The Price Elasticity of Heating and Cooling Energy Demand*

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Abstract. We create a large meta-dataset of price elasticities of energy demand for heating and cooling in buildings, comprising close to 5000 price elasticity estimates including study and observation characteristics from more than 400 primary studies. We find robust and strong signs of *p*-hacking and publication bias with insignificant or positive elasticities being underrepresented. Correcting for this bias, the price elasticities range from -0.05 to -0.2 for the short run and from -0.1 to -0.3 for the long run. This holds for all relevant fossil fuels and electricity, poor and rich countries, residential and business usage, and aggregate and survey data.

Keywords: Meta-analysis, Price elasticity, Energy demand, Publication Bias

JEL classification: C83, Q41, Q48

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1. Introduction

Heating and cooling in buildings account for about a quarter of global final energy consumption and about 20% of global energy-related carbon dioxide emissions (Whiteman et al. 2023; IEA 2021). Even as the share of fossil fuels in heat generation declines, global energy demand for cooling is expected to grow massively due to rising temperatures and the increasing availability of cooling appliances to a growing number of households (Scoccimarro et al. 2023; IEA 2023). Energy price shocks have returned in recent years, and geopolitical tensions as well as transition risks of fossil industries increase the likelihood of future shocks. The global price of CO2 is bound to rise as well. The price elasticity of demand for heating and cooling energy is a central metric for understanding the power of self-correcting market mechanisms after energy price shocks and the steering effects of CO2 prices.

This paper provides a meta-analysis (Stanley 2001), a quantitative synthesis of the vast amount of primary empirical studies that estimate the price elasticity of energy demand in buildings. These studies cover different fossil fuels and energy sources, countries and regions, time periods, sectors and sources of the price change; they use various estimation and identification approaches. We collect their point estimates, standard errors, and study characteristics.

Our study has three aims: (i) We provide an average estimate based on the statistical power of a large amount of primary empirical evidence. (ii) We identify patterns that explain the heterogeneity across studies and samples. (iii) We test and correct for possible publication selection bias in the literature that may lead to misleading conclusions about the average size and explanatory factors of the price elasticity. Publication bias may arise from a preference of researchers, referees, and editors for statistically significant findings (a.k.a. p-hacking) (Brodeur et al. 2016; Brodeur et al. 2023; Chopra et al. 2024) and, more generally, a selection of results that conform to conventional wisdom, dominant economic theories, or the findings of influential publications (Stanley 2005; Christensen and Miguel

2018; Ioannidis et al. 2017; Card et al. 2018; Neisser 2021; Brown et al. 2024; Andrews and Kasy 2019).

We adhered to a systematic study selection process and collected information from 421 primary studies that provided 4,974 comparable elasticity estimates plus the respective study and estimation characteristics. This is one of the largest meta datasets in the literature. Moreover, it is one of the first meta-analyses in economics that is based on a detailed pre-registered pre-analysis plan (PAP), documented at https://osf.io/zdche, to enhance the transparency and replicability of our analysis and findings. The PAP includes a set of testable hypothesis, search strings and study selection criteria, data to be collected, selection of statistical models and a risk-of-bias assessment.

Our main findings are as follows: (i) The unweighted average price elasticity for heating and cooling energy in buildings is around -0.3 in the short run and -0.45 in the long run. These numbers are roughly in line with, or slightly smaller than those of previous literature reviews and seminal papers in this field (Labandeira et al. 2017; Espey and Espey 2004; Reiss and White 2005; Alberini et al. 2011) and a brief summary requested from an artificial intelligence (AI).

(ii) However, we detect strong and robust signals of *p*-hacking and publication bias. Statistically insignificant, close to zero, and positive elasticities appear underrepresented in the skewed distribution of estimates. This pattern can be detected by suspicious heaping of estimates just below established significance thresholds (Brodeur et al. 2020) or by a significant correlation between standard errors and point estimates that should not occur according to standard econometric assumptions (Stanley and Doucouliagos 2012a).

The mechanisms that lead to this correlation can be described as follows: with classic sampling error, the distribution of estimates should be symmetrical around the true underlying elasticity. If the true value is negative and small, low-powered estimates with considerable standard errors should frequently find null results or positive elasticities. If such findings are considered implausible or hard to be published, researchers may consider alternative specifications or samples. Thus, large standard errors might lead researchers to seek specifications with more negative point estimates; vice versa, small negative point estimates can become significant by specifications involving smaller standard errors. In both cases, a negative correlation between standard errors and point estimates occurs.

The result is an over-representation of statistically significant and large negative estimates that inflates the average elasticity. Note that this process does not primarily involve fraudulent actions by authors or selection of results by editors and reviewers, but often a rational search for a plausible specification of authors before submission (Brodeur et al. 2024). If we correct for this effect via a battery of established approaches, the resulting elasticities are considerably smaller than the conventional wisdom, in a range of -0.05 to -0.2 in the short run and -0.1 to -0.3 in the long run. That is, the average correction factor is about 50 percent.

(iii) This strong downward correction of the elasticities is robust to a battery of test procedures, a subsample analysis for different energy sources, energy uses, sectors, country groups, journal ranks, and estimation approaches, as well as to the inclusion of various study or estimation characteristics as control variables both in a frequentist and Bayesian manner. A synthetic study that takes into account best practices from the literature would report an average elasticity of about -0.1 in the short run and -0.2 in the long run.

(iv) Electricity demand tends to be slightly more elastic to price changes than natural gas demand with an elasticity that is on average -0.1 units larger. (v) The residential sector also seem to be slightly more reactive to price changes than the business sector, the elasticity being around -0.1 units larger. (vi) There are no robust differences for heating vs cooling demand, for market vs administered price changes, for country groups, or over time.

The finding of a strong downward correction of the results in comparison to the unweighted average or the conventional wisdom by around 50 percent is not unusual. Meta-meta studies have reported similar correction factors in other fields (Gechert et al. 2024; Ioannidis et al. 2017). Price elasticities of demand might be prone to publication bias since there is a coherent and largely unchallenged theory that rules out positive elasticities. In addition, since the empirical literature is extensive and well-established, statistically insignificant or unconventional findings may even have lower chances of getting published.

Related literature. Our paper is related to a rich literature on energy price elasticities. Standard theory holds that energy products, in particular for heating and cooling, are necessities, implying small and negative price elasticities of demand. Monopolistic regional supply conditions and costly substitution possibilities contribute to inelastic demand (Davis 2023; Howie and Atakhanova 2017). Price sensitivity might also be low under rental agreements if landlords make contracts with energy suppliers while tenants pay the bills. Moreover, price signals to tenants often come with long delays under service charge settlements.

Overview studies and meta-analyses with respect to heating and cooling energy sources are rare. Espey and Espey (2004), based on 36 primary studies, report an average elasticity of residential electricity demand of around -0.35 in the short run and -0.85 in the long run. Chai et al. (2018) focus on studies about Chinese natural gas demand elasticities and find an elastic average short-run gas demand as well as a surprising positive average long-run elasticity. Labandeira et al. (2017), who cover all energy demand, estimate an average short-run elasticity of around -0.2 and a long-term elasticity of -0.6 for a subsample of electricity, natural gas and heating oil.

Meta-analyses have been more frequently applied to the literature on price elasticities for vehicle fuels in the transport sector (Havránek et al. 2012; Brons et al. 2008; Graham and Glaister 2004; Goodwin et al. 2004; Espey 1996; Espey 1998). The reported average elasticities for vehicle fuels are larger in absolute terms than for heating and cooling in buildings, indicating a higher potential for consumers to respond to fluctuations in fuel prices by substituting or modifying their consumption habits. Table A.2 in Appendix A summarizes the findings from these other quantitative reviews. Note that Havránek et al. (2012) is the only other meta-analysis in this field to account for publication bias. Their corrections are of a similar magnitude.

A related body of literature examines the impact of CO2 pricing on emissions. Drupp et al. (2024) provide evidence from a large-scale survey among experts from different countries and report rather coherent levels of recommended CO2 prices. Green (2021) reviews 37 ex-post evaluations of carbon pricing schemes with the lion's share focusing on European data and finding only a small effectiveness. Döbbeling-Hildebrandt et al. (2024) provide a systematic and more comprehensive review of 483 effect sizes found for 21 global carbon pricing schemes. They report a larger average effectiveness of about 10% reduction of emissions after introduction of a carbon pricing scheme. Their average effect sizes are reduced by about 1/3 after correcting for publication bias. Note, however, that effectiveness measures emission reductions after a (large or small) carbon price shock and cannot be compared to the elasticity estimates in our sample which give the percentage change in energy consumption after a one percent rise in the energy price.

Our estimation of relatively small short- and long-term elasticities also aligns well with observed consumption responses to energy-price shocks (Ruhnau et al. 2023). A back-of-the-envelope calculation for Germany in 2022, featuring a rise of +143 percent in the natural gas retail price and a demand reduction of about -11 percent would imply a short-run elasticity of -0.08 (Jamissen et al. 2024), which is fairly in line with our findings. Likewise, Reiss and White (2008) report an 11 percent fall of electricity consumption of San Diegan households in the six months after an unexpected and rapid market-price shock of +130 percent in the summer of 2000, pointing to a short run elasticity of -0.08, which might have been double the size at an annual horizon of measurement.

The remainder of the paper is structured as follows: Section 2 describes the data collection and presents descriptive statistics. Section 3 discusses the main findings regarding average elasticities and tests for publication bias. Section 4 considers the heterogeneity of findings in the primary studies in relation to study and estimation characteristics. The final section concludes.

2. Dataset

2.1. Search strategy

Collecting our final dataset involved the following steps: (i) selecting literature databases and defining search queries; (ii) determining inclusion/exclusion criteria for studies; (iii) determining relevant study and estimation characteristics derived from our hypotheses; (iv) screening titles and abstracts of the found entries; (v) full-text screening and coding of all estimates and characteristics. A detailed outline of the search strategy, including search queries, inclusion/exclusion criteria, rules for screening the data, and data management tools used, is available in the PAP. Appendix A.1 includes further information on the actual process of data collection, including Figure A.1, a flow chart of the steps involved, and the number of excluded and potentially relevant studies in each step.

The final sample includes 4,974 estimates of the demand elasticity of heating and cooling energy sources collected from 421 primary studies listed in Appendix D. For each of these entries we coded 29 study and estimation characteristics, resulting in a spreadsheet with around 140,000 entries. This is one of the largest meta datasets collected so far. Note that we employed AI tools to assist the title and abstract screening in step (iv), but that, at the time of data extraction, a high-quality full-text screening and coding of results and study characteristics was still beyond the capabilities of AI and required many hours of expert human labor.

2.2. Hypothesis and data collection

All coded characteristics relate to our set of pre-registered hypotheses. In the present paper, we focus on the following hypotheses for which we collected related information:

[H1] The simple mean of price elasticities is negative but small ($\varepsilon = [-1, 0]$). We collected all available price-elasticity point estimates per study. Almost all studies commonly estimated price elasticities via log-log equations such that we only needed to standardize a minor share of semi-elasticities (log-linear) and marginal propensities (linear-linear) to elasticities via standard transformations.

[H2]: There is substantial publication bias that leads to an inflated average of the reported price elasticity of demand. p-hacking and publication bias typically appear in suspicious distributions of z-statistics and correlations between point estimates and standard errors of the coefficient of interest (Egger et al. 1997; Stanley and Jarrell 2005; Andrews and Kasy 2019; Brodeur et al. 2020). We thus collected standard errors of the price elasticity estimates and transformed them in accordance with the point estimates as outlined above.[1]

We collect additional publication characteristics that might be related to publication bias. We code for the type of publication (journal vs non-journal publications like working papers, reports, etc.), and the number of citations according to Google Scholar, retrieved on August 19, 2024. We also measure journal impact according to the Scimago Journal Ranking (SJR) or the Journal Impact Score (JIS) from Scopus, as provided by resurchify.com, retrieved on August 19, 2024.

We also consider the heterogeneity of our sample in various dimensions, but place less emphasis on the following additional hypotheses that will be covered in more detail in follow-up studies:

[H3.] Non-market induced price changes exhibit a more elastic demand reaction compared to market-induced price changes. Energy consumers might react differently to different sources of price changes, including market-based demand and supply shocks, or policy-induced price changes (Andersson 2019; Basaglia et al. 2023; Broin et al. 2015; Edelstein and Kilian 2009; Grieder et al. 2021). Some studies argue that due to higher salience and expected persistence of policy-driven price changes, the elasticity could be higher than for market-driven changes. For the present paper, we categorize all estimates that are clearly related to market-price changes vs a residual category including different forms of policy

[1]Some studies report t-statistics or p-values, which we transform by the standard formulae. A few studies only report compliance with significance thresholds. In these cases, we conservatively code the p-value at the threshold itself. Note that this can lead to artificial spikes in the density distribution at the thresholds that would bias some tests of p-hacking. For these tests, we exclude the calculated threshold standard errors. Several studies calculate short-term and long-term elasticities from dynamic equations, such as autoregressive distributed lag (ARDL) models. A simple first-order generic form could look like $x_t = \alpha + \delta x_{t-1} + \varepsilon_{ST} p_t + u_t$, with x being log energy demand, δ the autoregressive term, ε_{ST} the short-term price elasticity and p log energy prices. The long-term elasticity can then be derived from $\varepsilon_{LT} = \varepsilon_{ST}/(1-\delta)$. Some studies report inference statistics only for the short-run elasticities. If sufficient information is given, we calculate the corresponding long-term standard errors using the Delta method.

measures and mixed cases. We explore the details of different policy-driven price changes in a follow-up paper (Gechert et al. 2025).

[H4.] Price elasticities differ between the types of energy sources. The dataset includes estimates for natural gas, heating oil, electricity, coal, LPG and a mixed category. Each of the fuels considered are used in different contexts and in heating/cooling appliances. Some energy sources can be substituted for and used more efficiently due to changing habits or replacement appliances. In addition, we categorize the use of energy sources for heating, cooling, or an unspecified mix of uses. Heating and cooling may be prone to different price sensitivities (De Cian et al. 2007).

[H5.] Price elasticities differ between sectors. Sector-specific regulations and energy-efficiency requirements, operating hours, contract details and building types might lead to different price elasticities. We distinguish residential, commercial, industrial and public sectors as well as an unspecified sector mix. Often, there is no clear distinction between commercial and industrial users in primary studies such that we subsumed them under a business sector. There is only a small number of distinct observations for the public sector, which we therefore count towards the sector-mix category.

[H6.] Energy demand is less price elastic in the short run becoming more elastic in the long run. Customers may not easily change their habits, appliances, energy sources or suppliers in the short term (Labandeira et al. 2017; Kwon et al. 2016). Moreover, innovations driven by price signals may take time to materialize. We categorize all estimates either as short-term or as long-term elasticities, according to the judgments of the primary studies.[2]

[H7.] Price elasticities are heterogeneous among countries and regions. Climatic and geographic conditions, natural resources, technology levels, energy systems, grids, population density, regulatory institutions, taxes and subsidies, energy efficiency requirements, customer preferences and habits, etc. may all contribute to different elasticities among

[2] If no clear categorization is given in the study, we apply the following rules: We coded price elasticity estimates to be short run if underlying data were of cross-sectional type, fixed effects estimates, static estimates and/or if authors controlled for any kind of capital stock variation (appliances/technology/energy efficiency) because in these cases no dynamic adaptions can be made by the household/firm/unit of interest so the price elasticity can be considered short-run (Boyd and Lee 2020; Burke and Liao 2015). Otherwise, estimates where coded as long run.

countries and regions. For the present paper, we collect the respective sample countries and simply classify them into OECD, non-OECD and a mix group.

[H8.] Price elasticities differ over time. Elasticities might exhibit trends according to technological developments or patterns for specific time periods in relation to major events or shocks, like oil crises, natural disasters political landmarks, etc. We collect the start and end-year of the respective sample and calculate an average year. Sample heterogeneity with respect to time and space will be investigated in more detail in a follow-up paper.

[H9.] There is heterogeneity in price elasticities between the various study designs applied in the primary studies. This is a universal hypothesis, reflecting a list of potentially relevant control factors with respect to sample, estimation and publication characteristics that are detailed in Appendix A.2.

2.3. Descriptive statistics

Table 1 provides descriptive statistics of the main variables in our dataset, separated for short-run and long-run elasticities. Statistics for the further control variables are shown in Table A.1 in Appendix A.2. Note that we winsorize elasticities (and their standard errors) at the 2nd and 98th percentile in order to reduce the impact of outliers, which is standard in the literature (e.g. Žigraiová et al. 2021; Gechert and Heimberger 2022). We later show that our main results are robust to different levels of winsorization.

The unweighted mean of short-run elasticities is around -0.3 while the long-run mean is ca. -0.45 with considerable standard deviations. These numbers are roughly in line with or slightly smaller than those from previous literature reviews and seminal papers in this field (Labandeira et al. 2017; Espey and Espey 2004; Reiss and White 2005; Alberini et al. 2011), see also Table A.2 in Appendix A.4. It is also consistent with a brief summary requested from an artificial intelligence (AI), GPT-4o mini, which answered that the point estimate is -0.3 for the short-run elasticity (and that the 95 percent confidence interval ranges from [-0.1,-0.5]) and -0.6 for the long run (95 percent confidence interval [-0.4,-0.8]) (see Appendix A.5 for details).

Table 1: Descriptive statistics for estimates and main study characteristics

	Time Horizon			
	short run		long run	
	mean or share	SD or freq	mean or share	SD or freq
price elasticity	-0.28	(0.41)	-0.44	(0.54)
SE	0.12	(0.18)	0.22	(0.28)
energy source				
electricity	57.39%	1,538	52.75%	1,210
coal	3.69%	99	5.71%	131
natural gas	25.11%	673	25.15%	577
LPG	1.12%	30	3.31%	76
heat oil	1.04%	28	0.83%	19
esource mix	11.64%	312	12.25%	281
energy use				
heating	25.30%	678	21.97%	504
cooling	3.96%	106	3.49%	80
euse mix	70.75%	1,896	74.54%	1,710
price change market	61.58%		67.14%	
country group				
OECD	68.02%	1,823	66.91%	1,535
non OECD	27.43%	735	27.24%	625
country mix	4.55%	122	5.84%	134
sector				
residential	67.20%	1,801	59.11%	1,356
business	23.47%	629	26.63%	611
sector mix	9.33%	250	14.25%	327

Notes: The table shows descriptive statistics of the elasticities, their standard errors and main study and estimation characteristics. We separate between short-run and long-run estimates. The numbers are calculated after winsorization of elasticities and standard errors at the 2nd and 98th percentile to contain the influence of outliers. We report the mean and standard deviation (SD) for continuous variables and percentage shares as well as frequencies (freq) for factor variables and dummies. The main study characteristics include the energy source (electricity, natural gas, coal, liquefied petroleum gas (LPG), heating oil, unspecified mix), the energy use (heating, cooling, unspecified mix), the sector (residential, business, unspecified mix), the source of the price change (market vs non-market and mixed) and the country group (OECD, non-OECD, mix). Statistics for further control variables are shown in Table A.1 in Appendix A.2.

Regarding energy sources, most estimates refer to electricity and natural gas. Since the other categories are rather small, in the estimations we lump them into the category "esource other" for our regression analysis. Regarding the use of energy, only a quarter of estimates can be clearly separated into heating and cooling uses, while more than 70 percent refer to an unspecified mix. More than 60 percent of the estimates come from studies that focus on market price fluctuations, while the rest look at policy-driven changes or unclear mixed cases. Estimates for the residential sector and for OECD countries represent the majority of the sample.

Figure A.2 in Appendix A.3 shows the histograms separately for the short-run and long-run elasticities as well for the other main study characteristics. The distributions are consistently left-skewed and exhibit a sizeable kink at zero. The variation in the elasticities might reflect the impact of different study characteristics that we explore in more detail in Section 4. However, at a first glance, the histograms for subsamples of the data according to study and data characteristics do not show clearly different patterns. That is why we first explore the zero kink and left-skewedness of the distribution which might signal publication selection bias as we explain in the next section.

3. p-hacking and publication selection bias

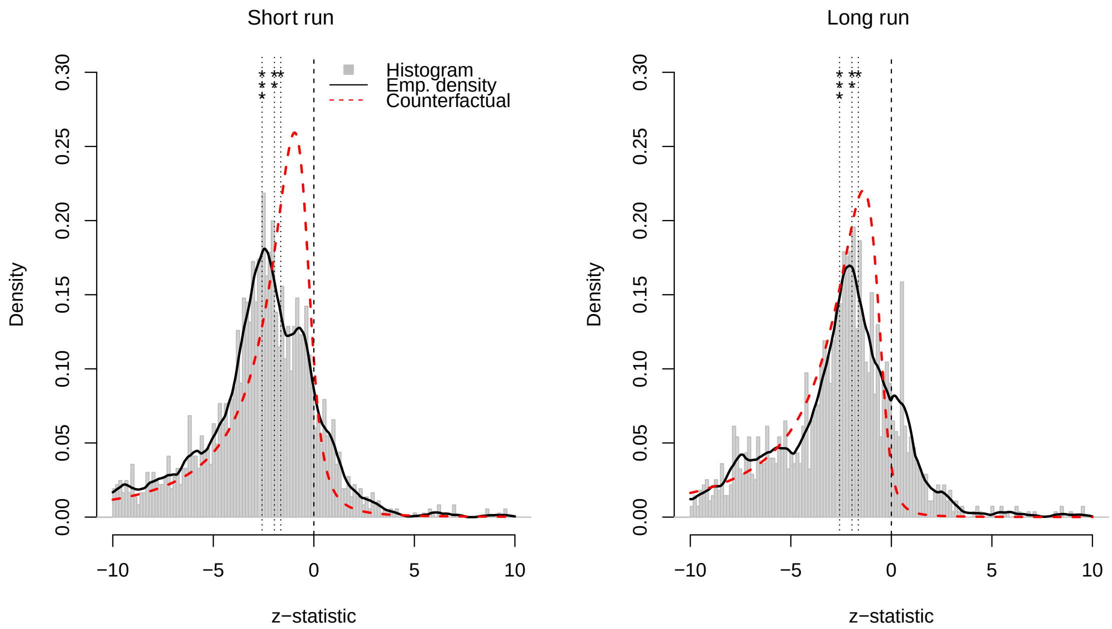
3.1. Signs of p-hacking

Publication bias may arise from a preference of researchers, referees and editors for statistically significant findings (a.k.a. p-hacking) (Brodeur et al. 2016; Brodeur et al. 2023; Chopra et al. 2024) and, in addition, a selection of results that conform with conventional wisdom, dominant economic theories or the findings of influential publications (Stanley 2005; Christensen and Miguel 2018; Ioannidis et al. 2017; Andrews and Kasy 2019).

The histograms of Figure A.2 in Appendix A.3 give a first visual impression of selective publication of findings with a strong discontinuity around zero. Authors might find positive elasticities implausible and therefore search for specifications that deliver theory-consistent negative results. While this behavior might be rational at the individual level, it leads to a skewed distribution of all estimates as the same plausibility filter may not apply for large negative elasticities.

p-hacking. In addition, results may be selected for statistical significance. Figure 1 shows density plots for the z-statistics that we calculated from the point estimates and standard errors of the elasticity estimates, in line with (Brodeur et al. 2016). Note that our

Figure 1: z-stat densities with counterfactuals



Notes: The figure shows density plots of the z-statistics of the short-run (left panel) and long-run (right panel) price elasticities of demand. Observations with missing exact inference statistics for which we calculated the standard error from given thresholds are excluded. The figures include histograms (gray bars) as well as the observed kernel densities (solid black line) together with a counterfactual density (dashed red line). The counterfactual is calibrated by matching the empirical mass for observations with $z < -5$, as p-hacking is unlikely in this range (Brodeur et al. 2016). Vertical dotted lines represent standard significance thresholds at the 10% (*), 5% (**) and 1% (***) level.

dataset includes both positive and negative values, while Brodeur et al. (2016) standardize their broad range of effect sizes to absolute values. Moreover, for this analysis we exclude observations with missing exact inference statistics for which we calculated the standard error from given thresholds, like significance stars. Including such values would bias the findings in favor of an over-representation of just-significant estimates.

We also follow Brodeur et al. (2016) in calibrating non-central t-distributions as counterfactuals. The key assumption is that test statistics above $z = 5$ represent a part of the distribution unaffected by p-hacking, since there is little incentive to manipulate already highly significant results. Through grid search, we find the degrees of freedom and non-centrality parameter that best replicate this tail behavior. Note, however, that Kranz and Pütz (2022) in a comment to Brodeur et al. (2016) show that this calibration approach does not generally correctly recover the true latent distribution. Our calibrated distribution may therefore only serve as a rough benchmark for what the full distribution could look like absent p-hacking.

In both sub-figures of Figure 1, it seems that the overall probability mass is shifted towards the left. Statistically insignificant elasticities seem to be underrepresented, while there also appears to be a heaping of estimates within the range of standard significance thresholds.[3] While this overall pattern can be spotted from the kernel densities, the light gray histogram bins do not show strong signals of clear cut-offs in frequencies just below and above the standard thresholds.

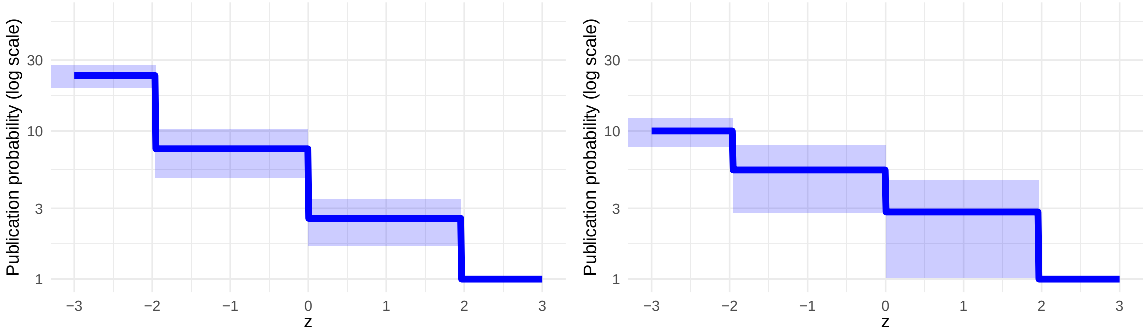
One might ask whether the differences in the densities around specific thresholds are themselves statistically significant. Figure B.4 in Appendix B.1 shows results of binomial proportion tests comparing the number of significant vs insignificant observations for wider or smaller symmetric intervals around the respective threshold, which should be similar in the absence of p-hacking. The results of this randomization test are less clear-cut than for the much larger sample of Brodeur et al. (2020). Narrowing the interval leads to a low number of suitable observations which reduces the power of the tests. In general, selection on statistical significance seems to be an issue in our sample, but the shift towards statistical significance seems more gradual over the range of significance thresholds.

3.2. Conditional publication probabilities

Andrews and Kasy (2019) provide an alternative assessment of publication selection, considering selection both on statistical significance and on the sign of the estimates. Their test calculates conditional publication probabilities in relation to a reference range of z-statistics. That is, in comparison to the randomization tests above, the focus is less on marginal differences in publication probabilities in the proximity of cut-off values, but on differences of average publication probabilities for typical significance levels.

[3] As can be seen from Figure B.1 in Appendix B.1, the heaping is stronger when we do not winsorize, and stronger still if we also include observations with precision measures based on threshold values as shown in Figure B.2. On the other hand, the heaping is less visible when we exclude low-quality studies (judged by coders) and non-preferred studies (judged by authors), see Figure B.3.

Figure 2: Conditional publication probabilities



(a) Short run

(b) Long run

Notes: The figures show relative publication probabilities on the vertical axis in logarithmic scale for typical ranges of the z-statistics of short-run (left panel) and long-run (right panel) price elasticities of demand displayed on the horizontal axis. Cutoffs are specified at $z = 0$ and $z = \pm 1.96$. The publication probability of observations with $z > 1.96$ is normalized to 1. Observations where the standard error was calculated from thresholds are excluded. Shaded areas show 89% confidence bands. Table B.1 in Appendix B.2 provides the precise numerical results and inference statistics. Figure B.5 shows the findings for additional cutoffs at the 90%, 95% and 99% significance thresholds.

Figure 2 shows the results of this test, separately for short- and long-run elasticities. Table B.1 in Appendix B.2 provides the precise numerical results and inference statistics. The reference range is a positive estimate that is statistically significant at least at the 5% level ($z > 1.96$), to which we assign a publication probability of 1. The other ranges then receive a relatively larger or smaller publication probability. Note that the vertical axes present a logarithmic scale such that the steps can be interpreted as factor changes.

Considering the short-run elasticities, the publication probability is about 3 times greater for a positive but statistically insignificant (at the 5% level) finding. When considering positive elasticities, there is no preference for statistical significance. Instead, researchers might rather shy away from significant estimates, if they happen to have an unexpected positive sign. This is consistent with a preference for theory-conformist results that can also be seen at the borderline from positive insignificant to negative insignificant estimates. The publication probability increases by a factor of about 3 at this threshold. Signs of p-hacking in line with Figure 1 above can be observed as negative statistically significant findings (at the 5% level) are about 4 times more likely to be published than negative statistically insignificant elasticities. Overall, observing a negative significant elasticity is

25 times more likely than observing a positive statistically significant one. Qualitatively similar patterns, but quantitatively weaker differences can be observed for the long-run elasticities, which generally show a greater variance. The steps are quite similar in relative terms but the overall increase in the publication probability is only half as large. This might be due to the fact that some of the reported long-run elasticities are derived from dynamic equations as described in footnote 1. The statistical significance of these long-run elasticities might thus not be of central concern to the primary study authors.

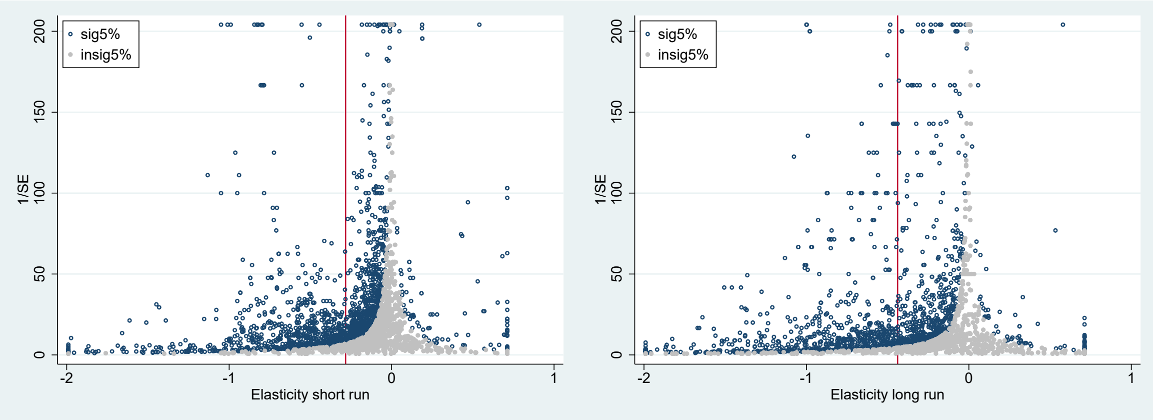
If we take into account additional standard significance thresholds, like the 10% ($z = \pm 1.65$) or 1% level ($z = \pm 2.58$), the single steps are expectedly smaller and the overall stair shape is less clear cut for the long-run estimates, but the qualitative results hold. As can be seen in Figure B.5 and Table B.2 in Appendix B.2, the most important thresholds are the change in sign as well as the 5% level in the negative range, which confirms a preference for negative statistically significant estimates in the literature.

3.3. Funnel asymmetry

Another established approach to detecting publication selection bias is a funnel plot – a scatter plot showing the effect size on the horizontal axis and the precision of the estimate (the inverse of the standard error, $1/SE$) on the vertical axis. According to standard econometric assumptions with random error, observations should scatter symmetrically around the most precise values (at the top of the funnel) which should be closest to the true effect. Publication selection bias, however, will lead to an asymmetric funnel due to a correlation between the standard error and the point estimate (Stanley and Doucouliagos 2012a; Egger et al. 1997).

The mechanisms leading to this correlation can be outlined as follows: When the true value is small and negative, low-powered estimates with large standard errors sometimes yield null results or positive elasticities. If these outcomes are deemed implausible or unlikely to be published, researchers might pursue different specifications or datasets. Consequently, specifications yielding more negative point estimates might address large standard errors,

Figure 3: Funnel plot: precision vs effect size



(a) Short run (b) Long run

Notes: The figures present funnel plots, which are scatter plots of the effect size on the horizontal axis and the precision of the estimate (the inverse of the standard error, $1/SE$) on the vertical axis. The left panel includes the short-run funnel, while the right panel shows a long-run funnel plot. For a better visual inspection of thresholds, the graph marks all statistically significant estimates (at the 5% level) in blue rings and the statistically insignificant values as gray dots. The red solid vertical lines represent the unweighted average elasticity of the short and long-term, respectively. An asymmetric funnel as well as a discrepancy between the unweighted average and the most precise estimates at the top of the funnel may point to publication selection bias. Figure B.6 in Appendix B.3 shows the same funnels with markers for observations that were deemed inferior by the primary study authors and quality concerns by us.

whereas small negative estimates can achieve significance via smaller standard errors through specific choices. Statistically significant positive results may be disregarded on theoretical grounds. All these scenarios contribute to a negative correlation between standard errors and point estimates.

Figure 3 shows the funnel plots for the winsorized estimates separately for the short and long-run elasticities. We mark estimates that are statistically significant at the 5% level as blue rings and the insignificant ones as gray dots. The funnels are clearly asymmetric with many more low precision (though statistically significant) estimates on the left-hand side. Moreover, the center regions of the funnels are less dense for positive elasticities. The tops of the funnels with the most precise estimates are close to zero and on average slightly negative both for the short and long run. The tops of the funnels are much closer to zero than the unweighted average of the estimates, displayed by the red solid vertical lines. As noted above, we flagged estimates that were deemed inferior by the primary authors and those with quality concerns according to our assessment. Figure B.6 in Appendix B.3 highlights these estimates in the funnel plots. Inferior estimates are more often large and positive than other observations so they conglomerate on the right-hand side of the funnel. Those with quality concerns often come with high precision and heap on the upper end of the graph.

3.4. Tests of publication bias

The meta-analysis literature has advanced a series of statistical procedures designed to identify potential publication bias and ascertain an unbiased true effect (Stanley 2008; Andrews and Kasy 2019; Stanley and Doucouliagos 2017). Formally, funnel asymmetry can be evaluated by performing a regression of effect sizes on the standard error. For this test, we compute variations of the following standard model:

$$\varepsilon_{ij} = \beta_0 + \beta_1 SE_{ij} + \nu_{ij} \tag{1}$$

Here, ε_{ij} stands for the estimated standardized elasticity i from study j. SE_{ij} denotes its standard error, and ν_{ij} is the random sampling error. This basic version is known as the Funnel-Asymmetry-Test-Precision-Effect-Test (FAT-PET) (Egger et al. 1997; Stanley 2008). In the absence of selection bias and other distortions, estimated effects are expected to be randomly dispersed around the true value. If β_1 is found to be significantly different from zero, it may indicate publication selection bias. Controlling for publication bias and sampling error, β_0 should reflect the mean effect, adjusted for bias, which is our primary coefficient of interest.

The preferred variant is to estimate Eq. (1) with weighted least squares (WLS), using the inverse of the variances ($1/SE^2$) as weights to deal with the observed heteroskedasticity (Stanley and Doucouliagos 2012b). The WLS assigns more weight to more precise studies as these should be less prone to publication selection. Alternatively, Stanley and Doucouliagos (2017) propose an unrestricted weighted least squares (UWLS) test, which does not contain $\beta_1 SE_{ij}$ from Eq. (1) and corrects for publication bias via inverse variance weights only. However, a simple OLS version and a variant with study fixed effects are also frequently estimated for robustness (e.g. Havránek et al. 2024). In addition, Stanley and Doucouliagos

(2014) show in Monte-Carlo simulations that Eq. (1) including the quadratic form SE^2 instead of the linear SE is often a better representation of the relation between the standard error and the point estimate in the presence of publication bias and a non-zero underlying effect. This specification is called precision effect estimate with standard error (PEESE).

Table 2: Linear publication bias tests

short run	(1)UA	(2)UWLS	(3)OLS	(4)FE	(5)PET	(6)PEESE	(7)Median
β_1			-0.917	-0.792	-1.148	-2.939	-2.414
pubbias			(0.148)	(0.143)	(1.089)	(1.047)	(1.266)
β_0	-0.283	-0.148	-0.169	-0.184	-0.134	-0.146	-0.130
mean	(0.0236)	(0.0488)	(0.0206)	(0.0179)	(0.0612)	(0.0492)	(0.0233)
Obs.	2680	2680	2680	2680	2680	2680	230
Adj. R^2	0.000	0.195	0.156	0.112	0.004	0.002	0.011
long run	(1)UA	(2)UWLS	(3)OLS	(4)FE	(5)PET	(6)PEESE	(7)Median
β_1			-0.471	-0.471	-1.764	-1.639	-3.963
pubbias			(0.142)	(0.138)	(0.770)	(0.553)	(1.442)
β_0	-0.439	-0.232	-0.335	-0.335	-0.212	-0.231	-0.0765
mean	(0.0395)	(0.0670)	(0.0364)	(0.0301)	(0.0753)	(0.0673)	(0.0340)
Obs.	2294	2294	2294	2294	2294	2294	191
Adj. R^2	0.000	0.368	0.060	0.065	0.012	0.001	0.033

Notes: The table shows tests for funnel asymmetry according to different versions of Eq. (1). The upper panel considers short run elasticities and the lower panel the long-run elasticities. Column (1) shows the unweighted average (UA) from a simple regression on the constant β_0. Column (2) estimates the unrestricted weighted least squares (UWLS) average, which is a WLS version of column (1) with inverse variance weights (Stanley and Doucouliagos 2017). Column (3) shows estimates of Eq. (1) as a simple OLS without weights and column (4) does the same, adding study fixed effects (FE). Column (5) shows the standard FAT-PET test of Eq. (1) with inverse variance weights. Column (6) (PEESE) does the same, but uses the SE^2 instead of the linear version. Column (7) resembles column (5), but uses only the median estimate per study. Standard errors in parentheses are clustered at the study level, except for column (7). Table B.4 in Appendix B.3 replicates these tests when excluding estimates that were deemed inferior by the primary study authors and those flagged with quality concerns by us. Table B.3 in Appendix B.3 replicates the PEESE test for different levels of winsorization of the data.

Regression results from the test variants are given in Table 2, where the upper panel focuses on short-run elasticities and the lower panel on long-run elasticities. Column (1) shows the unweighted average (UA) to facilitate comparison. In line with standard guidelines, we cluster standard errors at the study level since multiple estimates from one primary study might not be independent (Irsova et al. 2023). Column (7) shows estimates when only picking the median estimate per study, which can be a short or a long-run estimate.

Note that almost all tests in columns (3) to (7) find β_1 to be statistically significant and negative, pointing to funnel asymmetry and an over-representation of large negative elasticities. The corrected means, β_0 in columns (2) to (7), are all closer to zero than the unweighted average in column (1). Corrected short-run elasticities range from -0.13 to -0.18. Corrected long-term elasticities tend to be somewhat larger on average and more dispersed in the range of -0.08 to -0.34. The relative correction factor in comparison to the naïve average is around 25 to 80 percent – a range that is quite common in many meta-analyses (Gechert et al. 2024).

We robustify these baseline findings in several dimensions. First, we ask whether the choice of the winsorization level drives our results. Table B.3 in Appendix B.3 shows that the PEESE results are robust to different winsorization levels, except for the case of no winsorization at all. In this case, some outliers with very high precision receive a large weight in the WLS estimation. Winsorization contains their influence and leads to more conventional and conservative estimates. Second, Table B.4 in Appendix B.3 repeats the analysis based on a sample that excludes inferior estimates according to the authors of the primary studies and excludes estimates for which we raised quality concerns. The findings are qualitatively similar, but the resulting corrected means are even closer to zero.

The variants of Eq. (1) above assume a linear (or quadratic) relation between the standard error and the point estimate. However, if publication bias is not monotonous across the full distribution of point estimates but is concentrated on estimates in the proximity of standard significance thresholds, the relation between the standard error and the point estimate might be more complex. Moreover, Irsova et al. (2024) show that if p-hacking is performed on the standard error (instead of the point estimate), then Eq. (1) might suffer from reverse causality. Alternative nonlinear tests of publication bias can account for these complications.

The test for conditional publication probabilities by Andrews and Kasy (2019) also provides an estimate of the underlying mean beyond bias (AK). Moreover, Bom and Rachinger (2019) consider a kinked version of the FAT-PET regression, providing for the case that results are more likely to be published when they pass a certain significance threshold.[4] Ioannidis et al. (2017) develop an estimate of the underlying effect based on the weighted average of only the adequately powered primary estimates (WAAP). They argue that highly powered primary estimates are less likely to be p-hacked.

Similarly, the test of Furukawa (2019) focuses on a subsample of the most precise estimates, the so-called stem of the funnel plot. This subsample is determined by minimizing the trade-off between the variance per observation (which shrinks with an increasing sample) and publication bias (which rises with a larger sample). Finally, Irsova et al. (2024) propose a two-stage least squares (2SLS) regression, which they coin the meta-analysis instrumental variable estimator (MAIVE), where the inverse of the square root of the sample size of the primary estimates instruments the standard error in Eq. (1). The sample size could be a valid instrument as it is usually given to researchers due to limited data availability and should be highly negatively correlated with the standard error. Under these conditions, the second stage regression would provide an estimate of the mean elasticity, corrected for publication bias in the point estimates.

The results of these tests, shown in Table 3, are largely consistent with the linear tests in Table 2. The resulting means beyond bias for the WAAP method are quite similar to the PEESE estimates. The AK tests even find smaller underlying elasticities, close to zero in the short run and slightly negative in the long run. The Furukawa (2019) method reports large negative effects for the short run and close-to-zero effects for the long run, but the number of observations selected for the stem is extremely small, so the results may be interpreted with caution as they likely suffer from small sample issues. (see Figure B.7 in Appendix B). The MAIVE 2SLS approach finds somewhat larger corrected effect sizes. However the instrumentation is probably too weak in our sample as the first-stage F-statistics are below conventional thresholds and much smaller than the threshold of 100 recommended by Irsova et al. (2024). Again, if we exclude the inferior estimates and those with quality concerns,

[4] Note that the Bom and Rachinger (2019) endogenous kink test collapses to the simple FAT-PET test if the test does not signal a significant nonlinearity, which is the case in our sample. Thus we do not report the results here.

Table 3: Nonlinear tests – underlying effects

short run	(1)AK	(2)WAAP	(3)Stem	(4)MAIVE
mean beyond bias	0.028	-0.146	-0.6793	-0.226
	(0.014)	(0.0496)	(0.3529)	(0.100)
Obs.	2437	1335	3	2529
1st stage F				3.674
long run	(1)AK	(2)WAAP	(3)Stem	(4)MAIVE
mean beyond bias	-0.109	-0.230	-0.0137	-0.298
	(0.045)	(0.0685)	(0.1418)	(0.0919)
Obs.	1850	714	7	2213
1st stage F				7.270

Notes: The table shows the corrected effects according to further tests of publication bias. The upper panel considers short run elasticities and the lower panel the long-run elasticities. Column (1) refers to the test of conditional publication probabilities by Andrews and Kasy (2019) (AK). Column (2) refers to the test by Ioannidis et al. (2017) based on the weighted average of only the adequately powered primary estimates (WAAP). Column (3) refers to the test of Furukawa (2019), which focuses on a subsample of the most precise estimates, the 'stem' of the funnel plot. We distrust the test results due to the very small number of observations included. Column (4) (MAIVE) shows the second stage of a two-stage least squares (2SLS) regression, where the inverse of the square root of the sample size of the primary estimates instruments the standard error in Eq. (1) (Irsova et al. 2024). The first stage F-statistics are below recommended thresholds, so we distrust the estimates. Standard errors in parentheses are clustered at the study level, except for column (3). Table B.5 in Appendix B.3 replicates these tests when excluding estimates that were deemed inferior by the primary study authors and those flagged with quality concerns by us.

the corrected means are even closer to zero as can be seen from Table B.5 in Appendix B.3. Also, the Furukawa test on short-run elasticities is more in line with the other test results in the sample without inferior estimates and quality concerns.

Which of the multiple test variants are the preferred ones? FAT-PET and PEESE are established standards in most meta-analyses that assess publication bias (Havránek et al. 2020). A two-step procedure (PET-PEESE) detects whether there is a statistically significant genuine effect β_0 from the FAT-PET. If so, β_0 from the PEESE test is typically considered to be closer to the true underlying mean estimate. We follow this procedure and consider the PEESE specification as our baseline here and also in the multiple meta-regressions of Section 4. At any rate, considering the broad picture that emerges from the battery of tests, publication bias seems prevalent, and the range of corrected estimates is much lower than could be inferred from the simple average in our sample or a qualitative review from an AI.

4. Heterogeneity

This section investigates factors that might explain the variation in the reported price elasticities. As shown in Table 1, estimates can be classified by a number of study and estimation characteristics. We cover important dimensions of heterogeneity with a focus on different energy sources and uses, sectors, data and estimation choices, as well as publication characteristics. Subsection 4.1 provides a subsample analysis, Subsection 4.2 presents a frequentist multivariate meta-regression model and Subsection 4.3 uses a Bayesian model averaging approach to test the robustness of our main results with respect to model uncertainty. Subsection 4.4 discusses a best-practice specification.

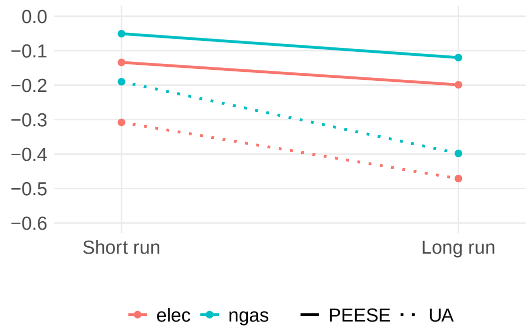
4.1. Subsample analysis

This section considers the corrected and uncorrected estimates from Section 3 for important subsamples of our dataset. Figure 4 shows short-run and long-run unweighted averages and estimates of the underlying effect based on the PEESE test. We chose the PEESE test as it is widely used and the correction factor for publication bias is somewhere in the middle of all methods. We consider subsamples for the largest groups of energy sources (electricity and natural gas), country groups (OECD vs non OECD), sectors (residential vs business) and regarding journal ranking (top journal publication vs any other publication).

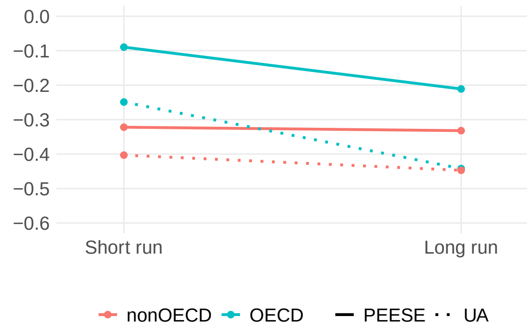
Comparing energy sources, price elasticities for electricity are somewhat stronger than for natural gas both in the short and long run, and the correction for publication bias is rather similar (about 1/3 of the effect size remains). Considering country groups, it turns out that uncorrected short-run estimates for OECD countries are smaller than for non-OECD countries, while the long-run estimates are rather similar. However, the measure of publication bias is stronger for OECD-country estimates such that the corrected effect size is much closer to zero both in the short and long run.

Looking at the differences between the sectors, the uncorrected effects are relatively similar for the residential and business sectors both in the short and long run. However, publication bias seems stronger in estimates for the business sector and the correction is

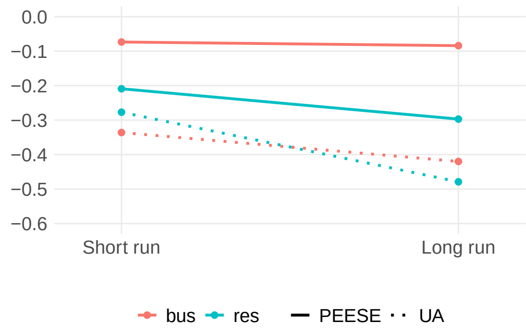
Figure 4: Uncorrected and corrected elasticities for subsamples, short run and long run



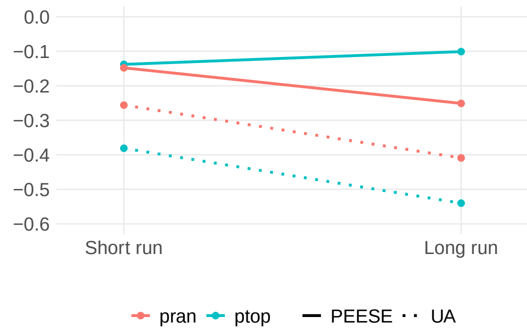
(a) Energy source



(b) Country group



(c) Sector



(d) Publication

Notes: The figures present the unweighted averages (UA, dotted lines) and corrected effect sizes (according to the PEESE method, solid lines) for short and long-run elasticities for major subsamples: energy source (electricity and natural gas), country group (OECD and non OECD), sector (residential and business), publication type (top journals vs other).

more than twice as strong. A similar effect can be seen for publication outlets. Uncorrected effects point to more negative elasticities in top journals both for the short and long run. However, publication bias seems to be stronger in high-ranking journals and after correction, the effects sizes are similar for the short run and even smaller for the long-run.

In general, the subsample analysis shows that publication bias seems to be a recurrent issue in most of the subsamples. Moreover, the corrected elasticities are more homogeneous among the considered subgroups than the uncorrected values.

4.2. Multivariate meta regression

In what follows, we test the impact of modeling and sample choices as well as publication characteristics in a multivariate meta-regression (MMR) framework, while also controlling for publication bias via the standard PEESE test as laid out in Section 3 above. That is, we modify Eq. (1) to include the squared standard error SE_{ij}^2 of estimation i of study j, as well as a vector X_{ij} of the moderator variables that capture the estimation and study characteristics introduced in Table 1. We can thus write the MMR model as follows (again estimated as WLS with inverse variance weights and with clustering at the study level):

$$\varepsilon_{ij} = \beta_0 + \beta_1 SE_{ij}^2 + \beta_2 X_{ij} + \nu_{ij} \tag{2}$$

X_{ij} primarily contains dummy variables or categorical variables of which one category is omitted from the regression. Thus, in the MMR, β_0 reflects the corrected effect sizes of the omitted reference categories, which are to be defined. In our baseline specification, we include the time horizon, the energy sources and uses, the sector, the source of the price change, and the considered country group as explanatory variables in X_{ij}. We select a short-run elasticity of electricity demand in the residential sector for an unspecific mix of heating / cooling after a market-price change in an OECD country as the reference. These references are chosen since they represent the largest groups in our sample. Due to the linear regression form, the choice of one omitted category does not affect the coefficients of other variables in β_2. However, it shifts the intercept β_0, which cannot be interpreted as the general mean beyond bias anymore. β_1 still estimates the strength of publication bias when controlling for study characteristics.

Table 4 shows the regression results for various versions of X_{ij} based on the full sample. Column (1) contains our baseline specification including factor and dummy variables of the time horizon of measurement, the main categories of energy sources, the energy use, the sector, the source of the price change and the country group. Column (2) shows a parsimonious specification in accordance with the PAP. Further columns are based on column (1) and include additional control variables concerning characteristics of the data used in the primary studies, their estimation methods and publication characteristics. Those additional coefficients are shown in Table C.1 in Appendix C.

The results for the baseline moderator variables are similar across the different specifications. The coefficient of the squared standard error remains large and highly statistically significant, irrespective of the list of included control variables. Thus, our assessment of publication bias does not seem to be an artifact of omitted variable bias. Short-run and long-run estimates do not differ strongly when taking into account other covariates. Regarding energy sources, natural gas tends to show less negative elasticities, in line with Figure 4 above. The other sources (like coal, heating oil, LPG) have somewhat more negative elasticities on average, about double the size of the underlying reference value.

There are no strong and robust differences for the elasticities of heating and cooling, the source of the price change and among the country groups. In some specifications, non-OECD countries tend to exhibit more negative demand reactions to price increases, but the results depend on the inclusion of further control variables and are uncertain. The sectoral differences are more noticeable: elasticities tend to be weaker in the business sector than in the residential sector and weakest for the mix of sectors which includes assessments for whole economies.

In Table 5, we consider the baseline specification for various subgroups of our sample regarding the time horizon, the main energy sources and the sectors. Columns (1) and (2) separate the effects for short and long-run estimates. Columns (3) and (4) look at the two

Table 4: Multivariate meta regression, baseline and robustness checks

	(1) base	(2) pars	(3) data	(4) estim	(5) public	(6) all
Constant	-0.177	-0.109	-0.140	-0.143	-0.193	-0.206
	(0.0591)	(0.0538)	(0.0799)	(0.0544)	(0.0941)	(0.111)
SE^2	-2.311	-2.098	-2.525	-2.942	-2.163	-2.585
	(0.440)	(0.473)	(0.422)	(0.430)	(0.421)	(0.413)
long run	-0.0160	-0.0454	0.00129	0.0153	-0.0227	-0.0474
	(0.0588)	(0.0689)	(0.0447)	(0.0402)	(0.0489)	(0.0349)
natural gas	0.115	0.0511	0.0305	0.121	0.0309	0.0684
	(0.0556)	(0.0452)	(0.0556)	(0.0485)	(0.0589)	(0.0512)
esource other	-0.160	-0.148	-0.151	-0.135	-0.212	-0.172
	(0.0777)	(0.0734)	(0.0635)	(0.0555)	(0.0664)	(0.0497)
heating	-0.0424		0.0202	0.0145	-0.00117	0.0296
	(0.0596)		(0.0593)	(0.0434)	(0.0606)	(0.0427)
cooling	0.100		0.163	0.117	0.0307	0.140
	(0.108)		(0.126)	(0.0862)	(0.106)	(0.0954)
non-market, mix	0.0279	-0.0118	0.00514	-0.0420	0.000373	-0.0431
	(0.0844)	(0.0914)	(0.0727)	(0.0578)	(0.0799)	(0.0554)
non OECD	-0.156	-0.137	-0.0716	-0.00633	-0.144	0.0252
	(0.100)	(0.118)	(0.0843)	(0.0636)	(0.0926)	(0.0565)
mixed countries	0.0294	0.163	0.0309	-0.0247	0.0542	0.00817
	(0.0576)	(0.0717)	(0.0524)	(0.0512)	(0.0574)	(0.0509)
business	0.116		-0.0123	0.123	0.0858	0.0617
	(0.0525)		(0.0499)	(0.0373)	(0.0494)	(0.0512)
sector mix	0.287		0.127	0.181	0.328	0.190
	(0.0744)		(0.0694)	(0.0547)	(0.0703)	(0.0551)
Further controls	none	none	data char.	estim. char.	publ. char.	all
Obs.	4974	4974	4970	4974	4974	4970
Adj. R^2	0.247	0.159	0.324	0.417	0.320	0.488

Notes: The table shows results of WLS regressions (with inverse variance weights) according to Eq. (2) with the PEESE correction for publication bias and including moderator variables in X. Moderators are primarily categorical factor variables of which one category is omitted due to perfect collinearity and is reflected in the intercept. The reference categories are given in italics. Column (1) uses our baseline specification and includes: the time horizon of measurement (*short* vs long run), the energy source (*electricity*, natural gas, other), the energy use (heating, cooling, *mix*), the sector (*residential*, business, mix), the source of the price change (*market-based* vs non-market and mixed), the country group (*OECD*, non-OECD, mix). Column (2) shows a parsimonious specification in accordance with the pre-analysis plan. Further columns are based on column (1) and include additional control variables. Column (3) includes controls characterizing the data used in the primary studies. Column (4) includes controls concerning estimation methods in the primary studies. Column (5) includes controls related to publication details. Column (6) combines all of the former. The additional coefficients are shown in Table C.1 in Appendix C. Standard errors in parentheses are clustered at the study level.

largest groups of energy sources, electricity and natural gas, respectively. Columns (5) and (6) consider the residential and business sector separately.

Most of the coefficients are relatively similar across subgroups and in line with Table 4. Importantly, the measure of publication bias (SE^2) is statistically significantly negative and large for all the subgroups. When looking at the different energy sources in combination with sectors, the weaker elasticities for natural gas seem to be driven by the residential sector, while the business sector's lower price sensitivity is specific to electricity demand. The finding of higher price elasticities in non-OECD countries seems to be specific to short-run responses, electricity consumption, and the residential sector. This makes sense since households in lower-income countries face stronger budget constraints and should be more price sensitive in the short run with respect to basic needs for a relatively expensive good like electricity, while their options to switch to more efficient appliances are more limited. Heating and cooling do not involve consistently different elasticities in comparison to the reference mix category. The large coefficient for cooling in the business sector is likely due to a small sample bias (there are only 18 such observations in our sample).

Table C.2 in Appendix C also uses the baseline selection of control variables and shows the findings for subsamples with different quality criteria (preferences of the primary authors or our quality concerns, identification strategies or accounting for income and cross-price effects). Again, there is a strong signal for publication bias in all sample selections and it seems to be stronger, when considering only the preferred estimates of the primary study authors. For some subsamples, single coefficients change considerably, but often this is related to categories with a small number of observations. After all, the results are largely robust when looking at these smaller samples with very different selection criteria.

4.3. Bayesian model averaging

Given the large amount of possible control variables there is considerable model uncertainty, which we address by Bayesian Model Averaging (henceforth, BMA). The BMA runs a universe of regression models including or excluding some of the moderator variables. In

Table 5: Multivariate meta regression, group-wise samples

	short run	long run	electricity	natural gas	residential	business
Constant	-0.154	-0.169	-0.175	-0.0554	-0.186	-0.0295
	(0.0530)	(0.0634)	(0.0588)	(0.0760)	(0.0688)	(0.0182)
SE^2	-2.546	-1.958	-3.352	-1.706	-2.406	-2.446
	(0.798)	(0.439)	(0.639)	(0.484)	(0.602)	(0.582)
long run			-0.0351	-0.0568	0.00702	-0.0290
			(0.0489)	(0.0458)	(0.0898)	(0.0259)
natural gas	0.0732	0.0719			0.191	0.0259
	(0.0821)	(0.0417)			(0.0826)	(0.0514)
esource other	-0.295	-0.0708			-0.154	-0.108
	(0.104)	(0.0739)			(0.0975)	(0.0569)
heat	0.0148	-0.138	-0.115	0.0241	-0.0535	-0.177
	(0.0803)	(0.0702)	(0.0615)	(0.0808)	(0.0861)	(0.0619)
cool	0.125	-0.0949	0.0527	-0.0729	0.123	-0.939
	(0.121)	(0.135)	(0.0998)	(0.0576)	(0.111)	(0.0449)
non-market	0.0280	-0.132	0.109	0.0208	0.0320	-0.0813
	(0.0924)	(0.142)	(0.0881)	(0.0740)	(0.0946)	(0.0872)
non OECD	-0.202	-0.0189	-0.237	-0.0902	-0.212	0.0420
	(0.111)	(0.167)	(0.113)	(0.0887)	(0.117)	(0.0941)
country mix	0.0291	0.00791	-0.0596	0.0109	0.0481	0.0642
	(0.0682)	(0.0619)	(0.0714)	(0.0651)	(0.0837)	(0.0725)
business	0.0939	0.114	0.155	-0.0589		
	(0.0579)	(0.0712)	(0.0557)	(0.0473)		
sector mix	0.379	0.188	0.295	0.0793		
	(0.0859)	(0.0726)	(0.100)	(0.0927)		
Obs.	2680	2294	2748	1250	3157	1240
Adj. R^2	0.347	0.183	0.232	0.073	0.234	0.171

Notes: The table shows results of WLS regressions (with inverse variance weights) according to Eq. (2) with the PEESE correction for publication bias and including moderator variables in X. See Table 4 for description of variables. Column (1) and (2) show subsamples for short and long-run elasticities, respectively. Column (3) and (4) show subsamples for the most frequent energy sources, electricity and natural gas, respectively. Columns (5) and (6) look at the residential and business sector subsamples, respectively. Standard errors in parentheses are clustered at the study level.

principle, model uncertainty generates 2^m possible subsets based on m, the number of moderator variables. Given the expected large amount of moderator variables, a full analysis of all models would be practically infeasible. Thus we use a Markov-Chain Monte-Carlo process with Metropolis-Hastings algorithm (Zeugner and Feldkircher 2015) focusing on the models with the highest probability. The result of the BMA is a weighted average of all these regressions, where the weights correspond to the posterior model probabilities according to the relevance of moderators across specifications. For each moderator variable we receive a posterior inclusion probability (PIP). See Figure C.1 and Table C.3 in Appendix C.2 for the BMA results.

Our core finding of the relevance of publication bias, represented by a PIP of 1 for SE^2 (PEESE) and a negative coefficient across models, is robust to BMA. Moreover, a publication-bias-corrected value of -0.166 for the BMA average of the price elasticity for the reference group is fairly in line with our previous MMR results. Regarding the relevance of moderator variables, the BMA results point to special importance of the time horizon, giving credence to our approach to split the sample into short and long-run elasticities.

Of the other moderators from our baseline MMR, in particular the energy use seems important from the BMA. In addition, the BMA results highlight the relevance of the macro-micro dimension, the frequency and dimension of the data, the role of estimation methods, distinguishing top-journal publications as well as the primary studies' authors assessments of estimates. All of the above-mentioned variables have a high PIP and are unambiguous regarding their sign across specifications. Note, however, that BMA is primarily useful for prediction purposes, and less for causal inference. Therefore, a high PIP does not necessarily reflect a direct causal relationship. Instead, the BMA primarily provides a model uncertainty-robustness check of our main results.

4.4. Best practice

Our results document some variation in the underlying elasticities depending on study and estimation characteristics. One might therefore ask for a best-practice point estimate from the meta-analysis that can guide modelers and policy makers. The specification for the best-practice estimate is informed by characteristics that are relevant on theoretical grounds as well as by the inclusion probabilities according to the BMA exercise. The estimate is based on Eq. (2) with a PEESE model of publication bias and is separately estimated for the short-run and long-run elasticities.

From a theoretical point of view, we include the variables in our baseline specification according to Table 4. Moreover, we consider a sophisticated identification method, income and cross-price controls, the journal rank, the number of citations and primary author preferences as important signals of best practices. In addition, we include a dummy for macro vs. micro estimates, the data dimension (cross section vs. times series vs. panel), the data frequency (sub-annual vs. (multi-)annual), a dummy for system estimations and a dummy for dynamic estimations as important variables according to the BMA's inclusion probabilities. We use the subsample without inferior estimates and excluding those with quality concerns in line with Figure B.6.

The best practice elasticities are linear predictions based on the PEESE multivariate meta regression, where we plug in sample maxima for variables reflecting best practice in the literature, sample minima for variables reflecting departures from best practice, and sample means for variables where we cannot clearly determine best practice. A best-practice estimate should have high precision, should be identified well, and should account for income and cross-price effects. It should be published in a top journal, the study should have the maximal number of citations in our sample, it should be preferred by the primary authors and should not be a byproduct of the study. As discussed above, these latter criteria might be even more strongly related to publication bias. However, since we already correct for publication bias, the chosen characteristics should rather represent estimates of higher quality. Finally, we impute sample averages for sample year and frequency, micro or macro studies, and estimation approaches. For the headline figures, we also use the sample-average shares of the energy source, energy use, source of the price change, country group and sector categories.

Table 6: Elasticities from a synthetic study representing best practices

	Short run		Long run	
	(1) Elasticity	(2) 95% CI	(3) Elasticity	(4) 95% CI
all	-.117	[-.026, -.207]	-.176	[-.012, -.341]
electricity	-.077	[.010, -.164]	-.189	[-.010, -.369]
natural gas	-.170	[-.053, -.288]	-.170	[-.010, -.329]
heating	-.084	[.016, -.185]	-.234	[-.046, -.422]
cooling	-.205	[-.096, -.313]	-.218	[-.010, -.420]
OECD	-.113	[-.025, -.200]	-.206	[-.041, -.370]
non OECD	-.135	[-.024, -.245]	-.096	[.106, -.298]
residential	-.131	[-.042, -.220]	-.235	[-.035, -.436]
business	-.124	[-.021, -.227]	-.125	[.029, -.279]

Notes: The table shows linear predictions of elasticities based on Eq. (2) of the sample without inferior estimates and observations with quality concerns. We plug in $SE^2 = 0$ (maximal precision), identified=1, income-control=1, cross-price control=1, topjournal=1, number of citations=max, preferred=1, byproduct=0. For the other moderator variables we chose sample averages. Columns (1) and (3) show the point estimates of the predictions for short and long run elasticities, respectively, and columns (2) and (4) the related 95 percent confidence intervals (CI).

Table 6 shows the resulting elasticities. The headline value in the first row represents the best-practice average elasticity for any heating or cooling purpose from any energy source in an average country and in any sector. For such a synthetic estimate, the elasticity is ca. -0.12 in the short run and -0.18 in the long run, well in the middle of the range of estimates from the various tests above.

The other rows show estimates for specific cases of the main energy sources, uses, country groups and sectors. Note that some cases are above or below the average estimate, but do not deviate strongly. The predicted values fall within the said range of -0.05 to -0.2 for the short run and -0.1 to -0.3 for the long run.

5. Conclusion

This paper provides a meta-analysis of more than 400 primary empirical studies that estimate the price elasticity of energy demand in buildings. The dataset contains almost 5000 estimated elasticities from different fossil fuels and energy sources, countries and regions, time periods, sectors and sources of the price change.

We have shown that a synthesis based on the naïve average of estimated elasticities would be consistent with previous literature reviews and an assessment made by ChatGPT. According to the simple average, heating and cooling would be necessities, though with still sizeable elasticities of about -0.3 in the short run and -0.45 in the long run, confirming hypothesis [H1].

However, such a conclusion would be misleading, since the literature suffers from publication bias according to a battery of established tests and in various subsamples. If we account for this problem, the resulting elasticities are considerably smaller than the conventional wisdom, in a range of -0.05 to -0.2 in the short run and -0.1 to -0.3 in the long run, confirming [H2].

Turning to factors that explain the variation in effect sizes, we do not detect relevant differences of elasticities after market-induced vs. non-market-induced price changes, rejecting [H3], but this topic might deserve a more detailed investigation than we can provide within the scope of this paper. We find a mixed picture regarding the elasticities of different energy sources and uses [H4]: in some specifications and subsamples, natural gas demand is somewhat less elastic than electricity demand and a mix of other sources (coal, oil, LPG), though not in best practice predictions and the differences are usually small. Heating and cooling do not exhibit robustly different elasticities either and the differences are usually small. When considering sectoral differences, however, residential energy demand consistently turns out to be slightly more elastic than business energy demand, in particular in the long run [H5]. In general, in line with our expectations, long-run elasticities exceed short-run elasticities, however, the differences are not large either [H6]. Differences between OECD and non-OECD countries are limited and vary with the chosen specification [H7]. There is only a negligible linear relation between the average sample year and the reported elasticity [H8], but country and time dependencies should be considered in more detail with more granular data. Study designs are partly correlated with the reported elasticities [H9]. However, in comparison to the corrections for publication bias, the impact of these heterogeneities is limited.

The estimated small short and long-term elasticities may not come as a big surprise when considering that heating and cooling energy in buildings is a necessity with difficult or costly substitution possibilities, partly delayed price signals to renters in ex-post service charge settlements, and often monopolistic market structures. They are also consistent with the observed consumption responses to recent energy price shocks (Ruhnau et al. 2023). A back-of-the-envelope calculation for Germany in 2022, featuring a rise of +143 percent in the natural gas retail price and a demand reduction of ca. -11 percent, notwithstanding weather effects, income elasticities, consumer fears of supply shortages, or ethical considerations, would imply a short-term elasticity of -0.08 (Jamissen et al. 2024), consistent with our findings. Likewise, Reiss and White (2008) report an 11 percent fall in electricity consumption of San Diegan households in the six months after an unexpected and rapid market price shock of +130 percent in the summer of 2000, pointing to a short-run elasticity of -0.08, which could have been double the size at an annual horizon of measurement.

Our results have implications for expectations of the workings of market mechanisms and the steering effects of pricing policies. The self-correcting effects after market price shocks through the demand side might be weaker than previously thought. Planning the security of supply in crisis situations and forecasts of energy-price dynamics should take into account a weak demand elasticity in this sector.

Likewise, CO2 pricing may have a less-than-expected steering effect through price signals alone. This could imply a prolonged trajectory of higher CO2 prices if demand responds sluggishly while certificate supply is shortened in line with sector targets. Climate policies accompanying CO2 prices or those that facilitate stronger price elasticities may turn out to have a better cost-benefit relation under such conditions of low demand responsiveness.

However, CO2 pricing and other administered price changes can induce different consumer reactions than market price fluctuations, as discussed in Andersson (2019), Basaglia et al. (2023), Broin et al. (2015), Edelstein and Kilian (2009), and Grieder et al. (2021), which deserves more detailed investigation. Our rich meta-data set is well suited to be extended in this direction in future research. Moreover, other country and time specifics could be analyzed by merging our dataset with more granular data on, e.g., temperatures, energy systems, CO2 price levels.

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Appendix A Further information on the search and data collection

A.1 Search and sample selection

Figure A.1: PRISMA Flow Chart

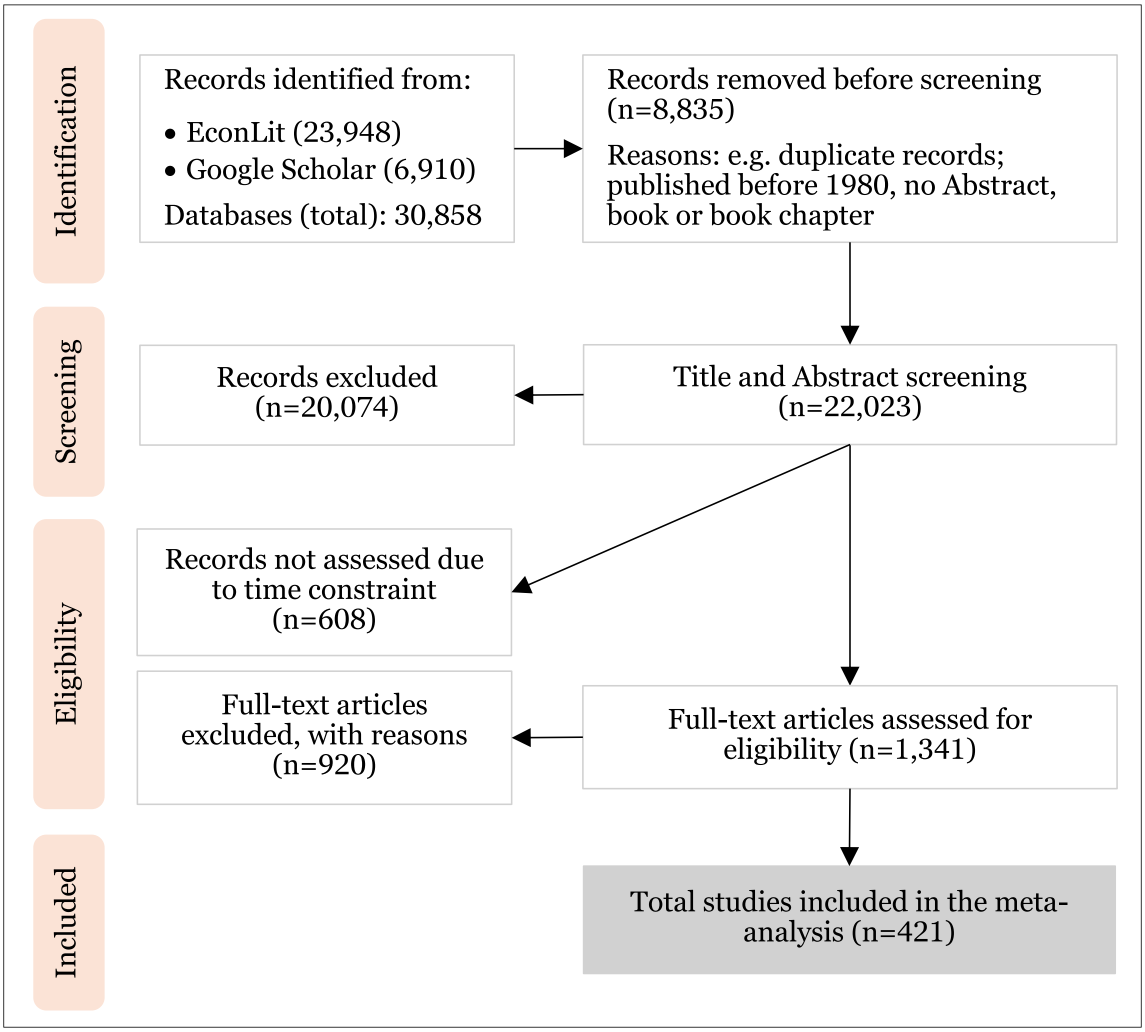


Figure A.1 provides a flow chart of the search, selection and screening process. We searched for studies in Google Scholar and EconLit (via EBSCO Host) and completed the search on January 25, 2023. Afterwards, we corrected for duplicates and added missing bibliographic information, which resulted in 22,023 unique and complete entries, finished on March 3, 2023.

These studies entered into title and abstract screening assisted by ASReview (van de Schoot et al. 2021), an open-source artificial intelligence (AI) tool that iteratively sorts entries based on previous decisions of researchers to mark similar studies as potentially relevant. Importantly, the decision about eligibility of any particular study is taken by the researcher, not by the AI. We performed a pilot run on a random 1% sample of entries to predict the expected share of truly relevant papers in all entries to determine statistical thresholds (in line with van Haastrecht et al. 2021) for stopping the screening as soon as the flow of relevant entries runs dry. After this pilot run, we finalized and registered the pre-analysis plan on March 10, 2023.

Two screeners then independently went through thousands of titles and abstracts and stopped screening on March 31, 2023. We included studies that were considered as eligible by at least one screener, leading to 1,949 potentially relevant studies for the full-text screening phase. This strongly outnumbered our predictions such that we suspected to run into our pre-determined time-resource constraint for the full-text screening (October 31, 2023). Thus, to avoid biases in the final dataset we randomized the order of screening and finally also extended the full-text screening and coding until January 31, 2024. At this point we needed to stop the full-text screening, but still could not assess 608 studies of which we suspect around 200 additional relevant studies. Due to binding time and resource constraints, we were unable to consider additions from either previous (meta)-studies or by other methods, such as backwards snowballing.

Nevertheless, we consider our final sample of 421 studies to be a representative random subsample of the true population. Two co-authors collected ca. 50% of the data each. Three co-authors double-checked around 70% of the collected data to correct potential coding errors and inconsistencies. The list of included papers can be found in Appendix D.

A.2 Additional descriptive statistics

In Table 1 in the main text we report descriptive statistics of the elasticities, their standard errors and the main study characteristics. It shows the mean and standard deviation for continuous variables, and percentage shares as well as number of observations of the values of factor variables and dummies. Table A.1 provides descriptive statistics of additional study and estimation characteristics that we use in several robustness checks.

We classify studies according to their use of micro vs macro data; the data dimension (cross section vs time series vs panel); data frequency (sub-annual vs (multi-)annual; the number of observations used; dynamic vs static estimation approaches; system vs single estimation approaches, the use of an elaborate identification strategy (like experiments, randomized control trials, difference-in-difference approaches, regression discontinuity designs, instrumental variables or generalized methods of moments approaches) or not; whether the estimation includes a control variable for income; a cross-price control variable for other energy sources; whether the heating/cooling energy price elasticity is the main focus of the paper or whether the elasticity is a mere byproduct; a judgment by the authors of the primary study as to whether they consider an estimate to be their preferred one, an inferior one or not; and a concern about the quality of the study on our side.[5] Note that in our baseline estimations we include observations with quality concerns and those that have been deemed inferior by the authors of the primary studies. However, we show that our main results are robust to excluding such estimates.

Data characteristics. The average year of the samples in the primary studies is ca. 1998 to 1999. The average number of observations is large due to some studies with sizeable micro datasets. Two-thirds of the estimates stem from macro data and around 75 percent use data with an annual or multi-year frequency. More than half of the estimates stem from panel studies (multi-country, multi-sector or micro panels) while a quarter uses time series data.

Estimation characteristics. Around 50 percent of the estimates stem from dynamic estimations and most of them are single equation estimations instead of system estimations (ca. 15 percent). About a third of the estimates, in particular short-run estimates use an

[5] Quality concerns flagged by us can be categorized as follows: (i) unclear or problematic research design, (ii) inconsistent results/reporting, (iii) implausible values of model validation, (iv) authors question their own results, (v) data limitations or (vi) issues with language, clarity, or formatting.

Table A.1: Descriptive statistics for additional study characteristics

	Time Horizon			
	short run		long run	
	mean or share	SD or freq	mean or share	SD or freq
no. of obs	348,833	(1.29e+06)	48,242	(433,795)
average year	1999	(12.65)	1998	(12.57)
macro data	60.04%		71.32%	
frequency >=Annual	71.90%		82.91%	
cross, time, panel				
cross section	17.39%	466	16.35%	375
time series	26.68%	715	28.51%	654
panel	55.93%	1,499	55.14%	1,265
dynamic estimation	56.79%		55.01%	
system estimation	13.25%		18.48%	
identified	34.78%		20.88%	
income control	82.05%		81.17%	
cross-price control	34.89%		38.27%	
top journal	21.60%		22.76%	
log citations	3.28	(1.69)	3.41	(1.53)
byproduct	12.20%		14.56%	
judgment estimate				
inferior	8.21%	220	7.41%	170
random	82.99%	2,224	79.08%	1,814
prefer	8.81%	236	13.51%	310
quality concern	13.36%		17.52%	

Notes: The table shows descriptive statistics of additional study and estimation characteristics. We separate between short-run and long-run estimates. We report the mean and standard deviation (SD) for continuous variables and percentage shares as well as frequencies (freq) for factor variables and dummies. The additional study characteristics include the number of observations and the average sample year of the estimate, the log no. of citations of the study, whether the estimate is based on micro or macro data, the frequency of the data (annual or multi-year vs below-annual), the dimension of the data (cross-section, time series, panel) estimation specifics (dynamic vs static estimation, system vs single-equation, specific identification approach (IV, DID, RCT, experiment, discontinuity) or not, controlling for income in the regression, controlling for other energy prices in the regression), whether the study is published in a top journal or not, whether the elasticity is the main focus of the study or merely a byproduct, how the primary study authors judge the estimate (preferred, random or inferior) and whether we flag a quality concern for the estimate or not. Statistics for the main variables are shown in Table 1 in Section 2.

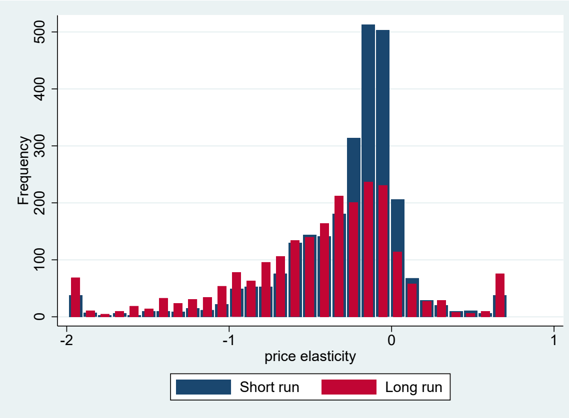
identification approach like IVs, natural experiments, or DID designs. Most estimations control for income and about one-third includes cross price controls.

Publication characteristics. About three quarters of the observations stem from journal publications, and we classify around 20 percent as published in high-ranking journals, according to a Scimago Journal Rating above 3 (the ranking is based on data from resurchify.com and was downloaded in April 2024). About 13 percent of the observations come from studies in which the price elasticity is a byproduct and not the main coefficient of interest. About 8 percent of estimates are considered inferior by the authors of the primary studies (usually because they doubt the regression specification) while around 10 percent are deemed preferential estimates. We flag around 15 percent of the estimates with a quality concern.

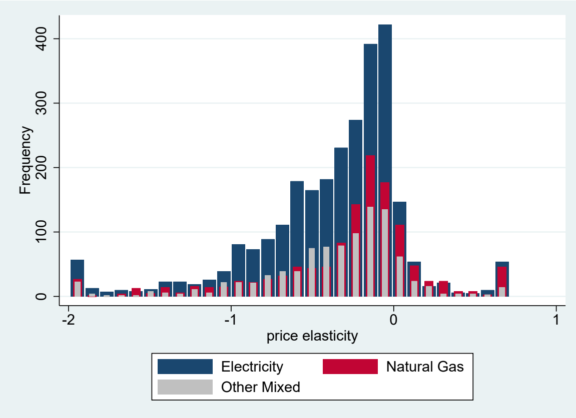
A.3 Histograms

Figure A.2 shows the histograms of the elasticities separately for a number of major study and estimation characteristics. Outliers are winsorized at the 2nd and 98th percentiles.

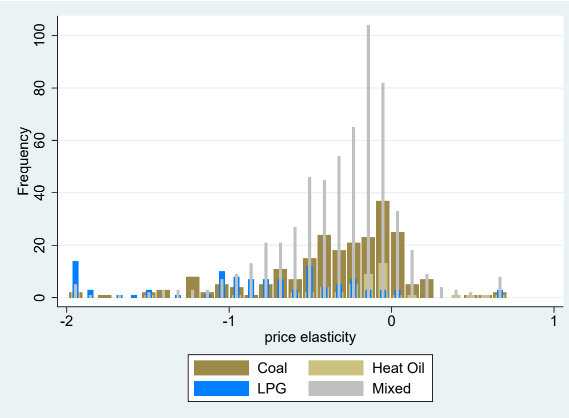
Figure A.2: Histograms of elasticities for major subsamples



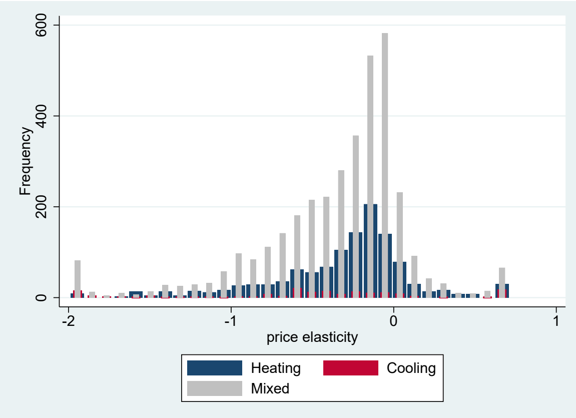
(a) Short-run vs long-run



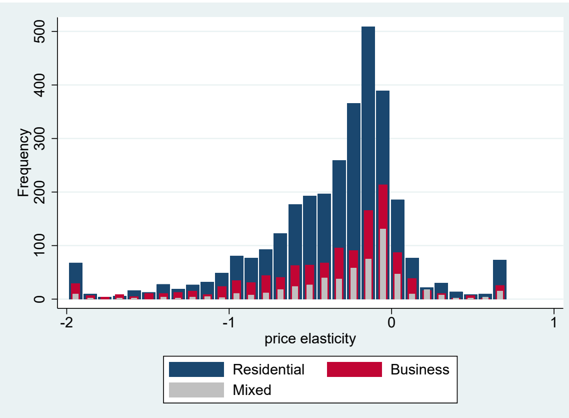
(b) Major energy sources



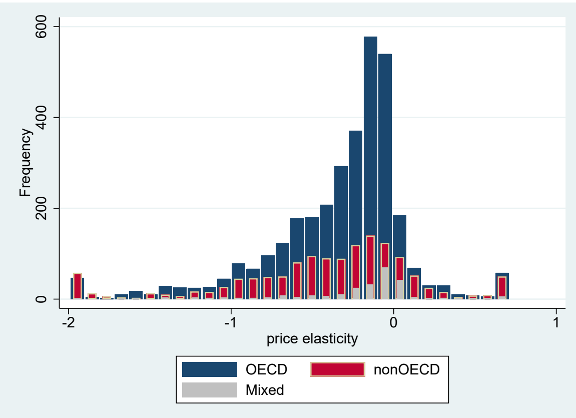
(c) Other energy sources



(d) Energy uses



(e) Sectors



(f) Country groups

Notes: The figure shows histograms of the price elasticities of demand for various subsamples based on study and estimation characteristics. Outliers are winsorized at the 2nd and 98th percentile.

A.4 Other reviews

Table A.2: Other systematic reviews on the price elasticity of energy demand

Study	Energy Source	Average Elasticity	
		Short Run	Long Run
Chai et al., 2018	Natural Gas	−0.718	0.281
Espey & Espey, 2004	Electricity	−0.35	−0.85
Labandeira et al., 2017	Electricity	−0.20	−0.51
	Natural Gas	−0.18	−0.57
	Heating Oil	−0.19	−0.54
	Diesel	−0.16	−0.39
	Gasoline	−0.20	−0.53
Havranek et al., 2012	Gasoline	−0.09	−0.31
Brons et al., 2008	Gasoline	−0.34	−0.84
Espey, 1998	Gasoline	−0.26	−0.58
Espey, 1996	Gasoline	−0.53	
Graham & Glaister, 2004	(car) Fuel	−0.25	−0.77
Goodwin et al., 2004	(vehicle) Fuel	−0.25	−0.64

Notes: The table summarizes the price elasticities of energy demand as found in other meta-analyses and systematic reviews.

A.5 AI summary

Prompt. We used the following prompt with GPT-4o mini via chatgpt.com on February 24, 2025: "Please provide an estimate of the price elasticity of demand for heating and cooling in buildings separately for short-run and long-run estimates. The estimate should take into account all available scientific studies, not just one prominent study. At the same time, the estimate should rigorously summarize the conventional wisdom in the literature. Answer like an economist and expert in this field. Provide the best possible point estimate of the effect together with the corresponding 95 percent confidence intervals."

Answer. "The price elasticity of demand for heating and cooling in buildings can vary significantly between the short run and the long run due to differences in consumer behavior and the ability to adjust to price changes over time. Based on a thorough review of the academic literature, we can provide separate estimates for both time frames.

Short-Run Estimates. In the short run, consumers have limited ability to adjust their heating and cooling usage in response to price changes. This is primarily due to the fixed nature of many heating and cooling systems and the immediate need for comfort. The literature generally suggests that the short-run price elasticity of demand for heating and cooling is relatively inelastic.

Long-Run Estimates. In the long run, consumers have more flexibility to adjust their energy consumption. They can invest in energy-efficient technologies, change their behavior, or even modify their living conditions (e.g., moving to different homes or retrofitting existing ones). As a result, the long-run price elasticity of demand for heating and cooling tends to be more elastic than in the short run.

- Point Estimate: Approximately -0.5 to -0.8
- Best Point Estimate: -0.6
- 95% Confidence Interval: $[-0.4, -0.8]$

Summary. In summary, the conventional wisdom in the literature indicates that the price elasticity of demand for heating and cooling in buildings is approximately:

- Short Run: -0.3 (95% CI: $[-0.1, -0.5]$)
- Long Run: -0.6 (95% CI: $[-0.4, -0.8]$)

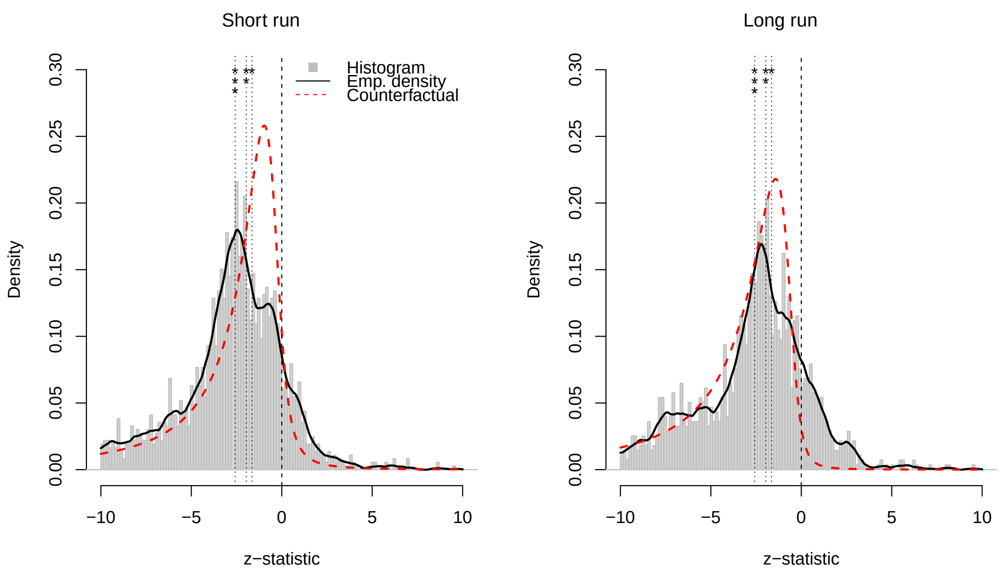
These estimates reflect the essential nature of heating and cooling services, with consumers showing a moderate response to price changes over the long term compared to the short term."

Appendix B Further details on publication bias

B.1 Robustness of density plots

This appendix provides further information on the z-statistic density plots developed by Brodeur et al. (2016), as discussed in Subsection 3.1. Figure B.1 considers the distribution of z-statistics if we do not winsorize point estimates and standard errors. The heaping just below standard significance thresholds is somewhat stronger in this case, but the qualitative results are unchanged.

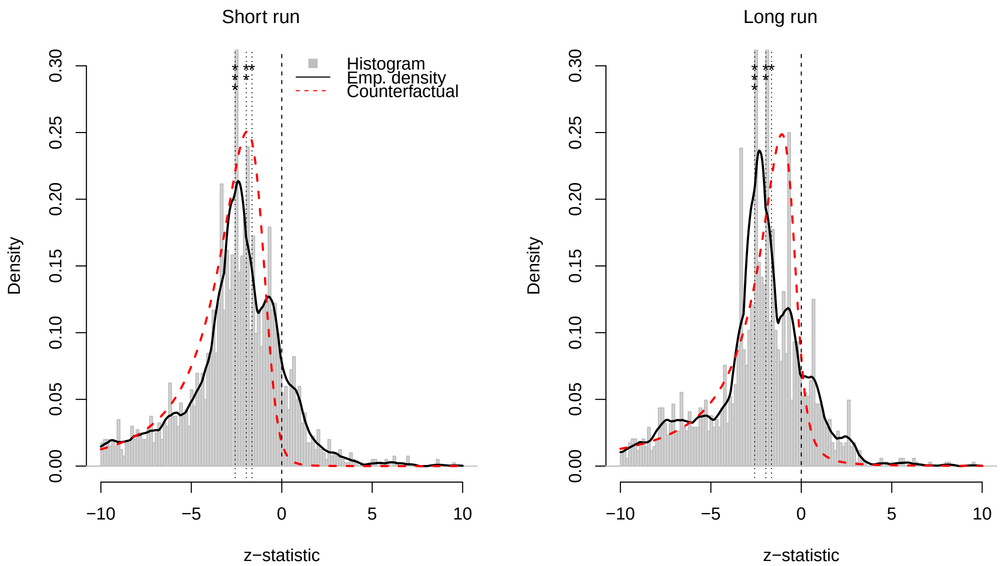
Figure B.1: z-stat densities with counterfactuals – without winsorization



Notes: The figures replicate Figure 1 when we do not winsorize the point estimates and standard errors in our sample.

Figure B.2 builds on Figure B.1 and additionally includes observations, whose standard errors were calculated based on significance thresholds. As expected, the heaping would be more extreme, but this is an artifact of the data collection with limited information on the exact inference statistics.

Figure B.2: z-stat densities with counterfactuals – without winsorization, including threshold-based precision measures

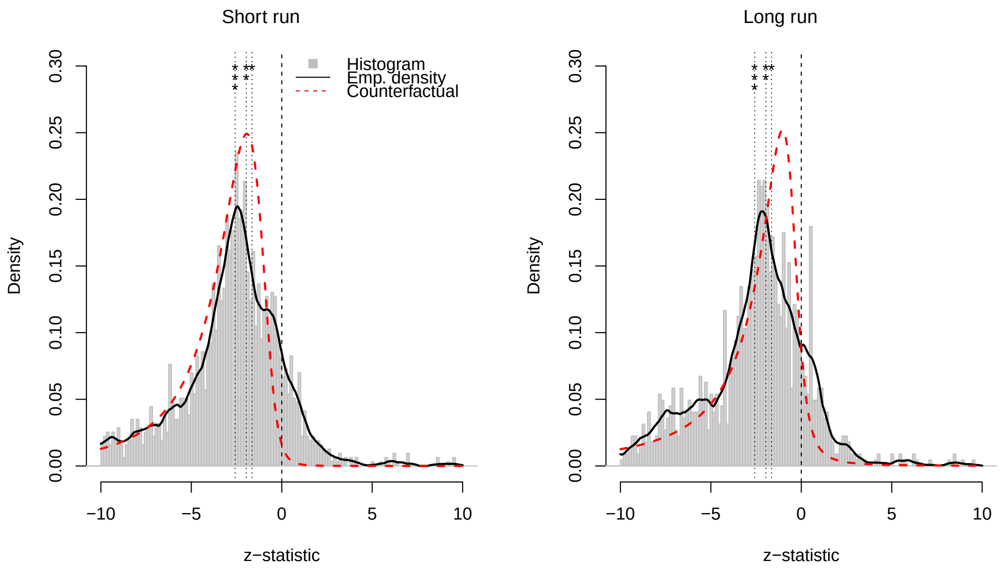


Notes: The figures replicate Figure 1 when we do not winsorize the point estimates and standard errors in our sample and if we don't exclude observations, whose standard errors were calculated based on significance thresholds (i.e. we only had information on significance stars and similar indicators.

Figure B.3 considers a sample excluding inferior estimates according to the judgment of primary study authors and estimates with quality concerns according to our assessment. The results are rather similar to the main sample.

Figure B.4 shows results of binomial proportion tests comparing the number of significant vs insignificant observations for wider or smaller symmetric intervals around the respective threshold. The shares of just significant and insignificant results should be similar in the absence of *p*-hacking. Clear signs of p-hacking can be observed for the popular 95 percent threshold for short-run estimates. This is also, where we would expect *p*-hacking to be most prevalent. Long-run estimates are often derived indirectly from dynamic estimation specifications, and statistical significance receives less attention.

Figure B.3: z-stat densities with counterfactuals – no inferior estimates, no quality concerns



Notes: The figures replicate Figure 1 for the sample excluding inferior estimates according to the judgment of primary study authors and estimates with quality concerns according to our assessment.

B.2 Conditional publication probabilities: details

This appendix provides additional information on the results from the publication bias test developed by Andrews and Kasy (2019), presented in Subsection 3.2. Table B.1 shows the precise numbers and inference statistics for the conditional publication probabilities shown in Figure 2.

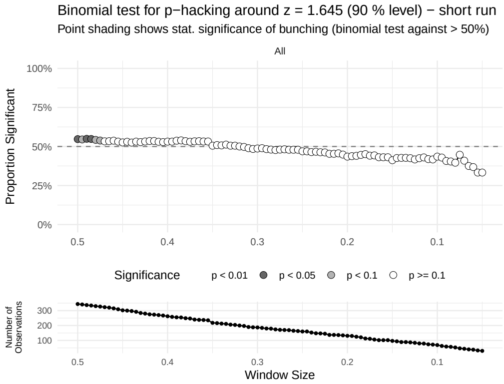
Figure B.5 replicates Figure 2 with more cutoffs specified at $z = 0$, $z = \pm 1.645$, $z = \pm 1.96$ and $z = \pm 2.576$. Table B.2 shows the related precise numbers and inference statistics.

B.3 Further information on funnel asymmetry and publication bias

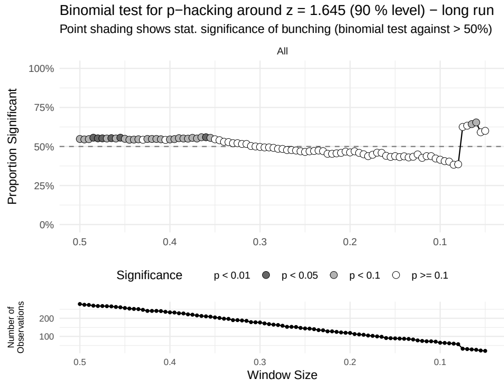
Figure B.6 replicates Figure 3 and highlights observations that the primary authors deemed inferior and quality concerns according to our assessment.

Table B.3 shows the robustness of our findings from the preferred PEESE specification in Table 2 with respect to different levels of winsorization. Note that the findings change strongly if we do not winsorize at all (column 1). This is due to the fact that the no-

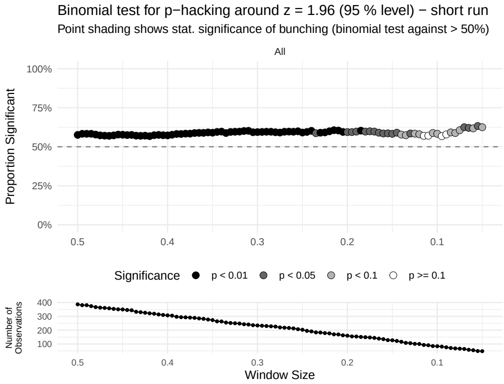
Figure B.4: Randomization tests at different significance thresholds



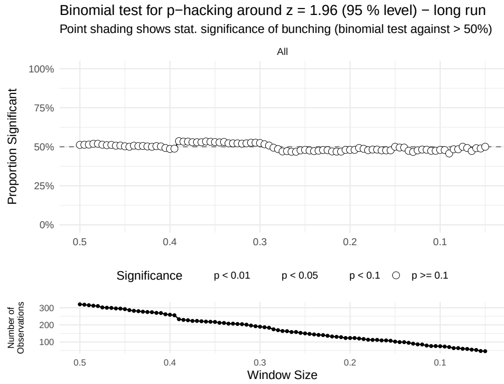
(a) Short run (90%)



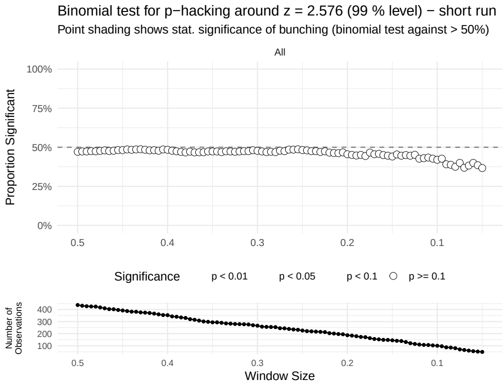
(b) Long run (90%)



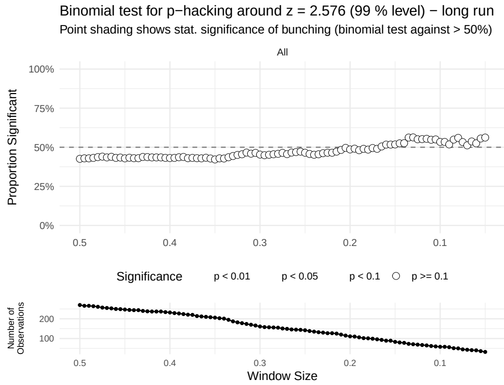
(c) Short run (95%)



(d) Long run (95%)



(e) Short run (99%)



(f) Long run (99%)

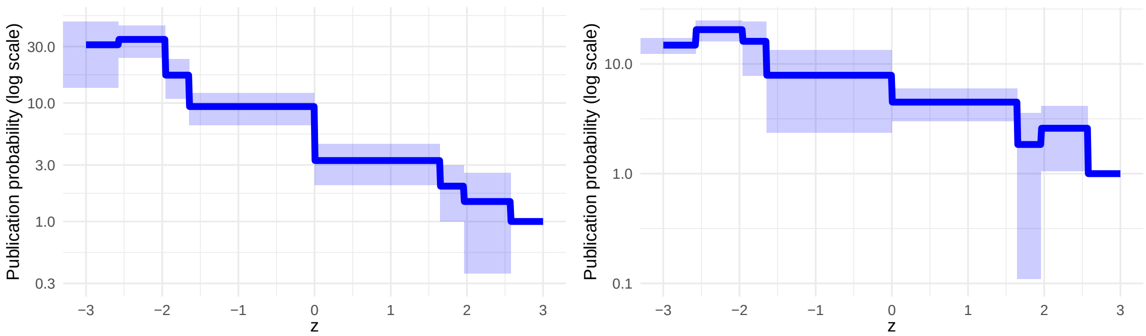
Notes: The figures provide formal tests for the visual inspection in Figure 1. They show results of binomial proportion tests comparing the number of significant vs insignificant observations for wider or smaller symmetric intervals around the respective threshold. The left column considers short-run estimates and the right column the long-run estimates. The rows refer to standard significance thresholds (90 percent, 95 percent, 99 percent). In each graph, the horizontal axis shows the size of the window around the significance thresholds, measured in z-values. The vertical axis measures the share of statistically significant observations according to the threshold. Thus, each dot shows the share of significant estimates in the respective window and the point shading signals whether this share is statistically significantly different from 50 percent. The smaller black dots in the little subfigures show the number of observations included in each window.

Table B.1: AK estimation; cutoff: $z = 1.96$

	Short run	Long run
mean beyond bias	0.028	-0.109
	(0.014)	(0.045)
$(-\infty, -1.96]$	23.614	9.988
	(0.961)	(1.022)
$(-1.96, 0]$	7.567	5.449
	(1.138)	(0.633)
$(0, 1.96]$	2.570	2.837
	(0.433)	(0.542)
Obs.	2437	1850

Notes: The table shows details of the test for conditional publication probabilities according to Andrews and Kasy (2019) separately for the short run and the long run estimates. It refers to Figure 2 in Subsection 3.2. Cutoffs are specified at $z = 0$ and $z = \pm 1.96$. The first row provides the estimate for the corrected effect. The other rows show the relative publication probabilities in comparison to the reference category of positive and statistically significant (at the 5 percent level, $z > 1.96$) estimates whose publication probability is normalized to 1. Clustered standard errors are given in parentheses.

Figure B.5: Conditional publication probabilities



(a) Short run

(b) Long run

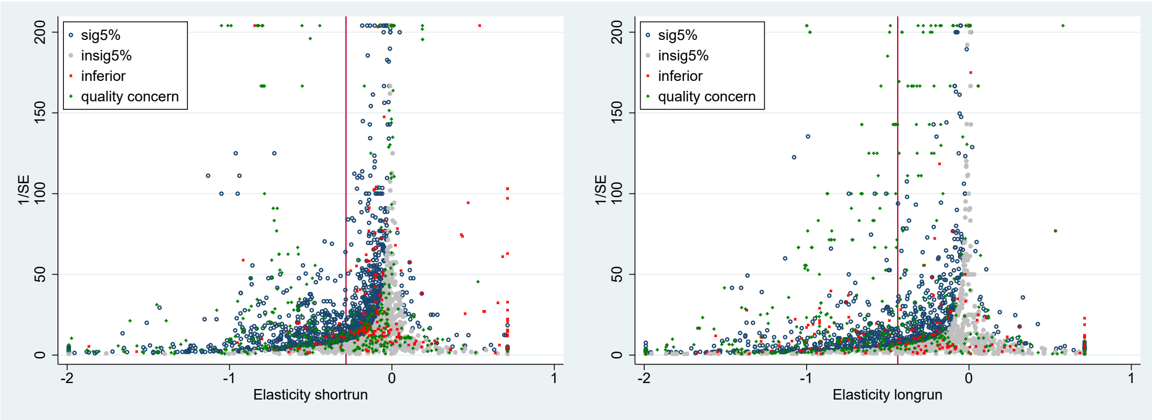
Notes: The figures show relative publication probabilities on the vertical axis in logarithmic scale for typical ranges of the z-statistics of short-run (left panel) and long-run (right panel) price elasticities of demand displayed on the horizontal axis. Cutoffs are specified at $z = 0$, $z = \pm 1.645$, $z = \pm 1.96$ and $z = \pm 2.576$. The publication probability of observations with $z > 2.576$ is normalized to 1. Observations where the standard error was calculated from thresholds are excluded. Shaded areas show 89% confidence bands. Table B.2 provides the precise numerical results and inference statistics. For further details see notes for Figure 2 and Subsection 3.2.

Table B.2: AK estimation; cutoffs: z =1.645, 1.96, 2.576

	Short run	Long run
mean beyond bias	0.049	-0.061
	(0.017)	(0.078)
$(-\infty, -2.576]$	31.038	14.828
	(1.909)	(0.889)
$(-2.576, -1.96]$	34.451	20.530
	(5.253)	(3.454)
$(-1.96, -1.645]$	17.202	16.072
	(3.450)	(2.340)
$(-1.645, 0]$	9.342	7.886
	(1.338)	(1.502)
$(0, 1.645]$	3.269	4.486
	(0.581)	(1.233)
$(1.645, 1.96]$	1.987	1.844
	(0.693)	(2.263)
$(1.96, 2.576]$	1.474	2.594
	(0.428)	(2.340)
Num.Obs.	2437	1850

Notes: The table shows details of the test for conditional publication probabilities according to Andrews and Kasy (2019) separately for the short run and the long run estimates. It refers to Figure B.5. Cutoffs are specified at $z = 0$, $z = \pm1.645$, $z = \pm1.96$ and $z = \pm2.576$. The first row provides the estimate for the corrected effect. The other rows show the relative publication probabilities in comparison to the reference category of positive and statistically significant (at the 1 percent level, $z > 2.576$) estimates whose publication probability is normalized to 1. Clustered standard errors are given in parentheses.

Figure B.6: Funnel plots: highlighted inferior estimates and quality concerns



(a) Short run

(b) Long run

Notes: The figures present funnel plots, which are scatter plots of the effect size on the horizontal axis and the precision of the estimate (the inverse of the standard error, $1/SE$) on the vertical axis. The left panel includes the short-run funnel, while the right panel shows a long-run funnel plot. The figures replicate Figure 3. The graph marks all statistically significant (at the 5% level) observations in blue rings and the statistically insignificant values as gray dots. Observations that were deemed inferior by the primary authors are marked as red 'x' and quality concerns by us are marked as green '+'. The red solid vertical lines represent the unweighted average elasticity of the short and long-term, respectively.

winsorization case features outliers with very high precision that would receive a large weight in the WLS estimation. Any other small or large winsorization produces coefficients that are close to the baseline case (2-98p).

Table B.4 and Table B.5 replicate Table 2 and Table 3 for the subsample without inferior estimates and quality concerns. The findings are qualitatively similar, but the corrected elasticities are even a bit closer to zero on average.

Figure B.7 shows the graphical output of the stem-based method, referred to in column (3) of Table 3.

Table B.3: PEESE with different winsorization levels

short run	0-100p	1-99p	2-98p	3-97p	4-96p	5-95p
β_1	-6.120	-2.426	-2.939	-3.339	-3.625	-3.960
pubbias	(2.656)	(1.171)	(1.047)	(1.117)	(1.051)	(1.022)
β_0	0.142	-0.145	-0.146	-0.151	-0.154	-0.155
mean	(0.148)	(0.0652)	(0.0492)	(0.0458)	(0.0399)	(0.0357)
Obs.	2680	2680	2680	2680	2680	
Adj. R^2	0.001	0.002	0.002	0.003	0.005	
long run	0-100p	1-99p	2-98p	3-97p	4-96p	5-95p
β_1	-0.354	-1.341	-1.639	-2.064	-2.210	-2.381
pubbias	(0.139)	(0.449)	(0.553)	(0.683)	(0.744)	(0.816)
β_0	-0.0126	-0.193	-0.231	-0.245	-0.262	-0.272
mean	(0.0122)	(0.0755)	(0.0673)	(0.0637)	(0.0606)	(0.0585)
Obs.	2294	2294	2294	2294	2294	
Adj. R^2	0.001	0.001	0.002	0.003	0.003	

Notes: The table replicates column (6) in Table 2 in Subsection 3.4 considering different levels of winsorization of the elasticity estimates and their squared standard errors from percentiles 0 and 100 to percentiles 5 and 95.

Table B.4: Linear publication bias tests: without inferior estimates and quality concerns

short run	(1)UA	(2)UWLS	(3)OLS	(4)FE	(5)PET	(6)PEESE	(7)Median
β_1			-0.981	-0.780	-2.530	-4.168	-3.622
pubbias			(0.181)	(0.153)	(0.375)	(0.752)	(0.576)
β_0	-0.285	-0.0960	-0.162	-0.187	-0.0605	-0.0938	-0.0470
mean	(0.0234)	(0.0228)	(0.0195)	(0.0190)	(0.0257)	(0.0230)	(0.0123)
Obs.	2113	2113	2113	2113	2113	2113	189
Adj. R^2	0.000	0.229	0.198	0.120	0.071	0.013	0.169
long run	(1)UA	(2)UWLS	(3)OLS	(4)FE	(5)PET	(6)PEESE	(7)Median
β_1			-0.460	-0.423	-2.366	-2.215	-1.782
pubbias			(0.183)	(0.185)	(0.326)	(0.450)	(1.084)
β_0	-0.400	-0.125	-0.298	-0.306	-0.0879	-0.123	-0.192
mean	(0.0349)	(0.0282)	(0.0289)	(0.0411)	(0.0287)	(0.0281)	(0.0340)
Obs.	1741	1741	1741	1741	1741	1741	160
Adj. R^2	0.000	0.227	0.064	0.056	0.070	0.010	0.010

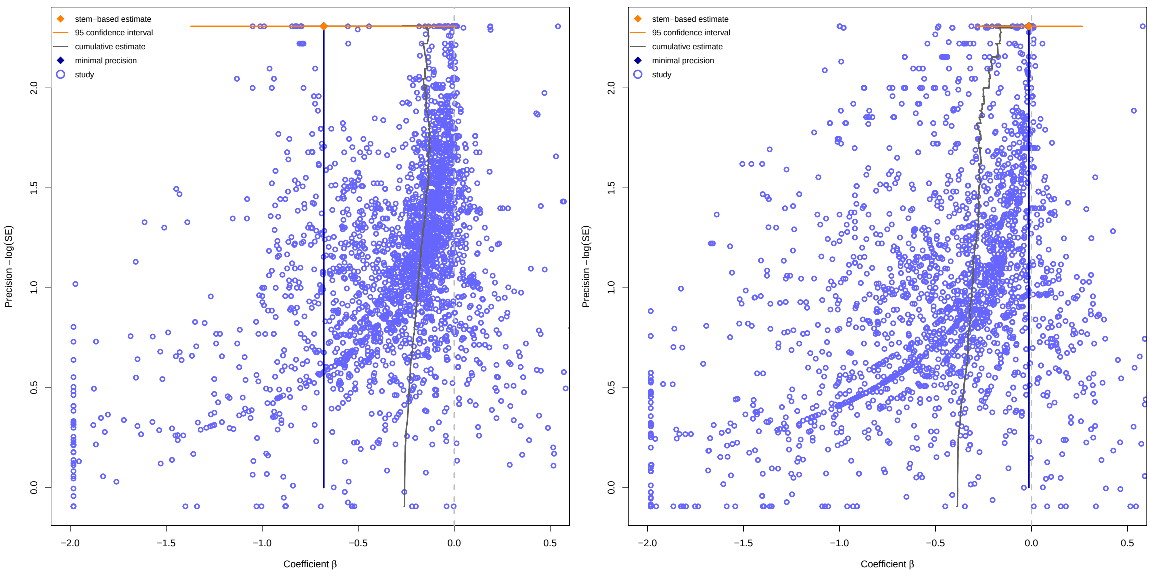
Notes: The table replicates Table 2 in Subsection 3.4 when excluding estimates that were deemed inferior by the primary study authors and those flagged with quality concerns by us.

Table B.5: Nonlinear underlying effects: without inferior estimates and quality concerns

short run	(1) AK	(2) WAAP	(3) Stem	(4) MAIVE
mean beyond bias	0.007	-0.0863	-0.055	-0.125
	0.020	(0.0241)	(0.09)	(0.0413)
Observations	1894	607	4	2022
1st stage F				11.39
long run	(1) AK	(2) WAAP	(3) Stem	(4) MAIVE
mean beyond bias	-0.080	-0.111	-0.016	-0.126
	0.030	(0.0282)	(0.0958)	(0.0395)
Obs.	1341	282	7	1670
1st stage F				13.81

Notes: The table replicates Table 3 in Subsection 3.4 when excluding estimates that were deemed inferior by the primary study authors and those flagged with quality concerns by us.

Figure B.7: Stem-based method



(a) Short run

(b) Long run

Notes: The figure shows the funnel plots and selection of the stem for the short and long-run estimates according to the method of Furukawa (2019). The resulting mean beyond bias is shown in Table 3 in Subsection 3.4.

Appendix C Further details on heterogeneity

C.1 Multivariate Meta Regression

This appendix provides further results of the multivariate meta-regressions in Subsection 4.2. Table C.1 shows the additional coefficients for columns (3) to (6) of Table 4.

Table C.2 uses the baseline selection of control variables and shows the findings for subsamples with different quality criteria. Column (1) excludes estimates that are either deemed inferior by the primary study authors or where we flagged a quality concern (due to inconsistent, incomplete or incomprehensible reporting, or suspicious research designs). Column (2) considers only those estimates that were marked as preferred by the authors of the primary studies. Column (3) looks at only those studies that control for income and prices of other energy sources, thus accounting for income and cross-price effects. Column (4) considers only those estimates that use a clear identification strategy, e.g. instrumentation, difference-in-differences, or quasi-experimental research designs.

C.2 Bayesian Model Averaging (BMA)

This appendix presents the results from the BMA as discussed in Subsection 4.3. Figure C.1 visualizes the inclusion probabilities of moderator variables and the sign of their coefficients for the universe of selected models in the BMA.

Table C.3 summarizes important statistics from the BMA for all moderator variables.

Table C.1: Multivariate meta regression, coefficients of further controls

	(3) data	(4) estim	(5) public	(6) all
sample avg.year	-0.00287			-0.00280
	(0.00224)			(0.00168)
macro data	0.159			0.128
	(0.0850)			(0.0612)
crossec	-0.0932			0.118
	(0.125)			(0.0992)
timeseries	-0.0353			-0.0468
	(0.0444)			(0.0386)
frequency >=Annual	-0.0884			-0.0788
	(0.0465)			(0.0402)
dynamic estimation		-0.00563		-0.00489
		(0.0378)		(0.0341)
system estimation		-0.218		-0.208
		(0.0618)		(0.0442)
identified		0.0532		0.0942
		(0.0381)		(0.0403)
income control		0.0725		0.0226
		(0.0510)		(0.0710)
cross-price control		-0.152		-0.167
		(0.0453)		(0.0410)
top journal			0.0128	-0.0502
			(0.0751)	(0.0578)
log citations			-0.00331	0.0182
			(0.0186)	(0.0148)
byproduct			0.173	0.128
			(0.0644)	(0.0418)
inferior			0.177	0.138
			(0.0823)	(0.0623)
prefer			0.0923	0.0618
			(0.0403)	(0.0441)

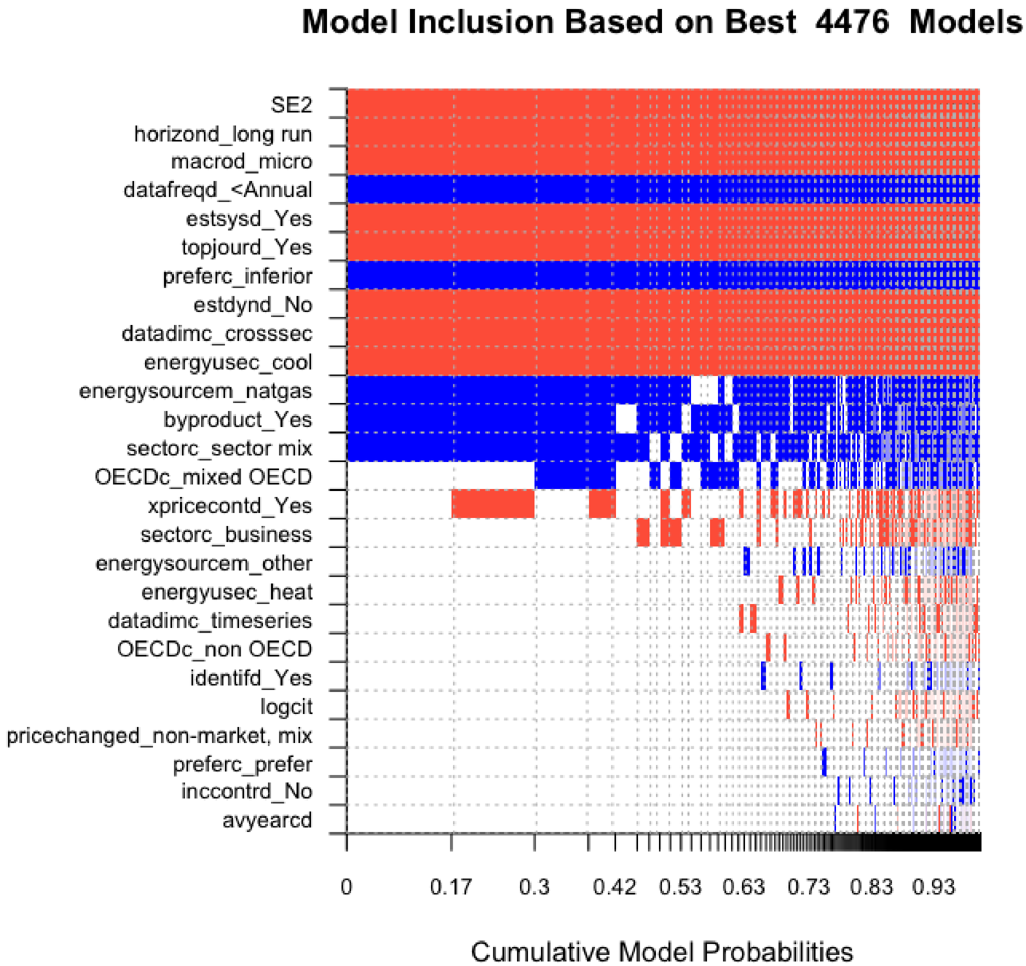
Notes: The table shows the additional coefficients for columns (3) to (6) of Table 4. Column (3) includes controls characterizing the data used in the primary studies (*micro* vs macro data; cross-sectional vs time series vs *panel* data; *higher* or lower than annual data frequency). Column (4) includes controls concerning estimation methods in the primary studies (dynamic estimation; system estimation; identification; controlling for income; controlling for other energy source prices). Column (5) includes controls related to publication details (top-journal publication vs *rest*; logarithm of number of citations; price elasticity is *main research question* or byproduct; primary authors judge estimate as preferred, *random* or inferior. The reference categories are given in italics. Standard errors in parentheses are clustered at the study level.

Table C.2: Multivariate meta regression, subsamples with quality criteria

	(1) quality	(2) prefer	(3) income-xprice	(4) identification
Constant	-0.0966	-0.0622	-0.250	-0.126
	(0.0294)	(0.0334)	(0.0681)	(0.0542)
SE^2	-2.604	-4.917	-2.330	-4.421
	(0.457)	(1.146)	(0.751)	(0.851)
long run	-0.0351	-0.111	-0.0140	-0.0133
	(0.0308)	(0.0707)	(0.0541)	(0.0358)
natural gas	-0.00776	0.0417	0.149	0.127
	(0.0519)	(0.0518)	(0.104)	(0.0470)
esource other	-0.0906	-0.0454	-0.274	0.0138
	(0.0642)	(0.0651)	(0.146)	(0.0695)
heating	-0.0259	-0.000110	0.106	-0.149
	(0.0474)	(0.0461)	(0.0913)	(0.0436)
cooling	-0.113	-0.140	0.0217	-0.0895
	(0.0439)	(0.0319)	(0.161)	(0.0173)
non-market, mix	-0.0117	-0.0294	-0.0469	0.0302
	(0.0274)	(0.0385)	(0.0350)	(0.0516)
non OECD	-0.0445	-0.00676	-0.193	-0.0268
	(0.0616)	(0.0868)	(0.169)	(0.0548)
mixed OECD	0.0504	0.0680	0.286	0.101
	(0.0332)	(0.0362)	(0.104)	(0.0342)
business	0.0619	0.0845	0.252	0.0976
	(0.0343)	(0.0862)	(0.0833)	(0.0528)
sector mix	0.108	0.109	0.258	-0.0917
	(0.0465)	(0.0819)	(0.0924)	(0.0531)
Obs.	3854	546	1480	1411
Adj. R^2	0.104	0.254	0.498	0.122

Notes: The table shows results of WLS regressions (with inverse variance weights) according to Eq. (2) with the PEESE correction for publication bias and including moderator variables in X. See Table 4 for description of variables. Column (1) excludes estimates that are either deemed inferior by the primary study authors or where we flagged a quality concern. Column (2) considers only those estimates that were marked as preferred by the authors of the primary studies. Column (3) looks at only those studies that control for income and prices of other energy sources, thus accounting for income and cross-price effects. Column (4) considers only those estimates that use a clear identification strategy, e.g. instrumentation, difference-in-differences, or quasi-experimental research designs. Standard errors in parentheses are clustered at the study level.

Figure C.1: BMA summary plot



Notes: The figure visualizes inclusion probabilities and signs of coefficients of the moderator variables in the selected models. The blue color corresponds to a positive BMA-average coefficient, red to a negative coefficient, and white to non-inclusion. The horizontal axis shows the best models, scaled by posterior model probability. The best model from the BMA is shown in the first column.

Table C.3: BMA results

	PIP	Post Mean	Post SD	Cond.Pos.Sign
SE^2	1.000	-0.393	0.025	0.000
horizond_long run	1.000	-0.122	0.013	0.000
macrod_micro	1.000	-0.109	0.019	0.000
datadimc_crosssec	1.000	-0.115	0.021	0.000
datafreqd_<Annual	1.000	0.094	0.016	1.000
estsysd_Yes	1.000	-0.122	0.018	0.000
topjourd_Yes	1.000	-0.156	0.016	0.000
preferc_inferior	1.000	0.145	0.024	1.000
estdynd_No	1.000	-0.085	0.015	0.000
energyusec_cool	1.000	-0.173	0.034	0.000
energysourcem_natgas	0.898	0.051	0.023	1.000
byproduct_Yes	0.869	0.057	0.028	1.000
sectorc_sector mix	0.844	0.067	0.036	1.000
OECDc_mixed OECD	0.389	0.033	0.046	1.000
xpricecontd_Yes	0.359	-0.013	0.019	0.000
sectorc_business	0.188	-0.007	0.017	0.000
energysourcem_other	0.077	0.003	0.010	1.000
energyusec_heat	0.056	-0.002	0.008	0.018
datadimc_timeseries	0.046	-0.001	0.006	0.000
OECDc_non OECD	0.041	-0.001	0.005	0.001
identifd_Yes	0.036	0.001	0.005	1.000
logcit	0.036	-0.000	0.001	0.000
pricechanged_non-market, mix	0.029	-0.000	0.003	0.000
preferc_prefer	0.024	0.000	0.004	1.000
inccontrd_No	0.022	0.000	0.003	1.000
avyearcd	0.019	0.000	0.000	0.574
(Intercept)	1.000	-0.166		

Notes: PIP shows the posterior inclusion probability for each variable. Post Mean shows the coefficients averaged across models, including models which excluded the variable (implying a zero coefficient). Cond.Pos.Sign indicates the sign certainty across models, 1 meaning a positive coefficient across all models and 0 implying a negative coefficient across all models.

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