

# Electricity demand has not become more price-responsive despite ninety years of technological change\*

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## Abstract

Energy planners have long assumed that electricity demand will grow more price-responsive as metering, automation, and storage spread, an assumption now embedded in decarbonization plans. We test it against the entire empirical record: 4,898 own-price elasticity estimates from 482 studies, with data spanning 1934–2024, ranked on a single ladder of identification quality from naive regressions to randomized experiments. Three findings emerge. First, better-identified studies find *smaller* responses: the corrected short-run elasticity is about  $-0.16$ , and only  $-0.09$  among the best-identified, design-based studies, whose adjusted value is statistically indistinguishable from zero. Second, responsiveness grows with time to adjust, roughly doubling from  $-0.16$  in the short run to  $-0.38$  in the long run as the capital stock turns over, but this pattern has itself been stable for decades. Third, and most important, responsiveness shows no upward trend across nine decades of data; if anything, the most technology-rich settings, including time-of-use pricing, are the *least* price-responsive in total consumption. Demand flexibility must be engineered and paid for; the historical record gives no reason to expect prices alone to deliver it.

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# 1 Introduction

Surveying, in 1981, the fifteen residential time-of-use pricing experiments the U.S. Department of Energy had run since 1975, Dennis Aigner recorded the profession’s expectation: with a full commitment to time-varying prices, “appliance choices will be made with an eye to TOU response; new appliances will become widely available,” and the pricing strategy “surely must be even more desirable in the long-run” (Aigner, 1985, p. 39). Four decades later, a recent working paper synthesizing time-based electricity rates opens with the same expectation, now attached to decarbonization: time-based rates “may be a useful and efficient tool for encouraging demand-side flexibility” in the transition to a high-renewable grid, and how the response will change “with widespread adoption of emerging technologies” remains, in the authors’ words, an open prediction problem (Kahn-Lang *et al.*, 2025). Between these two bookends, the assumption that price responsiveness of electricity demand is about to rise has underwritten smart-metering business cases (Faruqui & Sergici, 2010), demand-response program design (Torriti, 2014), and the demand-side assumptions of energy-system models (Huntington *et al.*, 2019). It is among the more consequential quantitative assumptions in energy policy: if demand will bend when prices ask it to, decarbonized power systems need less storage and less firm capacity; if it will not, plans built on flexible demand are underinsured.

The assumption is testable, and in the four decades since it was first recorded nobody has tested it head-on (Table 8 records it in the words of those who hold it). This paper brings the entire empirical record to bear on it: 4,898 own-price elasticity estimates from 482 studies, with underlying data spanning 1934–2024, to our knowledge the largest dataset assembled on the price elasticity of electricity demand, and one designed so that the question “has responsiveness changed?” can be separated from the two confounds that make a naive answer worthless.

The first confound is identification. Reported elasticities depend on how a study confronts the simultaneity of price and quantity: tariff schedules respond to consumption, regulators respond to demand growth, and average prices constructed from bills divide revenue by the quantity being explained (Alberini & Filippini, 2011; Ito, 2014). A literature whose identification standards improve over time will exhibit spurious elasticity “trends” that reflect method, not behavior. We therefore place every estimate on a single identification ladder (randomized experiments and mandated natural experiments at the top, difference-in-differences designs, in-

strumented specifications, panel fixed effects, and naive regressions below), in the spirit of the design-quality tiers that Kahn-Lang *et al.* (2025) apply to pricing pilots. The second confound is selective reporting. Statistically significant, correctly signed elasticities are easier to publish, which correlates estimates with their standard errors and inflates naive averages (Stanley *et al.*, 2008; Ioannidis *et al.*, 2017; Brodeur *et al.*, 2020); if selection pressure has itself drifted over time, so has the visible literature, independently of consumers. We therefore run modern selection corrections within tiers and horizons, and we separate two clocks that previous surveys usually conflated: the vintage of a study’s *data* (do consumers behave differently?) and the study’s *publication* date (does the literature report differently?).

Three findings follow, and they line up. First, *credibility shrinks the elasticity*. Moving up the ladder, the short-run elasticity falls in magnitude from  $-0.37$  (raw naive mean) to  $-0.09$  among design-based estimates (raw mean and PET intercept); and once observable study characteristics are held fixed, the design-based tier is statistically indistinguishable from zero ( $+0.02$ ,  $[-0.21, 0.25]$ ) and significantly less elastic than the naive baseline ( $p = 0.006$ ). Instrumented estimates, by contrast, are the one identification upgrade that remains as elastic as the naive literature ( $-0.19$  adjusted, against  $-0.24$  naive): instrumenting buys essentially none of the move toward the experimental benchmark that the rest of the ladder delivers. Selective reporting sharpens the divide: the observational literature shows pronounced funnel asymmetry (FAT =  $-0.85$ ,  $p < 0.001$ ), while the design-based sample shows none ( $p = 0.97$ ). The honest short-run consensus is small: bias-corrected estimates range from  $-0.04$  to  $-0.23$  across correction philosophies, with our preferred estimate at  $-0.16$ .

Second, *responsiveness grows with adjustment time, not with calendar time*. The bias-corrected elasticity roughly doubles from the short run ( $-0.16$ ) to the long run ( $-0.38$ ), a gradient consistent with the within-study adjustment path that Deryugina *et al.* (2020) recover from a municipal-aggregation natural experiment, where the elasticity triples from  $-0.09$  after six months to  $-0.27$  after two years, and with the mechanism that Costa & Kahn (2011) identify in the housing stock: the long-run response operates through durable capital, so it arrives with the turnover of buildings and appliances, and it plateaus at the substitution possibilities the stock allows. Crucially, the gradient itself is stable across eras of data: in every era since the mid-1970s the corrected long-run value has sat between  $-0.29$  and  $-0.47$ . The long run is not

a reservoir of future flexibility; it has already arrived, and it is  $-0.38$ , not  $-1$ .

Third, *the elasticity shows no trend in the data vintage*. In the raw record, in the bias-corrected record, and in the fully adjusted record (both clocks plus identification and composition moderators), the short-run trend per decade of data is statistically indistinguishable from zero, and in the specifications precise enough to be informative the 95% envelope admits at most two-thirds to four-fifths of the increase that a doubling of responsiveness, the canonical flexibility scenario, would require over three decades. The point estimate in the fully adjusted specification actually leans toward *less* elastic demand ( $+0.017$  per decade,  $p = 0.53$ ). Estimates from time-of-use settings, the environments the flexibility premise is most confident about, are closer to zero than the rest of the literature (coefficient  $+0.14$ ,  $p = 0.007$ ), and the design-based and time-of-use cells sit inside the pilot-study consensus of  $-0.02$  to  $-0.10$  reported by Faruqui & Sergici (2010) and the  $-0.075$  average of Kahn-Lang *et al.* (2025). The most technology-rich, best-identified corners of the record are the least price-responsive.

We are explicit about what this exercise is: a descriptive cross-study audit of a literature, anchored by design-based benchmarks and within-study contrasts where they exist, not a single design-identified causal parameter. That distinction is a feature of the design, not a hedge against it: no individual study spans ninety years or six identification tiers, so only a pooled audit can ask whether the corrections the literature trusts reproduce what clean designs deliver, and whether the parameter planners extrapolate has ever moved. Our contribution is therefore not one more summary elasticity but a credibility audit of the assumption that the summary is about to change, in the tradition of meta-research on selective reporting and the reliability of empirical economics (Ioannidis *et al.*, 2017; Brodeur *et al.*, 2020; DellaVigna & Linos, 2022; Christensen & Miguel, 2018).

Prior syntheses have touched the pieces separately, never the question. Espey & Espey (2004) include publication-year and data-vintage dummies in a 36-study sample and find drift; Labandeira *et al.* (2017) report, in a single sentence, that a technical-progress trend “is not significant at any level in any given specification”; Zabaloy & Viego (2022) find significant data-vintage drifts of opposite signs for short- and long-run elasticities in Latin America; Zhu *et al.* (2018) bin studies by period with no correction machinery; the surveys of Dahl (1993, 2011), Fatima (2023), and Marques *et al.* (2024) report static summaries; and an unpublished precursor

(Horacek, 2014) finds no significant time variation, without bias corrections or identification controls. None makes the time path its central question, none runs it with bias correction, identification controls, and clustered inference, and none but Espey & Espey (2004) separates the data clock from the publication clock, the separation that, on our corpus, can account for the contradictory findings of these syntheses.

The rest of the paper follows the argument. Section 2 describes the corpus and the three load-bearing variables. Section 3 climbs the identification ladder. Section 4 audits selective reporting along it. Section 5 estimates the adjustment-horizon path. Section 6 runs the calendar-time test and states the bound. Section 7 translates the results into the numbers planners and modelers should use. Section 8 concludes.

## 2 Data

**Corpus.** We collect estimates of the own-price elasticity of electricity demand together with their standard errors from published studies and working papers. The frozen corpus contains 4,898 estimates from 482 studies (listed in Appendix B); 4,720 estimates carry a usable standard error (estimates reported without any uncertainty metric are retained in the file but excluded from estimation; they are listed in the replication package). A small number of borderline rows carry audit flags in the corpus (notably: the construction-vintage elasticity of Costa & Kahn, 2011; a separate, author-disavowed wrong-signed estimate; and an aggregate event-study elasticity); they are retained, flagged, and individually immaterial at this sample size. Publication dates run from 1951 to 2026; the underlying data run from 1934 to 2024 (median data mid-year 1988). Each estimate carries 125 coded characteristics covering the data (country, sector, aggregation, frequency), the demand model (functional form, dynamics, controls), the price variable (average, marginal, time-of-use), the estimation method, and the publication outlet.<sup>1</sup>

Our baseline sample places all comparable own-price elasticities on a common Marshallian (uncompensated) footing: the 3,324 estimates from 366 studies (1,647 short-run, 954 intermediate-run, 723 long-run) that carry a usable standard error and are either reported

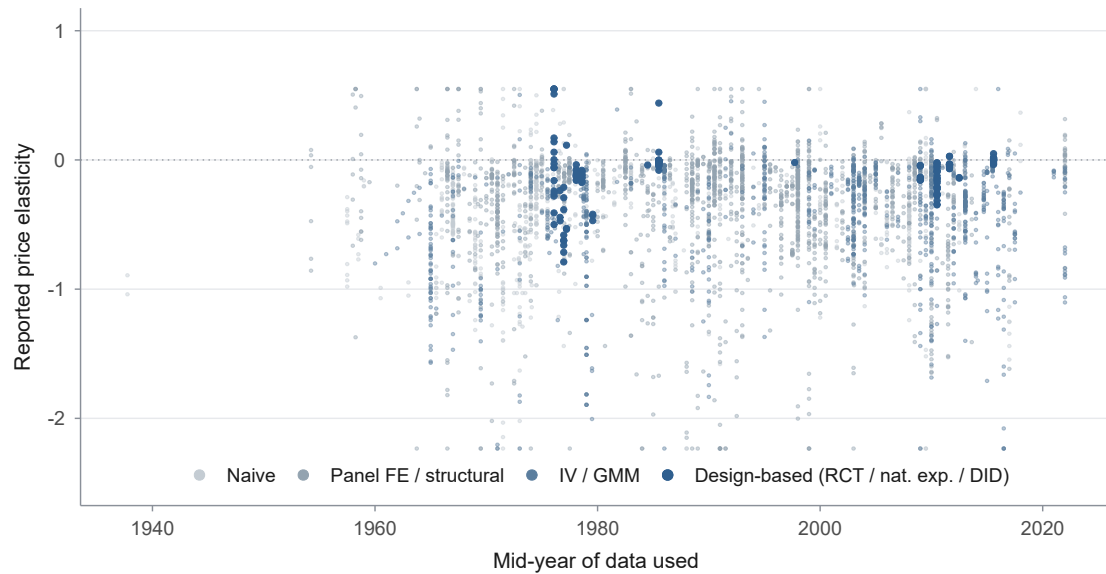
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<sup>1</sup>Country-level covariates are taken from external sources: GDP per capita (current US\$) and population from the World Bank's *World Development Indicators* (World Bank, 2025); surface temperature (degrees Celsius) from the Berkeley Earth record (Berkeley Earth, 2015); and the length of the longest day from the daylight calculator of Moesen (2010). Study-level bibliometrics: citation counts from Google Scholar and journal impact factors from the RePEc recursive ranking. The replication package's codebook maps every coded variable to its source.

directly as Marshallian or converted from a compensated (Hicksian) estimate. The conversion applies the Slutsky relation  $\varepsilon_M = \varepsilon_H - s\eta$  with electricity budget share  $s = 0.04$  (typical of household expenditure surveys) and each estimate’s own income elasticity at the matching horizon; we convert the 763 compensated estimates that report an income elasticity (745 of these carry a usable standard error and, with the 2,579 directly reported Marshallian estimates, make up the 3,324-estimate baseline) and leave the remainder in the corpus. Because  $s$  is small, the conversion shifts each estimate by roughly 0.01 and leaves standard errors essentially unchanged (the delta-method adjustment is negligible), so the directly reported and the converted estimates can be read on a single scale. Restricting to the 2,579 directly reported Marshallian estimates alone (the *pure-Marshallian* sample; Appendix D) leaves every headline result unchanged, as does varying  $s$  between 0.02 and 0.08. Indirectly derived (inverted-formula) estimates, and estimates whose only standard error is imputed from a reported significance level, are retained in the corpus but excluded from estimation (865 and 120 estimates respectively, 985 in total, since the two sets do not overlap). Effects and standard errors are winsorized at 1% throughout, and main-text inference clusters at the study level, with the model-averaging and full-battery appendices (Appendix F, Appendix E) additionally clustering two-way, by study and by underlying database. At the extremes of no winsorization and 5%, the bias-corrected short-run estimate is  $-0.1696$  and  $-0.1683$ , the long-run estimate  $-0.4639$  and  $-0.3572$ , so the 1% choice drives none of the headline conclusions.

**The identification ladder.** The load-bearing variable is the *price-identification strategy* of each estimate, coded on a six-tier ladder: (1) randomized experiments and randomized encouragement designs; (2) mandated natural experiments with exogenous price variation; (3) difference-in-differences designs; (4) instrumented specifications (IV, 2SLS/3SLS, GMM); (5) panel fixed effects and structural demand systems without instrumentation; (6) naive time-series or cross-section regressions with no attempt to address price endogeneity. Coding rules and study-level examples appear in Appendix C. Because tiers 1–3 are thin in this literature (136, 10, and 26 estimates respectively) we group them as *design-based* throughout the main text; 878 estimates whose strategy cannot be classified from the primary study are excluded from ladder analyses and retained elsewhere. In the coded corpus, sixteen studies report estimates on more than one tier. A pre-freeze audit of the primary papers identified several dozen

Figure 1: The corpus: nine decades of elasticity estimates



*Notes:* Reported own-price elasticities (winsorized at 1%) against the mid-year of the underlying data, colored by identification tier; the 3,843 tier-classified estimates with usable standard errors are shown (unclassified estimates are omitted from the plot but not from non-ladder analyses). Two features organize the paper: the raw record shows no visible drift over nine decades, and the design-based literature (dark blue) arrives late and thin (8 studies contribute short-run design-based estimates), so composition must be held fixed before any trend is read as behavior.

further studies whose lower-tier benchmark estimates exist in print but were never coded; those estimates lie outside the frozen corpus and would substantially enlarge the within-study sample.

**Adjustment horizon and the two clocks.** Each estimate is classified as short-run (within roughly a year), intermediate-run, or long-run, using the primary study’s own labels where given and the model structure otherwise (static single-equation estimates on levels with cointegration pre-tests are long-run; dynamic models contribute both a short- and a long-run estimate). We treat the horizon as an economic object, not a nuisance: Section 5 estimates the elasticity as a function of it.

Finally, every estimate carries two dates: the mid-year of the data used (the *data clock*; 23 estimates with no reported data years are assigned publication year minus three) and the publication year (the *publication clock*). Consumer behavior can only move the first; publication practices, methods fashion, and selection pressure move the second. Most previous surveys used one or the other; Section 6 uses both at once.

Table 1: Short-run price elasticity by identification tier

Tier	$N$	Studies	Unadjusted	Adjusted	95% CI
Design-based (RCT/nat. exp./DID)	81	8	-0.091	0.022	[-0.21, 0.25]
Instrumented (IV/GMM)	549	52	-0.304	-0.195	[-0.35, -0.04]
Panel FE / structural	655	98	-0.209	-0.126	[-0.28, 0.03]
Naive	316	70	-0.365	-0.238	[-0.37, -0.11]

*Notes:* Headline sample (Marshallian-equivalent, usable SE), short run; 1,601 estimates from 220 studies enter the adjusted regression (tier-classified subset). Unadjusted is the raw mean within the tier. Adjusted is the conditional mean from one meta-regression of the elasticity on its standard error, tier indicators (naive omitted), and study characteristics, evaluated at SE = 0 and characteristics at sample means; 95% study-clustered intervals. Relative to naive, the design-based shift is +0.260 ( $p = 0.006$ ), the fixed-effects shift +0.112 ( $p = 0.003$ ), the IV shift +0.043 ( $p = 0.32$ ).

### 3 The identification ladder

Does the elasticity survive scrutiny of its identification? Table 1 lines the short-run literature up along the ladder. The unadjusted column reports the raw mean within each tier; the adjusted column reports the conditional mean from a single precision-controlling meta-regression: elasticity on its standard error (absorbing small-study effects), tier indicators, and a common set of study characteristics (sector, price measurement, data structure, region, demand controls, functional form), evaluated at a zero standard error with other characteristics at sample means, with study-clustered 95% intervals. This is a descriptive cross-study association: which design a study uses is not randomly assigned. We state it narrowly and let the within-study evidence and the design-based benchmark carry the causal weight, noting that the within-study contrasts speak only to the IV and fixed-effects ranking (no within-study pair contrasts a design-based estimate), so the design-based benchmark itself rests on the cross-study comparison.

The pattern is a gradient with one deliberate exception. Raw means fall in magnitude from -0.37 (naive) to -0.09 (design-based); adjusted, the design-based tier sits at +0.02 with an interval of [-0.21, 0.25], and is significantly less elastic than the naive baseline; the width of that interval means the design-based cell bounds the anchor rather than pinning its level, so we read it as consistent with a small elasticity while unable to exclude a moderate one. In the menu (Table 5), -0.09 is the raw and PET value and +0.02 the composition-adjusted prediction. The exception is instrumenting: the IV tier is the only identification upgrade that stays as elastic as the naive literature after adjustment (-0.19 against -0.24 naive), statistically indistinguishable from naive and overshooting the design-based benchmark. Within the sixteen studies that

report estimates on more than one tier, the same picture appears in miniature: the within-study contrast between the cleaner and the lower tier averages  $-0.08$  (cleaner estimates *more* elastic, driven by IV-versus-naive pairs), but it is small relative to its dispersion ( $t = -0.79$ ) and positive in half the studies. We read the within-study evidence as underpowered and report it for completeness rather than leaning on it (Figure 7 in Appendix D plots every contrast); none of these within-study contrasts involves a design-based estimate, so the within-study evidence bears on the IV and fixed-effects ranking rather than on the design-based benchmark, and the cross-study ladder and the funnel evidence of the next section carry the argument.

Two readings of the IV overshoot are possible, and the data cannot separate them here: instruments may correct attenuation from classical average-price measurement error (Alberini & Filippini, 2011), legitimately raising magnitudes; or weak instruments and specification search may inflate them. Ito (2014) shows the average-price coefficient is itself the behaviorally relevant object rather than an attenuated one, which cuts against the attenuation-correction reading. What the ladder does establish is the direction that matters for the flexibility debate: *no identification upgrade makes demand look more price-responsive than the naive literature already claims*: the best designs point toward less responsiveness, not more.

The measurement of price brackets the ladder from a second direction. Average-price estimates carry classical division bias and inflated standard errors; marginal-price estimates are cleaner but scarcer. Bias-corrected short-run elasticities are  $-0.15$  (marginal price) and  $-0.17$  (average price): the correction for measurement regime moves the consensus by two hundredths, not by the multiples the flexibility premise would need.

## 4 Selective reporting along the ladder

A meta-analysis that ignores selective reporting mistakes the literature’s preferences for the world’s parameters (Stanley *et al.*, 2008; Ioannidis *et al.*, 2017). The concern is concrete here: correctly signed, significant elasticities are easier to publish, which leaves a correlation between estimates and standard errors that funnel-based estimators detect and net out.

Table 2 reports the battery. Three results matter. First, selection is real and it inflates: the short-run funnel-asymmetry test rejects strongly ( $FAT = -0.85$ ,  $p < 0.001$ ), and every corrector pulls the raw mean of  $-0.27$  toward zero, to  $-0.16$  (PET),  $-0.23$  (PEESE),  $-0.11$  (WAAP),

Table 2: Publication-bias battery by horizon and tier

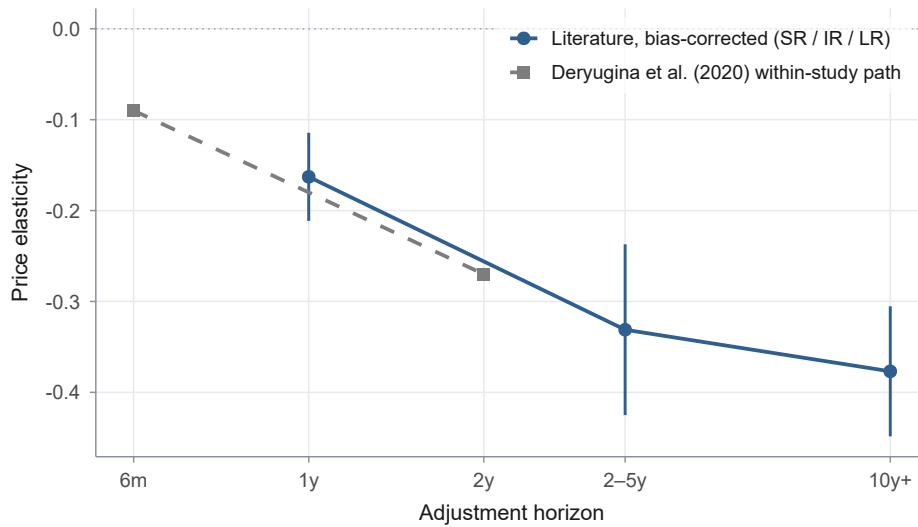
Sample	$N$	Studies	Mean	PW	PET	PEESE	WAAP
Long-run elasticities (full sample)	723	151	-0.583	-0.378	-0.377	-0.498	-0.376
Intermediate elasticities (full sample)	954	108	-0.440	-0.149	-0.331	-0.416	-0.148
Short-run elasticities (full sample)	1,647	226	-0.267	-0.114	-0.163	-0.231	-0.111
Short-run, design-based	81	8	-0.091	-0.078	-0.095	-0.114	-0.080
Short-run, instrumented (IV)	549	52	-0.304	-0.104	-0.203	-0.276	-0.102
Short-run, panel FE / structural	655	98	-0.209	-0.150	-0.088	-0.164	-0.151
Short-run, naive	316	70	-0.365	-0.108	-0.245	-0.318	-0.106
Short-run, marginal price	401	48	-0.301	-0.054	-0.148	-0.230	-0.049
Short-run, average price	1,030	166	-0.264	-0.107	-0.168	-0.236	-0.105
Short-run, preferred only	426	191	-0.302	-0.159	-0.182	-0.271	-0.158

*Notes:* Headline sample; the full-sample rows pool all estimates at each horizon, below which the short-run sample is split by tier and by price regime. Mean is the raw mean; PW the precision-weighted mean; PET and PEESE the funnel-asymmetry-corrected estimates (Stanley, 2005; Stanley *et al.*, 2008); WAAP the weighted average of adequately powered estimates (Ioannidis *et al.*, 2017). Study-clustered inference. FAT  $p$ -values for the asymmetry test: short-run all  $< 0.001$ ; design-based 0.97; marginal-price subsample 0.001. “Preferred only” uses the estimates flagged as preferred by the original studies (426 estimates from 191 studies). The sample-size-weighted corrector on the short-run full sample (1,647 estimates with a usable weight) gives  $-0.04$  (se 0.032), the range’s low end.

$-0.10$  (top decile by precision),  $-0.04$  (se 0.032; 1,647 estimates; sample-size weighted). We report the range rather than adjudicating among correction philosophies; the corrected short-run consensus lies between  $-0.04$  and  $-0.23$ , and nowhere near the naive mean. Second, it is the *observational* literature that is selected: the design-based sample shows no funnel asymmetry (FAT  $p = 0.97$ ), though with 81 estimates from 8 studies the test is under-powered, so selection can be neither detected nor excluded there; we rest the design-based benchmark on design credibility rather than on this null FAT. The slope itself is small ( $\gamma = 0.029$ ) and stays insignificant under few-cluster-robust inference (wild cluster bootstrap  $p = 0.918$  Rademacher,  $p = 0.941$  Webb; CR3-Satterthwaite  $p = 0.991$ ), so the null is not an artifact of the asymptotic approximation with only 8 clusters. Leave-one-study-out sharpens this caveat rather than resolving it: dropping any one of the 8 studies moves the FAT slope as low as  $-1.96$  and the  $p$ -value as high as 0.99, so the design-based null is a property of the full 8-study cell, not a result that survives study-by-study scrutiny. Third, the asymmetry is not an artifact of constructed standard errors or average-price measurement error: it survives, essentially undiminished, in the marginal-price subsample (FAT =  $-0.97$ ,  $p = 0.001$ ), which is the diagnostic that separates genuine selection from errors-in-variables mechanics.

Because the correctors themselves lean on the standard errors the selection story indicts, we triangulate with weights that never touch them: weighting by sample size yields a corrected

Figure 2: The adjustment path: elasticity by horizon, with the quasi-experimental benchmark



*Notes:* Bias-corrected (PET) elasticity by horizon on the headline sample, with study-clustered 95% intervals: short run  $-0.163$  (se  $0.025$ ; 1,647 estimates, 226 studies), intermediate run  $-0.331$  (se  $0.048$ ; 954/108), long run  $-0.377$  (se  $0.037$ ; 723/151). Dashed: the within-study adjustment path of Deryugina *et al.* (2020),  $-0.09$  at six months to  $-0.27$  at two years. Horizon placement of the literature bins is notational (SR  $\approx$  within a year; IR 2–5 years; LR beyond).

short-run elasticity of  $-0.04$  (se  $0.032$ ; 1,647 estimates), statistically indistinguishable from zero and the least elastic value in the battery. Restricting to the estimates the original studies flag as preferred (426 estimates from 191 studies) leaves the picture unchanged (PET  $-0.18$ ), as does the long run on the same restriction (251 estimates from 137 studies; PET  $-0.36$ ), and the calendar-time trend on the preferred-only short-run sample stays indistinguishable from zero ( $-0.056$  per decade,  $p = 0.084$ ), if anything more negative than the pooled headline. Every road leads to the same place: *the short-run price elasticity of electricity demand, honestly measured, is small*, and the corrections that economists use to strip the literature’s biases make it smaller, not larger.

## 5 Responsiveness grows with adjustment time

If short-run responsiveness is small, the flexibility premise retreats to the long run: given time, capital adjusts. The record supports the mechanism, and prices it.

Figure 2 plots the bias-corrected elasticity by horizon:  $-0.16$  in the short run,  $-0.33$  in the intermediate run,  $-0.38$  in the long run: the response roughly doubles as adjustment time accumulates and then plateaus. This is not an artifact of which studies happen to populate

which horizon bin: among the 100 studies that report both a short- and a long-run estimate, the same-study contrast goes from  $-0.238$  to  $-0.593$ , a within-study ratio of means of 2.492 (median study-level ratio 2.571), with 86.0% of studies showing a more elastic long run and a paired  $t$  of  $-8.68$ . A within-study fixed-effects regression on the 1,241 estimates from the 112 studies reporting more than one horizon, with study fixed effects absorbing every cross-study difference in composition, puts the same steepening at  $-0.12$  from short to intermediate run (se 0.065) and  $-0.22$  from short to long run (se 0.028): composition does not drive the gradient. Two study-level anchors discipline the shape. Within a single natural experiment, Deryugina *et al.* (2020) track the same set of over 250 municipally-aggregated communities in a difference-in-differences design and find the elasticity tripling from  $-0.09$  at six months to  $-0.27$  after two years: the literature-wide gradient is directionally consistent with what this dynamic design delivers, the one place where this literature and the credibility revolution agree. Because this study also supplies the short-run design-based cell, it anchors the horizon path but is not independent corroboration of both findings. And the plateau has a mechanism: Costa & Kahn (2011) show that electricity consumption is embodied in the housing stock at construction (an implied elasticity of about  $-0.22$  at construction time), so the long-run response arrives with the turnover of buildings and appliances and stops at the substitution possibilities the stock allows. Descriptive dynamics in the literature agree: the median adjustment speed among dynamic models implies half of the long-run response within roughly a year and a half and ninety percent within five and a half years (Dahl, 2011).

The policy-relevant corollary is the last row of the argument: *the long run is already in the data*. A planner invoking “the long run” is invoking a central estimate of  $-0.38$ , not  $-1$ ; we note this long-run value rests on the observational correctors rather than on a design-based benchmark (the design-based long-run cell is a single study, too thin to report), and that the within-study long-run projection of Deryugina *et al.* (2020) carries a 95% interval reaching  $-1$ , so  $-0.38$  is a central estimate rather than a design-audited bound. The gradient itself is stable across eras: within every data-vintage era from the 1970s to the 2010s, the long-run corrected elasticity sits between  $-0.29$  and  $-0.47$  (Table 3). In the pooled record, adjustment time roughly doubles responsiveness (era-by-era the gradient varies with the short-run dips of the 1980s but shows no tendency to steepen toward the present): there is no era in which adjustment time

Table 3: The horizon gradient is stable across eras of data

Horizon	$\leq 1975$	1976–90	1991–05	$\geq 2006$
Short run	−0.179 (0.038)	−0.090 (0.041)	−0.158 (0.034)	−0.223 (0.043)
Intermediate run	−0.418 (0.080)	−0.256 (0.081)	−0.258 (0.060)	−0.241 (0.112)
Long run	–	−0.429 (0.057)	−0.287 (0.046)	−0.472 (0.055)

*Notes:* Bias-corrected (PET) elasticity within horizon  $\times$  era cells, headline sample, study-clustered standard errors in parentheses. The long-run cell before 1976 has 51 estimates from 12 studies but is imprecisely estimated (a wide interval) and is suppressed. The intermediate run varies non-monotonically across eras; the short- and long-run values in the most recent era are not statistically distinguishable from the earliest era *shown* (for the long run this baseline is 1976–90, because the  $\leq 1975$  long-run cell is suppressed as imprecise), and no horizon exhibits a monotone rise toward the present. These era comparisons are imprecise and should be read as a limit on power rather than as evidence of exact stability.

began buying more response than it always had.

## 6 The calendar-time test

Sections 3–5 closed the escape routes: the true level is not secretly larger (the ladder), selection does not hide growth (the battery), and the long run is not still coming (the path). What remains is the premise’s core empirical claim, that responsiveness has been rising as technology diffuses, and it is directly testable on the data clock.

Table 4 reports the trend under progressively stricter specifications; Figure 3 shows the decade-by-decade profile. The answer does not depend on the machinery. In the raw record, the short-run trend is  $-0.010$  per decade of data ( $p = 0.43$ ). Controlling for precision,  $-0.018$  ( $p = 0.17$ ). Separating the two clocks leaves neither significant: the data clock is  $-0.033$  ( $p = 0.27$ ) and the publication clock  $+0.019$  ( $p = 0.57$ ); jointly, the two calendar coefficients are indistinguishable from zero as well (Wald  $\chi^2_2 = 2.18$ ,  $p = 0.34$ ), so the modest apparent drift that earlier surveys read as “elasticities changing over time” (Espey & Espey, 2004; Zabaloy & Viego, 2022) splits into two pieces, each indistinguishable from zero, rather than a behavioral trend in consumers. Holding identification and composition fixed, the data-clock point estimate turns positive:  $+0.017$  per decade ( $p = 0.53$ ),<sup>2</sup> if anything, demand drifting toward *less* price-

<sup>2</sup>Entering the controls one block at a time (data clock alone, then the publication clock, then the identification tier, then study composition) confirms the point estimate:  $+0.017$  ( $p = 0.53$ ) at the full, composition-adjusted specification, identical to the headline.

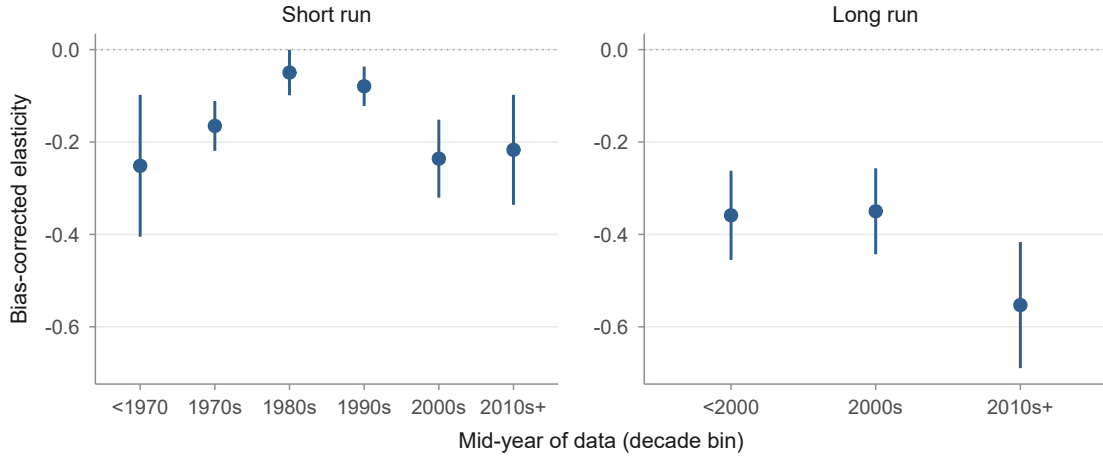
Table 4: Two clocks: the trend in the elasticity per decade of data and of publication

	Data clock	Publication clock
<i>Short run</i>		
Data clock only	-0.018 (0.013)	-
Two clocks	-0.033 (0.031)	0.019 (0.033)
Two clocks + composition	0.017 (0.027)	-0.030 (0.028)
<i>Intermediate run</i>		
Data clock only	0.012 (0.028)	-
Two clocks	0.000 (0.077)	0.012 (0.065)
Two clocks + composition	0.043 (0.064)	0.030 (0.057)
<i>Long run</i>		
Data clock only	0.011 (0.036)	-
Two clocks	-0.092 (0.048)	0.135 (0.047)
Two clocks + composition	-0.019 (0.053)	0.073 (0.050)

*Notes:* Each cell is a coefficient per decade from a study-clustered meta-regression that always controls for the standard error. The *Data clock* column is the trend in the elasticity per decade of the underlying *data* (do consumers behave differently over time?); the *Publication clock* column is the trend per decade of *publication* year (does the literature report differently over time?). The three rows are nested specifications: *Data clock only* includes just the data clock; *Two clocks* adds the publication clock, so the two are separated; *Two clocks + composition* further adds the identification tier and study characteristics of Table 1, that is, the changing *composition* of the literature, leaving any residual data-vintage trend. This specification runs on the full short-run sample rather than Table 1's 1,601-estimate tier-classified subset; estimates whose strategy could not be classified keep their identification tier at zero, alongside naive, instead of being dropped. Short-run sample: 1,647 estimates, 226 studies, data span 63 years; intermediate run: 954 estimates, 108 studies; long-run: 723 estimates, 151 studies, 54 years. A negative coefficient means the literature moves toward more elastic (more negative) values. No linear data-clock coefficient is significant at 5% in any specification. Quadratic specifications yield a significant squared term in the long run ( $p < 0.001$ ) but not the short run (squared  $p = 0.06$ ); we therefore label the long-run trend inconclusive rather than flat. The short-run quadratic does carry a significant linear term ( $-0.030$  per decade,  $p = 0.03$ ), but every linear short-run specification above is insignificant, consistent with the non-monotone mid-decade swings of Figure 3 rather than a trend.

responsiveness as the credibility revolution is netted out. We emphasize the sign convention: a rising-flexibility world requires this coefficient to be robustly *negative*. The pooled coefficient could still mask a rise concentrated in the settings the flexibility premise names, so we interact the data clock with sector and region and re-estimate it within each: none is robustly negative. The residential interaction is not distinguishable from the pooled trend ( $-0.021$  per decade,  $p = 0.30$ ), and the composition-adjusted data-clock slope is negative but insignificant in Europe ( $-0.067$ ,  $p = 0.18$ ), in post-2000 data ( $-0.037$ ,  $p = 0.60$ ), and in the residential-and-post-2000 intersection the premise names directly ( $-0.079$ ,  $p = 0.35$ ); the one subsample where the slope is significant, the United States ( $+0.099$ ,  $p = 0.002$ ), carries the wrong sign for a rising-flexibility

Figure 3: Bias-corrected elasticity by decade of data



*Notes:* PET-corrected elasticity within decade bins of the data mid-year, headline sample, 95% study-clustered intervals. Short run:  $-0.251$  (<1970),  $-0.165$  (1970s),  $-0.050$  (1980s),  $-0.079$  (1990s),  $-0.236$  (2000s),  $-0.217$  (2010s+). The 2010s+ value is statistically indistinguishable from the 1970s value; the series swings across the middle decades (the 1980s and 2000s intervals do not overlap), and the most recent bin is statistically indistinguishable from the 1970s bin. Long run:  $-0.36$ ,  $-0.35$ ,  $-0.55$  across coarse bins, the last on 9 studies.

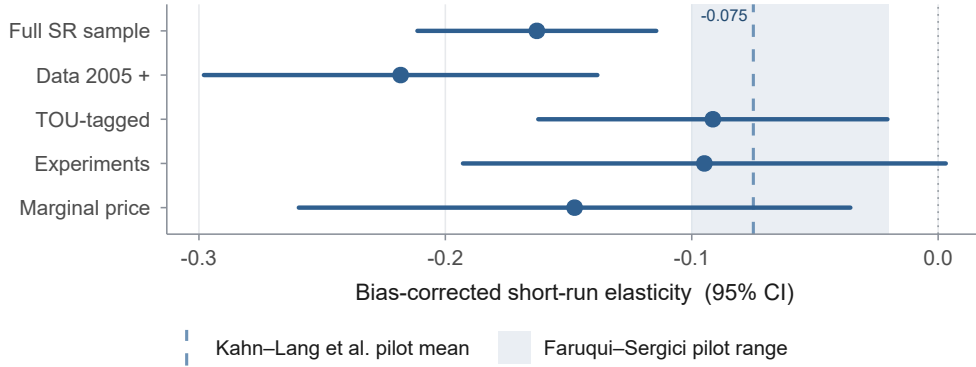
world. We detect no rise in the settings that matter most to the premise.

**The bound.** Statistical insignificance is not the finding; the width of the envelope is. Take the canonical flexibility scenario: short-run responsiveness roughly doubles over three decades, from  $-0.16$  to  $-0.33$ , an increase of about  $0.16$  in magnitude. Specification by specification, the edge of the 95% interval most favorable to that scenario allows a cumulative three-decade increase of  $0.10$  (raw record, 64% of the required movement),  $0.13$  (precision-controlled bivariate, 80%), and  $0.11$  (two clocks plus composition, 66%). The one exception proves the rule: splitting the two clocks *without* composition controls inflates the data-clock standard error enough (the two clocks are collinear) that its envelope alone could accommodate a doubling, which is precisely why the composition-adjusted specification, not the unrestricted split, is the informative one. In every specification precise enough to bound change, the record does not merely fail to show the assumed rise; it affirmatively bounds any concealed rise below what the scenario needs. A formal one-sided equivalence test against the doubling rate itself (a pre-specified margin of  $0.05428$  per decade, the increase the doubling scenario requires divided across three decades) rejects at the 5% level in all three specifications ( $p = 0.0002$  raw,  $p = 0.0026$  precision-controlled,  $p = 0.004$  two clocks plus composition), so the exclusion is not a reading of where the interval happens to end but a test the record passes. The minimum detectable effect at 80% power is 65–67% of

a doubling in the two precise specifications and 138% in the composition-adjusted one, whose wider standard error leaves it powered only to detect a rise larger than the scenario itself; the minimum detectable effect concerns detection against zero, whereas the equivalence test compares the estimate with the negative doubling margin, which the positive point estimate lies far enough above to reject. The long-run record, being noisier (a per-decade interval of  $\pm 0.10$  around zero and a significant quadratic), cannot support a symmetric claim, and we label it inconclusive at current power rather than flat.

**The new-regime objection.** The record’s data end in 2024, and the premise is about the future: perhaps smart meters, automation, and time-varying rates change the regime after the sample. We do not claim to settle that post-sample regime; what the in-sample technology-rich cells can do is show that where the premise’s favored environments already appear in the record, they are not more price-responsive. The objection can be addressed inside the data, because the record already contains the tariff instrument the premise favors, time-of-use pricing. We state the scope precisely: the record covers the pricing instrument rather than the full enabling-technology regime (mass smart metering, automation, and storage) that the premise projects for the post-sample future, and it samples that regime thinly, so we read the result below as suggestive of the technology-rich corner rather than dispositive for it. Figure 4 compares corrected short-run elasticities across cells: the full sample ( $-0.16$ ), data from 2005 onward ( $-0.22$ ), marginal-price studies ( $-0.15$ ), time-of-use settings ( $-0.09$ ), and design-based studies ( $-0.09$ ), the last two sitting at the elastic edge of the own-price pilot-literature consensus, the  $-0.02$  to  $-0.10$  range of Faruqui & Sergici (2010) and the  $-0.075$  average that Kahn-Lang *et al.* (2025) extract from post-2000 U.S. pilots. That same pilot literature reports substitution elasticities of 0.07 to 0.40 and peak own-price values as large as  $-0.79$ ; those describe intertemporal load-shifting, which is outside the own-price level object we measure. In the meta-regression, the time-of-use indicator enters at  $+0.14$  ( $p = 0.007$ ): conditional on precision, estimates from the most technologically enabled settings are closer to zero than the literature at large. The model-averaged counterpart is smaller and less firmly selected ( $+0.07$ , PIP = 0.74; Table 18), so we read the time-of-use signal as directionally consistent rather than robustly identified, in contrast to the design-based tier. Nothing in the technology-rich corner of the record hints at a rise in the own-price *level* elasticity we measure. The metric is narrow by construction: an own-price

Figure 4: The new-regime check: the technology-rich cells are the least elastic



Notes: PET-corrected short-run elasticity with 95% study-clustered intervals, by cell of the headline sample ( $n$ : full 1,647; post-2005 499; TOU 210; experiments 81; marginal price 401). Shaded band: the own-price elasticity range from dynamic-pricing pilots surveyed by Faruqi & Sergici (2010); dashed line: the  $-0.075$  pilot average of Kahn-Lang *et al.* (2025).

level elasticity does not capture intraday load-shifting or intertemporal substitution, which net out in total consumption, so these cells bound the level response but are silent on engineered demand-response flexibility. What does move pilot responses is hardware and program design (enabling technology adds over twenty percentage points to peak reductions in Kahn-Lang *et al.* (2025)), which is precisely our conclusion: flexibility is engineered, not summoned by prices.

Aigner’s conjecture can now be answered on its own terms. Appliances were indeed chosen anew, several times over; metering and displays arrived at scale; automated response began its rollout; and four further decades of estimates accumulated. The responsiveness they predicted would grow did not grow. The conservation effect the early experiments measured was not the seed of a more elastic future; it was, as Aigner himself suspected it might be, the whole crop.

## 7 What should planners and models use

Energy-system models rarely document their demand elasticities; where visible, household own-price values of  $-0.2$  to  $-0.6$  in the long run are common (Huntington *et al.*, 2019). Table 5 states what the audited record supports. Three planning rules follow. First, *use the horizon, not hope*:  $-0.16$  for operations within a year,  $-0.38$  for capital-turnover horizons; the upper end of the modeled range (beyond  $-0.5$ ) has little support in the corrected record. These menu values are residential-dominated; the long-run response is roughly half as large in industrial and commercial settings (Table 7), so a whole-system planner should apply a demand-weighted blend

Table 5: The elasticity menu: corrected consensus values for planning and modeling

Object	Preferred	Range across correctors
Short run, full sample	-0.16	-0.04 to -0.23
Short run, design-based	-0.09	-0.08 to -0.11
Short run, time-of-use settings	-0.09	-
Intermediate run	-0.33	-0.15 to -0.42
Long run	-0.38	-0.38 to -0.50
Trend allowance, per decade	0.00	95% envelope (-0.04, +0.07) adj. (-0.04, +0.01) bivariate

*Notes:* Bias-corrected values on the headline sample (ranges across PET, PEESE, WAAP, precision- and sample-size weighting where computed). The trend allowance row reports the 95% interval on the short-run data clock in the composition-adjusted specification and, for comparison, the precision-controlled bivariate one; the unrestricted two-clock split is too collinear to be informative (data and publication clocks correlate at  $r = 0.915$ , VIF = 6.12, inflating the data-clock standard error 2.35 $\times$ ; see Section 6).

smaller in magnitude than  $-0.38$ . Second, *any assumed growth in responsiveness is a technology assumption, not a demand assumption*: the historical envelope caps organic growth at roughly 0.04 per decade in magnitude in either specification, so a scenario in which the effective elasticity doubles must specify the hardware, contracts, and automation that deliver the difference, and should price them, because the same pilot literature that finds small elasticities finds large effects of enabling technology. Third, *treat headline literature means as upper bounds*: the raw mean of  $-0.27$  reflects the literature’s preferences; every correction philosophy and every identification upgrade moves the number toward zero.

## 8 Conclusion

We asked whether the price responsiveness of electricity demand has risen, as four decades of energy planning has assumed it would. On 4,898 estimates from 482 studies spanning nine decades of data: it has not. The short-run elasticity is small ( $-0.16$  corrected,  $-0.09$  in design-based studies, indistinguishable from zero once composition is held fixed), and nearly every step up the identification ladder, every publication-bias correction, and every technology-rich subsample moves it closer to zero, not further; the one exception is the post-2005 data cell ( $-0.22$ ), which is more elastic than the full sample yet still sits inside the corrected range and shows no upward trend. Responsiveness does grow with adjustment time, doubling toward  $-0.38$  as the durable stock turns over, exactly as the quasi-experimental adjustment path and

the embodied-capital mechanism predict; but that gradient has been in the data since the 1970s and has not steepened. And on the clock that matters (the vintage of the data, separated from the vintage of the publications), the elasticity shows no trend: in the specifications precise enough to bound it, the record caps any concealed three-decade rise at two-thirds to four-fifths of what a doubling scenario assumes, and the point estimate leans the other way.

We do not claim that responsiveness cannot rise. Automation that responds to prices without human attention, storage that arbitrages tariffs, and contracts that delegate curtailment are all technologies for manufacturing elasticity, and the pilot record shows they work. Our claim is about the assumption that does planning work today: demand has never yet become more price-elastic merely because prices became smarter, and a half-century of evidence puts an affirmative ceiling on how much such a change could have been hidden. Prices alone have never made electricity demand flexible; there is no evidence they are about to start. Flexibility, where plans require it, must be built and budgeted, not assumed.

Three limitations bound these conclusions. First, holding identification and composition fixed moves the short-run data clock from  $-0.033$  to  $+0.017$ ; because design quality is itself correlated with data vintage, the composition controls could in principle absorb a genuine behavioral trend, though the raw record independently shows no trend, which mitigates this concern. Second, the corrected values are read at a zero standard error, an out-of-support extrapolation of the funnel. Third, the design-based benchmark rests on eight short-run studies.

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# Appendices

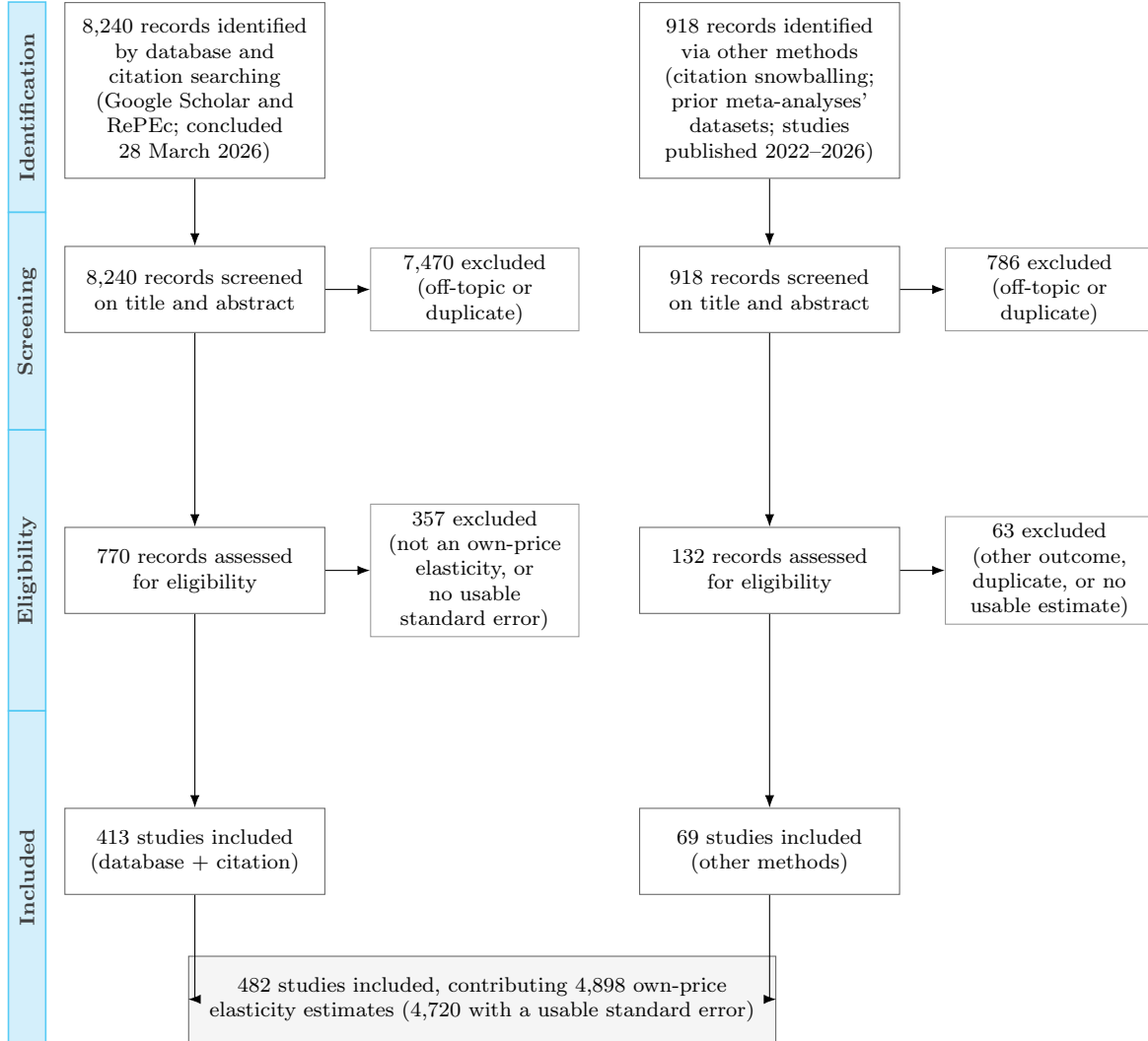
## A Literature search and summary statistics

The corpus was assembled by systematic database search and citation tracking; Figure 5 reports the flow. The database-and-citation arm builds on a systematic Google Scholar and RePEc search concluded on 28 March 2026, which produced a 413-study base collection; the other-methods arm adds backward and forward citation snowballing, the datasets of prior electricity-demand meta-analyses, and studies published through early 2026, extending the corpus to 482 studies that contribute 4,898 own-price elasticity estimates. Reporting follows the meta-analysis guidelines of Havranek *et al.* (2020). To qualify, a study had to report an own-price elasticity of electricity demand, not a cross-price or income elasticity, together with a usable measure of uncertainty (a standard error, a  $t$ -statistic, or a  $p$ -value from which a standard error could be recovered).

Figure 6 plots the distribution of the estimates, and Table 6 and Table 7 summarize them across the main subsamples for the short and long run separately. The estimates are overwhelmingly negative, as demand theory requires; wrong-signed (positive) estimates are rare, the mass concentrating in a narrow band between roughly  $-0.5$  and  $0$  in the short run and shifting away from zero at the long-run horizon.

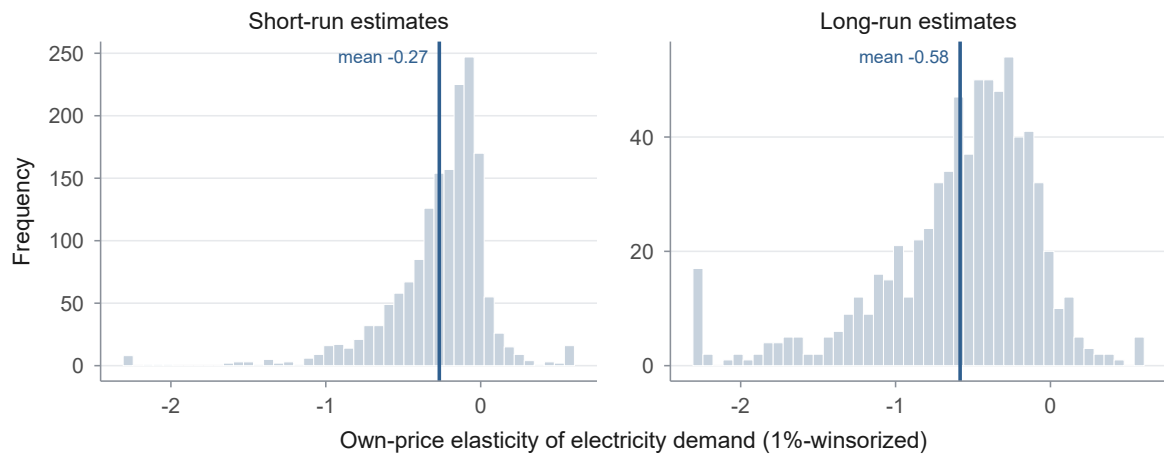
Table 8 collects the statements of the rising-flexibility premise that this paper tests, in the words of those who hold it: five sources across four decades, from the earliest time-of-use pricing experiments to the field's most recent quantitative synthesis, none of which estimates a time trend in price responsiveness.

Figure 5: PRISMA flow diagram of the literature search



Notes: Two-stream PRISMA flow for the electricity-demand price-elasticity corpus; reporting follows the meta-analysis guidelines of Havranek *et al.* (2020). *Database and citation arm*: a systematic Google Scholar and RePEc search (own-price elasticity of electricity demand paired with residential, industrial, tariff, and estimation-method terms) with forward and backward citation searching, concluded 28 March 2026, retrieves 8,240 records; 7,470 are removed at de-duplication and title/abstract screening and 770 are assessed in full text, of which 357 are excluded (a cross-price or income elasticity rather than own-price; no usable standard error,  $t$ -statistic, or  $p$ -value; or a superseded or out-of-scope version), leaving 413 studies. *Other-methods arm*: backward and forward citation snowballing on the most-cited estimates, the datasets of prior electricity-demand meta-analyses (Dahl, 2011; Espey & Espey, 2004; Labandeira *et al.*, 2017; Zhu *et al.*, 2018; Zabaloy & Viego, 2022; Fatima, 2023), and studies published between 2022 and 2026 identify 918 further records; 132 are assessed and 69 are included. Together the two arms give  $413 + 69 = 482$  studies, contributing 4,898 own-price elasticity estimates (4,720 with a usable standard error). PRISMA = Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

Figure 6: Distribution of the price-elasticity estimates



*Notes:* Histograms of the 1%-winsorized own-price elasticity of electricity demand, for the 1,647 short-run and 723 long-run headline estimates (Marshallian-equivalent, usable standard error); the two panels share a common horizontal scale. The vertical line marks the simple (unweighted) mean of each subsample, printed alongside. The long-run estimates sit farther from zero (more elastic) than the short-run estimates, reproducing the horizon gradient of Section 5.

Table 6: Summary of the short-run estimates by subsample

Subsample	$N$	Studies	Unweighted		Weighted	
			Mean	95% CI	Mean	95% CI
All estimates	1,647	226	-0.27	[-0.28, -0.25]	-0.11	[-0.11, -0.11]
<i>Identification tier</i>						
Design-based (tiers 1-3)	81	8	-0.09	[-0.15, -0.03]	-0.08	[-0.08, -0.07]
Instrumented (IV)	549	52	-0.30	[-0.33, -0.28]	-0.10	[-0.11, -0.10]
Panel FE / structural	655	98	-0.21	[-0.23, -0.18]	-0.15	[-0.15, -0.15]
Naive	316	70	-0.37	[-0.41, -0.32]	-0.11	[-0.11, -0.11]
<i>Demand sector</i>						
Residential	1,048	165	-0.29	[-0.31, -0.27]	-0.18	[-0.18, -0.18]
Industrial	220	56	-0.34	[-0.40, -0.27]	-0.07	[-0.08, -0.07]
Commercial	162	43	-0.24	[-0.29, -0.19]	-0.09	[-0.09, -0.08]
<i>Data structure</i>						
Time-series	555	104	-0.21	[-0.24, -0.18]	-0.04	[-0.05, -0.04]
Cross-section	128	26	-0.48	[-0.55, -0.41]	-0.26	[-0.26, -0.25]
Panel	964	106	-0.27	[-0.29, -0.25]	-0.13	[-0.13, -0.13]
<i>Price measurement</i>						
Average price	1,030	166	-0.26	[-0.28, -0.25]	-0.11	[-0.11, -0.11]
Marginal price	401	48	-0.30	[-0.34, -0.26]	-0.05	[-0.06, -0.05]
<i>Tariff regime</i>						
Time-of-use	210	19	-0.17	[-0.22, -0.12]	-0.01	[-0.01, -0.01]
Increasing-block	405	40	-0.31	[-0.34, -0.29]	-0.16	[-0.16, -0.16]
Decreasing-block	118	23	-0.27	[-0.33, -0.22]	-0.10	[-0.10, -0.09]
<i>Geographic aggregation</i>						
Country-level	396	87	-0.22	[-0.25, -0.18]	-0.08	[-0.08, -0.07]
Sub-country (region/city/micro)	1,244	142	-0.28	[-0.30, -0.27]	-0.14	[-0.14, -0.14]
<i>Region</i>						
United States	634	70	-0.25	[-0.27, -0.22]	-0.09	[-0.10, -0.09]
Europe	273	37	-0.26	[-0.30, -0.23]	-0.08	[-0.08, -0.08]
Other	781	127	-0.30	[-0.32, -0.27]	-0.17	[-0.17, -0.17]

*Notes:* Headline short-run sample (Marshallian-equivalent, usable standard error; 1,647 estimates from 226 studies). *Unweighted* is the simple mean of the 1%-winsorized elasticity with its analytic 95% interval ( $\pm 1.96$  s.d./ $\sqrt{N}$ ); *Weighted* is the inverse-variance (precision-)weighted mean with its fixed-effect 95% interval. Subsamples are defined by indicator variables and are not mutually exclusive, so they need not partition the full sample (an estimate can be, for example, both residential and panel). Tiers 1-3 are pooled as design-based; “Sub-country” pools regional, city, and micro-level estimates.

Table 7: Summary of the long-run estimates by subsample

Subsample	<i>N</i>	Studies	Unweighted		Weighted	
			Mean	95% CI	Mean	95% CI
All estimates	723	151	-0.58	[-0.62, -0.55]	-0.38	[-0.38, -0.38]
<i>Identification tier</i>						
Design-based (tiers 1–3)	4	1	-0.32	[-0.36, -0.28]	-0.29	[-0.35, -0.23]
Instrumented (IV)	190	36	-0.68	[-0.76, -0.61]	-0.24	[-0.25, -0.23]
Panel FE / structural	397	81	-0.55	[-0.60, -0.50]	-0.25	[-0.26, -0.25]
Naive	121	36	-0.54	[-0.62, -0.47]	-0.50	[-0.50, -0.50]
<i>Demand sector</i>						
Residential	464	106	-0.59	[-0.64, -0.55]	-0.43	[-0.43, -0.43]
Industrial	114	43	-0.50	[-0.60, -0.40]	-0.17	[-0.19, -0.16]
Commercial	73	29	-0.57	[-0.70, -0.44]	-0.28	[-0.31, -0.26]
<i>Data structure</i>						
Time-series	262	80	-0.56	[-0.64, -0.49]	-0.09	[-0.10, -0.09]
Cross-section	122	16	-0.64	[-0.70, -0.58]	-0.49	[-0.49, -0.48]
Panel	339	59	-0.58	[-0.63, -0.53]	-0.31	[-0.31, -0.30]
<i>Price measurement</i>						
Average price	587	126	-0.57	[-0.61, -0.53]	-0.42	[-0.42, -0.42]
Marginal price	52	17	-0.70	[-0.83, -0.57]	-0.49	[-0.50, -0.47]
<i>Tariff regime</i>						
Time-of-use	30	6	-1.00	[-1.27, -0.72]	-0.29	[-0.32, -0.26]
Increasing-block	152	23	-0.53	[-0.60, -0.46]	-0.47	[-0.48, -0.47]
Decreasing-block	45	10	-0.79	[-0.92, -0.65]	-0.85	[-0.87, -0.84]
<i>Geographic aggregation</i>						
Country-level	265	71	-0.52	[-0.59, -0.46]	-0.40	[-0.40, -0.39]
Sub-country (region/city/micro)	449	80	-0.62	[-0.67, -0.58]	-0.37	[-0.37, -0.37]
<i>Region</i>						
United States	177	30	-0.57	[-0.63, -0.51]	-0.54	[-0.55, -0.53]
Europe	200	38	-0.58	[-0.66, -0.50]	-0.27	[-0.28, -0.27]
Other	353	94	-0.60	[-0.65, -0.54]	-0.41	[-0.41, -0.41]

*Notes:* Headline long-run sample (Marshallian-equivalent, usable standard error; 723 estimates from 151 studies). Columns and conventions are as in Table 6; a subsample present at the short but not the long horizon is omitted.

Table 8: The rising-flexibility premise, 1985–2025

Source	The premise, in the source’s words
Aigner (1985, p. 39)	“[A]pppliance choices will be made with an eye to TOU response; new appliances will become widely available... the pricing strategy surely must be even more desirable in the long-run.”
Faruqui & Sergici (2010)	Roughly 40% of the \$40 billion advanced-metering investment “could be covered by reductions in power generation costs that could be brought about through demand response”; dynamic pricing “will have to be instituted.”
Torriti (2014)	“[M]uch will change with the introduction of In Home Displays (or ‘smart metres’) in every home.”
Huntington <i>et al.</i> (2019)	“[I]mportant transitions may eventually reshape future energy demand responses.”
Kahn-Lang <i>et al.</i> (2025)	Time-based rates “may be a useful and efficient tool for encouraging demand-side flexibility”; the response “may be increasingly important as the power sector becomes more decarbonized.”

*Notes:* Verbatim quotations from the cited works. The statements span four decades and include the field’s most recent quantitative synthesis, which treats the future path of responsiveness as an open prediction problem. The Aigner (1985) entry records the profession’s prevailing expectation at the time rather than his own belief; he himself doubted that the effect would grow. None of these sources estimates a time trend in price responsiveness.

## B Studies included in the meta-analysis

Table 9 lists every study contributing estimates to the corpus. Studies are ordered alphabetically by first author; each entry links by hyperlink to its full reference in the bibliography.

Table 9: Studies included in the meta-analysis

Ackah <i>et al.</i> (2014)	El-Shazly (2013)	Mansur <i>et al.</i> (2005)
Acton <i>et al.</i> (1976)	Eltony & Mohammad (1993)	Arthur <i>et al.</i> (2012)
Acton <i>et al.</i> (1980)	Eltony (1995)	Martins <i>et al.</i> (2023)
ADNDE (1981)	Eltony & Hoque (1996)	Masike & Vermeulen (2022)
Adom (2017)	Eltony & Hajeesh (1999)	Matsui (1979)
Agostini <i>et al.</i> (2015)	Eltony (2004)	Matsukawa <i>et al.</i> (1993)
Akmal & Stern (2001)	Eltony (2006)	Matsukawa (1996)
Al-Faris (2002)	Eltony & Al-Awadhi (2007)	Mattos & Lima (2005)
Al-Sahlawi (1999)	Enrich <i>et al.</i> (2024)	McFadden <i>et al.</i> (1977)
Al-Bajjali & Shamayleh (2018)	Erickson <i>et al.</i> (1973)	Mchugh (1977)
Al Irsyad <i>et al.</i> (2018)	Eskeland <i>et al.</i> (1994)	McRae & Meeks (2016)
Alberini <i>et al.</i> (2011)	Eskeland & Mideksa (2010)	Meher (2020)
Alberini & Filippini (2011)	Fan & Hyndman (2011)	Mendoza & Vargas (1987)
Alberini <i>et al.</i> (2019)	Fan <i>et al.</i> (2019)	Mikayilov <i>et al.</i> (2017)
Alberini <i>et al.</i> (2022)	Fatai <i>et al.</i> (2003)	Mikayilov <i>et al.</i> (2020)
Alter & Syed (2011)	Fell <i>et al.</i> (2014)	Miller & Alberini (2016)
Amarawickrama & Hunt (2008)	Filippini (1995a)	Modiano (1984)
Amusa <i>et al.</i> (2009)	Filippini (1995b)	Moghaddam (2003)
Anderson (1971)	Filippini (1999)	Moghimzadeh & Kymn (1986)
Anderson (1973a)	Filippini & Pachauri (2004)	Morovat <i>et al.</i> (2019)
Anderson (1973b)	Filippini (2011)	Mount <i>et al.</i> (1974)
Anderson (1974)	Filippini <i>et al.</i> (2018)	Mount & Chapman (1979)
Andrade & Lobao (1997)	Fisher & Kaysen (1962)	Mountain & Hsiao (1989)
Andrikopoulos <i>et al.</i> (1989)	Fouquet (1995)	Mountain (1989)
Ang <i>et al.</i> (1992)	Frondel <i>et al.</i> (2019)	Mountain <i>et al.</i> (1989)
Apte (1983)	Fullerton <i>et al.</i> (2012)	Munley <i>et al.</i> (1990)
Archibald <i>et al.</i> (1982)	Fuss (1977)	Murray <i>et al.</i> (1978)
Arimura <i>et al.</i> (2012)	Gabreyohannes (2010)	Nagata (2001)
Arisoy & Ozturk (2014)	Gam & Rejeb (2012)	Nakajima & Hamori (2010)
Aroonruengsawat <i>et al.</i> (2012)	Garbacz (1983a)	Nakajima (2010)
Arsenault <i>et al.</i> (1995)	Garbacz (1983b)	Nan & Murry (1991)
Asadoorian <i>et al.</i> (2006)	Garbacz (1983c)	Narayan & Smyth (2005)
Asadoorian <i>et al.</i> (2008)	Garbacz (1984a)	Narayan <i>et al.</i> (2007)
Aslam & Ahmad (2023)	Garbacz (1984b)	Nasir <i>et al.</i> (2008)
Atakhanova & Howie (2005)	Garbacz (1984c)	Okajima & Okajima (2013)
Atalla & Hunt (2016)	Garbacz (1986)	Oliveira (1993)
Athukorala & Wilson (2010)	Garcia-Cerrutti (2000)	Olivia & Gibson (2008)
Athukorala <i>et al.</i> (2019)	Gautam & Paudel (2018)	Ortiz-Velazquez <i>et al.</i> (2017)
Atkinson (1979a)	Ghaderi <i>et al.</i> (2006)	Ota <i>et al.</i> (2018)
Atkinson (1979b)	Gill & Maddala (1978)	Otero Prada (1984)
Auray <i>et al.</i> (2019)	Glakpe & Fazzolare (1985)	Otsuka & Haruna (2016)
Azevedo <i>et al.</i> (2011)	Gollnick (1975)	Otsuka (2017)
Babatunde & Shuaibu (2009)	Goo <i>et al.</i> (2019)	Otsuka (2023)
Badri (1992)	Green <i>et al.</i> (1986)	Parfomak & Lave (1996)
Baker & Blundell (1991)	Griffin (1974)	Parhizgari & Davis (1978)
Balabanoff (1994)	Gundimeda & Kohlin (2008)	Park & Acton (1984)
Banda & Verdugo (2007)	Guo & Tybout (1994)	Parti & Parti (1980)
Barnes <i>et al.</i> (1981)	Halicioglu (2007)	Paul <i>et al.</i> (2009)
Barrientos <i>et al.</i> (2018)	Hall (1986)	Pellini (2021)
Basu (1976)	Halvorsen (1975)	Pesaran <i>et al.</i> (1999)
Baughman <i>et al.</i> (1979)	Halvorsen (1976)	Pielow <i>et al.</i> (2012)
Beenstock <i>et al.</i> (1999)	Halvorsen (1977)	Pindyck (1979)
Beierlein <i>et al.</i> (1981)	Halvorsen & Ford (1979)	Pindyck (1980)
Bekhet & Othman (2011)	Halvorsen & Larsen (2001)	Pitt (1985)
Belanger <i>et al.</i> (1990)	Ham <i>et al.</i> (1997)	Polemis (2007)
Benavente <i>et al.</i> (2005)	Hartman & Werth (1981)	Polemis & Dagoumas (2013)
Berkhout <i>et al.</i> (2004)	Hasanov <i>et al.</i> (2016)	Pourazarm & Cooray (2013)
Bernard <i>et al.</i> (1987)	Hausman (1979)	Poyer & Williams (1993)
Bernard <i>et al.</i> (1996)	Hawkins (1975)	Qi <i>et al.</i> (2008)
Bernard <i>et al.</i> (2011)	Hawkins (1977)	Rahman (1982)

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Table 9 continued

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Berndt <i>et al.</i> (1980)	Hawkins (1978)	Rai <i>et al.</i> (2014)
Berndt & Samaniego (1984)	He & Lambert (2004)	Ramcharran (1988)
Bernstein & Griffin (2006)	Henderson (1983)	Ramcharran (1990)
Bernstein & Madlener (2011)	Henriksson <i>et al.</i> (2014)	Rapson (2014)
Bernstein & Madlener (2015)	Henson (1984)	Reilly & Shankle (1988)
Betancourt (1981)	Herriges & King (1994)	Reiss & White (2005)
Bianco <i>et al.</i> (2009)	Hesse & Tarkka (1986)	Romero-Jordan <i>et al.</i> (2014)
Bianco <i>et al.</i> (2010)	Hieronymus (1976)	Rosas-Flores (2017)
Bigano <i>et al.</i> (2006)	Hill <i>et al.</i> (1983)	Rossi & Tansini (1989)
Bigerna & Bollino (2014)	Hirth <i>et al.</i> (2022)	Roth (1981)
Bilgili <i>et al.</i> (2010)	Hisnanick & Kyer (1995)	Rouhani <i>et al.</i> (2022)
Biswas <i>et al.</i> (2026)	Hogan (1989)	Roy (1986)
Bjerkholt & Rinde (1983)	Holtedahl & Joutz (2004)	Ryan <i>et al.</i> (1996)
Bjorner <i>et al.</i> (2001)	Hondroyannis (2004)	Ryu <i>et al.</i> (2021)
Blasch <i>et al.</i> (2017)	Horowitz (2007)	Sa'ad (2009)
Blazquez <i>et al.</i> (2013)	Hosoe & Akiyama (2009)	Sabir <i>et al.</i> (2013)
Blundell & Robin (1999)	Houston (1982)	Saddler <i>et al.</i> (1980)
Boogen <i>et al.</i> (2017)	Houthakker (1951)	Saddler & Donnelly (1983)
Borenstein (2009)	Houthakker <i>et al.</i> (1974)	Saha & Bhattacharya (2018)
Bose & Shukla (1999)	Houthakker (1980)	Salari & Javid (2016)
Botero <i>et al.</i> (1990)	Hsiao <i>et al.</i> (1989)	Saunoris & Sheridan (2013)
Branch (1993)	Hsiao & Mountain (1994)	Schulte & Heindl (2017)
Brenton (1997)	Hsing (1994)	Schwarz (1984)
Burke & Kurniawati (2018)	Hsueh & Gerner (1986)	Setshedi & Mah (2025)
Burke & Abayasekara (2018)	Hu <i>et al.</i> (2019)	Shaffer (2020)
Bushnell & Mansur (2005)	Huang <i>et al.</i> (2024)	Shi <i>et al.</i> (2012)
Byrne <i>et al.</i> (2021)	Hughes-Cromwick (1985)	Shin (1981)
Cabral <i>et al.</i> (2020)	Hung & Huang (2015)	Shin (1985)
Campbell (2018)	Huntington & Soffer (1982)	Silk & Joutz (1997)
Cao <i>et al.</i> (2019)	Hyndman <i>et al.</i> (1980)	Silva <i>et al.</i> (2018)
Cao <i>et al.</i> (2023)	Idso <i>et al.</i> (2024)	Siqueira <i>et al.</i> (2006)
Cargill & Meyer (1971)	IEEJ (1986)	Smith (1980)
Carlevaro & Spierer (1983)	Ilmakunnas & Torma (1989)	Solaymani <i>et al.</i> (2015)
Casarin & Delfino (2011)	Inglesi-Lotz & Blignaut (2011)	Sterner (1985)
Cavoulacos & Caramanis (1983)	Inglesi-Lotz (2011)	Sterner (1989)
Cebula & Herder (2009)	Inglesi (2010)	Su (2018)
Cebula (2012)	Iqbal (1986)	Sun & Lin (2013)
Cetinkaya & Bagdadioglu (2015)	Irfi <i>et al.</i> (2006)	Sutherland (1983a)
Chama (2012)	Ishaque (2018)	Sutherland (1983b)
Chang & Chern (1981a)	Ishiguro & Akiyama (1995a)	S&Z Consultores (1999)
Chang & Chern (1981b)	Ishiguro & Akiyama (1995b)	Talbi <i>et al.</i> (2022)
Chang & Hsing (1991)	Ito (2014)	Tambe & Joshi (2014)
Chang & Martinez-Chombo (2003)	Ito <i>et al.</i> (2018)	Tanishita (2019)
Chang <i>et al.</i> (2014)	Jaffee <i>et al.</i> (1982)	Tatli (2017)
Chaudhary <i>et al.</i> (1999)	Jamil & Ahmad (2011)	Taylor <i>et al.</i> (1977)
Chaudhry (2010)	Javid & Qayyum (2014)	Taylor (1979)
Chen & Wang (2025)	Jeong <i>et al.</i> (2011)	Taylor <i>et al.</i> (2005)
Chern (1975)	Jeong & Kim (2025)	Terza (1986)
Chern (1978)	Jessoe & Rapson (2014)	THEC (1983)
Chern & Bouis (1988)	Jia <i>et al.</i> (2021)	Tiwari (2000)
Chindarkar & Goyal (2019)	Jin & Kim (2022)	Tiwari & Menegaki (2019)
Chishti (1993)	Johnsen (2001)	Tran <i>et al.</i> (2023)
Choi (2002)	Jones (1995)	Tserkezos (1992)
Christodoulakis & Kalyvitis (1997)	Jungeilges & Dahl (1986)	Turkekul & Unakitan (2011)
Christopoulos (2000)	Kamerschen & Porter (2004)	Twerefou & Abeney (2020)
Chung & Aigner (1981)	Karbuz <i>et al.</i> (1997)	Uhr <i>et al.</i> (2019)
Cialani & Mortazavi (2018)	Kaserman & Mayo (1985)	Urga & Walters (2003)
Cicchetti & Smith (1975)	KEEI (1989)	Uri (1977)
CISEPA (1998)	Keng (1991)	Uri (1978)
Cohn (1980)	Khan & Qayyum (2009)	Uri (1979a)
Considine (2000)	Khan & Abbas (2016)	Uri (1979b)
Costa & Kahn (2010)	Khanna <i>et al.</i> (2016)	Uri (1979c)
Costa & Kahn (2011)	Khazzoom (1986)	Uri (1979d)
Coughlin (1995)	Knaut & Paulus (2016)	Uri (1979e)
Cuddington & Dagher (2015)	Kohler & Mitchell (1984)	Uri (1982)
Dahan (1996)	Kohler (2014)	Uri (1983)
Damien <i>et al.</i> (2019)	Kokkelenberg & Mount (1993)	Vashist (1984)
Davis (2008)	Kolstad & Lee (1993)	Veall (1983)

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Continued on next page

Table 9 continued

De Cian <i>et al.</i> (2007)	Krishnamurthy & Kristrom (2015)	Veall (1987)
Delfino & Givogri (1979)	Kwon <i>et al.</i> (2016)	Velez <i>et al.</i> (1987)
Delfino (1995)	Labandeira <i>et al.</i> (2005)	Verleger (1973)
Denton <i>et al.</i> (1999)	Lanot & Vesterberg (2021)	Vlachou & Samouilidis (1986)
Denton <i>et al.</i> (2003)	Lareau & Darmstadter (1982)	Volland & Tilov (2018)
Dergiades & Tsoulfidis (2008)	Larsson (2004)	Wakashiro (2019)
Deryugina <i>et al.</i> (2020)	Larsson (2006)	Walfridson (1987)
De Vita <i>et al.</i> (2006)	Laumas & Williams (1981)	Walker (1979)
Diabli (1998)	Vasquez Lavin <i>et al.</i> (2011)	Wang (1985)
Dias-Bandaranaike & Munasinghe (1983)	Lee & Lee (2010)	Wang & Mogi (2017)
Dicembrino & Trovato (2013)	Lee & Chiu (2011)	Wang <i>et al.</i> (2020)
Dilaver & Hunt (2011a)	Li <i>et al.</i> (2020)	Wang & Lin (2021)
Dilaver & Hunt (2011b)	Liddle & Huntington (2021)	Westley (1984a)
Dobozi (1988)	Lijesen (2007)	Westley (1984b)
Dodgson <i>et al.</i> (1990)	Lillard & Acton (1981)	Westley (1989a)
Donatos & Mergos (1989)	Lim <i>et al.</i> (2014)	Westley (1989b)
Donatos & Mergos (1991)	Lin & Ouyang (2014)	Westley (1992)
Dong & Kim (2018)	Lin & Zhu (2020)	Wijemanne (1987)
Dong <i>et al.</i> (2020)	Liu (2005)	Wilson (1974)
Donnelly (1984)	Lohani (1992)	Woo <i>et al.</i> (2018)
Donnelly & Diesendorf (1984)	Loi & Ng (2018)	Yang (1978)
Donnelly & Saddler (1984)	Lyman (1994)	Yang & Liang (2023)
Donnelly (1985)	Lynk (1989)	Yin <i>et al.</i> (2016)
Donnelly (1987)	Ma & Stern (2016)	Yoo <i>et al.</i> (2007)
Douthitt (1989)	Macroconsult (2001)	Young <i>et al.</i> (1983)
Dubin (1985)	Maddala <i>et al.</i> (1994)	Yu & Xin (2020)
Dulleck & Kaufmann (2004)	Maddala <i>et al.</i> (1997)	Zachariadis & Pashourtidou (2007)
Duncan & Binswanger (1976)	Maddigan <i>et al.</i> (1983)	Zarnikau (1990)
Dunstan & Schmidt (1988)	Maddock & Castan (1991)	Zhai <i>et al.</i> (2023)
Durant (1990)	Maddock <i>et al.</i> (1992)	Zhou & Teng (2013)
Durmaz <i>et al.</i> (2020)	Madlener (2011)	Ziramba (2008)
Egorova & Volchkova (2004)	Mahmud & Chishti (1990)	Ziramba & Kavezeri (2012)
Ekpo <i>et al.</i> (2011)	Mamkhezri (2025)	

Notes: The 482 studies that make up the meta-analysis corpus, ordered alphabetically by first author and reading down each column. Each entry links to the reference list by hyperlink.

## C Coding the identification ladder

Each estimate’s price-identification strategy is coded from the primary study’s description of its price variation, not from its estimator label alone. Tier 1 (randomized): experimental assignment of tariffs or randomized encouragement (e.g., Wolak, 2011; Jessoe & Rapson, 2014; Ito *et al.*, 2018). Tier 2 (natural experiment): plausibly exogenous price variation from a mandated change or a spatial boundary design (e.g., Ito, 2014, exploiting spatial discontinuities across utility service-area boundaries; Deryugina *et al.*, 2020, municipal aggregation). Tier 3: difference-in-differences designs against untreated comparison groups. Tier 4: instrumented specifications (IV/2SLS/3SLS/GMM), including supply-shifter and Lewbel-type instruments. Of the 1,068 Tier 4 estimates from 91 studies in the full corpus, only 3 studies report any recoverable first-stage instrument-strength diagnostic (a first-stage  $F$ , Kleibergen-Paap, or comparable statistic), and none does so in a systematically coded field; we therefore cannot audit instrument strength across the tier and note the coverage rather than adjudicate it. Tier 5: panel fixed effects, cointegration systems, and structural demand systems without price instrumentation. Tier 6: naive OLS or time-series regressions of quantity on price. Estimates whose strategy cannot be determined are excluded from ladder analyses (878 estimates spanning 61 studies, of which 91 carry a usable Marshallian-equivalent estimate and so enter the headline pool that feeds every non-ladder analysis: the horizon path, the two-clock test, and the publication-bias battery). The full per-study mapping ships with the replication package. In the short-run headline sample the tiers contain 81 (design-based), 549 (IV), 655 (FE/structural), and 316 (naive) estimates. Coding in this literature, ours included, has historically prioritized each study’s pre-

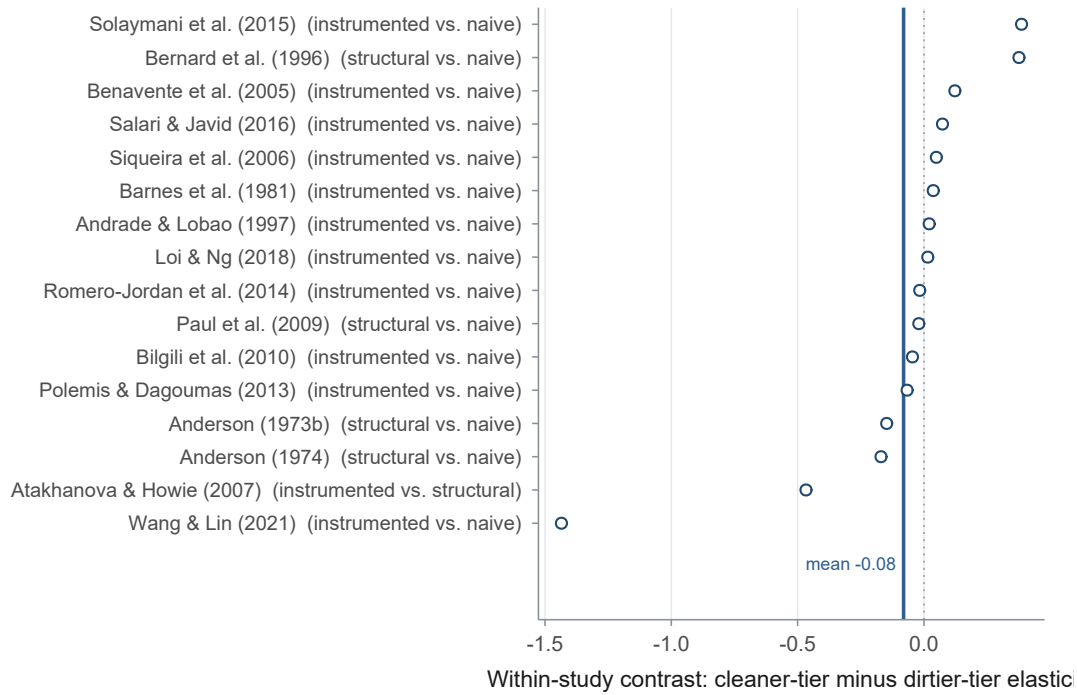
ferred estimates; the corpus carries a **preferred** flag (1,204 estimates), and all headline results are robust to restricting the sample to these flagged estimates (Section 4).

A limitation of this coding concerns reliability. Tiers were assigned by a single coder from each study’s description of its price variation, and the dataset then underwent a study-by-study audit against the primary sources; the assignments were not independently double-coded, so we report no formal inter-coder reliability statistic. Three features limit the exposure this creates. First, the complete per-study tier mapping ships with the replication package, so every assignment can be checked against the primary source. Second, the classification that carries the argument, design-based (Tiers 1–3) versus not, turns on observable, low-discretion features of a study’s design (random assignment, a mandated or plausibly exogenous price change, or an explicit untreated comparison group) rather than on a subjective judgment of estimator quality. Third, the headline analyses collapse the six tiers into four groups (design-based, instrumented, fixed-effects/structural, and naive), so boundary calls within a group do not bear on the design-based-versus-naive contrast that anchors the paper. Independent double-coding of the tier variable, with a reported reliability coefficient, would nonetheless strengthen the ladder and is a natural step for future work.

## D Within-study contrasts and robustness

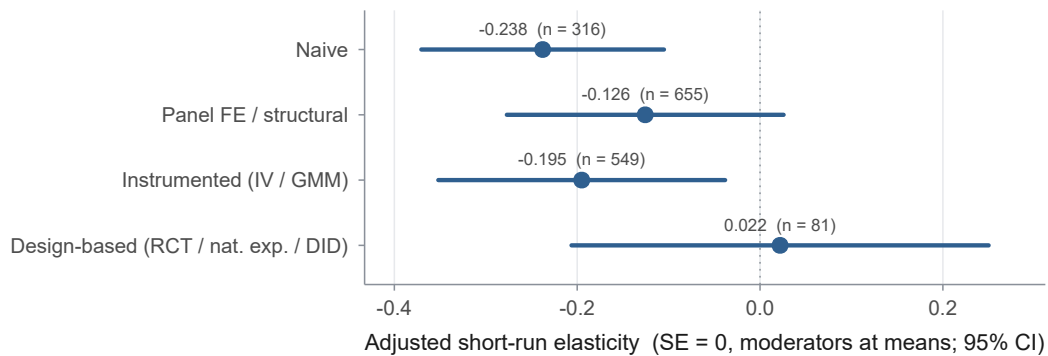
The imputed data-year offset is immaterial to the calendar-time results. Dropping all 23 estimates with no reported data years (assigned publication year minus three in the baseline) and re-estimating leaves the composition-adjusted short-run data-clock trend at +0.0168 per decade, against the reported +0.017 (unchanged across every horizon and specification at the reported precision); the short- and long-run two-clock data-clock coefficients are likewise unchanged at  $-0.0334$  and  $-0.0919$ .

Figure 7: Within-study tier contrasts



*Notes:* For each of the sixteen studies reporting estimates on more than one identification tier (tier pair in parentheses), the mean estimate on the cleaner tier minus the mean estimate on the lower tier, same study and data. The mean contrast is  $-0.081$  (cleaner estimates more elastic, driven by IV-vs-naive pairs), with a paired  $t$  of  $-0.79$  and positive contrasts in 50% of studies: no significant systematic within-study shift, consistent with the IV overshoot in Table 1. A pre-freeze identification audit flags several dozen further studies whose non-preferred lower-tier estimates exist in the primary papers but are not yet coded; enlarging this sample is a natural extension of the corpus.

Figure 8: The adjusted ladder (the key result of Section 3)



*Notes:* Graphical version of Table 1: adjusted short-run elasticity by tier with 95% study-clustered intervals.

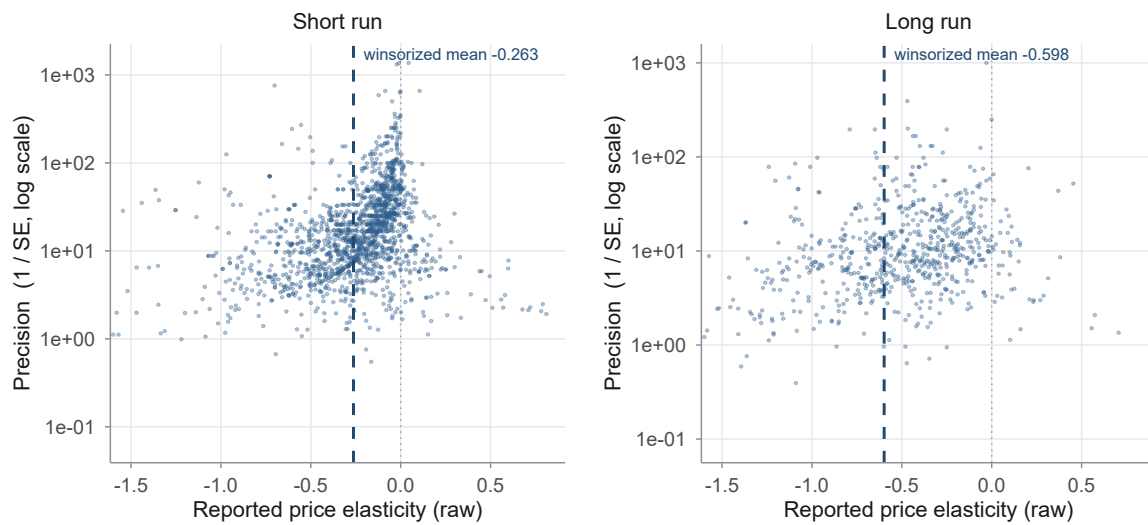
**Sample robustness: the pure-Marshallian core.** The baseline pools directly reported Marshallian estimates with Slutsky-converted compensated (Hicksian) ones (Section 2). Restricting instead to the 2,579 directly reported Marshallian estimates alone (from 272 studies) leaves the picture unchanged: short-run PET  $-0.172$  (se 0.028), long-run PET  $-0.356$  (se 0.041), funnel asymmetry  $p < 0.001$  in both. The calendar-time trend is unchanged as well: on this native-Marshallian sample the fully adjusted short-run data-clock coefficient is  $+0.043$

per decade ( $p = 0.234$ ), the same sign and the same statistical indistinguishability from zero as the pooled headline, and the time-of-use coefficient stays positive and significant ( $+0.19$ ,  $p < 0.001$ ). The harmonization is numerically inconsequential in either direction: because electricity’s budget share is small the Slutsky shift is about 0.012 per estimate, varying the budget share  $s$  over  $[0.02, 0.08]$  moves the short-run corrected value by less than 0.01, and the *converted-from-Hicksian* moderator in the model average (Appendix F) is robustly selected only in the long-run model (posterior mean  $-0.18$ ) and does not overturn the pooled short- and long-run corrections. One-preferred-estimate-per-study and sample-size weighting (no standard errors in the weights) likewise leave the short-run correction in the same range. The long-run ladder preserves the short-run IV overshoot (design-based cell too thin to report): IV  $-0.41$  against FE  $-0.33$ , with naive at  $-0.23$  adjusted.

## E Publication bias: the full battery

The main text corrects the short-run elasticity with a compact battery (Table 2); here we re-run the *full* battery: the linear FAT-PET family (Panel A), the nonlinear correctors, including the Bom–Rachinger endogenous kink (Panel B), and the endogeneity- and selection-robust estimators (MAIVE,  $p$ -uniform\*, and a selection model; Panel C), on each sample the paper leans on, so the correction can be read off estimator by estimator. Figure 9 plots the raw short- and long-run estimates against their precision. Table 10 runs the battery on the short-run and long-run headline samples; Table 11 and Table 12 split the short-run sample by identification tier (naive and panel fixed-effects; instrumented and design-based); and Table 13 splits it by price regime (time-of-use tariffs and marginal price). The message is broadly uniform: in nearly every cell the correctors pull the reported mean toward zero, the funnel-asymmetry (FAT) slope is negative wherever the sample is observational, and the least-selected cell, the design-based sample (baseline FAT slope  $+0.029$ ), carries one of the least price-responsive corrected values; the time-of-use sample is strongly selected (its FAT slope is among the largest in the battery) yet its corrected value is still near zero, which strengthens rather than weakens the low-elasticity conclusion. One further corrector, RTMA (the right-truncated meta-analysis of Mathur and VanderWeele), is not identified in this literature and we do not report it: nearly every estimate is *affirmative* (statistically significant with the expected negative sign), so the small residue of non-affirmative estimates on which the right-truncated likelihood relies is too thin to pin down the model, which then over-corrects implausibly past the raw mean (to  $+0.26$  in the short run).

Figure 9: Funnel plots of the short-run and long-run elasticities



*Notes:* Raw (un-winsorized) reported own-price elasticities against their precision ( $1/SE$ ), for the short-run and long-run headline samples; the dashed line marks the 1%-winsorized simple mean of each sample ( $-0.26$  short run,  $-0.59$  long run). The leftward asymmetry, a thinning of imprecise estimates on the positive side, is the funnel signature of selection against small or wrong-signed elasticities. The vertical axis (precision,  $1/SE$ ) is drawn on a logarithmic scale, which spreads the dense cluster of imprecise estimates near the bottom and makes the asymmetry easier to see; the horizontal axis is clipped to  $[-1.5, 0.8]$  for legibility, and a small number of outliers fall outside the frame but enter every calculation.

Table 10: Publication-bias battery: short-run and long-run elasticities

<b>Part 1: Short-run elasticities</b>					
<i>Panel A: Linear techniques</i>					
	OLS	FE	RE	Study	Precision
Publication bias (Standard error)	-0.851*** (0.154)	-2.055*** (0.708)	-0.953*** (0.072)	-1.281*** (0.128)	-1.171*** (0.202)
Effect beyond bias (Constant)	-0.163*** (0.025)	-0.093*** (0.034)	-0.158*** (0.007)	-0.143*** (0.019)	-0.124*** (0.024)
Observations	1,647	1,647	1,647	1,647	1,647
<i>Panel B: Nonlinear techniques</i>					
	Top 10%	WAAP	PEESE	STEM	EK
Effect beyond bias	-0.104*** (0.000)	-0.111*** (0.000)	-0.231*** (0.020)	-0.239*** (0.019)	-0.113*** (0.029)
Observations	1,647	1,647	1,647	1,647	1,647
<i>Panel C: Endogeneity- and selection-robust techniques</i>					
			p-uniform*	MAIVE	Selection
Effect beyond bias			-0.145*** (0.027)	-0.181*** (0.022)	-0.155*** (0.011)
First-stage $F$ (MAIVE)				23	
Observations			1,647	1,647	1,647
<b>Part 2: Long-run elasticities</b>					
<i>Panel A: Linear techniques</i>					
	OLS	FE	RE	Study	Precision
Publication bias (Standard error)	-0.993*** (0.160)	-1.337 (1.096)	-1.055*** (0.104)	-1.095*** (0.160)	-0.979*** (0.220)
Effect beyond bias (Constant)	-0.377*** (0.036)	-0.359*** (0.091)	-0.365*** (0.018)	-0.351*** (0.046)	-0.380*** (0.045)
Observations	723	723	723	723	723
<i>Panel B: Nonlinear techniques</i>					
	Top 10%	WAAP	PEESE	STEM	EK
Effect beyond bias	-0.372*** (0.001)	-0.376*** (0.001)	-0.498*** (0.032)	-0.424*** (0.114)	-0.378*** (0.076)
Observations	723	723	723	723	723
<i>Panel C: Endogeneity- and selection-robust techniques</i>					
			p-uniform*	MAIVE	Selection
Effect beyond bias			-0.385*** (0.052)		-0.420*** (0.023)
First-stage $F$ (MAIVE)				0	
Observations			723	723	723
<i>Notes:</i> Publication-bias-corrected mean price elasticity from each estimator, on the 1%-winsorized headline sample. <i>Panel A</i> is the FAT-PET meta-regression $\varepsilon_{is} = \varepsilon_0 + \gamma SE(\varepsilon_{is}) + u_{is}$ : “Publication bias” is the slope $\gamma$ and “Effect beyond bias” the intercept $\varepsilon_0$ (each coefficient’s standard error below it); the columns are OLS unweighted, FE inverse-variance weighted, RE random effects (DerSimonian–Laird), Study one median estimate per study, and Precision inverse-SE weighted. Standard errors are two-way clustered by study and by underlying database wherever a regression is estimated (OLS, FE, Precision, PEESE, and EK); RE reports the DerSimonian–Laird standard error and the study-level techniques are already one observation per study. <i>Panel B</i> : PEESE (Stanley & Doucouliagos, 2014), WAAP (Ioannidis <i>et al.</i> , 2017), the top-10% estimator, STEM (Furukawa, 2019), and EK, the endogenous-kink meta-regression of Bom & Rachinger (2019) whose kink point $a_1 =  \varepsilon_0 /1.96$ is iterated to convergence. <i>Panel C</i> : MAIVE (Irsova <i>et al.</i> , 2025) instruments the estimate variance with the inverse sample size (top 1% of SEs dropped) and is reported only where the first-stage instrument is adequate ( $F \geq 10$ ; otherwise only the $F$ is shown); $p$ -uniform* (van Aert & van Assen, 2026) works from study medians. “Selection” is a step-function selection model (a Vevea–Hedges / Andrews–Kasy-type maximum-likelihood correction (Vevea & Hedges, 1995; Andrews & Kasy, 2019) with a cut at the one-sided 2.5% threshold). “Observations” is the number of estimates in the sample, all of which enter every test; the study-level techniques (Study, STEM, $p$ -uniform*) then aggregate them to one value per study, and WAAP and the top-10% estimator average a precision-selected subset of the same estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.					

Table 11: Publication-bias battery: naive and panel fixed-effects tiers

<b>Part 1: Naive</b>					
<i>Panel A: Linear techniques</i>					
	OLS	FE	RE	Study	Precision
Publication bias (Standard error)	-0.750*** (0.204)	-3.187* (1.752)	-0.736*** (0.170)	-1.227*** (0.206)	-1.330*** (0.418)
Effect beyond bias (Constant)	-0.245*** (0.049)	-0.081 (0.093)	-0.248*** (0.023)	-0.188*** (0.044)	-0.152** (0.073)
Observations	316	316	316	316	316
<i>Panel B: Nonlinear techniques</i>					
	Top 10%	WAAP	PEESE	STEM	EK
Effect beyond bias	-0.083*** (0.001)	-0.106*** (0.001)	-0.318*** (0.041)	-0.300*** (0.052)	-0.106 (0.085)
Observations	316	316	316	316	316
<i>Panel C: Endogeneity- and selection-robust techniques</i>					
			p-uniform*	MAIVE	Selection
Effect beyond bias			-0.199*** (0.066)		-0.226*** (0.033)
First-stage $F$ (MAIVE)				0	
Observations			316	316	316
<b>Part 2: Panel FE / structural</b>					
<i>Panel A: Linear techniques</i>					
	OLS	FE	RE	Study	Precision
Publication bias (Standard error)	-0.965*** (0.174)	0.032 (1.165)	-0.885*** (0.124)	-1.589*** (0.146)	-0.683** (0.316)
Effect beyond bias (Constant)	-0.088*** (0.026)	-0.150** (0.069)	-0.098*** (0.014)	-0.085*** (0.023)	-0.123*** (0.041)
Observations	655	655	655	655	655
<i>Panel B: Nonlinear techniques</i>					
	Top 10%	WAAP	PEESE	STEM	EK
Effect beyond bias	-0.157*** (0.001)	-0.151*** (0.001)	-0.164*** (0.020)	-0.028 (0.134)	-0.149*** (0.057)
Observations	655	655	655	655	655
<i>Panel C: Endogeneity- and selection-robust techniques</i>					
			p-uniform*	MAIVE	Selection
Effect beyond bias			-0.128*** (0.031)		-0.115*** (0.013)
First-stage $F$ (MAIVE)				1	
Observations			655	655	655

*Notes:* Publication-bias-corrected mean price elasticity from each estimator, on the 1%-winsorized headline sample. *Panel A* is the FAT-PET meta-regression  $\varepsilon_{is} = \varepsilon_0 + \gamma SE(\varepsilon_{is}) + u_{is}$ : “Publication bias” is the slope  $\gamma$  and “Effect beyond bias” the intercept  $\varepsilon_0$  (each coefficient’s standard error below it); the columns are OLS unweighted, FE inverse-variance weighted, RE random effects (DerSimonian–Laird), Study one median estimate per study, and Precision inverse-SE weighted. Standard errors are two-way clustered by study and by underlying database wherever a regression is estimated (OLS, FE, Precision, PEESE, and EK); RE reports the DerSimonian–Laird standard error and the study-level techniques are already one observation per study. *Panel B:* PEESE (Stanley & Doucouliagos, 2014), WAAP (Ioannidis *et al.*, 2017), the top-10% estimator, STEM (Furukawa, 2019), and EK, the endogenous-kink meta-regression of Bom & Rachinger (2019) whose kink point  $a_1 = |\varepsilon_0|/1.96$  is iterated to convergence. *Panel C:* MAIVE (Irsova *et al.*, 2025) instruments the estimate variance with the inverse sample size (top 1% of SEs dropped) and is reported only where the first-stage instrument is adequate ( $F \geq 10$ ; otherwise only the  $F$  is shown);  $p$ -uniform\* (van Aert & van Assen, 2026) works from study medians. “Selection” is a step-function selection model (a Vevea–Hedges / Andrews–Kasy-type maximum-likelihood correction (Vevea & Hedges, 1995; Andrews & Kasy, 2019) with a cut at the one-sided 2.5% threshold). “Observations” is the number of estimates in the sample, all of which enter every test; the study-level techniques (Study, STEM,  $p$ -uniform\*) then aggregate them to one value per study, and WAAP and the top-10% estimator average a precision-selected subset of the same estimates. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% level.

Table 12: Publication-bias battery: instrumented and design-based tiers

<b>Part 1: Instrumented (IV/GMM)</b>					
<i>Panel A: Linear techniques</i>					
	OLS	FE	RE	Study	Precision
Publication bias (Standard error)	-1.037*** (0.241)	-3.819*** (0.695)	-1.462*** (0.109)	-1.722*** (0.361)	-1.841*** (0.375)
Effect beyond bias (Constant)	-0.203*** (0.039)	-0.063*** (0.023)	-0.177*** (0.009)	-0.119*** (0.039)	-0.124*** (0.036)
Observations	549	549	549	549	549
<i>Panel B: Nonlinear techniques</i>					
	Top 10%	WAAP	PEESE	STEM	EK
Effect beyond bias	-0.074*** (0.001)	-0.102*** (0.001)	-0.276*** (0.030)	-0.079*** (0.022)	-0.102*** (0.028)
Observations	549	549	549	549	549
<i>Panel C: Endogeneity- and selection-robust techniques</i>					
			p-uniform*	MAIVE	Selection
Effect beyond bias			-0.118** (0.056)	-0.278*** (0.042)	-0.164*** (0.020)
First-stage $F$ (MAIVE)				17	
Observations			549	549	549
<b>Part 2: Design-based (RCT/nat. exp./DID)</b>					
<i>Panel A: Linear techniques</i>					
	OLS	FE	RE	Study	Precision
Publication bias (Standard error)	0.029 (0.596)	0.467 (1.055)	-0.486** (0.204)	-0.556 (0.572)	-0.185 (0.612)
Effect beyond bias (Constant)	-0.095* (0.050)	-0.084** (0.036)	-0.046*** (0.011)	-0.117 (0.103)	-0.066** (0.030)
Observations	81	81	81	81	81
<i>Panel B: Nonlinear techniques</i>					
	Top 10%	WAAP	PEESE	STEM	EK
Effect beyond bias	-0.087*** (0.003)	-0.080*** (0.002)	-0.114** (0.056)	-0.108*** (0.038)	-0.077*** (0.028)
Observations	81	81	81	81	81
<i>Panel C: Endogeneity- and selection-robust techniques</i>					
			p-uniform*	MAIVE	Selection
Effect beyond bias			-0.116 (0.078)	-0.115*** (0.022)	-0.066*** (0.014)
First-stage $F$ (MAIVE)				123	
Observations			81	81	81
<i>Notes:</i> Publication-bias-corrected mean price elasticity from each estimator, on the 1%-winsorized headline sample. <i>Panel A</i> is the FAT-PET meta-regression $\varepsilon_{is} = \varepsilon_0 + \gamma SE(\varepsilon_{is}) + u_{is}$ : “Publication bias” is the slope $\gamma$ and “Effect beyond bias” the intercept $\varepsilon_0$ (each coefficient’s standard error below it); the columns are OLS unweighted, FE inverse-variance weighted, RE random effects (DerSimonian–Laird), Study one median estimate per study, and Precision inverse-SE weighted. Standard errors are two-way clustered by study and by underlying database wherever a regression is estimated (OLS, FE, Precision, PEESE, and EK); RE reports the DerSimonian–Laird standard error and the study-level techniques are already one observation per study. <i>Panel B</i> : PEESE (Stanley & Doucouliagos, 2014), WAAP (Ioannidis <i>et al.</i> , 2017), the top-10% estimator, STEM (Furukawa, 2019), and EK, the endogenous-kink meta-regression of Bom & Rächinger (2019) whose kink point $a_1 =  \varepsilon_0 /1.96$ is iterated to convergence. <i>Panel C</i> : MAIVE (Irsova <i>et al.</i> , 2025) instruments the estimate variance with the inverse sample size (top 1% of SEs dropped) and is reported only where the first-stage instrument is adequate ( $F \geq 10$ ; otherwise only the $F$ is shown); $p$ -uniform* (van Aert & van Assen, 2026) works from study medians. “Selection” is a step-function selection model (a Vevea–Hedges / Andrews–Kasy-type maximum-likelihood correction (Vevea & Hedges, 1995; Andrews & Kasy, 2019) with a cut at the one-sided 2.5% threshold). “Observations” is the number of estimates in the sample, all of which enter every test; the study-level techniques (Study, STEM, $p$ -uniform*) then aggregate them to one value per study, and WAAP and the top-10% estimator average a precision-selected subset of the same estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.					

Table 13: Publication-bias battery: time-of-use and marginal-price subsamples

<b>Part 1: Time-of-use tariff</b>					
<i>Panel A: Linear techniques</i>					
	OLS	FE	RE	Study	Precision
Publication bias (Standard error)	-0.517** (0.262)	-1.754*** (0.490)	-1.241*** (0.120)	-0.907* (0.484)	-1.001*** (0.372)
Effect beyond bias (Constant)	-0.091** (0.037)	0.001 (0.012)	-0.022*** (0.008)	-0.147* (0.083)	-0.018 (0.015)
Observations	210	210	210	210	210
<i>Panel B: Nonlinear techniques</i>					
	Top 10%	WAAP	PEESE	STEM	EK
Effect beyond bias	-0.005*** (0.001)	-0.011*** (0.001)	-0.154*** (0.056)	-0.051 (0.038)	0.001 (0.012)
Observations	210	210	210	210	210
<i>Panel C: Endogeneity- and selection-robust techniques</i>					
			p-uniform*	MAIVE	Selection
Effect beyond bias			-0.228*** (0.076)	-0.090*** (0.032)	-0.075*** (0.020)
First-stage $F$ (MAIVE)				15	
Observations			210	210	210
<b>Part 2: Marginal price</b>					
<i>Panel A: Linear techniques</i>					
	OLS	FE	RE	Study	Precision
Publication bias (Standard error)	-0.969*** (0.297)	-2.945*** (0.847)	-1.530*** (0.098)	-1.239*** (0.193)	-1.396*** (0.349)
Effect beyond bias (Constant)	-0.148*** (0.057)	-0.027 (0.022)	-0.114*** (0.008)	-0.135*** (0.047)	-0.080** (0.031)
Observations	401	401	401	401	401
<i>Panel B: Nonlinear techniques</i>					
	Top 10%	WAAP	PEESE	STEM	EK
Effect beyond bias	-0.041*** (0.001)	-0.049*** (0.001)	-0.230*** (0.047)	-0.076* (0.039)	-0.049** (0.025)
Observations	401	401	401	401	401
<i>Panel C: Endogeneity- and selection-robust techniques</i>					
			p-uniform*	MAIVE	Selection
Effect beyond bias			-0.124** (0.061)		-0.155*** (0.022)
First-stage $F$ (MAIVE)				7	
Observations			401	401	401
<i>Notes:</i> Publication-bias-corrected mean price elasticity from each estimator, on the 1%-winsorized headline sample. <i>Panel A</i> is the FAT-PET meta-regression $\varepsilon_{is} = \varepsilon_0 + \gamma SE(\varepsilon_{is}) + u_{is}$ : “Publication bias” is the slope $\gamma$ and “Effect beyond bias” the intercept $\varepsilon_0$ (each coefficient’s standard error below it); the columns are OLS unweighted, FE inverse-variance weighted, RE random effects (DerSimonian–Laird), Study one median estimate per study, and Precision inverse-SE weighted. Standard errors are two-way clustered by study and by underlying database wherever a regression is estimated (OLS, FE, Precision, PEESE, and EK); RE reports the DerSimonian–Laird standard error and the study-level techniques are already one observation per study. <i>Panel B</i> : PEESE (Stanley & Doucouliagos, 2014), WAAP (Ioannidis <i>et al.</i> , 2017), the top-10% estimator, STEM (Furukawa, 2019), and EK, the endogenous-kink meta-regression of Bom & Rachinger (2019) whose kink point $a_1 =  \varepsilon_0 /1.96$ is iterated to convergence. <i>Panel C</i> : MAIVE (Irsova <i>et al.</i> , 2025) instruments the estimate variance with the inverse sample size (top 1% of SEs dropped) and is reported only where the first-stage instrument is adequate ( $F \geq 10$ ; otherwise only the $F$ is shown); $p$ -uniform* (van Aert & van Assen, 2026) works from study medians. “Selection” is a step-function selection model (a Vevea–Hedges / Andrews–Kasy-type maximum-likelihood correction (Vevea & Hedges, 1995; Andrews & Kasy, 2019) with a cut at the one-sided 2.5% threshold). “Observations” is the number of estimates in the sample, all of which enter every test; the study-level techniques (Study, STEM, $p$ -uniform*) then aggregate them to one value per study, and WAAP and the top-10% estimator average a precision-selected subset of the same estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.					

## F Heterogeneity: Bayesian model averaging

The identification ladder and the two-clock test isolate the two dimensions the paper cares about most; the literature varies along many others, and this appendix maps that variation with Bayesian model averaging (BMA).<sup>3</sup> The exercise is descriptive and deliberately secondary to the trend and ladder findings: it asks which of the coded study characteristics move the reported elasticity once all of them compete for inclusion, and it doubles as a robustness check on the publication-bias term, which here must survive against every moderator at once. Because the paper’s spine is the adjustment horizon, we run the averaging on three samples (the full headline sample and, separately, its short-run and long-run subsamples) with variable definitions and summary statistics in Table 14 (full sample), Table 15 (short run) and Table 16 (long run). Table 17 reports the integrated averaging beside a two-way (study  $\times$  database) clustered OLS check and a frequentist model average; Table 18 and Table 19 report the horizon-specific models; Table 20 the sensitivity to the priors and Table 21 the convergence diagnostics; Figure 10 shows the model-inclusion picture for the three samples and Figure 11 the regressor correlation matrix.

The response variable is the 1%-winsorized price elasticity and the publication-bias term is its 1%-winsorized standard error, as throughout the paper. The moderators, their reference categories, and the log transforms follow the variable list that ships with the frozen data. Because the identification, model-form, data-structure and price-measurement indicators are strongly collinear (a naive static reduced-form model on yearly average prices is one bundle, a dynamic instrumented panel another), we average under the *dilution* prior of George (2010), which down-weights models whose regressor correlation matrix is near-singular and so prices in the redundancy the single-regression approach ignores; the scale-laden moderators (study size, data span, publication year, impact factor, citations) enter in logs. Two-way clustering by study and by underlying database sharpens the frequentist check: many estimates share a data source across studies, and clustering on the database as well as the study nets out that second layer of dependence. The two-way correction changes little, because shared datasets are rare in this corpus: the 366 headline studies draw on 344 distinct underlying data sources, only 17 databases are shared by two or more studies and none by more than four, so study-level clustering already absorbs almost all of the cross-estimate dependence. Re-running the headline inferences with two-way (study  $\times$  database) clustering leaves them materially unchanged: the short-run PET is  $-0.16$  with study-clustered and two-way-clustered standard errors identical to three decimals (0.025), the design-versus-naive ladder contrast moves from  $p = 0.006$  to  $p = 0.007$ , and the composition-adjusted data-clock trend stays null ( $p = 0.53$ ). The lone coefficient in which the second clustering dimension is visible is the pooled design-based term of the integrated model (Table 17), borderline at  $p = 0.08$ , a reflection of its larger moderator set and pooled horizons rather than of the clustering. Missing moderator values are median-imputed at the analysis step; the elasticity and its standard error are never imputed.

Table 14: Definition and summary statistics of the model-averaging variables: full sample

Variable	Description	Mean	SD
Price elasticity	The own-price elasticity of electricity demand (1%-winsorized), the response variable.	-0.39	0.44
Standard error	The standard error of the elasticity estimate (1%-winsorized); its coefficient measures publication selection.	0.15	0.20

*Continued on next page*

<sup>3</sup>The averaging is computed by a companion script, `bma.R`, on the same frozen data file; it uses the `BMS` package (Zeugner & Feldkircher, 2015) and generates every table and figure in this appendix. All scripts and the frozen data ship in the replication package.

Table 14 continued

Variable	Description	Mean	SD
<i>Horizon</i>			
Short run	=1 if the estimate is a short-run (within about one year) elasticity.	0.50	0.50
Long run	=1 if the estimate is a long-run (beyond about five years; capital fully adjusted) elasticity.	0.22	0.41
Intermediate run	=1 if the estimate is an intermediate-run (about 2–5 years) elasticity (reference category).	0.29	0.45
<i>Estimate type</i>			
Converted from Hicksian	=1 if the estimate is a compensated (Hicksian) own-price elasticity converted to a Marshallian footing via the Slutsky relation $\varepsilon_M = \varepsilon_H - s\eta$ ; =0 for a directly reported Marshallian estimate. Controls for any residual difference between the converted and the directly reported evidence.	0.22	0.42
<i>Identification</i>			
Design-based	=1 if a design-based strategy identifies the price response: a randomized or mandated pricing experiment, a natural experiment, or a difference-in-differences design (tiers 1–3).	0.05	0.21
Instrumented	=1 if the price is instrumented (IV, two- or three-stage least squares, or GMM; tier 4).	0.27	0.45
Panel FE / structural	=1 if identification rests on panel fixed effects or a structural demand system, without instrumentation (tier 5).	0.43	0.50
Naive	=1 if a naive regression with no price-identification strategy is used (tier 6; the reference category, which also absorbs estimates whose strategy could not be classified).	0.22	0.41
<i>Estimate aggregation</i>			
Aggregation: country	=1 if the data are aggregated to the country (or larger) level.	0.26	0.44
Aggregation: micro	=1 if the data are micro (household, plant or firm-level).	0.27	0.44
Aggregation: region	=1 if the data are aggregated to a sub-country region or city (reference category).	0.30	0.46
<i>Sector</i>			
Residential demand	=1 if the estimate refers to residential electricity demand.	0.62	0.49
Industrial demand	=1 if the estimate refers to industrial electricity demand.	0.19	0.39
Commercial demand	=1 if the estimate refers to commercial or mixed demand (reference category).	0.09	0.28
<i>Data characteristics</i>			
Time-series data	=1 if the study uses time-series data.	0.34	0.47
Cross-section data	=1 if the study uses cross-sectional data.	0.12	0.33
Panel data	=1 if the study uses panel data (reference category).	0.54	0.50
Yearly data	=1 if the data are of yearly frequency.	0.66	0.47
Sub-yearly data	=1 if the data are of monthly or quarterly frequency (reference category).	0.28	0.45
Data span	The logarithm of one plus the number of years the dataset covers.	2.35	1.00
Study size	The logarithm of the number of observations used in the estimation.	6.13	2.54
<i>Price and tariff</i>			
Average price	=1 if the price is measured as average price (revenue over quantity).	0.64	0.48
Marginal price	=1 if the price is measured as marginal price.	0.24	0.43
Other price	=1 if another price concept is used (lump-sum, Shin, undefined; reference category).	0.08	0.27
Increasing-block tariff	=1 if an increasing-block tariff is in force.	0.19	0.39
Decreasing-block tariff	=1 if a decreasing-block tariff is in force.	0.12	0.32

Continued on next page

Table 14 continued

Variable	Description	Mean	SD
Time-of-use tariff	=1 if a time-of-use rate (different unit prices by time block) is used.	0.14	0.34
Flat tariff	=1 if a flat tariff is in force (reference category).	0.04	0.20
<i>Demand controls</i>			
Control: demographics	=1 if the demand equation controls for demographic variation.	0.36	0.48
Control: temperature	=1 if the demand equation controls for temperature or degree-days.	0.50	0.50
Control: appliance stock	=1 if the demand equation controls for the stock of electrical appliances.	0.22	0.41
Control: other fuels	=1 if the demand equation controls for prices of substitute fuels.	0.41	0.49
<i>Model specification</i>			
Reduced form	=1 if a single-equation reduced-form model is estimated.	0.48	0.50
Structural form	=1 if a structural (multi-equation) model is estimated (reference category).	0.50	0.50
Static model	=1 if the model is static (no lagged demand).	0.32	0.47
Dynamic model	=1 if the model is dynamic, with lagged adjustment (reference category).	0.68	0.47
ARDL	=1 if an autoregressive distributed-lag (partial-adjustment) model is used.	0.04	0.20
Lagged endogenous	=1 if a lagged-endogenous model is used.	0.19	0.40
Linear demand	=1 if the demand function is linear.	0.13	0.34
Double-log demand	=1 if the demand function is double-logarithmic.	0.80	0.40
Semi-log demand	=1 if the demand function is semi-logarithmic or Box-Cox (reference category).	0.01	0.11
<i>Geography and context</i>			
United States	=1 if the estimate is for the United States.	0.41	0.49
Europe	=1 if the estimate is for a European country.	0.18	0.38
Other country	=1 if the estimate is for a country outside the US and Europe (reference category).	0.42	0.49
Daylight hours	The average length of the longest day for the country under examination (source: Moesen, 2010).	14.76	1.58
Population	The logarithm of the population of the country or entity (source: World Bank, 2025).	18.36	1.58
<i>Publication characteristics</i>			
Publication year	The logarithm of one plus the number of years since the earliest study in the sample.	3.91	0.35
Impact factor	The logarithm of one plus the journal's RePEc recursive impact factor; 0 for unpublished work.	0.26	0.55
Citations	The logarithm of one plus the study's mean annual Google Scholar citation count.	0.87	0.67

*Notes:* Mean and standard deviation over all 3,324 headline estimates (Marshallian-equivalent: directly reported or Slutsky-converted, usable standard error) from 366 studies. Binary indicators are reported as means (shares); the elasticity and its standard error are the 1%-winsorized values used throughout. Reference categories are shown for completeness but drop from the averaging. The identification reference category (Naive) totals 821 estimates, of which 730 are labeled naive (tier 6) and 91 could not be classified; the averaging does not distinguish between the two. Scale-laden variables enter the averaging in logs, as noted in their descriptions.

Table 15: Definition and summary statistics of the model-averaging variables: short-run estimates

Variable	Description	Mean	SD
Price elasticity	The own-price elasticity of electricity demand (1%-winsorized), the response variable.	-0.27	0.33

*Continued on next page*

Table 15 continued

Variable	Description	Mean	SD
Standard error	The standard error of the elasticity estimate (1%-winsorized); its coefficient measures publication selection.	0.12	0.17
<i>Horizon</i>			
Short run	=1 if the estimate is a short-run (within about one year) elasticity.	1.00	0.00
Long run	=1 if the estimate is a long-run (beyond about five years; capital fully adjusted) elasticity.	0.00	0.00
Intermediate run	=1 if the estimate is an intermediate-run (about 2–5 years) elasticity (reference category).	0.00	0.00
<i>Estimate type</i>			
Converted from Hicksian	=1 if the estimate is a compensated (Hicksian) own-price elasticity converted to a Marshallian footing via the Slutsky relation $\varepsilon_M = \varepsilon_H - s\eta$ ; =0 for a directly reported Marshallian estimate. Controls for any residual difference between the converted and the directly reported evidence.	0.18	0.39
<i>Identification</i>			
Design-based	=1 if a design-based strategy identifies the price response: a randomized or mandated pricing experiment, a natural experiment, or a difference-in-differences design (tiers 1–3).	0.05	0.22
Instrumented	=1 if the price is instrumented (IV, two- or three-stage least squares, or GMM; tier 4).	0.33	0.47
Panel FE / structural	=1 if identification rests on panel fixed effects or a structural demand system, without instrumentation (tier 5).	0.40	0.49
Naive	=1 if a naive regression with no price-identification strategy is used (tier 6; the reference category, which also absorbs estimates whose strategy could not be classified).	0.19	0.39
<i>Estimate aggregation</i>			
Aggregation: country	=1 if the data are aggregated to the country (or larger) level.	0.24	0.43
Aggregation: micro	=1 if the data are micro (household, plant or firm-level).	0.26	0.44
Aggregation: region	=1 if the data are aggregated to a sub-country region or city (reference category).	0.34	0.47
<i>Sector</i>			
Residential demand	=1 if the estimate refers to residential electricity demand.	0.64	0.48
Industrial demand	=1 if the estimate refers to industrial electricity demand.	0.13	0.34
Commercial demand	=1 if the estimate refers to commercial or mixed demand (reference category).	0.10	0.30
<i>Data characteristics</i>			
Time-series data	=1 if the study uses time-series data.	0.34	0.47
Cross-section data	=1 if the study uses cross-sectional data.	0.08	0.27
Panel data	=1 if the study uses panel data (reference category).	0.59	0.49
Yearly data	=1 if the data are of yearly frequency.	0.65	0.48
Sub-yearly data	=1 if the data are of monthly or quarterly frequency (reference category).	0.27	0.44
Data span	The logarithm of one plus the number of years the dataset covers.	2.40	0.98
Study size	The logarithm of the number of observations used in the estimation.	6.55	2.56
<i>Price and tariff</i>			
Average price	=1 if the price is measured as average price (revenue over quantity).	0.63	0.48
Marginal price	=1 if the price is measured as marginal price.	0.24	0.43
Other price	=1 if another price concept is used (lump-sum, Shin, undefined; reference category).	0.07	0.25

Continued on next page

Table 15 continued

Variable	Description	Mean	SD
Increasing-block tariff	=1 if an increasing-block tariff is in force.	0.25	0.43
Decreasing-block tariff	=1 if a decreasing-block tariff is in force.	0.07	0.26
Time-of-use tariff	=1 if a time-of-use rate (different unit prices by time block) is used.	0.13	0.33
Flat tariff	=1 if a flat tariff is in force (reference category).	0.04	0.20
<i>Demand controls</i>			
Control: demographics	=1 if the demand equation controls for demographic variation.	0.39	0.49
Control: temperature	=1 if the demand equation controls for temperature or degree-days.	0.58	0.49
Control: appliance stock	=1 if the demand equation controls for the stock of electrical appliances.	0.21	0.40
Control: other fuels	=1 if the demand equation controls for prices of substitute fuels.	0.42	0.49
<i>Model specification</i>			
Reduced form	=1 if a single-equation reduced-form model is estimated.	0.55	0.50
Structural form	=1 if a structural (multi-equation) model is estimated (reference category).	0.43	0.50
Static model	=1 if the model is static (no lagged demand).	0.21	0.40
Dynamic model	=1 if the model is dynamic, with lagged adjustment (reference category).	0.79	0.41
ARDL	=1 if an autoregressive distributed-lag (partial-adjustment) model is used.	0.03	0.18
Lagged endogenous	=1 if a lagged-endogenous model is used.	0.32	0.47
Linear demand	=1 if the demand function is linear.	0.09	0.29
Double-log demand	=1 if the demand function is double-logarithmic.	0.88	0.33
Semi-log demand	=1 if the demand function is semi-logarithmic or Box-Cox (reference category).	0.01	0.10
<i>Geography and context</i>			
United States	=1 if the estimate is for the United States.	0.38	0.49
Europe	=1 if the estimate is for a European country.	0.17	0.37
Other country	=1 if the estimate is for a country outside the US and Europe (reference category).	0.47	0.50
Daylight hours	The average length of the longest day for the country under examination (source: Moesen, 2010).	14.66	1.64
Population	The logarithm of the population of the country or entity (source: World Bank, 2025).	18.32	1.56
<i>Publication characteristics</i>			
Publication year	The logarithm of one plus the number of years since the earliest study in the sample.	3.99	0.29
Impact factor	The logarithm of one plus the journal's RePEc recursive impact factor; 0 for unpublished work.	0.26	0.56
Citations	The logarithm of one plus the study's mean annual Google Scholar citation count.	0.91	0.59

Notes: Mean and standard deviation over the 1,647 short-run headline estimates. The horizon indicators are constant on this subsample and drop from the short-run averaging. Binary indicators are reported as means (shares); the elasticity and its standard error are the 1%-winsorized values.

Table 16: Definition and summary statistics of the model-averaging variables: long-run estimates

Variable	Description	Mean	SD
Price elasticity	The own-price elasticity of electricity demand (1%-winsorized), the response variable.	-0.58	0.52

Continued on next page

Table 16 continued

Variable	Description	Mean	SD
Standard error	The standard error of the elasticity estimate (1%-winsorized); its coefficient measures publication selection.	0.21	0.26
<i>Horizon</i>			
Short run	=1 if the estimate is a short-run (within about one year) elasticity.	0.00	0.00
Long run	=1 if the estimate is a long-run (beyond about five years; capital fully adjusted) elasticity.	1.00	0.00
Intermediate run	=1 if the estimate is an intermediate-run (about 2–5 years) elasticity (reference category).	0.00	0.00
<i>Estimate type</i>			
Converted from Hicksian	=1 if the estimate is a compensated (Hicksian) own-price elasticity converted to a Marshallian footing via the Slutsky relation $\varepsilon_M = \varepsilon_H - s\eta$ ; =0 for a directly reported Marshallian estimate. Controls for any residual difference between the converted and the directly reported evidence.	0.36	0.48
<i>Identification</i>			
Design-based	=1 if a design-based strategy identifies the price response: a randomized or mandated pricing experiment, a natural experiment, or a difference-in-differences design (tiers 1–3).	0.01	0.07
Instrumented	=1 if the price is instrumented (IV, two- or three-stage least squares, or GMM; tier 4).	0.26	0.44
Panel FE / structural	=1 if identification rests on panel fixed effects or a structural demand system, without instrumentation (tier 5).	0.55	0.50
Naive	=1 if a naive regression with no price-identification strategy is used (tier 6; the reference category, which also absorbs estimates whose strategy could not be classified).	0.17	0.37
<i>Estimate aggregation</i>			
Aggregation: country	=1 if the data are aggregated to the country (or larger) level.	0.37	0.48
Aggregation: micro	=1 if the data are micro (household, plant or firm-level).	0.18	0.38
Aggregation: region	=1 if the data are aggregated to a sub-country region or city (reference category).	0.38	0.49
<i>Sector</i>			
Residential demand	=1 if the estimate refers to residential electricity demand.	0.64	0.48
Industrial demand	=1 if the estimate refers to industrial electricity demand.	0.16	0.36
Commercial demand	=1 if the estimate refers to commercial or mixed demand (reference category).	0.10	0.30
<i>Data characteristics</i>			
Time-series data	=1 if the study uses time-series data.	0.36	0.48
Cross-section data	=1 if the study uses cross-sectional data.	0.17	0.37
Panel data	=1 if the study uses panel data (reference category).	0.47	0.50
Yearly data	=1 if the data are of yearly frequency.	0.82	0.39
Sub-yearly data	=1 if the data are of monthly or quarterly frequency (reference category).	0.12	0.32
Data span	The logarithm of one plus the number of years the dataset covers.	2.74	0.95
Study size	The logarithm of the number of observations used in the estimation.	5.62	2.10
<i>Price and tariff</i>			
Average price	=1 if the price is measured as average price (revenue over quantity).	0.81	0.39
Marginal price	=1 if the price is measured as marginal price.	0.07	0.26
Other price	=1 if another price concept is used (lump-sum, Shin, undefined; reference category).	0.10	0.30

Continued on next page

Table 16 continued

Variable	Description	Mean	SD
Increasing-block tariff	=1 if an increasing-block tariff is in force.	0.21	0.41
Decreasing-block tariff	=1 if a decreasing-block tariff is in force.	0.06	0.24
Time-of-use tariff	=1 if a time-of-use rate (different unit prices by time block) is used.	0.04	0.20
Flat tariff	=1 if a flat tariff is in force (reference category).	0.04	0.20
<i>Demand controls</i>			
Control: demographics	=1 if the demand equation controls for demographic variation.	0.35	0.48
Control: temperature	=1 if the demand equation controls for temperature or degree-days.	0.44	0.50
Control: appliance stock	=1 if the demand equation controls for the stock of electrical appliances.	0.19	0.39
Control: other fuels	=1 if the demand equation controls for prices of substitute fuels.	0.32	0.47
<i>Model specification</i>			
Reduced form	=1 if a single-equation reduced-form model is estimated.	0.40	0.49
Structural form	=1 if a structural (multi-equation) model is estimated (reference category).	0.57	0.50
Static model	=1 if the model is static (no lagged demand).	0.25	0.43
Dynamic model	=1 if the model is dynamic, with lagged adjustment (reference category).	0.74	0.44
ARDL	=1 if an autoregressive distributed-lag (partial-adjustment) model is used.	0.12	0.32
Lagged endogenous	=1 if a lagged-endogenous model is used.	0.05	0.21
Linear demand	=1 if the demand function is linear.	0.05	0.21
Double-log demand	=1 if the demand function is double-logarithmic.	0.93	0.26
Semi-log demand	=1 if the demand function is semi-logarithmic or Box-Cox (reference category).	0.01	0.11
<i>Geography and context</i>			
United States	=1 if the estimate is for the United States.	0.24	0.43
Europe	=1 if the estimate is for a European country.	0.28	0.45
Other country	=1 if the estimate is for a country outside the US and Europe (reference category).	0.49	0.50
Daylight hours	The average length of the longest day for the country under examination (source: Moesen, 2010).	14.64	1.88
Population	The logarithm of the population of the country or entity (source: World Bank, 2025).	18.21	1.79
<i>Publication characteristics</i>			
Publication year	The logarithm of one plus the number of years since the earliest study in the sample.	4.09	0.22
Impact factor	The logarithm of one plus the journal's RePEc recursive impact factor; 0 for unpublished work.	0.40	0.78
Citations	The logarithm of one plus the study's mean annual Google Scholar citation count.	1.08	0.99

Notes: Mean and standard deviation over the 723 long-run headline estimates. The horizon indicators are constant on this subsample and drop from the long-run averaging. Binary indicators are reported as means (shares); the elasticity and its standard error are the 1%-winsorized values.

*What the averaging shows.* Four features reinforce the body. First, publication selection is the most robust regularity in the corpus: the standard-error term enters every model in every subsample (PIP = 1.00) with a negative posterior mean that grows with the horizon (−0.83 integrated, −0.85 short run, −1.07 long run), the funnel asymmetry of Table 2 recovered as a model-averaged coefficient that survives against every moderator at once. Second, and most telling for the flexibility premise, the best-identified and most technology-rich settings are, conditional on everything else, the *least* price-responsive: in the short-run averaging the design-based tier (+0.26, PIP = 1.00, and OLS-significant) is robustly included and pulls the

Table 17: Bayesian model averaging of the price elasticity: integrated headline sample

	Bayesian model averaging (UIP $g$ -prior, dilution)			OLS, two-way clustered (study $\times$ database)			FMA (smoothed AIC)	
	P. mean	P. SD	PIP	Coef.	SE	$p$	Coef.	Wt.
Constant	-0.476	–	1.000	-0.938	0.257	0.000	-0.661	1.000
Standard error	-0.830	0.034	1.000	-0.830	0.100	0.000	-0.834	1.000
<i>Horizon</i>								
Short run	0.114	0.017	1.000	0.096	0.034	0.005	0.114	1.000
Long run	-0.103	0.021	1.000	-0.122	0.039	0.002	-0.101	1.000
<i>Estimate type</i>								
Converted from Hicksian	-0.000	0.002	0.009	-0.011	0.028	0.687	-0.000	0.003
<i>Identification</i>								
Design-based	0.160	0.038	0.996	0.122	0.070	0.079	0.148	0.999
Instrumented	-0.000	0.004	0.011	-0.022	0.038	0.557	-0.000	0.001
Panel FE / structural	0.076	0.018	0.994	0.058	0.030	0.054	0.072	1.000
<i>Estimate aggregation</i>								
Aggregation: country	0.000	0.002	0.007	-0.009	0.028	0.739	0.000	0.001
Aggregation: micro	-0.022	0.031	0.381	-0.039	0.040	0.337	-0.018	0.333
<i>Sector</i>								
Residential demand	-0.012	0.020	0.291	-0.029	0.028	0.309	-0.020	0.510
Industrial demand	0.000	0.003	0.013	0.002	0.036	0.963	0.000	0.002
<i>Data characteristics</i>								
Time-series data	0.002	0.010	0.065	0.022	0.032	0.494	0.002	0.064
Cross-section data	-0.166	0.028	1.000	-0.145	0.046	0.002	-0.152	1.000
Yearly data	-0.125	0.018	1.000	-0.108	0.033	0.001	-0.127	1.000
Data span	0.005	0.011	0.186	0.019	0.021	0.362	0.021	0.690
Study size	-0.017	0.004	0.998	-0.018	0.006	0.005	-0.018	1.000
<i>Price and tariff</i>								
Average price	-0.000	0.002	0.008	0.009	0.034	0.784	-0.000	0.002
Marginal price	0.005	0.015	0.106	0.087	0.055	0.116	0.049	0.874
Increasing-block tariff	0.000	0.001	0.005	0.024	0.035	0.489	0.000	0.000
Decreasing-block tariff	-0.174	0.024	1.000	-0.142	0.054	0.008	-0.180	1.000
Time-of-use tariff	0.000	0.003	0.011	0.039	0.046	0.404	0.002	0.046
<i>Demand controls</i>								
Control: demographics	-0.068	0.015	0.999	-0.057	0.028	0.039	-0.058	1.000
Control: temperature	-0.000	0.002	0.007	-0.012	0.025	0.618	-0.000	0.001
Control: appliance stock	-0.004	0.014	0.107	-0.032	0.034	0.345	-0.022	0.465
Control: other fuels	-0.074	0.016	1.000	-0.061	0.028	0.029	-0.070	1.000
<i>Model specification</i>								
Reduced form	0.013	0.021	0.328	0.041	0.029	0.157	0.025	0.703
Static model	-0.000	0.003	0.012	-0.041	0.035	0.241	-0.000	0.011
ARDL	0.001	0.010	0.028	0.050	0.043	0.251	0.001	0.023
Lagged endogenous	0.101	0.020	1.000	0.083	0.036	0.020	0.096	1.000
Linear demand	0.010	0.023	0.179	0.049	0.052	0.348	0.037	0.631
Double-log demand	-0.004	0.013	0.086	-0.005	0.041	0.907	-0.002	0.055
<i>Geography and context</i>								
United States	0.037	0.030	0.669	0.054	0.034	0.111	0.029	0.559
Europe	0.002	0.009	0.039	0.043	0.041	0.295	0.012	0.220
Daylight hours	0.019	0.005	0.995	0.018	0.007	0.009	0.019	0.999
Population	0.004	0.007	0.303	0.014	0.010	0.149	0.011	0.739
<i>Publication characteristics</i>								
Publication year	0.001	0.007	0.017	0.065	0.050	0.192	0.006	0.078
Impact factor	0.035	0.027	0.673	0.031	0.024	0.188	0.027	0.615
Citations	0.018	0.021	0.469	0.019	0.021	0.351	0.020	0.605
Observations	3,324			3,324			3,324	
Studies / databases	366 / 344							

*Notes:* The response variable is the 1%-winsorized own-price elasticity of electricity demand. *Bayesian model averaging* (left) uses the unit-information  $g$ -prior with the dilution model prior, which penalizes collinearity by down-weighting models whose regressor correlation matrix is close to singular; P. mean, P. SD and PIP are the posterior mean, posterior standard deviation and posterior inclusion probability. *OLS, two-way clustered* (middle) regresses the elasticity on the same moderators with standard errors clustered simultaneously by study and by underlying database. *FMA* (right) is a frequentist model average with smoothed-AIC weights over the models visited by the sampler, reporting the model-averaged coefficient and its inclusion weight. The standard error of the estimate absorbs residual publication selection. Scale-laden moderators enter in logs; reference categories are omitted. Variables are defined in Table 14.

Table 18: Bayesian model averaging of the short-run price elasticity

	Bayesian model averaging (UIP $g$ -prior, dilution)			OLS, two-way clustered (study $\times$ database)			FMA (smoothed AIC)	
	P. mean	P. SD	PIP	Coef.	SE	$p$	Coef.	Wt.
Constant	-0.448	–	1.000	-0.773	0.366	0.035	-0.452	1.000
Standard error	-0.847	0.045	1.000	-0.862	0.139	0.000	-0.835	1.000
<i>Estimate type</i>								
Converted from Hicksian	0.033	0.038	0.480	0.055	0.034	0.107	0.062	0.966
<i>Identification</i>								
Design-based	0.258	0.040	1.000	0.258	0.095	0.006	0.249	1.000
Instrumented	0.001	0.005	0.018	0.057	0.049	0.248	0.005	0.124
Panel FE / structural	0.067	0.029	0.910	0.086	0.037	0.019	0.064	0.991
<i>Estimate aggregation</i>								
Aggregation: country	-0.000	0.002	0.006	-0.038	0.031	0.211	-0.000	0.019
Aggregation: micro	-0.020	0.032	0.315	-0.023	0.042	0.580	-0.031	0.526
<i>Sector</i>								
Residential demand	-0.014	0.030	0.224	-0.039	0.034	0.243	-0.019	0.358
Industrial demand	-0.013	0.032	0.165	-0.070	0.046	0.127	-0.021	0.322
<i>Data characteristics</i>								
Time-series data	0.006	0.015	0.143	0.028	0.035	0.416	0.007	0.206
Cross-section data	-0.000	0.004	0.009	-0.006	0.071	0.935	-0.000	0.018
Yearly data	-0.019	0.031	0.316	-0.068	0.040	0.086	-0.050	0.864
Data span	0.003	0.009	0.087	0.019	0.024	0.426	0.007	0.300
Study size	-0.001	0.004	0.110	-0.012	0.007	0.109	-0.003	0.308
<i>Price and tariff</i>								
Average price	-0.000	0.004	0.017	-0.036	0.035	0.305	-0.000	0.025
Marginal price	0.000	0.003	0.009	0.006	0.059	0.919	0.000	0.014
Increasing-block tariff	-0.000	0.004	0.014	-0.035	0.033	0.282	-0.001	0.027
Decreasing-block tariff	0.000	0.002	0.005	0.018	0.049	0.717	0.000	0.003
Time-of-use tariff	0.066	0.045	0.735	0.056	0.063	0.370	0.062	0.819
<i>Demand controls</i>								
Control: demographics	-0.064	0.023	0.953	-0.052	0.034	0.125	-0.055	0.989
Control: temperature	-0.000	0.002	0.007	-0.015	0.027	0.583	-0.000	0.024
Control: appliance stock	-0.084	0.029	0.956	-0.068	0.043	0.113	-0.085	1.000
Control: other fuels	-0.010	0.020	0.227	-0.018	0.032	0.583	-0.024	0.595
<i>Model specification</i>								
Reduced form	0.060	0.025	0.918	0.084	0.030	0.005	0.073	1.000
Static model	-0.133	0.023	1.000	-0.122	0.047	0.009	-0.133	1.000
ARDL	0.000	0.005	0.008	0.014	0.038	0.710	0.000	0.006
Lagged endogenous	-0.001	0.006	0.018	-0.024	0.041	0.552	-0.000	0.006
Linear demand	0.056	0.049	0.626	0.145	0.097	0.136	0.091	0.988
Double-log demand	0.001	0.010	0.025	0.057	0.065	0.386	0.003	0.050
<i>Geography and context</i>								
United States	-0.000	0.002	0.007	-0.004	0.044	0.919	-0.000	0.011
Europe	-0.000	0.002	0.007	-0.027	0.047	0.575	-0.000	0.015
Daylight hours	0.020	0.005	0.997	0.024	0.006	0.000	0.021	1.000
Population	0.000	0.000	0.006	0.009	0.011	0.426	0.000	0.011
<i>Publication characteristics</i>								
Publication year	0.000	0.005	0.012	0.032	0.084	0.705	0.001	0.018
Impact factor	-0.000	0.001	0.005	-0.009	0.021	0.678	-0.000	0.011
Citations	0.000	0.002	0.012	0.010	0.036	0.774	0.001	0.033
Observations	1,647			1,647			1,647	
Studies / databases	226 / 218							

Notes: As in Table 17, estimated on the 1,647 short-run headline estimates; the horizon indicators are constant here and drop out. Variables are defined in Table 15.

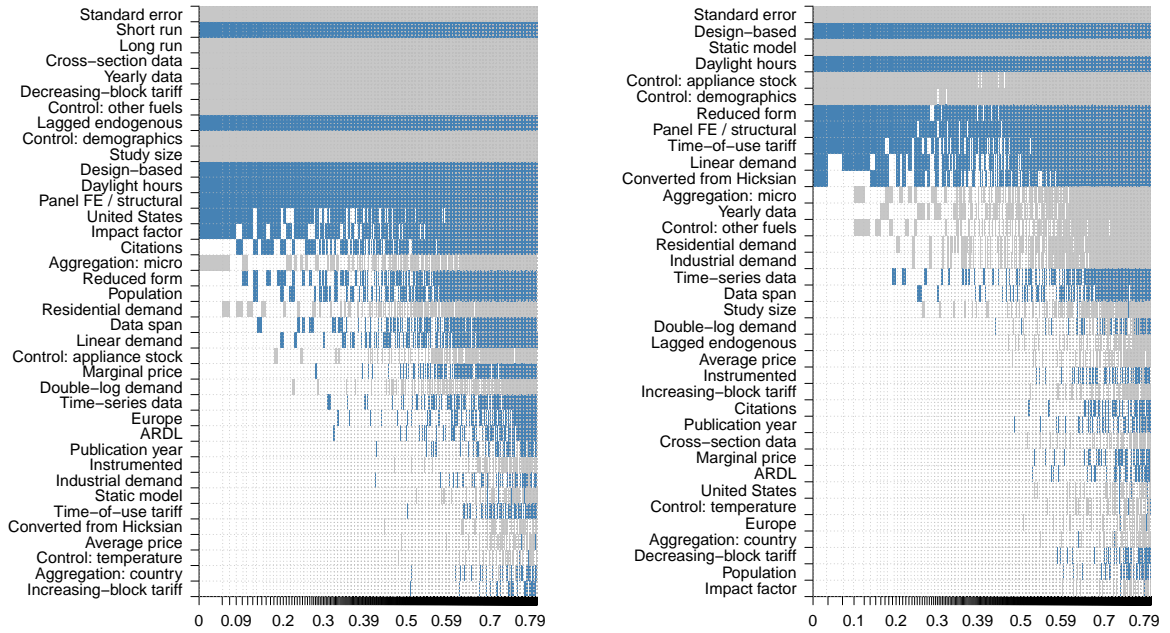
Table 19: Bayesian model averaging of the long-run price elasticity

	Bayesian model averaging (UIP $g$ -prior, dilution)			OLS, two-way clustered (study $\times$ database)			FMA (smoothed AIC)	
	P. mean	P. SD	PIP	Coef.	SE	$p$	Coef.	Wt.
Constant	-0.507	–	1.000	-1.525	0.839	0.069	-0.959	1.000
Standard error	-1.072	0.069	1.000	-1.085	0.138	0.000	-1.072	1.000
<i>Estimate type</i>								
Converted from Hicksian	-0.187	0.043	0.995	-0.135	0.067	0.045	-0.207	1.000
<i>Identification</i>								
Design-based	0.001	0.019	0.006	-0.260	0.191	0.174	0.000	0.001
Instrumented	-0.028	0.057	0.214	-0.102	0.077	0.185	-0.047	0.375
Panel FE / structural	0.088	0.070	0.657	0.079	0.082	0.337	0.087	0.638
<i>Estimate aggregation</i>								
Aggregation: country	0.000	0.004	0.007	0.022	0.063	0.729	0.000	0.014
Aggregation: micro	-0.000	0.005	0.006	0.068	0.105	0.518	0.000	0.001
<i>Sector</i>								
Residential demand	-0.000	0.004	0.006	0.044	0.066	0.499	-0.000	0.002
Industrial demand	0.011	0.036	0.105	0.114	0.076	0.133	0.031	0.337
<i>Data characteristics</i>								
Time-series data	0.000	0.003	0.005	-0.002	0.075	0.983	0.000	0.001
Cross-section data	-0.084	0.120	0.373	-0.064	0.114	0.572	-0.062	0.398
Yearly data	-0.004	0.022	0.042	-0.071	0.100	0.477	-0.003	0.033
Data span	0.047	0.043	0.582	0.062	0.046	0.178	0.073	0.933
Study size	0.000	0.001	0.007	-0.012	0.019	0.521	-0.000	0.019
<i>Price and tariff</i>								
Average price	0.000	0.003	0.005	0.099	0.110	0.366	0.000	0.001
Marginal price	0.001	0.010	0.009	0.247	0.173	0.154	0.007	0.054
Increasing-block tariff	0.000	0.004	0.006	-0.054	0.084	0.519	0.000	0.001
Decreasing-block tariff	-0.001	0.016	0.012	-0.028	0.149	0.850	-0.000	0.004
Time-of-use tariff	-0.001	0.014	0.009	0.010	0.183	0.957	-0.000	0.003
<i>Demand controls</i>								
Control: demographics	-0.001	0.011	0.018	-0.056	0.075	0.455	-0.000	0.004
Control: temperature	0.000	0.003	0.005	-0.084	0.059	0.156	-0.000	0.008
Control: appliance stock	-0.175	0.072	0.918	-0.151	0.059	0.011	-0.158	0.994
Control: other fuels	-0.000	0.004	0.007	-0.051	0.068	0.457	-0.000	0.008
<i>Model specification</i>								
Reduced form	0.000	0.005	0.008	0.071	0.080	0.373	0.001	0.011
Static model	-0.001	0.007	0.011	-0.069	0.090	0.447	-0.000	0.007
ARDL	0.006	0.030	0.055	0.110	0.066	0.096	0.081	0.625
Lagged endogenous	0.013	0.051	0.075	0.235	0.085	0.006	0.194	0.901
Linear demand	-0.015	0.055	0.080	-0.446	0.179	0.013	-0.152	0.785
Double-log demand	0.001	0.011	0.011	-0.200	0.100	0.047	0.000	0.005
<i>Geography and context</i>								
United States	0.033	0.061	0.260	0.313	0.096	0.001	0.166	0.996
Europe	0.083	0.073	0.624	0.206	0.078	0.008	0.176	1.000
Daylight hours	0.000	0.001	0.009	-0.012	0.016	0.454	-0.000	0.012
Population	0.000	0.001	0.006	-0.007	0.020	0.704	-0.000	0.014
<i>Publication characteristics</i>								
Publication year	0.011	0.050	0.058	0.342	0.197	0.083	0.085	0.380
Impact factor	0.000	0.005	0.010	-0.016	0.048	0.745	0.000	0.009
Citations	0.015	0.029	0.226	0.065	0.033	0.048	0.031	0.535
Observations	723			723			723	
Studies / databases	151 / 143							

Notes: As in Table 17, estimated on the 723 long-run headline estimates; the horizon indicators are constant here and drop out. Variables are defined in Table 16.

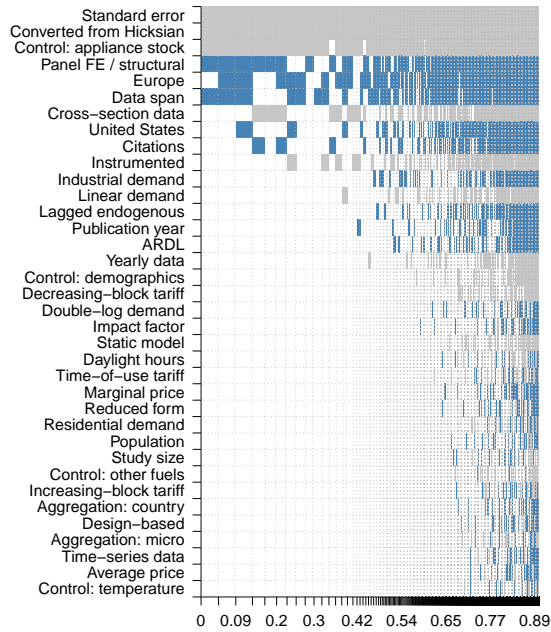
elasticity toward zero, while the time-of-use indicator (+0.07, PIP = 0.74, its OLS coefficient not distinguishable from zero) is weakly included and only sign-consistent, the model-averaging counterpart of the identification ladder and of the new-regime and time-of-use findings (Figure 4, Section 6). Third, the vintage dimension itself carries *negligible* weight: in the integrated model the publication-year moderator is almost never selected and the data span only rarely (PIP = 0.02 and 0.19), both entering with posterior means near zero, so once composition is averaged over there is no heterogeneity along the calendar axis a rising-flexibility world would light up, the averaging counterpart of the flat *publication* clock; the data mid-year itself is not among the model-averaging moderators, so this appendix speaks to the reporting clock rather than directly to the behavioral one. Fourth, the heterogeneity that *is* robust is compositional and methodological: data structure and frequency (cross-section  $-0.17$ , yearly  $-0.12$ , both PIP = 1.00), study size, the dynamic-adjustment indicators (lagged endogenous  $+0.10$  integrated, static model  $-0.13$  short run), decreasing-block tariffs ( $-0.18$ ) and a demographics control, exactly the study characteristics the ladder and horizon analyses hold fixed. The frequentist model average and the two-way clustered OLS agree in sign with the posterior means on every reliably included moderator, the load-bearing integrated-sample terms are stable across the four prior specifications (Table 20), while the horizon-specific positives (the short-run design-based and time-of-use coefficients) are checked only for sign in that integrated sweep, and the chains have converged (posterior-model-probability correlation 0.99; Table 21), with the regressor correlations (Figure 11) justifying the dilution prior.

Figure 10: Model inclusion in Bayesian model averaging



(a) Full headline sample

(b) Short-run estimates



(c) Long-run estimates

Notes: Each column is a regression model; column width is proportional to the model's posterior probability, cumulated along the horizontal axis. Rows are regressors, sorted by posterior inclusion probability. Blue means the variable enters with a positive coefficient, grey negative, white excluded. The estimation uses the unit-information  $g$ -prior and the dilution model prior.

Table 20: Model averaging under alternative priors (integrated sample)

Variable	A		B		C		D	
	Mean	PIP	Mean	PIP	Mean	PIP	Mean	PIP
Standard error	-0.830	1.000	-0.830	1.000	-0.830	1.000	-0.829	1.000
Short run	0.114	1.000	0.114	1.000	0.114	1.000	0.114	1.000
Long run	-0.103	1.000	-0.103	1.000	-0.103	1.000	-0.102	1.000
Cross-section data	-0.167	1.000	-0.166	1.000	-0.167	1.000	-0.161	1.000
Yearly data	-0.125	1.000	-0.125	1.000	-0.125	1.000	-0.124	1.000
Decreasing-block tariff	-0.174	1.000	-0.174	1.000	-0.174	1.000	-0.175	1.000
Control: other fuels	-0.074	1.000	-0.074	1.000	-0.074	1.000	-0.073	1.000
Lagged endogenous	0.101	1.000	0.100	1.000	0.101	1.000	0.098	1.000
Control: demographics	-0.068	0.999	-0.067	0.999	-0.068	1.000	-0.064	1.000
Study size	-0.017	0.998	-0.017	0.999	-0.017	0.997	-0.017	1.000
Design-based	0.160	0.997	0.160	0.998	0.160	0.996	0.155	0.998
Daylight hours	0.019	0.994	0.019	0.995	0.019	0.995	0.019	0.997
Panel FE / structural	0.076	0.993	0.075	0.993	0.076	0.992	0.074	0.999
Impact factor	0.035	0.679	0.034	0.670	0.035	0.669	0.033	0.698
United States	0.037	0.670	0.038	0.680	0.037	0.666	0.036	0.645
Citations	0.018	0.464	0.019	0.485	0.018	0.470	0.019	0.548
Aggregation: micro	-0.022	0.384	-0.023	0.392	-0.023	0.385	-0.024	0.438
Reduced form	0.013	0.321	0.014	0.355	0.013	0.320	0.018	0.484
Population	0.004	0.299	0.004	0.317	0.004	0.304	0.007	0.453
Residential demand	-0.012	0.290	-0.013	0.319	-0.012	0.288	-0.016	0.422
Data span	0.005	0.176	0.006	0.209	0.005	0.178	0.010	0.364
Linear demand	0.010	0.180	0.010	0.195	0.009	0.175	0.021	0.373
Control: appliance stock	-0.004	0.101	-0.004	0.104	-0.004	0.101	-0.010	0.238
Marginal price	0.004	0.098	0.005	0.116	0.004	0.096	0.020	0.399
Double-log demand	-0.003	0.082	-0.004	0.094	-0.004	0.085	-0.005	0.116
Time-series data	0.002	0.062	0.002	0.071	0.002	0.068	0.004	0.104
Europe	0.002	0.038	0.002	0.044	0.001	0.034	0.004	0.098
ARDL	0.001	0.026	0.002	0.029	0.001	0.025	0.003	0.046
Publication year	0.001	0.015	0.001	0.020	0.000	0.012	0.002	0.045
Industrial demand	0.000	0.011	0.000	0.011	0.000	0.013	0.000	0.015
Static model	-0.000	0.010	-0.000	0.012	-0.000	0.009	-0.001	0.024
Instrumented	-0.000	0.013	-0.000	0.015	-0.000	0.012	-0.000	0.007
Time-of-use tariff	0.000	0.006	0.000	0.011	0.000	0.009	0.000	0.012
Converted from Hicksian	-0.000	0.008	-0.000	0.009	-0.000	0.009	-0.000	0.011
Average price	-0.000	0.006	-0.000	0.010	-0.000	0.009	-0.000	0.012
Control: temperature	-0.000	0.008	-0.000	0.008	-0.000	0.004	-0.000	0.007
Aggregation: country	0.000	0.006	0.000	0.007	0.000	0.005	0.000	0.005
Increasing-block tariff	0.000	0.005	0.000	0.006	0.000	0.004	0.000	0.003

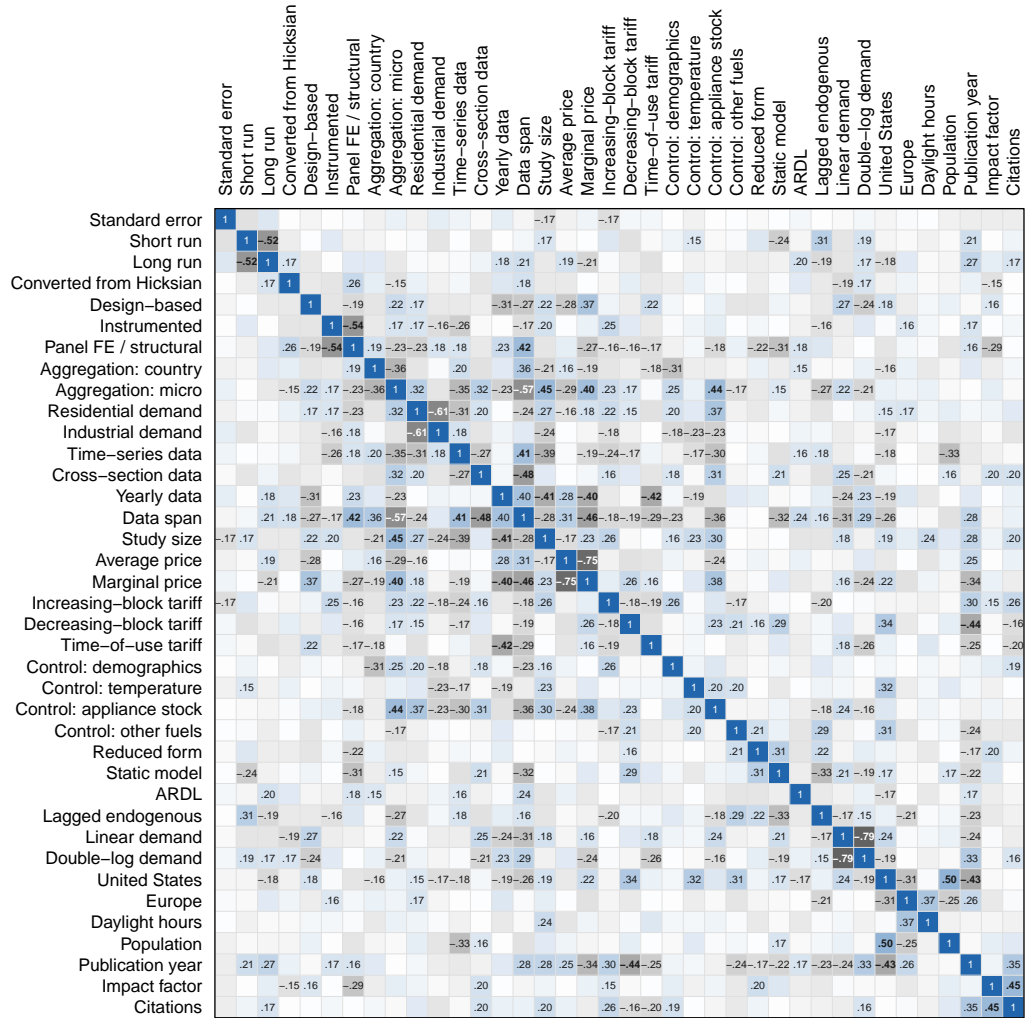
*Notes:* Posterior mean and inclusion probability (PIP) for the integrated model average under four prior specifications, ordered by baseline PIP. (A) unit-information  $g$ -prior, dilution model prior (baseline); (B) unit-information  $g$ -prior, uniform model prior; (C) BRIC  $g$ -prior, random (beta-binomial) model prior; (D) Hannan–Quinn  $g$ -prior, dilution model prior. The load-bearing moderators keep their sign and inclusion across priors.

Table 21: Model-averaging diagnostics

Model	Obs.	E[model size]	Models visited	Corr PMP	Shrinkage
Integrated	3,324	16.99	26,198	0.99	1.00
Short run	1,647	11.35	26,830	0.99	1.00
Long run	723	6.43	21,076	0.99	1.00

*Notes:* Birth–death MCMC ( $1.5 \times 10^5$  draws after  $5 \times 10^4$  burn-in) for each averaging. “E[model size]” is the sum of inclusion probabilities; “Corr PMP” is the correlation between analytical and MCMC posterior model probabilities (one indicates convergence); “Shrinkage” is  $g/(1+g)$ .

Figure 11: Regressor correlation matrix (collinearity diagnostic)



Notes: Pairwise correlations among the integrated-model regressors; cells with  $|r| < 0.15$  are left blank and  $|r| \geq 0.4$  are bold. Blue = positive, grey = negative. The visible clusters (among the identification, model-form and data-structure indicators) are why we average under the collinearity-penalizing dilution prior rather than reporting a single regression.

## F.1 Best-practice prediction, country by country

The model average also lets us read off a *best-practice* elasticity for each country: the response an ideally-designed study would report there. We predict from the integrated two-way-clustered regression of Table 17, evaluated not at the average study but at a best-practice profile  $x^*$ . Following the variable list, (i) the standard error is set to zero, stripping out the publication-selection component; (ii) the journal impact factor and citation count are set to their 95th percentiles, a well-cited outlet without letting a single citation outlier drive the prediction; (iii) the country-level moderators (the US and Europe indicators, daylight hours and log population) are set to *each country's own level*; (iv) the identification tier is set to best practice, with the design-based indicator on and the instrumented and panel-FE/structural indicators off (naive is the reference); (v) the demand equation is fully controlled (demographics, temperature, appliance stock, and substitute-fuel prices) and estimated on micro, household-level data; and (vi) every remaining moderator (model specification and functional form) is held at its sample mean, so the prediction describes a fully-specified, design-based, de-biased study conducted in that country. We report the prediction at both horizons: the short-run profile sets the short-run indicator on, the long-run profile the long-run indicator, with the other horizon dummies off.

The best-practice elasticity is the linear prediction  $r^* = x^{*'}\hat{\beta}$ , and because it is a linear combination of the estimated coefficients its sampling variance is  $x^{*'}\hat{V}x^*$ , where  $\hat{V}$  is the two-way (study  $\times$  database) clustered covariance; we report the Wald interval  $r^* \pm 1.96\sqrt{x^{*'}\hat{V}x^*}$ . Figure 12 shows the two predictions for the countries with at least thirty headline estimates, ranked by the short-run value; Table 22 lists every country with at least five. The message mirrors the pooled result: the best-practice short-run elasticity is small and negative everywhere (typically around  $-0.18$ ), in line with the corrected short-run consensus of the body, though its confidence interval still spans zero in fewer than half the countries; the long-run prediction is roughly twice as large and significantly negative in most countries, the horizon gradient reproducing country by country. Because only the four country-level moderators vary across predictions while all method, sector, and horizon slopes are held common, the compressed cross-country spread is imposed by the pooled model rather than demonstrated as genuine homogeneity. The cross-country spread is modest relative to the within-country uncertainty, so the audit's central finding travels: no country's best-practice *prediction* supports the large, rising responsiveness the flexibility premise assumes. For countries without an in-country design-based study, this value is a model extrapolation that sets the design-based indicator on, not an observed record.

Table 22: Best-practice price elasticity by country

Country	Short run		Long run	
	$r^*$	95% CI	$r^*$	95% CI
Argentina	-0.201	[-0.374, -0.028]	-0.419	[-0.588, -0.250]
Australia	-0.218	[-0.395, -0.042]	-0.437	[-0.608, -0.266]
Austria	-0.148	[-0.339, 0.044]	-0.366	[-0.546, -0.185]
Azerbaijan	-0.469	[-0.757, -0.182]	-0.687	[-0.970, -0.405]
Bangladesh	-0.189	[-0.365, -0.013]	-0.407	[-0.583, -0.232]
Brazil	-0.206	[-0.386, -0.026]	-0.424	[-0.603, -0.246]
Canada	-0.158	[-0.328, 0.011]	-0.377	[-0.542, -0.212]
Chile	-0.215	[-0.390, -0.039]	-0.433	[-0.603, -0.263]
China	-0.145	[-0.327, 0.038]	-0.363	[-0.548, -0.178]
Colombia	-0.235	[-0.416, -0.054]	-0.453	[-0.630, -0.277]
Costa Rica	-0.265	[-0.455, -0.076]	-0.483	[-0.664, -0.302]
Denmark	-0.124	[-0.313, 0.066]	-0.342	[-0.520, -0.164]

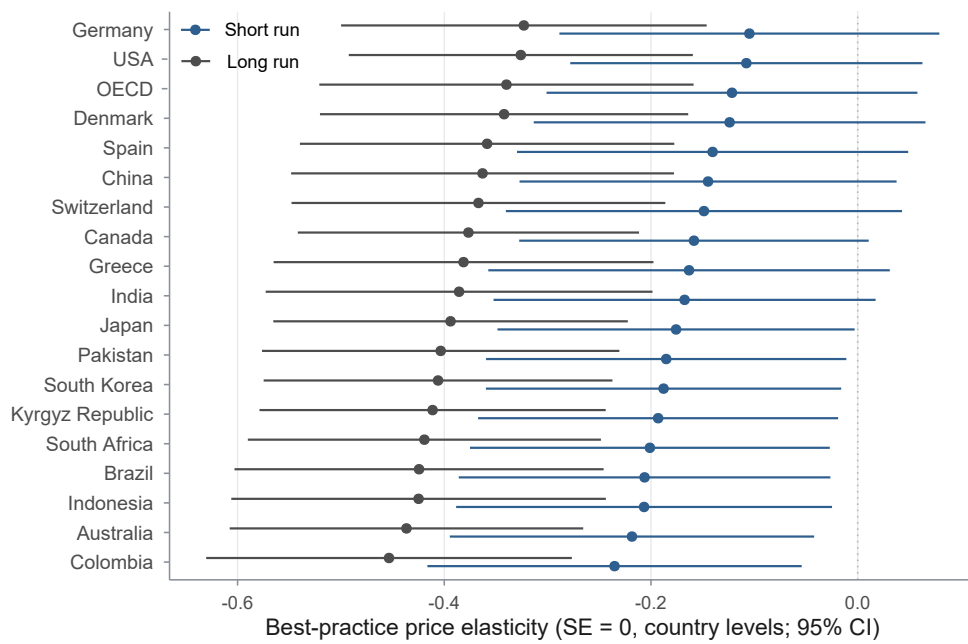
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Table 22 continued

Country	Short run		Long run	
	$r^*$	95% CI	$r^*$	95% CI
Egypt	-0.198	[-0.372, -0.024]	-0.416	[-0.588, -0.245]
EU	-0.091	[-0.278, 0.096]	-0.309	[-0.493, -0.126]
Finland	-0.064	[-0.254, 0.125]	-0.282	[-0.461, -0.104]
France	-0.123	[-0.309, 0.063]	-0.341	[-0.520, -0.162]
Germany	-0.105	[-0.289, 0.079]	-0.323	[-0.500, -0.146]
Ghana	-0.237	[-0.418, -0.056]	-0.455	[-0.631, -0.280]
Greece	-0.163	[-0.357, 0.031]	-0.381	[-0.565, -0.198]
Hong Kong	-0.236	[-0.416, -0.056]	-0.454	[-0.628, -0.280]
India	-0.167	[-0.352, 0.017]	-0.386	[-0.573, -0.199]
Indonesia	-0.207	[-0.389, -0.025]	-0.425	[-0.606, -0.244]
Iran	-0.188	[-0.362, -0.015]	-0.406	[-0.577, -0.236]
Ireland	-0.141	[-0.334, 0.052]	-0.359	[-0.540, -0.178]
Israel	-0.226	[-0.404, -0.047]	-0.444	[-0.616, -0.272]
Italy	-0.131	[-0.318, 0.057]	-0.349	[-0.529, -0.169]
Jamaica	-0.255	[-0.442, -0.068]	-0.473	[-0.652, -0.295]
Japan	-0.176	[-0.349, -0.003]	-0.394	[-0.565, -0.222]
Kazakhstan	-0.180	[-0.351, -0.009]	-0.398	[-0.564, -0.232]
Kuwait	-0.245	[-0.431, -0.059]	-0.463	[-0.641, -0.286]
Kyrgyz Republic	-0.193	[-0.367, -0.019]	-0.411	[-0.579, -0.244]
Malaysia	-0.242	[-0.424, -0.060]	-0.460	[-0.637, -0.283]
Mexico	-0.200	[-0.376, -0.025]	-0.418	[-0.592, -0.245]
Middle income	-0.076	[-0.274, 0.123]	-0.294	[-0.492, -0.095]
Mozambique	-0.458	[-0.743, -0.172]	-0.676	[-0.957, -0.395]
Netherlands	-0.122	[-0.308, 0.064]	-0.340	[-0.517, -0.164]
New Zealand	-0.218	[-0.397, -0.039]	-0.436	[-0.607, -0.265]
Niger	-0.247	[-0.430, -0.063]	-0.465	[-0.641, -0.289]
Nigeria	-0.217	[-0.396, -0.038]	-0.435	[-0.611, -0.259]
Norway	-0.089	[-0.278, 0.100]	-0.307	[-0.485, -0.130]
OECD	-0.122	[-0.301, 0.058]	-0.340	[-0.521, -0.159]
Pakistan	-0.185	[-0.360, -0.011]	-0.403	[-0.576, -0.231]
Peru	-0.231	[-0.411, -0.052]	-0.449	[-0.625, -0.274]
Philippines	-0.216	[-0.395, -0.038]	-0.435	[-0.610, -0.259]
Portugal	-0.161	[-0.355, 0.032]	-0.380	[-0.563, -0.196]
Saudi Arabia	-0.222	[-0.399, -0.045]	-0.441	[-0.612, -0.269]
South Africa	-0.201	[-0.375, -0.027]	-0.419	[-0.590, -0.248]
South Korea	-0.188	[-0.360, -0.016]	-0.406	[-0.575, -0.237]
Spain	-0.140	[-0.330, 0.049]	-0.359	[-0.540, -0.177]
Sri Lanka	-0.239	[-0.420, -0.058]	-0.457	[-0.633, -0.281]
Sweden	-0.078	[-0.263, 0.107]	-0.296	[-0.471, -0.121]
Switzerland	-0.149	[-0.340, 0.043]	-0.367	[-0.548, -0.186]
Taiwan	-0.217	[-0.394, -0.041]	-0.436	[-0.607, -0.264]
Tunisia	-0.215	[-0.391, -0.039]	-0.433	[-0.603, -0.263]
Turkey	-0.138	[-0.328, 0.051]	-0.357	[-0.538, -0.175]
Ukraine	-0.150	[-0.342, 0.041]	-0.369	[-0.552, -0.185]
United Kingdom	-0.099	[-0.282, 0.084]	-0.317	[-0.493, -0.142]
USA	-0.108	[-0.278, 0.063]	-0.326	[-0.492, -0.159]
Yemen	-0.237	[-0.417, -0.056]	-0.455	[-0.629, -0.280]

Notes: Best-practice short- and long-run predictions with 95% two-way (study  $\times$  database) clustered intervals, for the countries contributing at least five headline estimates, ordered alphabetically; a few rows are country aggregates (EU, OECD, Middle income) rather than individual countries. Country-level moderators are set to each country's own level; all other moderators follow the best-practice profile of Figure 12.

Figure 12: Best-practice price elasticity by country, short and long run



*Notes:* Best-practice prediction from the integrated regression of Table 17 (standard error set to zero, impact factor and citations at their 95th percentiles, country-level moderators at each country’s own level, identification at best practice (design-based tier on; instrumented and structural tiers off, naive reference), demand controls on and micro-level data, remaining moderators at sample means), with 95% intervals from the two-way (study × database) clustered covariance. Countries with at least thirty headline estimates, ranked by the short-run prediction.