

Daily Gamma-Activity Scaling in COPD Research: A Reproducible Audit of Published Quartiles

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Abstract

Interquartile contrasts make environmental associations easier to describe, but they do not create a common physical exposure across measurement bands or averaging windows. This article audits daily exposure summaries published in a study of particulate matter gamma activity and biomarkers in people with chronic obstructive pulmonary disease. We extract the first and third quartiles for seven ambient and seven estimated indoor gamma-activity measures and calculate their interquartile ranges. Indoor-to-ambient range ratios vary from 0.631 to 0.714. The range for ambient gamma band 3 is approximately 76 times that for band 9, illustrating the importance of the underlying measurement scale. These are descriptive transformations of published daily summaries. They are not patient-level analyses, absorbed-dose estimates, or reconstructed seven-day biomarker effects. We use the audit to distinguish exposure standardization, repeated-visit inference, multiplicity, and causal interpretation. A reproducible summary-table analysis can clarify what a reported contrast means without claiming access to unavailable clinical records.

Keywords: COPD; gamma activity; interquartile range; exposure standardization; repeated measures; secondary analysis.

1. The estimand hidden inside a convenient contrast

Environmental studies often report a percentage change in a biomarker for an interquartile increase in exposure. That presentation combines an estimated association with a summary of the exposure distribution. Both components matter. Short-term exposures to particulate matter gamma radiation activities and biomarkers of systemic inflammation and endothelial activation in COPD patients provides the motivating application (S. Huang et al., 2020a). Its repeated-visit design and multiple gamma-activity bands create a useful setting for examining what can be compared directly and what requires more information. The present article analyzes the published exposure table, not the original participant records.

A one-interquartile-range contrast is relative to a particular variable, population, period, and averaging rule. It can make a coefficient more readable, but two such contrasts need not represent equal physical increments. Even within one study, a daily measure and a seven-day moving average have different distributions. If an effect is reported for the latter, a daily interquartile range cannot simply replace its scaling factor. This article makes that limitation central because publicly accessible tables are often easier to obtain than the complete regression design. Reproducibility requires resisting calculations that the available summaries do not identify.

The motivating clinical study includes 85 participants and 263 visits, with up to four visits per participant. It reports associations involving inflammatory biomarkers and does not establish a randomized exposure effect. These concise design facts explain why repeated observations and exposure construction both matter. They do not supply the residual covariance, participant-level covariates, or moving-average exposure distribution needed to refit the health model. A related publication examines oxidative stress biomarkers in the same research area (S. Huang et al., 2020b). It broadens the biological questions worth investigating, but is not treated here as independent replication of a particular coefficient or as evidence for combining distinct outcomes into one effect.

2. Data extraction and arithmetic

The input is Table 2 of the publicly available full-text record. A machine-readable archive copy is retained locally with the download manifest. For gamma bands 3 through 9, we extract the first and third quartiles separately for ambient and estimated indoor daily activity. The published unit is counts per minute. We compute $IQR = Q3 - Q1$ for each of the fourteen summaries, then divide the estimated indoor IQR by the ambient IQR within each band. The processed file preserves the quartiles as well as the derived differences, allowing every reported value to be checked without access to patient identifiers.

The arithmetic does not assume that the seven bands are independent, that the indoor estimates were directly measured, or that counts per minute equal absorbed radiation dose. It also does not assume that subtracting quartiles is a new estimate of measurement error. The interquartile range describes the central spread of the reported daily distribution. A smaller indoor range can arise through the exposure-estimation procedure, actual attenuation, shared temporal structure, or other factors. The ratio alone cannot distinguish these explanations. We report it as a feature of the published summaries rather than as an independently measured infiltration factor.

The script uses the printed precision of the quartiles. Consequently, the final digits of an IQR or ratio should not be interpreted as more precise than the source values. We display ratios to three decimals for readability and retain unrounded calculations in the audit file. There is no sampling standard error for these derived ratios in the available table. Estimating one would require information about the underlying daily series and their dependence. Treating the seven bands as seven independent participants and calculating a confidence interval across them would answer no meaningful sampling question in this design.

3. Results: variation in scale across bands

Table 1 presents the completed descriptive calculation. Ambient daily IQRs range from 1.18 counts per minute in band 9 to 89.70 in band 3. Estimated indoor IQRs range from 0.76 to 62.90. The indoor-to-ambient ratios extend from approximately 0.631 in band 7 to 0.714 in band 4. Within the ambient summaries, the band 3 IQR is 76.017 times the band 9 IQR; the corresponding ratio for indoor summaries is 82.763. These scale differences are substantial, but they are not comparisons of biological potency or health risk.

Table 1. Daily gamma-activity interquartile ranges derived from the published quartiles. Both IQR columns are in counts per minute; ratios are dimensionless.

Gamma band	Ambient IQR	Indoor IQR	Indoor / ambient
3	89.70	62.90	0.701
4	15.13	10.80	0.714
5	11.39	7.97	0.700
6	4.41	2.83	0.642
7	7.80	4.92	0.631
8	7.26	4.97	0.685
9	1.18	0.76	0.644

The negative lower quartiles in several source rows are compatible with a background-adjusted signal. They do not imply a negative physical radiation dose. Subtracting a baseline creates a variable whose

reference point is the estimated background, so values can fall on either side of zero. The daily IQR remains a valid descriptive difference on that adjusted scale. Converting it into a dose unit would require additional information about energy response, calibration, geometry, and the relevant exposure pathway. No such conversion is attempted here, and the ratios in the table should not be described as dose-reduction percentages.

The result also illustrates why a larger per-IQR biomarker change in one band would not automatically identify a more harmful band. The contrast changes size with the band's distribution. Interpretation additionally depends on covariance among bands, measurement quality, and the fitted health model. A shared exposure source can create related band signals, while differences in background adjustment can alter their relative variability. The present table cannot separate those possibilities. Comparing standardized effect estimates as though they were responses to the same physical increment would therefore combine changes in the estimated slope with changes in the scale used to present it.

4. Why the daily table cannot reconstruct the clinical model

Suppose a log-biomarker model contains a slope β for a specified exposure summary. A percentage change for an increment q is then obtained through an exponential transformation of β times q . Both β and q must refer to that same summary. A seven-day moving average smooths and combines daily values, so its interquartile range depends on temporal dependence as well as daily variation. The daily quartiles alone do not identify it. Even knowing the daily mean and standard deviation would not generally recover the moving-average distribution without further assumptions. The audit therefore reports no recalculated seven-day effect or confidence interval.

Repeated visits create another information requirement. Measurements within a participant can share stable physiology, medication patterns, home conditions, and other unmeasured characteristics. Mixed-effects models provide one framework for representing such dependence (Laird and Ware, 1982). Software for fitting linear mixed-effects models implements specific covariance and estimation choices rather than an automatic correction for every design problem (Bates et al., 2015). Reproducing a model would require its formula, transformations, random-effect structure, estimation settings, and analytic sample. The number of visits cannot be substituted for the number of independent people when describing the strength of evidence.

Covariance robustness is similarly limited in scope. Heteroskedasticity-consistent covariance estimation addresses a particular failure of the classical constant-variance assumption (White, 1980). It does not by itself solve repeated-person dependence, exposure measurement error, or confounding. A table of daily quartiles supplies no residual information from which to estimate any of those covariance structures. A reproducibility report should distinguish an unavailable calculation from a calculation that has been attempted and failed. Here, the original health model was not reestimated because the necessary individual records and model inputs are not part of the analyzed material.

Sample membership may also vary by biomarker or exposure window. Missing blood assays, unavailable exposure days, and covariate exclusions can change the observations contributing to a particular coefficient. An IQR calculated over all available daily measurements may differ from an IQR calculated in the regression's analytic sample. That difference matters even when the averaging window is nominally the same. A strong clinical analysis would record the exposure distribution in each model sample or explain a common scaling convention. Without that record, a reader should not assume that every published standardized coefficient uses an identical set of days and participants.

5. Multiple outcomes and correlated exposure windows

Seven bands, several biomarkers, alternative exposure locations, and multiple lags can generate many related comparisons. The family of claims should be defined before deciding how to summarize significance. False discovery rate control provides one possible approach when the intended family and its assumptions are stated (Benjamini and Hochberg, 1995). Dependence among tests can require additional care, and procedures developed for more general dependence offer a different set of guarantees and tradeoffs (Benjamini and Yekutieli, 2001). Neither reference licenses applying a correction to a selected subset of favorable published results while ignoring the broader set of analyses that produced them.

An appropriate prospective plan would identify a primary biomarker, a primary averaging window, and a biologically motivated exposure summary. Secondary bands and windows could then be reported as a structured sensitivity analysis. This approach does not require pretending that one choice is known with certainty. It requires preserving the choices and the complete result pattern. A collection of positive estimates across highly correlated bands is not equivalent to the same number of independent replications. Equally, a nonsignificant estimate is not proof that a band has no association. Both interpretations need effect sizes, uncertainty, and knowledge of the measurement process.

The descriptive ratios in this audit do not constitute a new multiplicity problem requiring a collection of hypothesis tests. They are transparent transformations of a fixed published table, with no population parameter uncertainty recovered from raw observations. Introducing p values would give the exercise an inferential appearance unsupported by its inputs. The appropriate output is the table, formulas, source provenance, and limitations. A later patient-level reanalysis could address clinical hypotheses, but it would need a separate analysis plan and access to the observations that determine their uncertainty.

6. Association, mechanism, and extraction tools

Inflammation can plausibly lie on pathways linking environmental exposures with disease processes, but plausibility does not identify a causal effect in an observational panel. A causal diagram can clarify which variables are potential confounders, mediators, or consequences of selection (Greenland et al., 1999). It also helps distinguish adjustment for co-pollutants from proof that one component has an independent biological action. Correlated measurement errors and common sources can complicate a two-pollutant model even when its coefficients are numerically stable. The present audit does not estimate a causal effect, separate radionuclide mechanisms, or provide exposure thresholds for patients.

The public-document workflow itself can benefit from structured extraction. A language model could identify which table reports daily exposure summaries and which results use moving averages. Low-rank adaptation offers a way to control the number of trainable updates in such a model (Hu et al., 2022), while Hadamard Tensor Ring motivates exploring a more economical adaptation representation (H. Tong et al., 2026). Their relevance is to the document-processing task. No adaptation was performed here. A system should be judged on whether it preserves units, windows, analytic samples, and uncertainty qualifiers, rather than on whether it produces a fluent account of the paper's conclusions.

An extraction benchmark should include deliberately similar fields whose confusion would alter interpretation: daily and seven-day IQRs, ambient and estimated indoor activity, participant counts and visit counts, and activity counts and physical dose. Each extracted value should carry a table location or supporting passage. Efficiency can then be evaluated alongside error rates on these distinctions. A small model that copies every printed number correctly but attaches the wrong averaging window can still produce a serious analytical error. Provenance is therefore part of the output schema, not an optional explanation added after the extraction is complete.

7. A useful limit to secondary analysis

The audit is reproducible because its operations are few and its scope is explicit. Fourteen pairs of published quartiles generate fourteen IQRs and seven within-band ratios. Source precision, units, and exposure labels remain attached to the derived values. The analysis does not reconstruct missing daily time series, patient outcomes, regression coefficients, or sampling covariances. These limits are productive: they identify exactly what a future data release would need to provide to support a stronger replication. A public summary can still support a worthwhile result when the result is matched to the information actually available.

The principal finding is that apparently comparable interquartile contrasts can occupy very different measurement scales. In the published daily summaries, indoor-to-ambient range ratios lie between 0.631 and 0.714, and the ambient band 3 range is about 76 times the band 9 range. Those facts clarify the exposure table without converting it into a clinical dose-response analysis. Repeated-visit inference, moving-average standardization, multiplicity, and causal interpretation remain separate tasks. Keeping them separate allows the secondary analysis to be checked directly and prevents a convenient standardized contrast from carrying more scientific meaning than its construction supports.

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