

Does electricity scaling improve annual water-withdrawal estimates? Evidence from US Meta data centers

Jaivime Evaristo

Warnell School of Forestry and Natural Resources

University of Georgia, Athens, Georgia, USA

Email: evaristo@uga.edu

ORCID: 0000-0003-1387-229X

Peer review status

This is a non-peer-reviewed preprint submitted to EarthArXiv.

Later versions may differ. If the manuscript is accepted, the published version will be available through the Peer-reviewed Publication DOI link on this preprint's EarthArXiv page.

Does electricity scaling improve annual water-withdrawal estimates? Evidence from US Meta data centers

Jaivime Evaristo^{1*}

¹Warnell School of Forestry and Natural Resources, University of Georgia, Athens, Georgia, USA

*Correspondence evaristo@uga.edu

Abstract

Planners often need to estimate data-center water use before detailed operating data exist, and a common shortcut multiplies electricity use by a historical water intensity. The logic is appealing: computing produces heat, and removing heat often takes water. Yet whether this shortcut beats the simplest alternative, persistence (assuming that each facility withdraws what it withdrew the year before), remains an open question. Here we test three electricity-scaling rules against persistence, using operator-reported withdrawal and electricity for 15 US Meta data-center sites in 2016-2024, cross-checked across eleven report versions. In 64 year-to-year comparisons at 13 sites, persistence had the lowest pooled error (a weighted absolute percentage error of 30.9%, against 41.6-56.2%), even though the scaling rules were given each target year's observed electricity. All three scaling rules overestimated withdrawal on average. The ranking was not fixed, however: scaling by a site's own previous-year ratio had lower error in three of eight target years. And in an exploratory comparison, more history made the pooled rules worse, not better: because the withdrawal-to-electricity ratio fell every year from 2017 to 2023, histories that began in 2016 carried outdated ratios forward and raised errors on 2021-2024 targets by 5.4-7.0 percentage points. These retrospective results come from one operator and establish neither a universal ranking nor forecasting skill. But they set a simple bar for anyone converting electricity projections into water demand: test the conversion against last year's withdrawal, year by year as well as on average.

Keywords: data centers; water withdrawal; water intensity; persistence; disclosure; retrospective estimation

1 Introduction

Annual water-demand screening often requires estimates before detailed information on facility operations is available. Electricity use is a plausible basis for estimating data-center water use: computing and support systems generate heat that must be removed. But electricity use does not uniquely determine external water use, which also depends on operating conditions and accounting boundaries. Reviews and commentaries highlight incomplete disclosure and the limitations of average water-efficiency values [1,2]. Electricity scaling is therefore useful only if its assumptions are appropriate for the facility and year being estimated.

Existing studies address this estimation problem at several scales. Spatial footprint models combine electricity estimates with direct and indirect water factors [3]. Lei and Masanet model power and water usage effectiveness for different climates and cooling systems [4]; Sharma and colleagues document monthly water and energy use at a single data center [5]. Workload-level accounting shows that water use per unit of computational service depends on hardware and utilization as well as cooling [6]. Hourly water-efficiency datasets help characterize temporal variation, but modeled efficiency and reported facility withdrawal are different quantities [7].

Recent preprints use operator disclosures to develop national water and infrastructure scenarios [8] and to assess whether the assumptions behind water estimates are representative [9]. We stress that neither electricity scaling nor variation in water intensity is new.

A practical question, however, remains: for a facility with a withdrawal history, does electricity scaling estimate annual withdrawal more accurately than carrying forward the previous year's withdrawal? We compare four rules against the same reported withdrawals at named US Meta sites over eight target years. The targets are the same, but the rules use different information. Our contribution is an auditable empirical benchmark. We examine errors both in aggregate and by year, including years that reverse the overall ranking. We also check whether later disclosures revised the values being estimated. We do not evaluate machine-learning models, forecasts of electricity use, or water demand at genuinely new sites.

2 Data and methods

2.1 Disclosure panel and accounting boundary

We compiled facility electricity use and operational water withdrawal for named data-center sites from Meta's annual environmental disclosures. The 2025 Environmental Data Index, retrieved from its October 2025 URL on 14 October 2026, reports electricity in MWh (page F), withdrawal in megaliters (ML; page I), and reporting notes and methods (pages M and R) [10]. Each disclosure since 2020 also reports four or five earlier years, allowing us to extend the record to 2016 using disclosures dated from September 2020 to August 2024 [11-15]. For each site-year, we used the most recent disclosure that reported it (supplementary table S1). We converted withdrawal from cubic meters to ML ($1,000 \text{ m}^3 = 1 \text{ ML}$) and matched three sites reported under alternative place names using values that agreed within rounding in overlapping years. A July 2017 disclosure reported 2014-2016 withdrawal in gallons [16]. Later disclosures restated the 2015-2016 withdrawals in cubic meters, and we used these restatements. The primary panel includes all 15 named US sites for the nine calendar years 2016-2024. We included 2015, which only the September 2020 disclosure reports in cubic meters, in a sensitivity analysis. We excluded international sites and pooled rows for leased facilities, other facilities, and offices because their spatial or reporting boundaries differ. A named site may contain multiple buildings; these series specify neither a fixed building count nor a cooling configuration.

Our target is annual operational withdrawal within the operator's reporting boundary. It is not consumption, potable-only demand, cooling-only use, construction water, AI-attributable demand, or indirect water use from electricity generation. Each disclosure since 2020 excludes construction withdrawal from the operational total for its latest year—for example, 1,019 ML in 2024 [10]—but does not explain how construction water was treated in earlier years. The reporting methods allow utility records, metering, and estimation when activity data are incomplete; they do not identify how each facility-year value was obtained. These are therefore operator-reported values, not uniformly metered observations.

We retained every report version, preserving how it marked missing values, its numeric zeros, its source pages, and its units before conversion. We treated dashes, the words "not yet online", and a site's absence from a report as missing values, never as zeros. Of 135 possible site-years, 82 had numeric values for both withdrawal and electricity: three pairs contained zeros, and 79 were positive for both variables. The primary cohort required positive values for both variables at the same site in consecutive years. This yielded 64 transitions across 13 sites in target years 2017-

2024. We did not bridge missing years. The July 2023 indexes report all three zero withdrawals as 0 m^3 , so they are not rounded small values; a missing value is neither a zero nor a commissioning date. Let $W_{i,t}$ denote reported withdrawal (ML) and $E_{i,t}$ total facility electricity (MWh) at site i in year t . We call their ratio, $R_{i,t}$ in equation (1), the water intensity; it differs from water usage effectiveness (WUE), which uses IT energy [1,10]. Estimator coefficients are calculated in ML per MWh and reported in L/kWh, using the conversion in equation (1).

$$R_{i,t} = \frac{W_{i,t}}{E_{i,t}} \quad 1 \frac{\text{ML}}{\text{MWh}} = 1,000 \frac{\text{L}}{\text{kWh}} \quad (1)$$

2.2 Estimators and available information

Equations (2)-(5) define the four estimates of $W_{i,t}$, written $\hat{W}_{i,t}$ with superscripts P, S, H, and O for persistence, site ratio, pooled history, and other-site history. Persistence carries forward the site's previous-year withdrawal (equation (2)). Site ratio multiplies the target year's electricity by the site's previous-year water intensity, $R_{i,t-1}$ (equation (3)). The two pooled rules instead multiply it by the coefficients a_t and $a_{-i,t}$, respectively (equations (4) and (5)). Each coefficient divides summed withdrawal by summed electricity over a set of historical site-years (j, s), where j denotes a site and s a year (equations (6) and (7)). The set H_t contains every site-year before the target year with positive withdrawal and electricity ($s < t, W_{j,s} > 0, E_{j,s} > 0$); the set $H_{-i,t}$ is the same set without the target site ($j \neq i$). Neither coefficient is an unweighted average of site ratios or a previous-year fleet ratio.

$$\hat{W}_{i,t}^{\text{P}} = W_{i,t-1} \quad (2)$$

$$\hat{W}_{i,t}^{\text{S}} = W_{i,t-1} \frac{E_{i,t}}{E_{i,t-1}} \quad (3)$$

$$\hat{W}_{i,t}^{\text{H}} = a_t E_{i,t} \quad (4)$$

$$\hat{W}_{i,t}^{\text{O}} = a_{-i,t} E_{i,t} \quad (5)$$

$$a_t = \frac{\sum_{(j,s) \in H_t} W_{j,s}}{\sum_{(j,s) \in H_t} E_{j,s}} \quad (6)$$

$$a_{-i,t} = \frac{\sum_{(j,s) \in H_{-i,t}} W_{j,s}}{\sum_{(j,s) \in H_{-i,t}} E_{j,s}} \quad (7)$$

We scored all four rules against the same 64 targets, but the rules use different information. Persistence uses only the site's prior-year withdrawal. The site-ratio rule also uses prior-year and observed target-year electricity; the pooled rules use broader historical data and observed target-year electricity. We therefore compare conditional retrospective estimates, not operational forecasts based on identical information. Excluding the target site's history tests cross-site scaling, but inclusion in the cohort still requires prior observations at that site. This comparison does not validate estimates at newly observed facilities or across operators.

Historical coefficients used only records from 2016 onward and before the target year. This chronological filter would not, by itself, reproduce the information available at the time: the panel uses the latest disclosure for each site-year, which could include later revisions. We therefore compared every value with each earlier disclosure that reported it. We counted a difference as a revision only if it exceeded half the sum of the two rounding units—for example, 1,000 MWh when both values are reported to the nearest 1,000 MWh. We also repeated the analysis with first-release values, the first value published since September 2020 for each site-year. We neither tuned coefficients against the targets nor fitted a statistical learner.

2.3 Error metrics and exploratory checks

Let C be the common set of $N = 64$ transitions scored by every estimator, each identified by its site and target year, (i, t) . The signed error $e_{i,t}$ is the prediction minus reported withdrawal (equation (8)), where $\hat{W}_{i,t}$ stands for any of the four estimates. Weighted absolute percentage error (WAPE; equation (9)) was our primary metric. We also calculated mean absolute error (MAE; equation (10)) and mean signed error (equation (11)), both in ML. All methods share the same WAPE denominator: total reported withdrawal of 10,019 ML. We assessed errors by target year and separately for target years 2017-2020 and 2021-2024. We also compared methods within each transition. WAPE gives greater weight to larger water volumes; it does not describe the mean percentage error for a typical facility. Supplementary summaries report equal-site averages and per-site results (supplementary section S4 and table S3).

$$e_{i,t} = \hat{W}_{i,t} - W_{i,t} \quad (8)$$

$$\text{WAPE} = 100 \frac{\sum_{(i,t) \in C} |e_{i,t}|}{\sum_{(i,t) \in C} W_{i,t}} \quad (9)$$

$$\text{MAE} = \frac{1}{N} \sum_{(i,t) \in C} |e_{i,t}| \quad (10)$$

$$\text{Mean signed error} = \frac{1}{N} \sum_{(i,t) \in C} e_{i,t} \quad (11)$$

This analysis is retrospective: we specified the rules, eligibility criteria, and metrics after the results of a preliminary analysis of 2020-2024 (hereafter, the 2020-2024 analysis) were known. We added the 2016-2019 records after the 2020-2024 results were known, without changing any estimator, eligibility rule, or metric. The added years supply targets for 2017-2020 and lengthen the pooled histories. In exploratory influence checks, we omitted each site's errors in turn from pooled scoring without refitting the historical coefficients. A separate reweighting diagnostic sampled the 13 sites with replacement 2,000 times (seed 20261014). Each repetition retained the sampled sites' eligible transitions and original predictions and calculated paired WAPE differences. The 2.5th-97.5th percentile ranges describe site reweighting with predictions held fixed. They are not calibrated confidence or prediction intervals for new facilities, future years, reporting error, or industry performance. We also used an exact site-level randomization test, chosen after the 2020-2024 results were known. Assuming that the two methods' absolute errors are exchangeable within each site, we reversed the sign of each site's summed absolute-error difference in all 2^K combinations for the K sites scored ($2^{13} = 8,192$ for all 13 sites). We report the two-sided share of WAPE differences at least as large in magnitude as the observed

difference. After the 2016-2024 results were known, we chose to apply the same test and reweighting to the difference between pooled histories beginning in 2016 and in 2020, scored on the same 2021-2024 targets. Every site’s difference in that comparison stems from the same added years of history. Because these differences share a common cause, we interpret this test as descriptive.

In a sensitivity analysis, we allowed nonnegative withdrawal while requiring positive electricity in both adjacent years and applied the same eligibility rule to historical pooling. This added two transitions and changed the historical intensity denominators; the positive-only analysis remained primary. To assess the effect of rounding in the reports, we perturbed every positive value uniformly within its rounding interval in 10,000 repetitions. We calculated logarithmic changes in withdrawal only for positive values. The accompanying materials include code, cleaning rules, eligibility criteria, and predictions. Twenty-two integrity tests check the source files against the versions analyzed, agreement between two PDF text extractors, missing values and zeros, units, site names, revisions, adjacency, chronological filtering, target-site history removal, scoring, and the comparison of history start years. They also check that the analysis, run on the October 2025 index alone, reproduces the 2020-2024 analysis’s transition and pair counts, WAPE denominator, and pooled WAPEs to 0.1 percentage point.

3 Results

Persistence had the lowest WAPE across all 64 transitions: 30.9%, compared with 41.6% for site ratio, 52.3% for pooled history, and 56.2% for other-site history (table 1; figure 1A). MAE ranged from 48.3 ML for persistence to 88.0 ML for other-site history. Persistence underestimated withdrawal on average (mean signed error -24.6 ML); the ratio rules overestimated it by 28.5-44.3 ML. The ranking was unchanged between the two halves of the record, which share no target years. For the four rules in the same order, WAPEs were 31.3%, 42.1%, 42.8%, and 49.5% in target years 2017-2020, and 30.7%, 41.3%, 56.9%, and 59.4% in 2021-2024. Site ratio and pooled history differed by less than 1 percentage point in 2017-2020. When pooled histories began in 2020, as in the 2020-2024 analysis, WAPEs for pooled history and other-site history were 49.9% and 54.0% on the 2021-2024 targets. These errors alone do not establish whether any method is accurate enough for a particular planning decision.

Table 1. Pooled errors for 64 common targets. Signed error is prediction minus reported withdrawal. MAE and mean signed error are in ML; WAPE is expressed as a percentage. Target years are 2017-2024, and pooled histories begin in 2016. Estimator definitions and their distinct data requirements are given in section 2.2.

Estimator	WAPE (%)	MAE (ML)	Mean signed error (ML)
Persistence	30.9	48.3	-24.6
Site ratio	41.6	65.1	+28.5
Pooled history	52.3	81.8	+44.3
Other-site history	56.2	88.0	+44.2

The pooled ranking concealed year-to-year differences (figure 1B; supplementary table S2). Persistence had lower WAPE than site ratio in five of eight target years (2019-2023); site ratio

had lower WAPE in 2017, 2018, and 2024. The 2024 reversal involved the most transitions. Across that year's 13 transitions, WAPE was 32.7% for site ratio, 39.8% for persistence, 44.6% for pooled history, and 46.7% for other-site history. Site ratio also had the smaller absolute error in 9 of the 13 transitions. The 2017 and 2018 reversals each rested on four transitions, with the smaller absolute errors split evenly between the two rules. Across all years, site ratio had the smaller absolute error in 27 of the 64 transitions. The eligible sites differed among years.

Persistence retained the lowest WAPE in every pooled scoring check that omitted one site's errors and in the equal-site summaries. The reweighting ranges were nevertheless wide: pooled WAPE spanned 25.3-38.5% for persistence and 31.6-58.8% for site ratio between the specified percentiles. The paired WAPE difference (site ratio minus persistence) was 10.7 percentage points, with a conditional reweighting range of 3.9-23.2 points (supplementary table S4). The exact site-level randomization test asks how often swapping the two rules' absolute errors within sites would produce a WAPE difference at least this large in magnitude. Only 76 of the 8,192 sign combinations matched or exceeded the observed difference in magnitude ($p = 0.009$). The 2021-2024 targets alone gave $p = 0.054$; the seven sites scored in 2017-2020 gave $p = 0.17$. Adding the earlier years lowered the p -value without changing the direction of the difference.

The ranking was also unchanged in the data-sensitivity analyses. Including zero withdrawals (66 transitions) gave WAPes of 31.1%, 41.8%, 52.6%, and 56.5% for persistence, site ratio, pooled history, and other-site history, respectively. Starting the record in 2015 (67 transitions) lowered each pooled WAPE by 0.2-1.2 percentage points; the randomization test for this analysis gave $p = 0.017$. Across 341 comparisons among report versions with distinct site tables, no US site value reported in more than one disclosure since September 2020 changed beyond rounding. Using first-release values changed pooled WAPes by at most 0.03 points; also replacing the restated 2016 withdrawals with those first reported in gallons changed them by as much as 0.61 points. In 10,000 random perturbations within the rounding intervals, no pooled WAPE changed by more than 0.31 points.

Changes in water intensity provide descriptive context, not evidence of a mechanism.

Withdrawal increased in 19 of the 44 transitions with declining withdrawal-to-electricity ratios. At the four sites with positive withdrawal and electricity in both 2016 and 2024, summed withdrawal tripled from 294 to 897 ML (+205.1%), while electricity rose almost fivefold from 1,024,000 to 4,958,242 MWh (+384.2%). Their aggregate ratio declined by 37.0%. The eight sites reporting in both 2020 and 2024 show the same pattern (withdrawal +14.0%, electricity +85.3%, ratio -38.5%). These changes and ratios are calculated from sums for selected matched subsets. They are neither averages of site-level changes nor sector-wide estimates, and they establish neither causal efficiency improvements nor rebound effects (supplementary figures S1-S2).

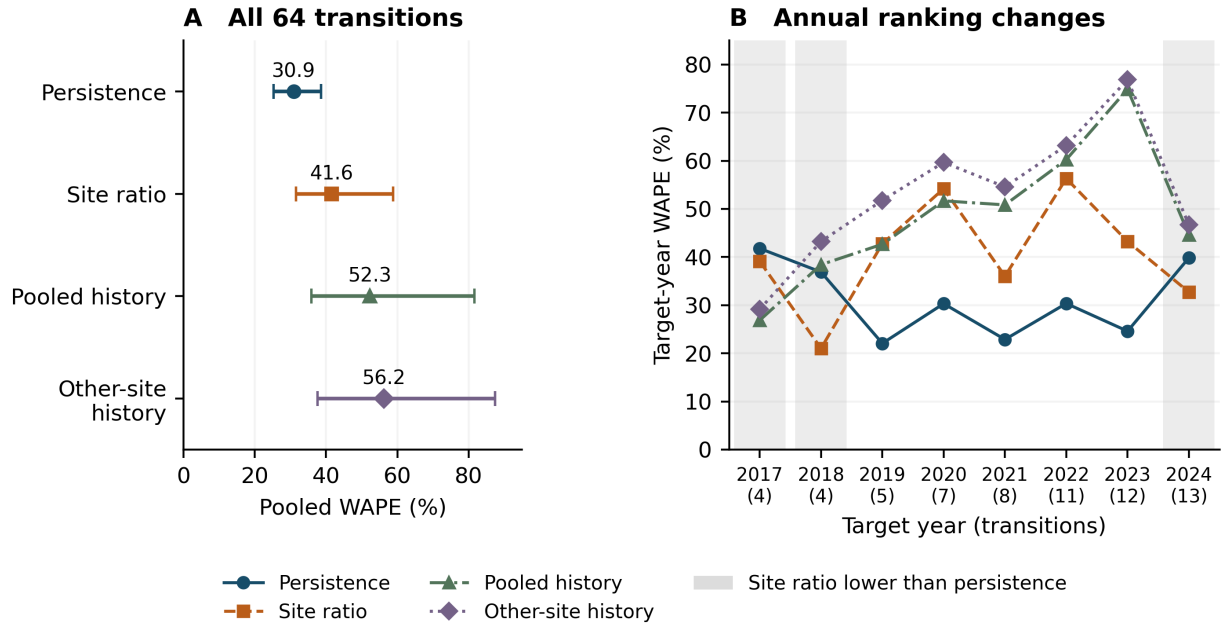


Figure 1. Errors in conditional retrospective withdrawal estimates for the same 64 positive adjacent-year transitions at 13 US Meta reporting sites, target years 2017-2024. (A) pooled WAPE; horizontal lines show the 2.5th-97.5th percentiles from 2,000 reweightings of whole-site error blocks with predictions held fixed. These are not calibrated confidence or prediction intervals. **(B)** target-year WAPE; counts in parentheses give the number of transitions in each year, with site coverage varying among years. Shading marks the years in which site ratio has lower WAPE than persistence (2017, 2018, and 2024). Ratio rules use observed target-year total facility electricity and the historical intensities defined in section 2.2, with histories beginning in 2016. Sources: the most recent disclosure reporting each year [10,12,14,15]. Panel A uses a pooled withdrawal denominator of 10,019 ML; panel B uses the sum of eligible target withdrawals for each year.

4 Discussion

The rule that used the least information did best. Persistence knew only the previous year's withdrawal, yet it beat every electricity-scaling rule, overall and in each half of the record, although the scaling rules were given the electricity used in the very year being estimated. This does not mean that electricity is uninformative or that physically informed models fail; the result applies only to the rules tested. It means that the ratio of withdrawal to electricity changed faster than these rules assumed. At the four sites reporting in both 2016 and 2024, electricity rose almost fivefold while withdrawal tripled. A historical intensity applied to such growth carries yesterday's ratio into today's facility, so the scaling rules overestimated withdrawal on average, whereas persistence, which lags growth, underestimated it (table 1). The two kinds of rule err in opposite directions; on this record, the lag was the smaller error.

Two growth factors decide the contest between persistence and site ratio. Let $g_{W,i,t}$ and $g_{E,i,t}$ be the year-to-year growth factors of withdrawal and electricity (equation (12)). For the positive cohort, the absolute errors of persistence and site ratio, $|e_{i,t}^P|$ and $|e_{i,t}^S|$, then depend only on these growth factors and the previous year's withdrawal (equation (13)).

$$g_{W,i,t} = \frac{W_{i,t}}{W_{i,t-1}} \quad g_{E,i,t} = \frac{E_{i,t}}{E_{i,t-1}} \quad (12)$$

$$|e_{i,t}^P| = W_{i,t-1} |g_{W,i,t} - 1| \quad |e_{i,t}^S| = W_{i,t-1} |g_{W,i,t} - g_{E,i,t}| \quad (13)$$

Persistence wins when withdrawal changes little from one year to the next; site ratio wins when withdrawal grows in step with electricity. A falling ratio alone does not settle the matter, because withdrawal can rise while the ratio falls, as it did in 19 of the 44 transitions with declining ratios. At these sites, withdrawal growth more often lay closer to 1 than to electricity growth: site ratio had the smaller absolute error in only 27 of the 64 transitions. Four of its five largest overestimates occurred in a site's first scored transition, when electricity rose three- to elevenfold while withdrawal rose far less or fell.

The same arithmetic produced the year-to-year reversals. Site ratio was more accurate in 2017, 2018, and 2024, the last on 13 transitions, more than in any other year. The within-year randomization tests cannot distinguish these reversals from chance ($p = 1.00, 0.88$, and 0.12 ; with four sites, 2017 and 2018 allow no p below 0.125), but they show how a pooled ranking can mislead: a rule chosen for its average performance may still lose in the year that matters. Errors need to be assessed by year as well as in aggregate.

More history did not help the pooled rules either. Beginning their histories in 2016 rather than 2020 raised WAPE on the 2021-2024 targets from 49.9% to 56.9% for pooled history and from 54.0% to 59.4% for other-site history. These contrasts are exploratory, and every site's difference arises from the same added years, so we read their randomization tests ($p = 0.037$ and 0.10) descriptively. The reason is arithmetic. A cumulative ratio of sums is an electricity-weighted average of earlier ratios, and those were higher: summed across sites with positive values, withdrawal divided by electricity was 0.29-0.33 L/kWh in 2016-2019 against 0.16-0.27 L/kWh in 2020-2024. An index chained across sites reporting in consecutive years, and so not driven by sites entering or leaving the record, fell by 45% from 2016 to 2024 and in every year from 2017 to 2023 (supplementary table S5). From 2019 onward, the pooled coefficient exceeded the ratio realized in every target year, and in 2021-2024 the longer history raised it further. Long histories help when the quantity being estimated changes slowly relative to its year-to-year noise; here, each added year pulled the estimate toward intensities the sites had left behind.

The falling ratio fits operational evidence. Sharma et al. explain that fixed monthly water uses (windage, blowdown, and filtration backwash) become a smaller share of total use as computing load grows, lowering WUE [5]. Our data agree with this explanation but cannot confirm it: the withdrawal-to-total-electricity ratio carries no information on IT load, hardware productivity, cooling details, or utilization, so it cannot compare computational or cooling efficiency across sites. Climate- and cooling-specific efficiency models [4] and workload-normalized accounting [6] describe relationships that a pooled annual coefficient cannot resolve; we did not test those models and cannot claim to outperform them.

These findings bear on a wider practice of converting electricity into water. Footprint studies combine electricity estimates with water factors [3]; national scenarios use operator disclosures, including the 2025 Environmental Data Index [10], to project new water capacity [8]; and facility-scale projections [17] and coupled scenarios of cooling, electricity dispatch, and subbasin depletion [18] also link water to energy. We do not test those studies, whose targets and scales differ from ours. Every such conversion assumes something about how the ratio of water to

electricity will change. At these sites, assuming that a historical ratio would hold overestimated withdrawal, and a longer history made the overestimate larger. Our contribution is a way to test that assumption against reported withdrawal: an auditable comparison on common targets, built on eleven report versions (supplementary table S1) and examined year by year.

For annual screening at an existing site, persistence is therefore best treated as the baseline that any other rule must beat, not as a default answer. It lags real change: it underestimated withdrawal in years of rapid growth, including 2024, and its pooled error of 30.9% is still substantial, so it is no substitute for information on expansion or operational changes. The reversals are consistent with electricity scaling doing better under some conditions, but identifying those conditions requires independently supported predictors, not post hoc attribution.

Several limitations bound these conclusions. The sites come from one operator's disclosures, not a probability sample of data centers. The 64 transitions include repeated sites and shared adjacent-year endpoints, eight target years cannot characterize future variation, and only four to seven sites contribute to each of the four earliest target years. Requiring positive reports in consecutive years excludes first observations, gaps, and the three reported zero withdrawals. The reported values proved stable: where disclosures since September 2020 overlap, their US site values agree within rounding, and the only US revision we found was the restatement of the 2015-2016 withdrawals first reported in gallons. Yet campus expansion, changing boundaries, climate, cooling operation, and hardware may all affect these series, and we cannot separate their effects. Nor do we know the measurement status or errors of individual records, and rounding can affect whether a change is classified as an increase or a decrease. Neither site reweighting nor numerical integrity tests can recover this missing information.

The statistical checks have limits, too. Historical pooling creates dependence across sites, but the reweighting diagnostic and randomization test hold the coefficients fixed and treat sites as independent; they do not capture uncertainty from the full estimation procedure, shared year-specific shocks, or operators outside the sample. More fundamentally, this is a retrospective comparison, not a forecast. The scaling rules used observed target-year electricity, and the analysis uses records published after the target years; the earliest disclosure we found with site values for 2017-2019 is dated September 2020. Forecasts need independently specified future inputs and the records available at the forecast date, and errors in forecast electricity could either enlarge or offset scaling errors. Because the cohort requires a prior report at each site, even the other-site rule is not a test at new sites. The analysis therefore provides no prospective, new-site, or cross-operator validation.

Annual withdrawal is also a narrow target. It does not measure monthly peaks, consumptive loss, drought-season depletion, or the effects of reclaimed-water diversion on downstream returns, and evaluating those requires explicit spatial boundaries, counterfactuals, and time-resolved observations. This benchmark can help select an annual estimator, but annual totals alone cannot establish hydrologic consequences.

The next tests should widen the sample and sharpen the comparison: more operators and sites, the reporting records available at the time, simple baselines kept alongside every new method, and detailed models tested against compatible targets and genuinely withheld observations. Until such tests exist, the burden of proof belongs with electricity scaling: before it is used to estimate a facility's annual withdrawal, it should beat last year's withdrawal, year by year as well as on average.

5 Conclusion

Simple annual withdrawal rules should be tested against observations using information available for their intended use. Across nine years (2016-2024) of disclosed Meta site data, persistence had the lowest pooled error in both halves of the record. Yet the site-ratio rule performed better in three of eight target years. In an exploratory comparison on the 2021-2024 targets, starting the pooled histories in 2016 rather than 2020 made the pooled rules less accurate, not more. The year-to-year reversals, the rules' differing inputs, and the cohort's selection limits constrain the planning lesson: historical electricity scaling should be tested against prior-year withdrawal, not assumed to improve annual estimates. A favorable pooled ranking in one retrospective sample establishes neither a universal ranking nor forecasting accuracy.

Code and data availability

Meta's source reports are publicly accessible [10-16], and the Internet Archive holds a copy of every report version used, identical to the file analyzed. Extracted values from every report version, revision checks, exclusions, row-level estimates, code, and source-file records, including where each report is archived, are supplied in Supplementary Data 1. Full source PDFs retain their original rights. Supplementary Data 1 is deposited in Zenodo under the reserved DOI 10.5281/zenodo.23565086, which becomes active when the record is published; during peer review it is available at

https://zenodo.org/records/23565086?preview=1&token=eyJhbGciOiJIUzUxMiJ9.eyJpZCI6ImY5MDI0NjU4LTQxN2UtNDk5Yy1hNTZhLTc3MmNjNzVmZmNkOSIsImRhdGEiOnt9LCJyYW5kb20iOiJkNTA1ODlkMjc0MmQyOWI0NTkxMDY5MzQ0TI2MmY4MyJ9.DBi5R9PiGs_6Wid2aFrVPu2p10IRHidPwGxaa-d_MC2Ow2U7pNkd9XbkEmBoqFUuV8u2lfWFQugilZV-NP0Thw

Acknowledgements and author declarations

This work received no specific funding. The author is a member of the Editorial Board of Environmental Research: Water and was not involved in the peer review of this manuscript. The author declares no other competing interests.

AI disclosure: During this work, JE used Anthropic Claude (Opus 5.5) for code development and review, data auditing, and reproducibility. JE reviewed, validated, and critically revised all AI-assisted code, and takes full responsibility for the content of the manuscript.

References

- [1] Mytton D 2021 Data centre water consumption npj Clean Water 4 11. <https://doi.org/10.1038/s41545-021-00101-w>
- [2] Privette A P, Barros A and Cai X 2026 Data Centers Water Footprint: The Need for More Transparency AGU Advances 7 e2025AV002140. <https://doi.org/10.1029/2025AV002140>
- [3] Siddik M A B, Shehabi A and Marston L 2021 The environmental footprint of data centers in the United States Environmental Research Letters 16 064017. <https://doi.org/10.1088/1748-9326/abfb1>
- [4] Lei N and Masanet E 2022 Climate- and technology-specific PUE and WUE estimations for U.S. data centers using a hybrid statistical and thermodynamics-based approach Resources, Conservation and Recycling 182 106323. <https://doi.org/10.1016/j.resconrec.2022.106323>

- [5] Sharma P, Pegus II P, Irwin D, Shenoy P, Goodhue J and Culbert J 2017 Design and Operational Analysis of a Green Data Center IEEE Internet Computing 21(4) 16-24. <https://doi.org/10.1109/MIC.2017.2911421>
- [6] Lei N, Lu J, Shehabi A and Masanet E 2025 The water use of data center workloads: A review and assessment of key determinants Resources, Conservation and Recycling 219 108310. <https://doi.org/10.1016/j.resconrec.2025.108310>
- [7] Gupta P S, Hossen M R, Li P, Ren S and Islam M A 2024 A Dataset for Research on Water Sustainability Proceedings of the 15th ACM International Conference on Future and Sustainable Energy Systems pp 442-446. <https://doi.org/10.1145/3632775.3661962>
- [8] Han Y, Li P, Wierman A and Ren S 2026 Small Bottle, Big Pipe: Quantifying and Addressing the Impact of Data Centers on Public Water Systems arXiv:2603.02705v2, preprint, revised 18 March 2026. <https://doi.org/10.48550/arXiv.2603.02705>
- [9] Masanet E 2026 Estimating data center water use: Best practices and critical questions arXiv:2610.04735v1, preprint, submitted 3 October 2026. <https://doi.org/10.48550/arXiv.2610.04735>
- [10] Meta 2025 Environmental Data Index, October URL vintage, pp F, I, J, M and R (accessed 14 October 2026). https://sustainability.atmeta.com/wp-content/uploads/2025/10/Meta_2025-Environmental-Data-Index.pdf (archived at https://web.archive.org/web/20261012212710/https://sustainability.atmeta.com/wp-content/uploads/2025/10/Meta_2025-Environmental-Data-Index.pdf)
- [11] Facebook 2020 Facebook Sustainability Data 2019, dated September 2020, pp 1, 3 and 4 (accessed 15 October 2026). https://sustainability.atmeta.com/wp-content/uploads/2020/12/FB_Sustainability-Data-Disclosure-2019.pdf (archived at https://web.archive.org/web/20260212165939/https://sustainability.atmeta.com/wp-content/uploads/2020/12/FB_Sustainability-Data-Disclosure-2019.pdf)
- [12] Facebook 2021 Facebook Sustainability Data 2020, dated June 2021, pp 2, 5 and 6 (accessed 15 October 2026). https://sustainability.atmeta.com/wp-content/uploads/2021/06/2020_FB_Sustainability-Data.pdf (archived at https://web.archive.org/web/20251028215352/https://sustainability.atmeta.com/wp-content/uploads/2021/06/2020_FB_Sustainability-Data.pdf)
- [13] Meta 2022 2021 Meta Sustainability Report: Data Index, pp 12, 14-15 and 19 (accessed 15 October 2026). <https://sustainability.atmeta.com/wp-content/uploads/2022/06/2021-Meta-Sustainability-ESG-Data-Index.pdf> (archived at <https://web.archive.org/web/20251028214145/https://sustainability.atmeta.com/wp-content/uploads/2022/06/2021-Meta-Sustainability-ESG-Data-Index.pdf>)
- [14] Meta 2023 2023 Environmental Data Index, pp 9 and 11 (accessed 15 October 2026). <https://sustainability.atmeta.com/wp-content/uploads/2023/07/Meta-2023-Environmental-Data-Index.pdf> (archived at <https://web.archive.org/web/20260728194353/https://sustainability.atmeta.com/wp-content/uploads/2023/07/Meta-2023-Environmental-Data-Index.pdf>)
- [15] Meta 2024 2024 Sustainability Report, data index pp G, J and N (accessed 15 October 2026). <https://sustainability.atmeta.com/wp-content/uploads/2024/08/Meta-2024-Sustainability-Report.pdf> (archived at

<https://web.archive.org/web/20261010180418/https://sustainability.atmeta.com/wp-content/uploads/2024/08/Meta-2024-Sustainability-Report.pdf>)

[16] Facebook 2017 Facebook Sustainability Data 2016, pp 1-2 (accessed 15 October 2026). <https://sustainability.atmeta.com/wp-content/uploads/2023/04/2016-Facebook-Sustainability-Data-Disclosure.pdf> (archived at

<https://web.archive.org/web/20260212165945/https://sustainability.atmeta.com/wp-content/uploads/2023/04/2016-Facebook-Sustainability-Data-Disclosure.pdf>)

[17] Jiang F, Duan C and Chen B 2025 Facility-level energy-driven water footprint and scarcity implications of Chinese data centers: a bottom-up analysis and scenario-based projection *Applied Energy* 399 126522. <https://doi.org/10.1016/j.apenergy.2025.126522>

[18] Alston G, Love N and Haider R 2026 Data center cooling choices shift water impacts across the grid: An integrated water-energy model for sustainable data center development *arXiv:2609.25437v1*, preprint, submitted 21 September 2026. <https://doi.org/10.48550/arXiv.2609.25437>

Supplementary information for annual withdrawal benchmarks

Does electricity scaling improve annual water-withdrawal estimates? Evidence from US Meta data centers

Jaivime Evaristo

S1 Data boundaries and exclusions

For 2016-2024, the empirical estimator panel pairs Meta's operational withdrawal with total facility electricity, using the most recent cubic-meter or megaliter report version that covers each year (table S1). Across 15 named US sites, the panel has 135 site-year slots, 82 of them with numeric pairs, of which 79 are positive. It excludes international sites, pooled leased/other-facility rows and offices. Supplementary Data 1 keeps every value exactly as reported, with its report version and source page. Consecutive positive observations yield 64 transitions at 13 sites, with target years 2017-2024; gaps in annual reporting are not bridged.

Table S1. Report versions examined, ordered by the modification date recorded in each PDF (Dated). Years reported are those in the site tables. Panel years identifies which years each version supplies to the analysis panel; every version enters the revision checks.

Report version	Dated	Years reported	Water unit	Panel years
Facebook Sustainability Data 2016	Jul 2017	2011-2016 (water 2014-2016)	gallons	none (restated)
Facebook Sustainability Data 2019	Sep 2020	2015-2019	m ³	2015 (sensitivity)
Facebook Sustainability Data 2020	Jun 2021	2016-2020	m ³	2016
2021 Meta Sustainability Report: Data Index	Jul 2022	2017-2021	m ³	none
2023 Responsible Business Practices Report: Data Index	Jul 2023	2017-2022	m ³	none (site tables identical to next row)
2023 Environmental Data Index	Jul 2023	2017-2022	m ³	2017-2018
2024 Sustainability Report: Data Index	Aug 2024	2019-2023	ML	2019
2025 Environmental Data Index, three August URLs (archived 13, 19 and 22 Sep 2025)	Sep 2025	2020-2024	ML	none (site tables identical to next row)
2025 Environmental Data Index, October URL	Oct 2025	2020-2024	ML	2020-2024

Three numeric pairs contain a zero: DeKalb 2021 (0 ML, 4,724 MWh), Gallatin 2021 (0 ML, 0 MWh), and Gallatin 2022 (0 ML, 6,264 MWh). They remain numeric in the raw panel. Fifty-three site-year slots lack a numeric pair; in two, only electricity is reported (Sarpy 2018, 5,000 MWh; Henrico 2019, 10,000 MWh). The September 2020 and June 2021 disclosures list eight of the 15 sites and show "not yet online" where later reports show dashes. Both entries, as well as a site's absence from a report, are treated as missing. So is the "<500" given for Fort Worth's 2015 electricity in the September 2020 disclosure. Kansas City and Mesa have numeric values only in 2024 and contribute no adjacent positive transitions. Neither missingness nor the first positive reported value independently identifies commissioning.

The response excludes construction use. Each disclosure since 2020 reports construction withdrawal omitted from its latest year (504,000 m³ in 2019, 388,000 m³ in 2020, 827,289 m³ in 2021, 1,780,000 m³ in 2022, 1,724 ML in 2023 and 1,019 ML in 2024), but none states how construction water was treated in earlier years. Withdrawal does not identify consumption, source depletion, the potable fraction or AI workloads; total facility electricity does not identify IT energy. Line-item rounding and unspecified measurement or estimation for individual rows limit interpretation. In the panel, electricity is reported to the nearest 1,000 MWh for 2016-2020 and to the MWh thereafter; withdrawal is reported to the nearest 1 ML in every year. Methodology and footnotes appear on pages M and R of the October 2025 index and on the notes pages of earlier reports; Supplementary Data 1 records page locators (code/config.py) and SHA-256 checksums, digital fingerprints that change if a file is altered (sources/source_manifest.csv), for every version.

S2 Analysis sequence, report revisions and numerical checks

We specified the four rules, the positive-pair cohort and WAPE after preliminary 2020-2024 results were known, and we chose the site reweighting, the nonnegative-withdrawal analysis and the descriptive summaries and figures after the benchmark results were known. The 2016-2024 extension, also made after the 2020-2024 results were known, changed no estimator, eligibility rule or metric; its randomization test, revision and closure checks, and first-release and rounding analyses were likewise chosen after those results were known. We added the September 2020 and July 2017 disclosures (and with them the 2015 sensitivity analysis), the randomization test and site reweighting of the history-start comparison, and the continuing-site ratio index (section S8) after the 2016-2024 results had been calculated. All of these are exploratory analyses of one retrospective sample, not a confirmatory test. No machine-learning model or hyperparameter search was used.

Twenty-two integrity tests pass. They check source checksums; agreement of two independent PDF text extractors (Poppler pdftotext and pdfplumber) on each of the 431 table rows found and on the absence of the other 75 rows sought; missing versus zero, including "not yet online"; cubic-meter and gallon conversion; matching values for renamed sites; the absence of US-site revisions since September 2020 and the restatement of the July 2017 gallon values; adjacency; exclusion of later-dated and target-site history; shared-denominator WAPE and missing-prediction failure; the error identities in section S7; reproducible resampling; the 2015 panel; the history-start comparison and continuing-site index of section S8, recomputed independently; and reproduction of the 2020-2024 analysis from the October 2025 index alone, at the precision it reported (44 transitions; 59 positive and 62 numeric pairs; 6,748 ML denominator; WAPE 30.7%, 41.3%, 49.9% and 54.0%). Passing these tests does not show that the source values are accurate.

Across the ten cubic-meter or megaliter versions, every US value reported more than once agrees with its latest version to within half the sum of their rounding units: 815 comparisons (341 excluding the four versions whose site tables duplicate another), none revised. The largest differences, 447 MWh and 0.50 ML, arise from rounding. Values for 2016 are cross-checked between the September 2020 and June 2021 disclosures; among these versions, 2015 appears only in the September 2020 disclosure. Within these ten versions, apart from sites that later reports add with dashes for earlier years, a US site-year's reported status changes only when "not yet online" becomes a dash or when DeKalb and Gallatin first appear, in July 2023, with 2021 values. The July 2017 disclosure reports withdrawal in US gallons. Its 2015-2016 site values differ from their cubic-meter restatements by -1% to +27%, whereas its electricity values agree with later reports. Seven pooled or international values (ten earlier entries) were revised beyond rounding, including leased-facility electricity in 2019 (674,000 MWh in the September 2020 and June 2021 disclosures, 647,000 MWh thereafter) and Clonee electricity in 2017 (17,000 MWh in the September 2020 disclosure, 1,000 MWh thereafter). Reported totals equal the sum of their rows within rounding in 172 of 208 checks. The 36 exceptions follow four of those revisions, which left reported totals unchanged, plus a 60 MWh gap in the 2022 electricity total. Using the first value published since September 2020 for each site-year changes pooled WAPes by at most 0.03 percentage points; also admitting the July 2017 gallon values for 2016 changes them by as much as 0.61 points. In 10,000 draws (seed 20261014) that perturb each positive value uniformly within its rounding interval, no pooled WAPE changes by more than 0.31 points. Across these draws, the site-ratio-minus-persistence difference ranges from 10.3 to 11.0 points, and site ratio has the smaller absolute error in 26-28 transitions.

S3 Target-year errors

Table S2. WAPE (%) by target year, evaluated on the same targets for every method within each year. Eligible-site coverage varies between years; pooled histories begin in 2016.

Year	n	Persistence	Site ratio	Pooled history	Other-site history
2017	4	41.8	39.1	26.9	29.2
2018	4	36.9	21.0	38.4	43.2
2019	5	22.1	42.8	42.7	51.8
2020	7	30.3	54.2	51.7	59.7
2021	8	22.9	36.0	50.8	54.6
2022	11	30.4	56.3	60.2	63.2
2023	12	24.6	43.2	74.9	76.9
2024	13	39.8	32.7	44.6	46.7

Site ratio has smaller absolute error than persistence in 27 of the 64 primary transitions, with no exact ties (26-28 across 10,000 perturbations within the rounding intervals). It wins 9 of 13 transitions in 2024 and 2 of 4 in each of 2017 and 2018. The corresponding counts are 1 of 5 in 2019, 3 of 7 in 2020, 4 of 8 in 2021, 2 of 11 in 2022 and 4 of 12 in 2023. This paired comparison is descriptive, not a binomial test that treats transitions as independent. Within individual years,

exact site-level randomization p-values for the WAPE difference range from 0.009 (2022) to 1.00 (2017).

S4 Site errors and weighting

Table S3. Per-site WAPE (%) on each site's eligible transitions for target years 2017-2024. All sites are nested within one operator; the rows are not independent industry samples.

Site	n	Persistence	Site ratio	Pooled history	Other-site history
Altoona	8	17.7	17.9	63.9	79.4
DeKalb	2	46.9	146.0	27.2	27.6
Eagle Mountain	3	25.6	32.4	86.7	88.1
Forest City	8	20.6	33.8	112.0	124.3
Fort Worth	8	26.9	28.2	27.3	32.6
Gallatin	1	98.5	95.5	61.2	61.1
Henrico	4	46.8	53.4	178.4	184.3
Huntsville	3	36.6	19.6	4.9	5.4
Los Lunas	6	26.7	38.7	18.1	18.0
New Albany	5	37.7	99.3	82.9	84.6
Prineville	8	38.9	46.4	31.1	28.0
Sarpy	5	16.2	30.9	108.4	113.4
Stanton Springs	3	45.4	127.1	136.8	138.4

In the method order of main-text table 1, the equal-site mean of within-site WAPE is 37.3%, 59.2%, 72.2% and 75.8%; the equal-site mean of transition-level absolute percentage error is 38.4%, 73.2%, 83.3% and 86.9%. These estimands differ from each other and from pooled WAPE. Small positive withdrawals can strongly affect percentage-error summaries. Omitting each site in turn from scoring leaves persistence with the lowest pooled WAPE, but does not rebuild the historical coefficients.

S5 Conditional site reweighting and zero sensitivity

Table S4. WAPE differences from persistence (percentage points), with 2.5th-97.5th percentile ranges from fixed-prediction site reweighting.

Estimator	Observed difference	Lower percentile	Upper percentile
Site ratio	10.7	3.9	23.2

Estimator	Observed difference	Lower percentile	Upper percentile
Pooled history	21.4	2.6	51.5
Other-site history	25.3	4.1	58.3

Using seed 20261014, the site reweighting makes 2,000 draws of 13 site indices with replacement. Selecting a site more than once repeats its full target-volume and error contribution; the same draws compare every method. Pooled historical coefficients and predictions remain fixed; percentiles interpolate linearly between order statistics. The procedure does not reproduce shared coefficient dependence, common-year shocks, reporting revisions or meter error, and its percentile ranges have no established nominal population or forecast coverage. The exact site-level randomization test instead enumerates all 8,192 sign patterns of the 13 site-summed absolute-error differences. Its two-sided p-values are 0.009, 0.052 and 0.047 for site ratio, pooled history and other-site history versus persistence. The corresponding values are 0.054, 0.033 and 0.035 for the 2021-2024 targets and 0.17, 0.44 and 0.38 for the seven sites scored in 2017-2020; with 128 patterns, the smallest possible p-value is 2/128 (0.0156). This test is conditional on the predictions and treats sites as independent; it does not reproduce shared pooled coefficients or common-year shocks.

The nonnegative-withdrawal sensitivity analysis requires positive electricity in both adjacent years and applies the same eligibility to historical pooling. Adding DeKalb 2021-2022 and Gallatin 2022-2023 yields 66 targets; Gallatin 2021 remains excluded because its electricity is zero. Both the historical denominator and the target set change. The four WAPes, in main-table order, are 31.1%, 41.8%, 52.6% and 56.5%. Logarithmic changes remain restricted to the original positive cohort. Starting the record in 2015, the first year in the September 2020 disclosure, adds the Altoona, Forest City and Prineville 2015-2016 transitions (67 targets). The resulting WAPes are 30.7%, 40.7%, 51.5% and 55.0%; site ratio's WAPE exceeds persistence's by 10.0 points (randomization $p = 0.017$).

S6 Reproducibility files and remaining limits

Supplementary Data 1 contains every value read from the eleven report versions, exactly as reported, with its status and converted value, the comparison of both text extractors, revision and closure checks, the site-name map, the analysis panel and its first-release and 2015 counterparts, transition predictions, all reported diagnostic tables, executable scripts, and the source URLs, Internet Archive captures and checksums. The portable package omits the third-party PDFs. Its first script checks local copies or downloads each report from Meta's URL or, if that copy is unavailable or has changed, from an Internet Archive capture, and keeps a file only if it matches the recorded SHA-256 value. Each of the eleven versions has a capture identical to the analyzed file (confirmed on 21 October 2026); with every Meta URL disabled, the package retrieved all eleven from the archive and reproduced every data and result file. The three August 2025 URLs returned HTTP 404 on 22 October 2026; those versions were retrieved from Internet Archive captures made on 13, 19 and 22 September 2025. Captures also show that the June 2021 URL served a re-issued file (created 7 October 2021) from at least November 2021 to April 2022; its text differs from the June 2021 file only in the 2020 location-based emissions total, so its site tables are identical. Meta's resources page lists no site-level disclosure for the 2017 and 2018

reporting years (only company-wide water verification statements), and no report covering 2025 had been published by 22 October 2026. Disclosures for the 2014 and 2015 reporting years, also listed there, report no year of the 2016-2024 record and were not read; the 2015 sensitivity analysis uses the September 2020 restatement.

The full pipeline produced byte-identical data and result files (every CSV and JSON output; logs/cross_system_check.txt) in two environments: Python 3.10.12 (NumPy 2.2.6, pandas 2.3.3, matplotlib 3.10.9, Poppler pdftotext 22.02) and Python 3.13.16 (NumPy 2.5.3, pandas 3.0.5, matplotlib 3.11.2, Poppler pdftotext 24.02). Both used pdfplumber 0.11.10. Both systems drew figures from those identical files, but the figure files were not compared: the two matplotlib versions write PNG, PDF and SVG bytes differently.

S7 Secondary ratio and growth descriptions

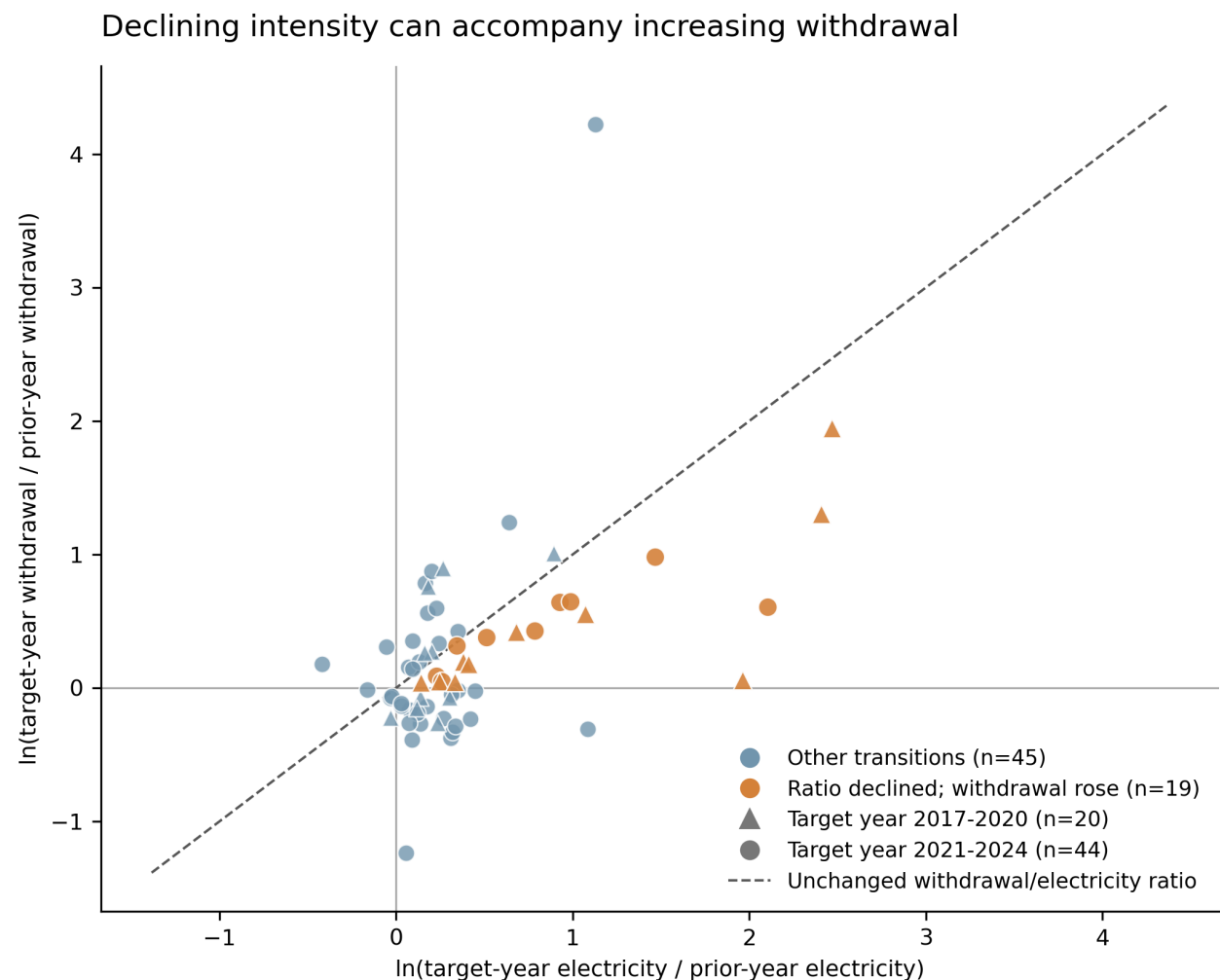


Figure S1. Log changes in reported withdrawal and total facility electricity for 64 positive adjacent-year transitions (triangles: target years 2017-2020; circles: 2021-2024). The diagonal marks an unchanged withdrawal/electricity ratio. Of the 44 transitions with a declining ratio, 19 have increasing withdrawal (orange); the other 45 transitions are blue. These arithmetic associations are not causal estimates of efficiency or rebound. Missing and zero pairs remain in the source data but cannot enter the logarithmic display.

Main-text equation (13) gives the absolute errors of persistence and site ratio. Thus a declining ratio with increasing withdrawal does not, by itself, mean that persistence has the smaller error. The comparison depends on whether the withdrawal growth factor is closer to 1 or to the electricity growth factor.

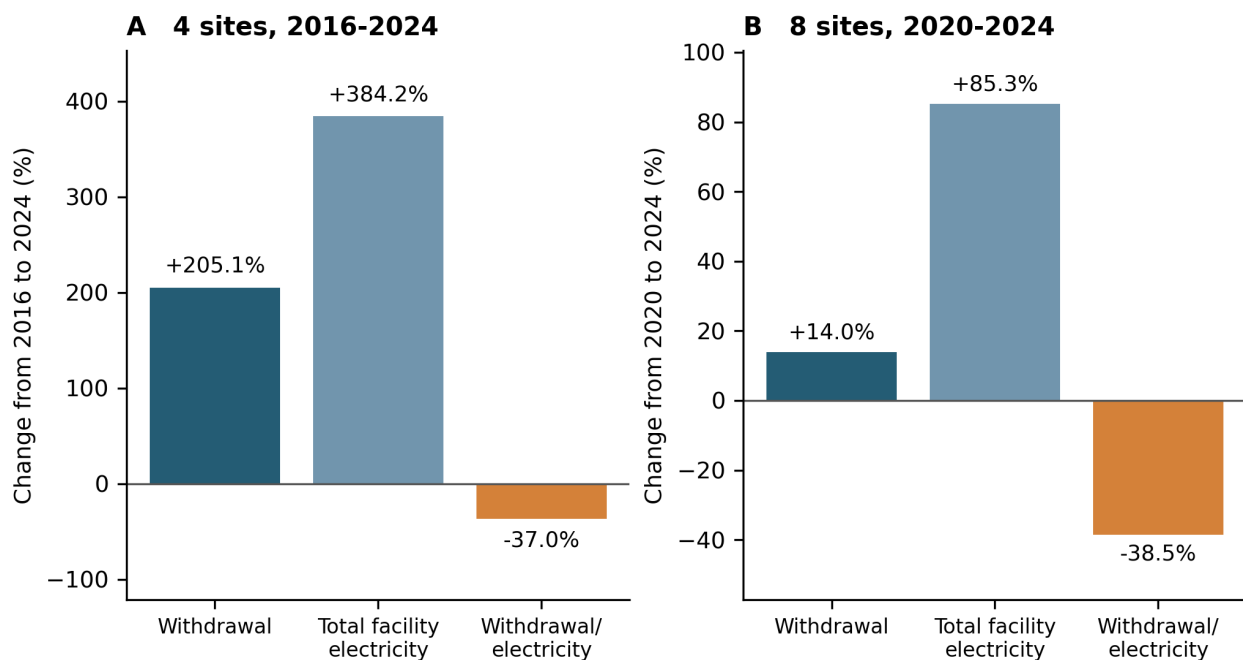


Figure S2. Aggregate change at sites with positive endpoint pairs. (A) From 2016 to 2024, at the four sites with positive values in 2016—Altoona, Forest City, Fort Worth and Prineville—withdrawal rises 205.1%, total facility electricity rises 384.2%, and their ratio of sums falls 37.0%. Fort Worth reported only 16,000 MWh in 2016, so panel A includes its start-up. (B) From 2020 to 2024, at eight sites—Altoona, Forest City, Fort Worth, Henrico, Los Lunas, New Albany, Prineville and Sarpy—withdrawal rises 14.0%, electricity rises 85.3%, and the ratio falls 38.5%. These endpoint-selected subsets exclude the other listed US sites and are not estimates of sector growth. Source and boundaries are the same as the primary panel.

S8 Pooled-history coefficients

A cumulative ratio of sums averages earlier years' ratios, weighted by electricity, and therefore lags a sustained trend in the ratio. Table S5 compares the coefficient applied in each target year with that year's realized ratio of sums for all sites with positive values and for the scored targets. The all-site ratio was 0.29-0.33 L/kWh in 2016-2019 and 0.16-0.27 L/kWh in 2020-2024; it was 36% lower in 2024 than in 2016. This decline is not an artifact of which sites reported: an index chained across sites reporting in consecutive years fell by 45% over the same period. From 2019 onward, the coefficient exceeded the ratio realized by the scored targets every year, so pooled history overestimated their summed withdrawal in each year. For every 2021-2024 transition and both pooled rules, histories beginning in 2016 gave higher coefficients than histories beginning in 2020. The mean signed error rose from 39.9 to 60.2 ML for pooled history and from 38.8 to 59.2 ML for other-site history. WAPE rose from 49.9% to 56.9% and from 54.0% to 59.4%, respectively. The longer history gave larger absolute errors in 32 and 31 of the 44 transitions and at 8 and 7 of the 13 sites, respectively. The randomization test and site reweighting of section S5 give $p = 0.037$ and a range of 1.2 to 14.3 points for the first difference (7.0 points), and $p = 0.10$

and a range of -0.3 to 13.3 points for the second (5.4 points). Every site's difference stems from the same added years of history, so the site differences share a common cause; these values describe the contrast rather than test it.

Table S5. Pooled-history coefficients and realized withdrawal-to-electricity ratios (L/kWh; the index is dimensionless). For target year t , the coefficient is the ratio of sums over positive site-years before t , using histories beginning in 2016 (primary analysis) or 2020. Realized ratios are ratios of sums for all sites with positive values that year and for the scored targets. The continuing-site index chains each year's change in the ratio of sums for sites with positive values in that year and the previous year (2016 = 1).

Year	Coefficient, from 2016	Coefficient, from 2020	All positive sites	Scored targets	Continuing-site index
2016	-	-	0.287	-	1.00
2017	0.287	-	0.326	0.326	1.14
2018	0.311	-	0.324	0.316	1.10
2019	0.317	-	0.292	0.282	0.96
2020	0.307	-	0.270	0.273	0.90
2021	0.293	0.270	0.228	0.215	0.71
2022	0.271	0.246	0.186	0.183	0.57
2023	0.245	0.221	0.159	0.160	0.49
2024	0.221	0.199	0.185	0.178	0.55