type, tariffs, daylight hours, and citation counts, whereas average prices, marginal prices, and time-of-use tariffs have limited explanatory power.

2 Data

The dataset includes 4521 elasticity estimates from 413 studies, covering 109 variables. The studies span from 1951 to 2023, with only two studies published before 1971 (Houthakker 1951; Fisher and Kaysen 1962). Between 1971 and 2023, publication dates appear uniformly distributed, with a median year of 1995. Figure A1 illustrates the wide variability in price elasticity estimates, showing no systematic trends in their variance.

To qualify for inclusion, studies had to meet the following criteria. Provide estimates of own price elasticity of electricity (excluding cross-price or income elasticities). Report an uncertainty metric, such as standard errors or confidence intervals. Studies with only p-values were included if standard errors could be approximated, as these are critical for meta-analysis and quantifying publication bias. The dataset includes estimates from peer-reviewed and non-peer-reviewed sources, with 210 estimates from books (Chern 1978; Donnelly 1984) or government studies (Verleger 1973; Matsui 1979; Wijemanne 1987). However, the dataset lacks many unpublished studies, limiting conclusions about publication bias based solely on summary statistics.

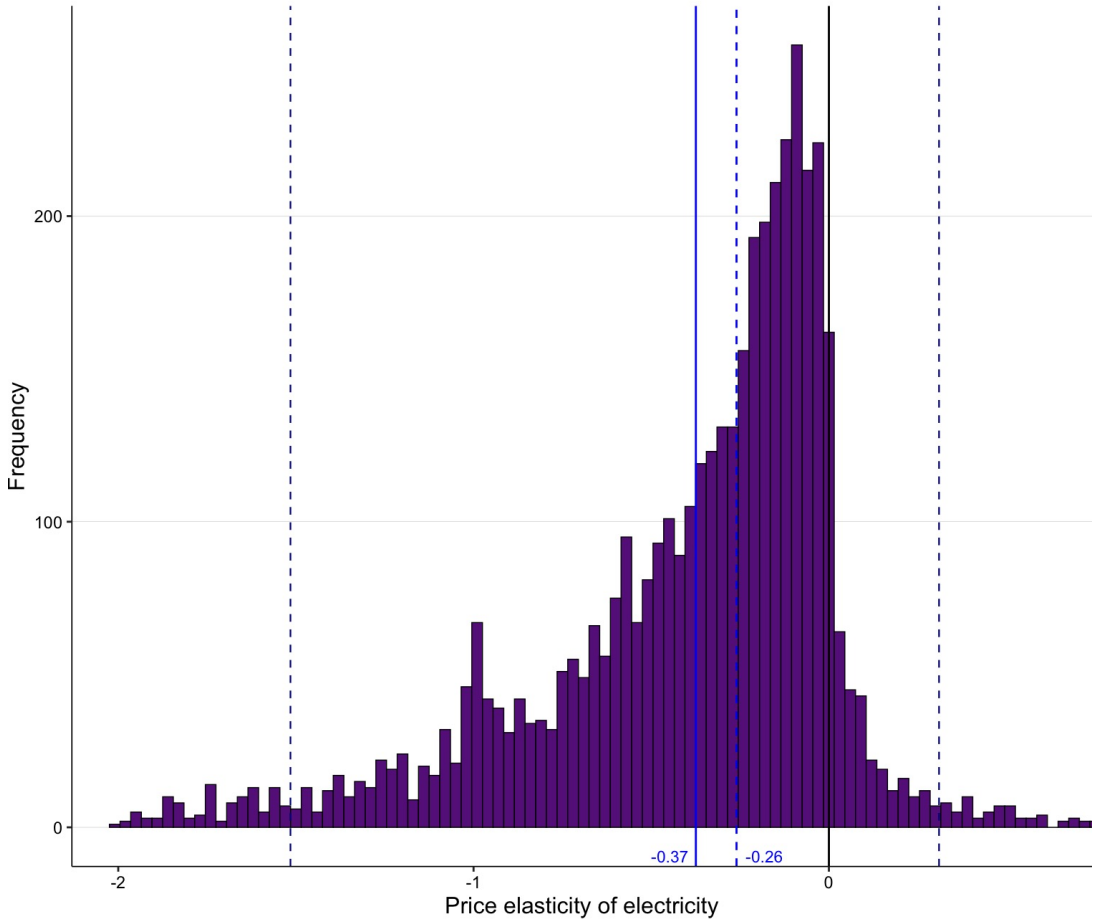
For studies lacking standard errors for long-run elasticity estimates (Blázquez et al. 2013; Saha and Bhattacharya 2018; Frondel et al. 2019), we applied the delta method to derive them from short-run coefficients and lagged consumption terms. The delta method, as discussed by Oehlert (1992), approximates transformations and their distributions using Taylor expansions. On average, each study contributes 10 to 11 elasticity estimates. Lastly, a 1% winsorization was applied to mitigate skewness without significant data loss, as recommended by Irsova et al. (2023b), who argue that outlier removal should be a last resort.

While comprehensive, the dataset is not exhaustive, as the search concluded on December 15, 2023. Citation counts for included studies, based on Google Scholar, were updated on March 3, 2024. The dataset is available upon request.

Table A1 summarizes the variables. The mean price elasticity is -0.395, indicating that a 1-unit price increase reduces demand by approximately 39.5%. Absolute elasticity values are higher for studies focusing on periods before 1984 and shorter time frames. Consistent with theoretical expectations, short-run elasticities are smaller than intermediate- and long-run elasticities. Granularity influences elasticity, with household-level data showing higher elasticity (-0.523) compared to country-level data (-0.289). Consumers responding to marginal prices exhibit greater elasticity than those reacting to average prices. Tariff structures also affect responsiveness: decreasing tariff structures elicit the highest elasticity, reflecting sensitivity to cost savings. Time-of-use tariffs lead to more modest adjustments, with higher usage during off-peak hours and limited flexibility during peak periods. These observations, derived from simple averages, should be interpreted with caution. Table A.3 contains the list of studies included in the meta-analysis.

Figure 1 shows the distribution of all elasticity estimates, which are negatively skewed. Most estimates range between -1 and 0, though extreme values are present, with approximately 50 estimates above 1 and 50 below -3.

Figure 1: Distribution of the effect



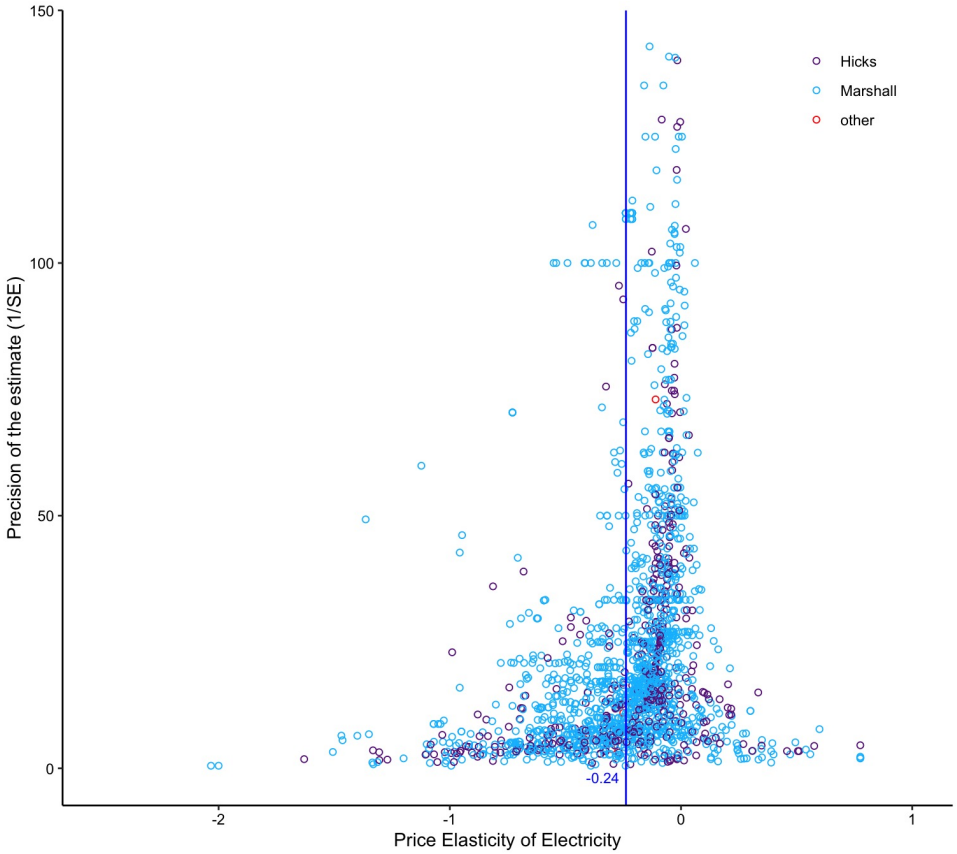
Notes: The figure depicts the distribution of elasticity effect for all estimates. Outliers are bunched to improve graphical interpretability but are included in all subsequent tests. The dashed vertical line denotes median and the solid vertical line is mean. The dark dashed lines represent 95% confidence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

3 Publication bias

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. Figure 2 displays the funnel plot for short-run elasticity. The plot exhibits a slightly skewed funnel shape. The most precise estimates cluster within a narrow range, spanning from a very slight negative effect (approximately -0.1) to a near-zero effect. Notably, precise positive estimates are scarce, and the negative estimates exhibit higher precision and greater density, creating asymmetry in the funnel plot.

Given that the most precise estimates are expected to cluster around the true effect, this pattern suggests that the true short-run price elasticity (-0.231) is significantly lower than the sample average.

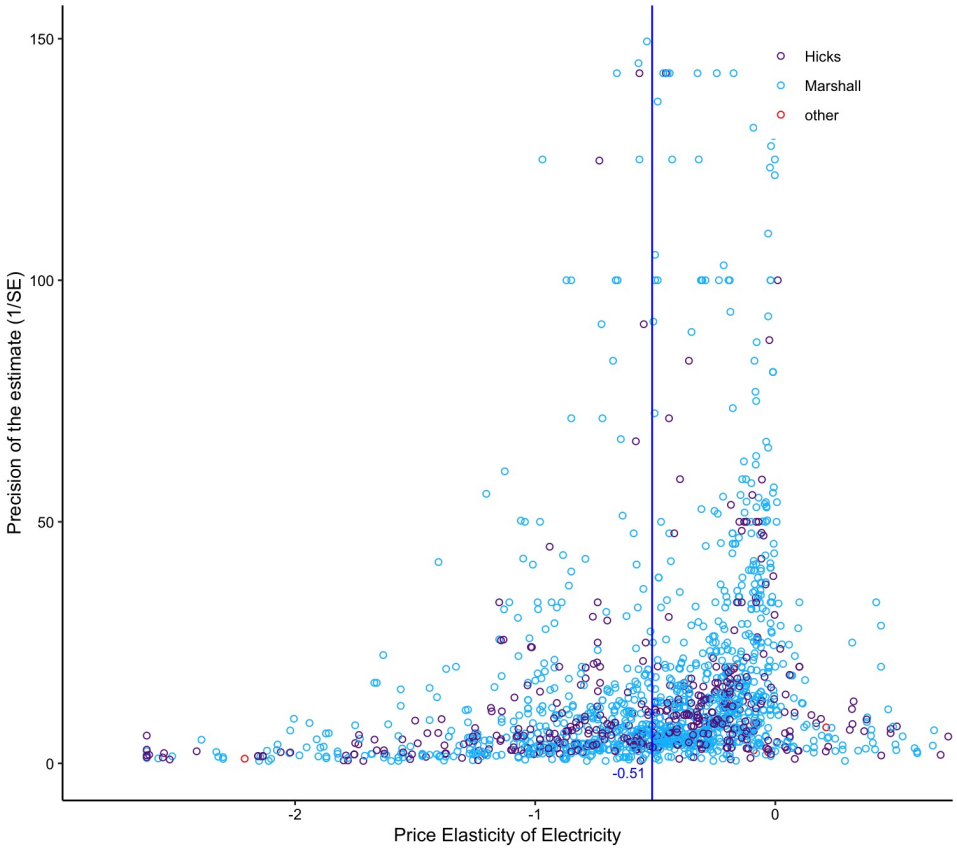
Figure 2: Funnel plot for short-run elasticity



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). For ease of exposition, extreme outliers are excluded from the figure but included in all statistical tests.

Figure 3 reveals distinct characteristics for intermediate-run elasticity estimates. Notably, the most precise estimates cluster between -0.5 and -0.05. Unlike the short-run funnel plot, the intermediate-run funnel plot lacks a clear funnel shape. A noticeable discrepancy exists between the number of precise positive estimates and their negative counterparts, with fewer precise positive estimates present. This lack of symmetry, coupled with a greater density of observations on the left side of the most precise estimates, suggests that positive elasticity estimates tend to exhibit lower precision. However, interpreting this funnel plot requires caution, given the lack of a universally accepted definition for intermediate-run elasticity.

Figure 3: Funnel plot for intermediate-run elasticity

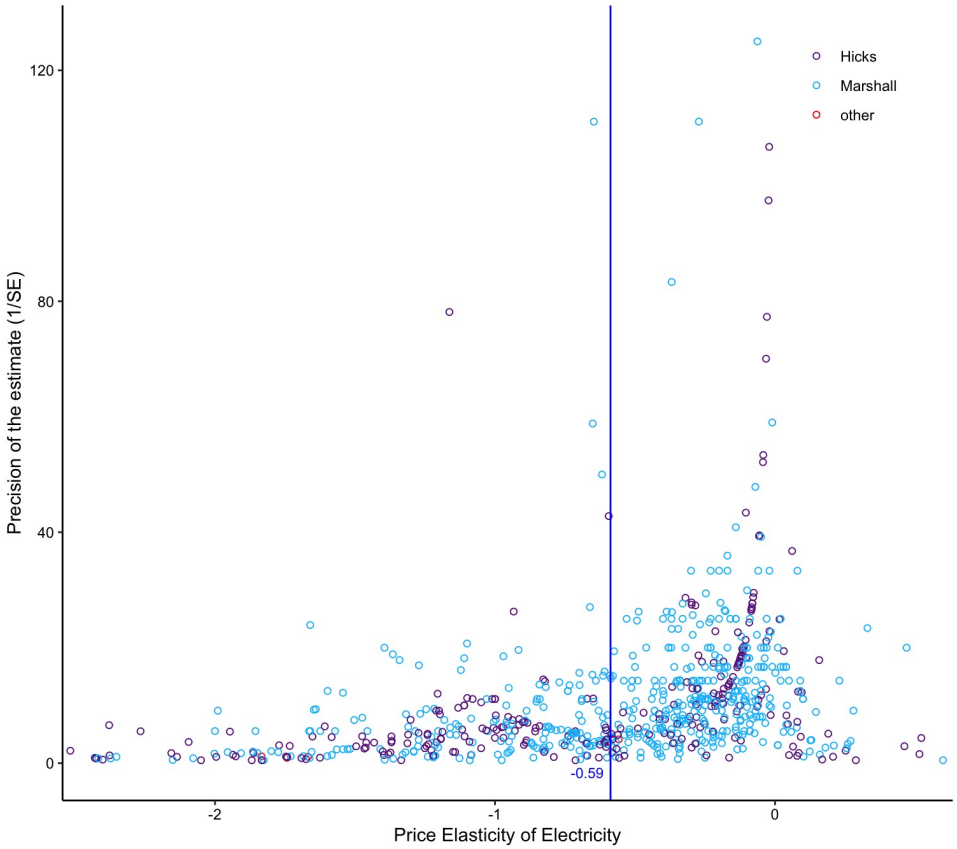


Notes: The figure displays a funnel plot for the intermediate-run subsample. The plot should be symmetrical in the absence of publication bias. Estimates with higher precision form funnel-like pattern. A vertical blue line marks the average value of these estimates, with a total count of 1671 data points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

The asymmetry observed in the intermediate-run funnel plot becomes even more pronounced in the long-run analysis. Assuming that the most precise estimates are centered around -0.05, the right side of the funnel plot is almost entirely absent. This observation may indicate a reluctance among researchers to report significant positive elasticity estimates.

While the short-run and long-run funnel plots exhibit considerable asymmetry, it remains premature to conclude the presence of publication bias solely based on these visualizations. A key point of concern is that the most precise estimates deviate from the overall sample mean elasticity.

Figure 4: 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). For ease of exposition, extreme outliers are excluded from the figure but included in all statistical tests.

It is important to note that the dataset comprises various types of elasticity estimates, which may not be directly comparable. In cases where Marshallian and Hicksian elasticities are not estimated separately, Hicksian elasticities were converted into Marshallian elasticities using the income elasticity reported alongside Hicksian estimates in individual studies. The delta method was applied to approximate the standard errors of these transformed effects.

3.1 Linear tests for detecting publication bias

A key approach for modeling publication selection bias is the Funnel Asymmetry Test (FAT), which examines the relationship between the estimate of price elasticity of electricity and its standard error (Card and Krueger 1995; Stanley 2005):

$$PE_{ij} = \alpha + \beta * SE(PE_{ij}) + \epsilon_{ij} \tag{1}$$

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 and ϵ is the (heteroskedastic) error term. As pointed out by Havranek and 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.

At this stage, the models were assumed to be exogenous. The analysis first employed ordinary least squares (OLS) estimation, followed by the inclusion of study-level fixed effects (FE) to control for unobserved heterogeneity. Subsequently, a between-study variation (BE) model was applied, along with a random-effects (RE) specification to assign weights for both within-study and between-study variation, as outlined in Bom and Rachinger (2019). Two weighting schemes were introduced to enhance robustness.

Study-Level Weighting (SW) to account for discrepancies in the number of reported estimates per study, estimates were weighted by the inverse of the number of estimates from each study. This ensures that each study contributes equally to the estimation process. A high constant (in absolute value) in this scheme could indicate that publication bias is concentrated in a small subset of studies. Precision Weighting (PW) following Stanley (2005), the precision of each estimate was used as a weight. By dividing Equation 1 by the standard error, the equation becomes:

$$\frac{PE_{ij}}{SE(PE_{ij})} = \frac{\alpha}{SE(PE_{ij})} + \beta + \varepsilon_{ij} \tag{2}$$

Note that the left-hand side of Equation 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 summarized in Table 1.

Significant evidence of publication bias was detected across all specifications and elasticity periods. The random-effects specification generated the highest corrected elasticity estimates. Nonetheless, careful interpretation is required, as studies reporting only a single price elasticity estimate may disproportionately influence the observed within-study variation.

Table 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Overall, the short-run elasticity corrected for publication bias ranges from -0.046 to -0.161. As observed in prior research (Stanley et al. 2008), the magnitude of publication bias often exceeds the true effect, which aligns with the findings here. While most models produced relatively consistent estimates, study-level and precision-weighting techniques yielded lower elasticity values and higher publication bias magnitudes. According to the fixed-effects model, a 1% increase in electricity prices reduces demand by approximately 0.15% in the short run, 0.38% in the intermediate run, and 0.43% in the long run.

3.2 Nonlinear tests for detecting publication bias

While linear methods are widely used in meta-analyses, they rely on a critical assumption: the relationship between price elasticity and its standard error is linear. However, this assumption may not always hold true. Bom and Rachinger (2019) argue that Equation 1 fails to adequately

account for the reduced likelihood of selection bias in highly precise estimates. Although linear methods provide valuable initial insights into publication bias, they may lead to imprecise results, introducing upward or downward bias in the estimated effect.

To address the potential non-linearity between price elasticity and standard error, alternative methods are applied. These approaches assume that the most precise estimates are less susceptible to publication bias but employ different strategies to accommodate non-linearities. Such methods have been utilized in published meta-analyses, including those by Cazachevici et al. (2020) and Havranek et al. (2022).

Following methods have been used Top10 method, introduced by Stanley and Doucouliagos (2010), the stem-based method, developed by Furukawa (2019),the WAAP method, proposed by Ioannidis et al. (2017), the Selection Model by Andrews and Kasy (2019), the Endogenous Kink method, described by Bom and Rachinger (2019), and finally the Hierarchical Bayes model (Allenby and Rossi 2006).

The results of non-linear tests, presented in Table 2, confirm the presence and significant magnitude of publication bias. The corrected elasticity estimates, based on median values, are approximately -0.11, -0.34, and -0.33 for the short, intermediate, and long-term periods, respectively.

For the short run, the corrected elasticity (-0.11) is less than half the sample mean (-0.231), highlighting the extent of publication bias. Notably, the intermediate-run and long-run corrected elasticities are of similar magnitude, which challenges the expectation that long-run elasticities should exhibit the largest absolute values. The stem-based method produces insignificant corrected elasticities for both the short-run and long-run, but it identifies a significant intermediate-run effect of -0.446, the largest among the methods applied.

The similarity in intermediate and long-run corrected elasticities suggests potential inconsistencies in how authors define elasticity periods. These findings underscore the importance of considering publication bias in meta-analyses and highlight the utility of non-linear methods for refining elasticity estimates.

Table 2: Non-linear tests for publication bias

Effect beyond bias *(Short-run)*			
WAAP	-0.127***	-0.123***	**Selection model**
	(0.006)	(0.006)	
Top10	-0.104***	-0.107***	**Hierarchical Bayes**
	(0.009)	(0.021)	
Stem-based method	-0.08	-0.046***	**Endogenous kink**
	(0.011)	(0.003)	
Publication bias			
Hierarchical Bayes	-1.262***	-2.854***	**Endogenous kink**
	(0.142)	(0.227)	
Number of observations = 1846			
Effect beyond bias *(Intermediate-run)*			
WAAP	-0.335***	-0.337***	**Selection model**
	(0.013)	(0.014)	
Top10	-0.245***	-0.341***	**Hierarchical Bayes**
	(0.022)	(0.035)	
Stem-based method	-0.446***	-0.153***	**Endogenous kink**
	(0.011)	(0.009)	
Publication bias			
Hierarchical Bayes	-0.940***	-3.148***	**Endogenous kink**
	(0.146)	(0.484)	
Number of observations = 1723			
Effect beyond bias *(Long-run)*			
WAAP	-0.320***	-0.345***	**Selection model**
	(0.020)	(0.030)	
Top10	-0.241***	-0.388***	**Hierarchical Bayes**
	(0.038)	(0.104)	
Stem-based method	0.002	-0.062***	**Endogenous kink**
	(0.057)	(0.008)	
Publication bias			
Hierarchical Bayes	-0.999***	-3.468***	**Endogenous kink**
	(0.155)	(0.295)	
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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

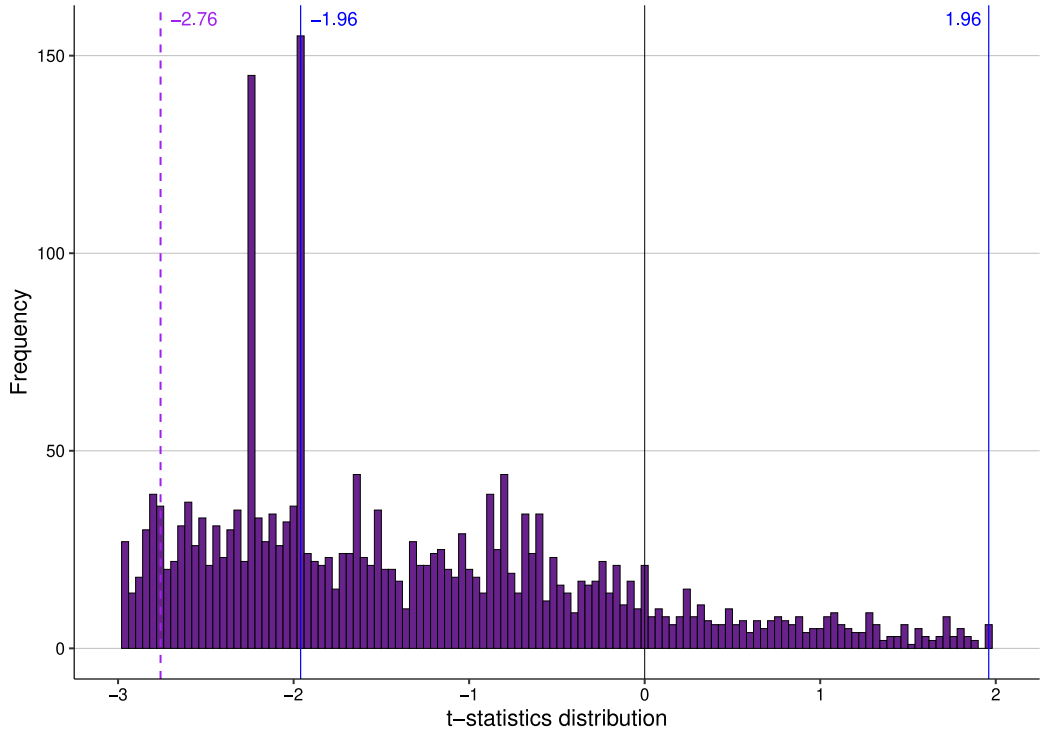
3.3 P-Hacking

We now turn to tests investigating the presence of p-hacking within the dataset. Although the boundary between publication bias and p-hacking is often blurred, p-hacking typically refers to deliberate or inadvertent manipulation of data or methodologies to achieve statistically significant results, such as more favorable p-values (Irsova et al. 2023b). Examples of such

practices include selectively adding data points, altering estimation procedures, or excluding specific subsamples to enhance significance (Brodeur et al., 2023).

An inspection of the t-statistic distribution in Figure 5 shows a sharp concentration just below the critical value of -1.96, with nearly 200 estimates in $[-1.99, -1.96]$. Even with a narrower bin of 0.01, more than 130 estimates fall immediately below the threshold. A smaller discontinuity is also visible around 1.96.

Figure 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.

To assess publication bias, we apply the caliper tests of Gerber et al. (2008). Unlike traditional methods focusing on the relationship between effect sizes and standard errors, caliper tests analyze the distribution of t-statistics near critical thresholds, such as 1.645 and 1.96, which correspond to 10% and 5% significance levels, respectively. The test assesses asymmetry in these distributions, with a spike in t-statistics above critical values serving as clear evidence of bias (Gerber et al. 2008).

The results, summarized in Table 3, show that 61% of observations fall below the negative critical value of -1.96, indicating significance. This proportion increases with wider caliper intervals, underscoring the asymmetry. Similar results are observed for the 10% significance threshold, where over 80% of estimates exceed the critical value of -1.645. For positive t-statistics, significant asymmetry is also detected around 1.96, although the lower sample size for positive estimates warrants caution. Importantly, the caliper test is robust to distributions centered away from the critical value, as demonstrated in our dataset where the mean t-statistic is -2.76 (Gerber et al. 2008).

To further explore p-hacking, we employ p-curve analysis following Elliott et al. (2022). This methodology examines the distribution of p-values under the null hypothesis of no true effect. A right-skewed p-curve suggests genuine effects, while a left-skew indicates p-hacking. The theoretical constraints on the monotonicity and exponential bounds of the p-curve depend on the critical value and the nature of the t-statistic. As shown in Panel A of Table 4, the null hypothesis of no p-hacking is rejected across all reasonable significance levels. This trend persists across subsets of the data, emphasizing the pervasive nature of p-hacking.

Additionally, we employ the Meta-Analysis Instrumental Variable Estimator (MAIVE) developed by Irsova et al. (2023a), which builds on the Egger regression. MAIVE addresses endogeneity in standard errors caused by p-hacking by instrumenting standard error with the inverse of sample size. This instrument is expected to correlate with the standard error while remaining unaffected by measurement error or estimation procedures (Opatrny et al. 2023). However, the F-test for instrument strength reveals that the instrument is relatively weak. Despite this limitation, the MAIVE coefficient is estimated at 0.605, as reported in Table 4.

The analysis reveals a significant prevalence of publication bias and p-hacking within the dataset. Corrected elasticity estimates exhibit some variability. Short-run elasticity consistently approximates -0.1 across different methodologies, while intermediate and long-run elasticities range between -0.33 and -0.38. These corrected values are generally higher than those reported by Horáček (2014), though they remain smaller in magnitude compared to the findings of Zhu et al. (2018).

Table 3: 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.

Table 4: 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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

3.4 Endogeneity bias

Endogeneity is a critical challenge in estimating electricity demand, arising from the simultaneous determination of consumption and price (Paul et al. 2009), variable tariffs, or omitted variable bias (e.g., seasonal effects). Consumer behavior, such as bunching under increasing block tariffs, further complicates isolating the causal impact of price changes, leading to biased elasticity estimates if not addressed (Borenstein 2009; Rapson 2014).

To explore the effects of endogeneity, we classified studies into four groups. Natural experiments, leveraging exogenous events or policies (e.g., Byrne et al. 2021). Quasi-experiments, using methods like regression discontinuity designs (RDD) or matching (e.g., Zhou et al. 2019). Non-experimental studies, controlling for endogeneity by employing techniques like IV, 2SLS, or GMM(e.g., Alberini et al. 2019). Studies not controlling for endogeneity, often relying on OLS or random effects (Apte 1983; Hesse and Tarkka 1986; Fan and Hyndman 2011).

While natural and quasi-experiments enable stronger causal inference, techniques addressing endogeneity directly (e.g., IV or GMM) are also essential for mitigating bias. Comparing results across these subsamples highlights how methodological choices impact elasticity estimates, enhancing the reliability of conclusions.

Table 5: 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 the linear tests (Table 5) indicate substantial differences between experimental and non-experimental data, with minimal variation between endogeneity subsamples (control vs. no control). The average price elasticity corrected for publication bias in natural experiments is approximately -0.06, about one-fifth of the corresponding value for non-experimental data. Despite these corrections, publication bias remains evident, with the median corrected elasticity for non-experimental subsamples ranging from -0.3 to -0.28. Due to the limited observations and the short-run focus of most experiments, we did not segment elasticities into short-run,

intermediate-run, and long-run categories.

Non-linear methods for detecting publication bias yield similar conclusions. Corrected price elasticity is lower for studies controlling for endogeneity (median around -0.243) and for those without endogeneity controls (median around -0.2), compared to the sample average of around -0.4. Notably, the stem-based method produces an insignificant estimate at the 10% level for experimental studies. These findings confirm significant evidence of publication bias across subsamples, with corrected elasticities from natural experiments markedly lower than those from non-experimental data. However, even experimental studies appear affected by publication bias.

Table 6: Non-linear tests for publication bias for endogeneity subsamples

Effect beyond bias *(Natural Experiments)*			
WAAP	−0.078***	−0.071***	**Selection model**
	(0.007)	(0.005)	
Top10	−0.035***	−0.055***	**Hierarchical Bayes**
	(0.007)	(0.161)	
Stem-based method	−0.014	−0.039***	**Endogenous kink**
	(0.027)	(0.004)	
Publication bias			
Hierarchical Bayes	−2.458	−2.313***	**Endogenous kink**
	(1.768)	(0.501)	
Number of observations = 299			
Effect beyond bias *(Quasi Experiments)*			
WAAP	−0.140***	−0.145***	**Selection model**
	(0.017)	(0.017)	
Top10	−0.051***	−0.148**	**Hierarchical Bayes**
	(0.011)	(0.065)	
Stem-based method	−0.032	−0.047***	**Endogenous kink**
	(0.047)	(0.011)	
Publication bias			
Hierarchical Bayes	−1.109*	−2.037***	**Endogenous kink**
	(0.868)	(0.473)	
Number of observations = 63			
Effect beyond bias *(Endogeneity Control)*			
WAAP	−0.292***	−0.296***	**Selection model**
	(0.013)	(0.016)	
Top10	−0.161***	−0.201***	**Hierarchical Bayes**
	(0.026)	(0.045)	
Stem-based method	−0.426***	−0.167***	**Endogenous kink**
	(0.016)	(0.014)	
Publication bias			
Hierarchical Bayes	−1.367***	−2.022***	**Endogenous kink**
	(0.194)	(0.779)	
Number of observations = 944			
Effect beyond bias *(No Endogeneity Control)*			
WAAP	−0.232***	−0.212***	**Selection model**
	(0.008)	(0.008)	
Top10	−0.198***	−0.249***	**Hierarchical Bayes**
	(0.015)	(0.023)	
Stem-based method	−0.081***	−0.08***	**Endogenous kink**
	(0.026)	(0.004)	
Publication bias			
Hierarchical Bayes	−1.196***	−3.265***	**Endogenous kink**
	(0.110)	(0.208)	
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: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

4 Heterogeneity

The magnitude of estimated effects exhibits considerable variation depending on specific factors and control variables. Out of 109 collected variables, 74 were selected for analysis after omitting variables with insufficient data or relevance. These variables are categorized into nine groups: study-level data characteristics, data aggregation, types of price elasticity, types of electricity demand, types of electricity price, electricity tariff structures, demand controls, model and function specifications, and publication characteristics. The full variable description summary is presented in Table 7.

Table 7: Definition and summary statistics of selected variables

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 the study covers	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

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Variable	Description	Mean	SD
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
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 a 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

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Variable	Description	Mean	SD
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.

Study-Level Data Characteristics

Variables such as elasticity estimates, t-statistics, number of observations, temporal scope, and demographic factors are included. Differences by region (e.g., USA vs. Europe) and temporal factors, such as daylight and temperature, are also analyzed (Auray et al. 2019; Dahl 1993; Schwartz et al. 2013).

Data Aggregation

Electricity demand studies draw on data ranging from national aggregates to household-level meter readings, each influencing elasticity estimates differently (Krishnamurthy and Kriström, 2015). While aggregated data are easier to obtain, mixing them with disaggregated sources without caution may lead to aggregation bias and hinder policy relevance (Dahl, 1993). Recent findings, however, suggest no consistent pattern in how data aggregation affects elasticity estimates (Miller and Alberini, 2016).

Type of Price Elasticity

Electricity demand studies employ Marshallian, Hicksian, and Morishima elasticities to capture distinct consumer responses to price changes. While these measures are grounded in different theoretical assumptions, few studies clearly state whether they follow utility maximization or cost minimization frameworks (Jones, 1995; Pitt, 1985; Cao et al., 2023), complicating comparability. To account for temporal dynamics, short-run elasticities capture immediate behavioral

responses, long-run estimates reflect structural adjustments such as appliance stock changes (Houthakker, 1980; Filippini, 2011), and intermediate-run estimates provide a transitional view using pooled or annual data.

Electricity Demand Types

Electricity demand is typically categorized into residential, commercial, and industrial segments, each with distinct characteristics and policy implications. Residential demand reflects household usage and is shaped by income and efficiency factors (Sa'ad, 2009), while commercial demand supports business operations and exhibits the highest short-run variability in the U.S. (Woo et al., 2018). Industrial demand, often linked to economic output, involves high energy intensity and has been shown to correlate with economic growth (Bildirici et al., 2012).

Electricity Price Types

The choice between average and marginal electricity prices is crucial in demand estimation, as consumers may respond differently depending on which price they perceive. Although marginal price better reflects the cost of additional consumption, its complexity, especially under increasing block tariffs, often leads consumers to rely on average price instead (Shin, 1985; Borenstein, 2009). Shaffer (2020) finds that 85% of consumers respond to average price, while only 7% react to marginal price, highlighting a significant gap between theoretical assumptions and actual behavior. These misperceptions can reduce policy effectiveness; as Schneider and Sunstein (2017) note, cost-reflective tariffs may not lead to efficient outcomes. However, Jessoe and Rapson (2014) show that providing high-frequency price information improves consumer responsiveness substantially.

Electricity Tariff Structures

Electricity pricing structures are generally categorized into four types: increasing, decreasing, flat, and time-of-use (TOU) tariffs. TOU tariffs, which vary prices by time of day, have gained particular attention for their ability to incentivize consumption shifting away from peak periods, thus reducing strain on the grid and improving efficiency (Filippini, 2011; Holland and Mansur, 2008). Experimental evidence shows that consumers do respond to dynamic hourly

pricing (Wolak, 2011), and TOU policies may also promote cleaner energy use, though the net environmental impact depends on the composition of peak and base-load energy sources.

Demand Controls

To capture potential sources of heterogeneity in electricity price elasticity, five dummy control variables are included in the dataset. These are: *demographics_control*, reflecting household characteristics (e.g., size, construction date); *temperature_control*, which accounts for climate effects through cooling and heating degree days (CDD/HDD), crucial in capturing electricity demand variations due to weather (Dong and Kim, 2018; Auray et al., 2019); *stock_control*, indicating inclusion of appliance stock in demand equations; $fuels_control$, denoting whether substitute fuels are considered; and *income_control*, which captures the influence of income levels. These controls help reduce omitted variable bias, a concern emphasized by Lanot and Vesterberg (2021) and Miller and Alberini (2016), especially in studies using detailed household-level data.

Model and Estimation Specifications

Different model specifications and estimation techniques yield systematically varying elasticity estimates due to differing assumptions and treatment of unobserved heterogeneity. To capture this variation, we code nine model and estimation types, including indicators for reduced-form vs. structural-form models and static vs. dynamic specifications. Dynamic models, which incorporate lagged consumption, are particularly important for distinguishing short-run from long-run responses but may suffer from multicollinearity between current and lagged consumption variables. We also track the use of linear, semi-logarithmic, double-logarithmic functional forms, and usage of Box-Cox transformations, along with a dummy variable (*endo_control*) to flag whether studies correct for endogeneity bias (Dahl, 1993; Kamerschen and Porter, 2004).

Publication Characteristics

We include indicators for whether a study was published in a high-impact journal ($impact_journal$) and the *publication_year* to reflect the temporal context of the research. Additionally, we gathered the total number of citations for each study and applied a transformation ($citations(t)$) to normalize citation counts across studies published in different years.

This comprehensive categorization reveals the significant heterogeneity in price elasticity estimates and their dependency on methodological and contextual factors.

4.1 Model Averaging

To explore the heterogeneity of electricity price elasticity estimates, we constructed a dataset including 74 explanatory variables. A direct regression using all variables would risk inflated standard errors and multicollinearity (Irsova et al., 2023b). Moreover, given the ex-ante uncertainty regarding the appropriate model specification, such as the choice between average or marginal price, or the inclusion of demand-side controls, we face considerable model uncertainty (Steel, 2020). Relying on a single specification could misrepresent the underlying economic reality. To address this, we employ Bayesian Model Averaging (BMA), a standard practice in meta-analyses (Havranek et al., 2018; Bajzik, 2021).

BMA integrates over a space of models, weighting each by its posterior model probability, thereby accounting for model uncertainty in a probabilistically coherent manner. The importance of each variable is captured by its Posterior Inclusion Probability (PIP), which aggregates the probabilities of all models including that variable (Kass and Raftery, 1995). We use the Unit Information Prior (UIP) for coefficient priors and adopt a dilution prior for model probabilities to mitigate multicollinearity among correlated variables (George, 2010). This approach has been applied successfully in prior meta-analyses (Cala et al., 2022; Elminejad et al., 2023).

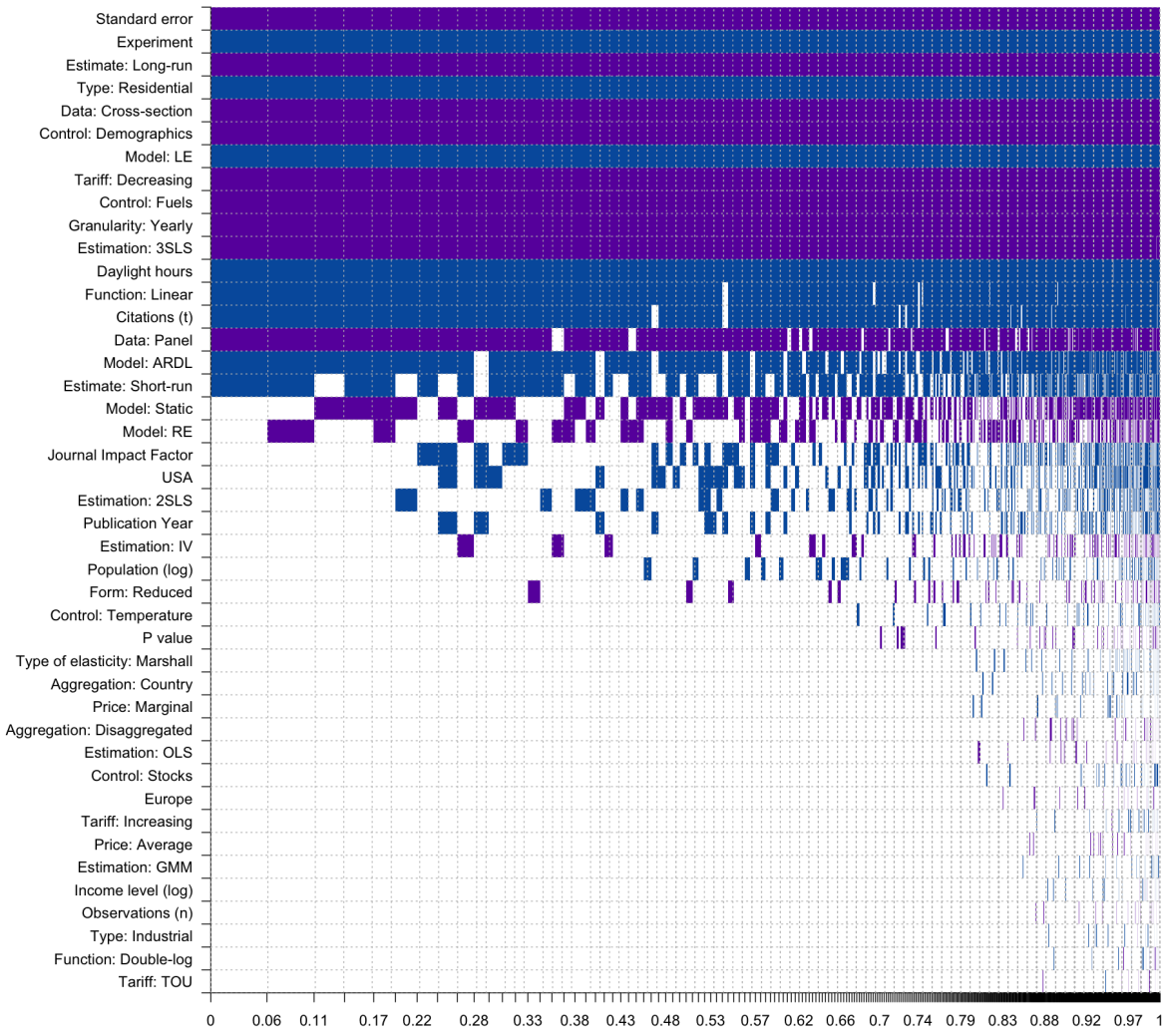
To ensure robustness, we also apply Frequentist Model Averaging (FMA), which requires no prior specification and weights models based on data-driven criteria. Prior to estimation, we assessed multicollinearity via the correlation matrix and Variance Inflation Factors (VIF). Highly correlated or redundant variables (e.g., *mid_year*, *monthly_granularity*) were excluded, and we avoided the dummy variable trap by omitting redundant dummy group members.

Following these adjustments, our final model includes 43 variables and is estimated as:

$$PE_{ij} = \alpha + \beta * SE(PE_{ij}) + \sum_{p=1}^{43} \gamma_p * X_{ijp} + \epsilon_{ij} \tag{3}$$

This equation presents the general regression of price elasticity estimation. The individual terms are defined as follows (in line with setting from Equation 1): α denotes the price elasticity corrected for publication bias, and β describes the magnitude and direction of publication bias.

Figure 6: 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. Numerical results can be referred to in Table 8.

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.

4.2 Bayesian Model Averaging Results

Figure 6 presents a graphical summary of the Bayesian Model Averaging (BMA) results, illustrating the posterior inclusion probability (PIP) of variables. Variables are arranged by

inclusion frequency, with *Standard error*, *experiment*, and *long-run elasticity* being the most frequently included. On the horizontal axis, variable effects are color-coded: blue for positive effects, purple for negative effects, and white for variables excluded from the models.

The results indicate that approximately 40% of characteristics significantly contribute to explaining price elasticity heterogeneity. The inclusion frequency declines rapidly for subsequent variables, with about one-third appearing in the fewest models. The BMA approach provides posterior means, standard deviations, and PIP values, analogous to coefficient estimates, standard errors, and p-values in frequentist methods. According to Jeffreys (1998), PIP values are classified as follows: *weak effect* (PIP between 0.5 and 0.75), *substantial effect* (PIP between 0.75 and 0.95), and *strong* or *decisive effect* for PIP intervals of [0.95, 0.99) and [0.99, 0.1], respectively.

Table 8 summarizes the numerical BMA results. Of nineteen variables with PIP above 0.5, two exert a weak effect (*Estimate: Short-run, Model: Static*), one a substantial effect (*Model: ARDL*), three a strong effect (*Panel data, Function: Linear, Citations (t)*), and 13 a decisive effect (*Constant, Standard error, Experiment, Daylight hours, Estimate: Long-run, Cross-sectional (data), Granularity: Yearly, Type: Residential, Tariff: Decreasing, Control: Demographics, Control: Fuels, Model: LE, Estimation: 3SLS*). While the constant term appears statistically significant, its interpretation is limited due to a lack of posterior inclusion probability (PIP). Consistent with earlier evidence, both Bayesian and Frequentist Model Averaging (BMA and FMA) confirm the presence of publication bias, with magnitudes comparable to those identified via linear and non-linear bias tests.

Focusing on key determinants, only a few variables show robust explanatory power. Experimental settings significantly reduce elasticity estimates, likely due to their stronger identification of causal effects. Daylight hours, correlated with ambient temperature, are also associated with less elastic demand. Although US-based studies tend to report lower elasticity magnitudes, this effect is statistically insignificant. Notably, both panel and cross-sectional data structures, as well as annual data granularity, significantly affect the estimates, aligning with findings by Zhu et al. (2018). However, unlike Zhu et al. (2018), we find the 3SLS estimator to exert a strong influence, while GMM appears insignificantâ€"similar to results reported by Zabaloy and Viego (2022) for the Caribbean region.

Interestingly, neither average nor marginal price levels significantly explain heterogeneity, contrary to claims in the literature suggesting stronger consumer sensitivity to average prices (Ito, 2014; Shaffer, 2020). Among tariff types, only decreasing tariffs show a significant effect, whereas time-of-use tariffs do not, despite extensive literature supporting their impact (Holland and Mansur, 2008; Torriti, 2020). Of the demand-side controls, only demographic and fuel substitution variables are significant; appliance stock and temperature controls (CDD/HDD) are not, diverging from earlier work by Holtedahl and Joutz (2004).

Among publication-related variables, only the adjusted citation count (*Citations (t)*) shows a significant and positive relationship with elasticity estimates. BMA and FMA results are highly consistent in the direction and significance of effects; all variables deemed significant in BMA also show significance in FMA ($p < 0.05$). FMA does, however, identify additional variables, such as U.S. data, random-effects models, IV estimation, and publication year, as significant, particularly within the Marshallian subsample.

Table 8: 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 9: 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.

4.3 Best practice estimate

Constructing best practice estimates involves using coefficients derived from model averaging and selecting variables that reflect the ideal conditions for elasticity estimation. This process is inherently subjective, as the inclusion of variables depends on BMA performance, which is influenced by prior beliefs or preliminary tests. Consequently, this approach should be viewed as an additional robustness check rather than an independent finding.

Initially, we set variable values to their sample means, with a few exceptions. To address publication bias, we set the standard error to 0. Additionally, we assigned the maximum values

to journal impact factor and citation characteristics, as these are assumed to represent reliable methodologies. Panel data was chosen for its superior informational value. For short-run results, we set the short-run variable to 1 and the long-run value to 0, and vice versa for long-run results.

The short-run best practice estimate of -0.116 is borderline insignificant, compared to the sample average of -0.231. This estimate is consistent with the elasticity from IV regression in Table A2. The long-run estimate is -0.303, slightly lower than Labandeira et al. (2017) and about half of Zhu et al. (2018)'s estimates. Horáček (2014) reports a lower short-run elasticity of -0.06 but a higher long-run estimate of -0.430.

Cross-country elasticity estimates are also provided. The robustness of these estimates is constrained by the limited number of reports for some countries. Estimates largely align with our best practice estimate, except for Norway, Pakistan, and Japan. The results for Norway and Pakistan are less reliable due to fewer than 20 observations. In contrast, Japan's 185 elasticity reports suggest a significant estimate of -0.200, implying that a 1% price increase leads to a 0.2% decrease in electricity demand.

Table 10: 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.

5 Conclusion

This paper presents a comprehensive meta-analysis of electricity price elasticity, synthesizing 4,521 estimates from 413 empirical studies. While prior literature has emphasized the substantial heterogeneity in elasticity estimates (Dahl, 1993), our findings suggest that much of this variation can be systematically linked to methodological choices, data characteristics, and publication dynamics. Short-run elasticities cluster between -0.12 and -0.07, while intermediate- and long-run estimates range from -0.38 to -0.34, all indicating inelastic demand. Importantly, we find robust evidence of publication bias and p-hacking, and report that elasticities from natural experiments (-0.07 to -0.05) are significantly lower than those from non-experimental settings. Endogeneity correction modestly increases elasticity values, though not substantially.

By coding over 100 variables and incorporating 43 into Bayesian and Frequentist model averaging, we reveal that key drivers of elasticity estimates include experimental design, demand

type, time horizon, and estimation technique. Residential demand, short-run specifications, and linear functional forms are associated with higher elasticities, while panel data, long-run horizons, and inclusion of demographic or fuel controls yield more inelastic responses. Although average electricity price is widely considered more salient for consumer behavior (Borenstein, 2009; Ito, 2014), it does not consistently emerge as a dominant predictor. Our best-practice estimates suggest electricity demand remains inelastic, with corrected values of -0.116 (short-run) and -0.303 (long-run). Sensitivity analysis confirms that uncorrected biases may overstate price responsiveness, even producing artificially elastic estimates in some cases.

Our findings offer several implications for policymakers and researchers. First, accounting for publication and methodological biases is essential when interpreting elasticity estimates for regulatory or pricing policy. Second, further disaggregation, by demand type, income group, industrial subsector, and energy mix, could help refine policy instruments. For instance, elasticity may differ across household income levels (Gundimeda and Kohlin, 2008), industrial activities (Chaudhry, 2010), or appliance types (Volland and Tilov, 2018), each warranting tailored policy responses. Lastly, future work should explore how the energy transition toward renewables and nuclear sources may reshape price responsiveness across sectors and regions.

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Appendices

Figure A1: 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).

Table A1: Full statistics summary

Variable Name	Sample Mean	Weighted Mean	n
All Data	-0.395	-0.417	4521
Observations (n) $\geq$ 608	-0.383	-0.430	2263
Observations (n) $<$ 608	-0.408	-0.409	2258
If appropriate transformation is used	-0.385	-0.409	4501
t-statistic $\geq$ -2.68	-0.230	-0.235	2263
t-statistic $<$ -2.68	-0.561	-0.560	2258
Standard error $\geq$ 0.086	-0.544	-0.588	2261
Standard error $<$ 0.086	-0.247	-0.252	2260
Experiment	-0.129	-0.180	362
P value	-0.347	-0.369	381
Estimate: Short-run	-0.231	-0.247	1866
Estimate: Intermediate-run	-0.502	-0.495	1842
Estimate: Long-run	-0.532	-0.624	813
Type: Marshall	-0.398	-0.414	3326
Type: Hicks	-0.385	-0.420	1176
Type: other	-0.548	-0.465	20
Start year $\geq$ 1977	-0.390	-0.440	2315
Start year $<$ 1977	-0.401	-0.398	2206
End year $\geq$ 1991	-0.348	-0.390	2448
End year $<$ 1991	-0.452	-0.445	2073
Mid year $\geq$ 2000	-0.389	-0.410	1166
Mid year $<$ 2000	-0.398	-0.418	3355
Number of years$\geq$ 14.5	-0.322	-0.354	2262
Number of years $<$ 14.5	-0.470	-0.473	2259
Daylight hours $\geq$ 14.767	-0.386	-0.404	3100
Daylight hours $<$ 14.767	-0.415	-0.433	1421
Annual temperature $\geq$ 9.146	-0.355	-0.381	2290
Annual temperature $<$ 9.146	-0.437	-0.462	2231
Electricity exporter dummy	-0.398	-0.430	3738
Carbon intensity production $\geq$ 6.124	-0.394	-0.397	3307
Carbon intensity production $<$ 6.124	-0.399	-0.464	1214
Population (log) $\geq$ 19.096	-0.358	-0.396	2263
Population (log) $<$ 19.096	-0.433	-0.432	2258
Income level (log) $\geq$ 9.012	-0.350	-0.384	2261
Income level (log) $<$ 9.012	-0.441	-0.439	2260
Hottest months	-0.213	-0.247	68
Coolest months	-0.427	-0.367	78
USA	-0.395	-0.389	2151
Europe	-0.410	-0.432	833
Other location	-0.391	-0.427	1598
Aggregation: Country	-0.289	-0.307	1224
Aggregation: Region	-0.397	-0.443	1082
Aggregation: City	-0.387	-0.393	654
Aggregation: Disaggregated	-0.523	-0.552	1099
Type: Residential	-0.355	-0.379	1710
Type: Commercial	-0.248	-0.309	884
Type: Industrial	-0.413	-0.417	2893
Demand: Peak	-0.256	-0.360	269
Demand: Mid-peak	-0.190	-0.186	108
Demand: Off-peak	-0.432	-0.713	83
Data: Panel	-0.389	-0.418	2290
Data: Time-series	-0.328	-0.343	1718
Data: Cross-section	-0.652	-0.649	513
Granularity: Yearly	-0.436	-0.455	3291
Granularity: Quarterly	-0.316	-0.393	126
Granularity: Monthly	-0.302	-0.289	948
Price: Average	-0.409	-0.420	2385
Price: Marginal	-0.430	-0.446	957
Price: Lump sum	-0.302	-0.302	1

Continued on next page

Table A1 – continued from previous page

Variable Name	Sample Mean	Weighted Mean	n
Price: Shin	-0.151	-0.137	11
Price: Other	-0.340	-0.392	453
Tariff: Increasing	-0.350	-0.359	565
Tariff: Decreasing	-0.625	-0.474	462
Tariff: Flat	-0.495	-0.530	119
Tariff: TOU	-0.283	-0.375	559
Tariff: Undefined	-0.420	-0.428	1058
Control: Demographics	-0.484	-0.516	1515
Control: Temperature	-0.366	-0.381	2203
Control: Stocks	-0.479	-0.465	821
Control: Fuels	-0.427	-0.458	1844
Control: Income	-0.414	-0.429	2543
Form: Reduced	-0.407	-0.410	1873
Form: Structural	-0.370	-0.422	2386
Model: Dynamic	-0.327	-0.377	3100
Model: Static	-0.546	-0.501	1407
Lag: Dependent	-0.299	-0.347	1831
Lag: Other	-0.279	-0.345	442
Model: RE	-0.579	-0.487	63
Model: FE	-0.406	-0.379	436
Model: VAR	-0.519	-0.498	30
Model: ARDL	-0.284	-0.350	348
Model: ECM	-0.369	-0.389	217
Model: VECM	-0.263	-0.362	85
Model: DS	-0.445	-0.527	497
Model: LE	-0.254	-0.287	1025
Model: Other model	-0.282	-0.350	112
Estimation: ML	-0.302	-0.419	265
Estimation: GMM	-0.367	-0.300	176
Estimation: Error component	-0.398	-0.252	111
Estimation: OLS	-0.412	-0.447	1641
Estimation: GLS	-0.412	-0.418	220
Estimation: SUR	-0.359	-0.459	494
Estimation: 2SLS	-0.488	-0.400	440
Estimation: 3SLS	-0.431	-0.562	112
Estimation: IV	-0.367	-0.375	377
Estimation: other	-0.253	-0.391	100
Endogeneity: Control	-0.382	-0.371	1306
Endogeneity: No control	-0.401	-0.431	3215
Function: Linear	-0.329	-0.433	832
Function: Semi-log	-0.513	-0.344	231
Function: Double-log	-0.406	-0.412	2474
Function: Box-Cox	-0.453	-0.464	16
Function: non available	-0.397	-0.435	968
Publication Year $\geq$ 2000	-0.362	-0.406	2161
Publication Year ¡ 2000	-0.427	-0.425	2375
Journal Impact Factor $\geq$ 0.061	-0.391	-0.409	2347
Journal Impact Factor $<$ 0.061	-0.401	-0.426	2189
Citations (t) $\geq$ 1.128	-0.385	-0.383	2286
Citations (t) $<$ 1.128	-0.407	-0.444	2250
Number of citations $\geq$ 50	-0.368	-0.422	1094
Number of citations $<$ 50	-0.404	-0.415	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.

Table A2: Tests accounting for potential endogeneity

	IV Regression	**p-uniform***
Publication Bias	-1.640^{***}	L=46.03
Standard Error	(0.293)	(pj0.001)
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 and van Assen (2021)) uses maximum likelihood estimation. Asterisks denote significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Literature Collection

Table A.3: List of studies included in the Meta-analysis

List of studies included in the Meta-analysis (Continued)		
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 and Robin (1999)	Duncan and Binswanger (1976)
ADNDE (1981)	Boogen et al. (2017)	Dunstan and Schmidt (1988)
Akmal and Stern (2001)	Borenstein (2009)	Durmaz et al. (2020)
Al-Bajjali and Shamayleh (2018)	Bose and 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 and Filippini (2011)	Burke and Abayasekara (2018)	Eltony and Al-Awadhi (2007)
Alberini et al. (2011)	Burke and Kurniawati (2018)	Eltony and Hajeeh (1999)
Alberini et al. (2019)	Bye (1986)	Eltony and Hoque (1996)
Allcott (2011)	Byrne et al. (2021)	Eltony and Mohammad (1993)
Alter and Syed (2011)	Campbell (2018)	Erickson et al. (1973)
Amarawickrama and Hunt (2008)	Cao et al. (2019)	Eskeland and Mideksa (2010)
Amusa et al. (2009)	Cao et al. (2023)	Eskeland et al. (1994)
Anderson (1971)	Cargill and Meyer (1971)	Fan and Hyndman (2011)
Anderson (1973a)	Carlevaro and Spierer (1983)	Fan et al. (2019)
Anderson (1973b)	Cavoulacos and Caramanis (1983)	Fatai et al. (2003)
Anderson (1974)	Cebula (2012)	Fell et al. (2014)
Andrikopoulos et al. (1989)	Chang and Chern (1981a)	Filippini (1995a)
Ang et al. (1992)	Chang and Chern (1981b)	Filippini (1995b)
Apte (1983)	Chang and Hsing (1991)	Filippini (1999)
Archibald et al. (1982)	Chaudhary et al. (1999)	Filippini and Pachauri (2004)
Arimura et al. (2012)	Chaudhry (2010)	Filippini (2011)
Arisoy and Ozturk (2014)	Chern (1975)	Filippini et al. (2018)
Aroonruengsawat et al. (2012)	Chern (1978)	Fisher and Kaysen (1962)
Arsenault et al. (1995)	Chern and Bouis (1988)	Fouquet (1995)
Asadoorian et al. (2006)	Chishti (1993)	Frondel et al. (2019)
Asadoorian et al. (2008)	Choi (2002)	Fuss (1977)
Aslam and Ahmad (2023)	Christodoulakis and Kalyvitis (1997)	Gam and Rejeb (2012)
Atakhanova and Howie (2005)	Christopoulos (2000)	Garbacz (1983a)
Athukorala and Wilson (2010)	Chung and Aigner (1981)	Garbacz (1983b)
Athukorala et al. (2019)	Cialani and Mortazavi (2018)	Garbacz (1983c)
Atkinson (1979a)	Cicchetti and 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 and Shuaibu (2009)	Coughlin (1995)	Garcia-Cerrutti (2000)
Badri (1992)	Cuddington and Dagher (2015)	Gautam and Paudel (2018)
Baker and Blundell (1991)	Dahan (1996)	Gill and Maddala (1978)
Balabanoff (1994)	Davis (2008)	Glakpe and Fazzolare (1985)
Banda and 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)
L. et al. (1979)	Denton et al. (1999)	Gundimeda and Kohlin (2008)
Beenstock et al. (1999)	Denton et al. (2003)	Guo and Tybout (1994)
Beierlein et al. (1981)	Dergiades and Tsoulfidis (2008)	Halicioglu (2007)
Bekhet and Othman (2011)	Deryugina et al. (2020)	Hall and Rosenthal (1995)
Belanger et al. (1990)	Diabli (1998)	Halvorsen (1975)
Berkhout et al. (2004)	Dias-Bandaranaike and Munasinghe (1983)	Halvorsen (1976)

Continued on next page

List of studies included in the Meta-analysis (continued)

Bernard et al. (1987)
Bernard et al. (1996)
Bernard et al. (2011)
Berndt and Samaniego (1984)
Berndt et al. (1980)
Bernstein and Griffin (2006)
Betancourt (1981)
Bianco et al. (2009)
Bianco et al. (2010)
Bigano et al. (2006)
Bildirici et al. (2012)
Bjerkholt and Rinde (1983))
Bjorner et al. (2001)
Herriges and King (1994)
Hesse and Tarkka (1986)
Hieronymus (1976)
Hill et al. (1983)
Hirth et al. (2022)
Hisnanick and Kyer (1995)
Hogan (1989)
Holtedahl and Joutz (2004)
Hondroyiannis (2004)
Horowitz (2007)
Houston (1982)
Houthakker (1951)
Houthakker et al. (1974)
Houthakker (1980)
Hsiao et al. (1989)
Hsiao and Mountain (1994)
Hsueh and Gerner (1986)
Hughes-Cromwick (1985)
Hung and Huang (2015)
Huntington and Soffer (1982)
Hyndman et al. (1980)
IEEJ (1986)
Ilmakunnas and Torma (1989)
Inglesi (2010)
Inglesi-Lotz and Blignaut (2011)
Inglesi-Lotz (2011)
Iqbal (1986)
Ishaque (2018)
Ishiguro and Akiyama (1995a)
Ishiguro and Akiyama (1995b)
Ito (2014)
Ito et al. (2018)
Jaffee B. L. and Olshavsky (1982)
Jamil and Ahmad (2011)
Javid and Qayyum (2014)
Jessoe and Rapson (2014)
Jin and Kim (2022)
Jones (1995)
Jungeilges and Dahl (1986)
Kamerschen and Porter (2004)
Karbuz et al. (1997)
KEEI (1989)
Kaserman and Mayo (1985)
Dilaver and Hunt (2011a)
Dilaver and Hunt (2011b)
Dobozi (1988)
Dodgson et al. (1990)
Donatos and Mergos (1989)
Donatos and Mergos (1991)
Dong and Kim (2018)
Dong et al. (2020)
Donnelly (1984)
Donnelly (1985)
Donnelly (1987)
Donnelly and Diesendorf (1984)
Donnelly and Saddler (1984)
Lim et al. (2014)
Lin and Ouyang (2014)
Lin and Zhu (2020)
Liu (2005)
Lohani (1992)
Lyman (1994)
Lynk (1989)
Ma and Stern (2016)
Macroconsult (2001)
Maddala et al. (1994)
Maddala et al. (1995)
Maddala et al. (1997)
Maddigan et al. (1983)
Maddock et al. (1992)
Madlener (2011)
Mahmud and Chishti (1990)
Mansur et al. (2005)
Maria de Fátima et al. (2012)
Masike and Vermeulen (2022)
Matsui (1979)
Matsukawa (1996)
Matsukawa (2018)
Matsukawa et al. (1993)
McFadden et al. (1977)
Mchugh (1977)
McRae and Meeks (2016)
Meher (2020)
Mendoza and Vargas (1987)
Micklewright (1989)
Mikayilov et al. (2017)
Miller and Alberini (2016)
Moghaddam (2003)
Moghimzadeh and Kymn (1986)
Morovat et al. (2019)
Mount and Chapman (1979)
Mount et al. (1974)
Mountain (1982)
Mountain and Hsiao (1989)
Mountain (1989)
Mountain et al. (1989)
Munley et al. (1990)
Murray et al. (1978)
Nagata (2001)
Halvorsen (1977)
Halvorsen and Ford (1979)
Halvorsen and Larsen (2001)
Hartman and Werth (1981)
Hasanov et al. (2016)
Hausman (1979)
Hawkins (1975)
Hawkins (1977)
Hawkins (1978)
He and Lambert (2004)
Henderson (1983)
Henriksson et al. (2014)
Henson (1984)
Pindyck (1980)
Pitt (1985)
Polemis (2007)
Pourazarm and Cooray (2013)
Poyer and Williams (1993)
Rahman (1982)
Rai et al. (2014)
Ramcharran (1988)
Rapson (2014)
Reilly and Shankle (1988)
Reiss and White (2005)
Rossi and Tansini (1989)
Roth (1981)
Rouhani et al. (2022)
Roy (1986)
Ryan et al. (1996)
S&Z Consultores (1999)
Sa'ad (2009)
Saddler and Donnelly (1983)
Saddler et al. (1980)
Sadorsky (2012)
Saha and Bhattacharya (2018)
Schulte and Heindl (2017)
Schwarz (1984)
Shaffer (2020)
Shi et al. (2012)
Shin (1981)
Shin (1985)
Silk and Joutz (1997)
Silva et al. (2018)
Smith (1980b)
Sterner (1985)
Sterner (1989)
Su (2018)
Sutherland (1983a)
Sutherland (1983b)
Talbi et al. (2022)
Tambe and Joshi (2014)
Tanishita (2019)
TatlÄ (2017)
Taylor (1979)
Taylor et al. (1977)
Terza (1986)

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List of studies included in the Meta-analysis (continued)		
Keng (1991)	Nakajima (2010)	THEC (1983)
Khan and Abbas (2016)	Nakajima and Hamori (2010)	Tiwari (2000)
Khan and Qayyum (2009)	Nan and Murry (1991)	Tiwari and Menegaki (2019)
Khanna et al. (2016)	Narayan and Smyth (2005)	Tran et al. (2023)
Khazzoom (1986)	Narayan et al. (2007)	Tserkezos (1992)
Knaut and Paulus (2016)	Nasir et al. (2008)	Türkekul and Unakıtan (2011)
Kohler (2014)	Okajima and Okajima (2013)	Uhr et al. (2019)
Kohler and Mitchell (1984)	Oliveira (1993)	Urga and Walters (2003)
Kokkelenberg and Mount (1993)	Olivia and Gibson (2008)	Uri (1977)
Kolstad and Lee (1993)	Otero Prada (1984)	Uri (1978)
Krishnamurthy and Kriström (2015)	Otsuka (2017)	Uri (1979a)
Labandeira et al. (2005)	Otsuka (2023)	Uri (1979b)
Lanot and Vesterberg (2021)	Otsuka and Haruna (2016)	Uri (1979c)
Lareau and Darmstadter (1982)	Parfomak and Lave (1996)	Uri (1979d)
Larsson (2004)	Parhizgari and Davis (1978)	Uri (1979e)
Larsson (2006)	Park and Acton (1984)	Uri (1982)
Laumas and Williams (1981)	Parti and Parti (1980)	Uri (1983)
Lee and Chiu (2011)	Paul et al. (2009)	Vashist (1984)
Lee and Lee (2010)	Pellini (2021)	Veall (1983)
Liddle and Huntington (2021)	Pesaran et al. (1999)	Veall (1987)
Lijesen (2007))	Pielow et al. (2012)	Velez et al. (1987)
Lillard and Acton (1981)	Pindyck (1979)	Verleger (1973)
Vlachou and Samouilidis (1986)	Westley (1984b)	Yang and Liang (2023)
Volland and 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 and Pashourtidou (2007)
Wang (1985)	Wilson (1974)	Zhang et al. (2017)
Wang et al. (2020)	Wolak (2011)	Zhou et al. (2019)
Wang and Mogi (2017)	Woo et al. (2018)	Ziramba (2008)
Westley (1984a)	Yang (1978)	

Table A.4: 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 and White (2005)
Byrne et al. (2021)	Ito et al. (2018)	Shaffer (2020)
Cao et al. (2023)	Jessoe and Rapson (2014)	Tran et al. (2023)
Davis (2008)	Kohler and Mitchell (1984)	Wolak (2011)
Deryugina et al. (2020)	Lillard and Acton (1981)	Zhou et al. (2019)
Herriges and King (1994)	Matsukawa (2018)	

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Univerzita Karlova v Praze, Fakulta sociálních věd
Institut ekonomických studií [UK FSV – IES] Praha 1, Opletalova 26
E-mail : ies@fsv.cuni.cz http://ies.fsv.cuni.cz