

# Revisión crítica del preprint EGUSPHERE-2026-1317

I have read with interest your preprint entitled “Maximum Certainty Principle applied to rainfall modelling and regionalisation in Ecuador” (EGUSPHERE-2026-1317), and I must state the following:

## Abstract and Introduction

The abstract claims that statistical independence has been demonstrated between short-duration rainfall intensities (less than 2 hours) and daily intensities, based on a correlation coefficient of  $R^2 \leq 0,64$ . This value alone does not imply independence; it confuses basic statistical concepts and exaggerates the findings. Furthermore, the introduction invokes Noether’s theorem and symmetries to justify a “probabilistic invariant”, but the symmetry is never identified, so the analogy is forced.

## Foundations of the Maximum Certainty Principle (sections 2.1 to 2.2)

The variational formulation of the PCM does not constitute a new principle, but rather a reformulation of the Maximum Entropy Principle (MaxEnt) with an arbitrary prior. The central functional

$$\mathfrak{N}_{\text{máx}} = C(t) - \ln(f_T(t)),$$

where  $C(t)$  is vaguely defined as a “knowledge potential”, directly leads to the solution  $f_T(t)$ . Mathematically, this is equivalent to standard entropy maximization considering an exponential distribution, as described in Kapur (1989). More seriously, explicit Lagrange multipliers are not derived, nor is the Euler-Lagrange equation solved rigorously; it is mentioned in a circular reference that this is done in Beltrán (2023), but no demonstration is provided. The resulting truncated exponential distribution for the intra-event structure is a classic model in hydrology (Eagleson, 1978; Rodríguez-Iturbe et al., 1987), used for decades. Renaming it as the “Maximum Certainty Principle” does not represent a real methodological advance. Moreover, assuming that  $C(t)$  is monotonic and independent of elevation is physically unsustainable in the Metropolitan District of Quito (DMQ), where rainfall intensity exhibits strong altitudinal gradients between 2600 and 4555 m a.s.l., as shown in [missing reference]. The mathematical development presented to justify the proposed distribution contains a key structural flaw.

In particular, the manuscript states that an expression of the form:

$$C(n) + I_n = -\alpha \frac{n}{m} - \ln p_n$$

is “invariant”, and based on this proceeds to take it outside the expectation operator in order to derive a functional form for  $p_n$ .

However, this step is not justified. For a quantity to be considered invariant (and thus removable from an expectation), it must be constant with respect to the index  $n$ , which is not demonstrated at any point. On the contrary, both  $C(n)$  and  $I_n = -\ln p_n$  depend explicitly on  $n$ .

Furthermore, assuming that:

$$-\alpha \frac{n}{m} - \ln p_n = \text{constant}$$

directly implies that:

$$p_n \propto e^{-\alpha n/m}$$

That is, the exponential form of the distribution is not derived, but rather implicitly imposed by the invariance assumption. This constitutes a problem of mathematical circularity: the result is already contained in the premise.

## MIT-Q Model (sections 2.3 to 2.3.4)

The model is calibrated with daily data from a single station (Quito-Observatory) for the period 1916-1992, completely ignoring modern high-resolution rain gauge networks (5-minute data) that have been operating in the DMQ for at least 20 years. In a context of climate change, using such old records violates the stationarity assumption and fails to adequately capture recent extreme events, especially short-duration ones. The author does not explain how sub-hourly intensities are derived from daily data, which is a serious limitation. A descriptive analysis of the pluviometric information from the Quito-Observatory station should have been performed, indicating whether the records are sub-hourly, hourly, daily, or band data. Nothing is explained about the data source; it only says “the century-old Quito-Observatory station”, here and in all of Beltrán’s circular references.

Furthermore, the model includes a dimensionless parameter calibrated to a value close to 10 and vaguely related to the acceleration of gravity ( $g$ ). Note that in Beltrán (2023) it is stated that “gravity ( $g$ ) induces information that forces the formation of preferably early random patterns”, without any physical derivation justifying this relationship. The explanation of “upward pulses suppressed by gravity” is a metaphor or pseudoscience. Later, this same parameter varies between 2 and 35 depending on the station, contradicting the supposed universality of the gravitational constant.

Model validation is presented only through visual comparisons of IDF curves (Figure 5), without quantitative error metrics that show model skill, such as RMSE, BIAS, confidence intervals, or statistical tests. Validation based solely on graphical inspection is insufficient for a work that claims to provide a new methodology for extreme rainfall regionalisation.

## Regionalisation and Potential IDF curves (sections 2.4 to 3.1)

The derivation of the key equation relating the parameter  $\tau$  to the empirical coefficient  $b$  of the INAMHI IDF curves is presented opaquely, with algebraic steps omitted. It is not

clear how the expression  $1 - b$  is reached nor why  $\tau$  is assumed constant for all durations. The introduction of the “transition storm” appears to be a mathematical artifice to join two duration regimes, without observational evidence that storms with that property exist.

The scaling factor  $\phi(b) = b^{-0.237}$  is obtained through an empirical regression (Figure 8a), but goodness-of-fit measures are not reported nor is estimation uncertainty discussed. Most worryingly, this regression is not derived from the Maximum Certainty Principle; it is a purely empirical correction that has nothing to do with the variational functional presented in the first part of the paper.

The scaling factor maps (Figure 9) are presented as deterministic results, but no uncertainty propagation from station parameters to map cells is included. Nor is the spatial interpolation method specified. In a region with strong orographic gradients (exceeding 20%), ignoring altitude and topography is a serious omission. Note: precipitation is not random.

## Discussion and Conclusions

The conclusions exaggerate the scope of the work. It is claimed that the PCM “modifies the inferential interpretation of extreme event modelling”, but this is not demonstrated with concrete examples or comparisons against established methods (L-moments, maximum likelihood, two-state Poisson models). The potential IDF curves are presented as an operational contribution, but they lack independent validation with dense high-resolution networks. There is no analysis quantifying the improvement over IDF curves generated by EPMAPS or INAMHI.

## Appendix and Reproducibility

The calibration and validation are insufficient for the regionalisation the author seeks to justify. The Storm Information Model (MIT-Q) is calibrated at a single station (Quito Observatory, 1916-1992) and validated only at four additional stations over approximately 2500 km<sup>2</sup>. This control point density is extremely low to capture convective and mesoscale processes, as well as the dominant orographic effects in the Andes. The approach lacks quantitative cross-validation metrics and does not evaluate model skill using statistics such as MAE, RMSE, NSE, or MAPE, nor does it demonstrate superiority for short-duration (< 2h) extreme events, where tail behaviour is critical.

## Finally

The work mixes a variational theory that provides no real novelty (equivalent to maximum entropy) with a storm model calibrated using obsolete data and an empirical regionalisation that is not derived from the proposed principle.

**Additional shortcoming (not addressed in the original preprint):** There is no sensitivity analysis of the parameter  $\alpha$  (or  $\phi$ ) with respect to the length of the calibration period, nor any discussion of non-stationarity. Given that the calibration period (1916-1992) predates the most intense warming decades, the assumption that the same IDF curves hold for future or even present-day conditions is unjustified. The author should

at least test for trends in extreme rainfall and, if none are found, provide evidence of stationarity. The complete absence of this analysis weakens any practical recommendation derived from the model.