Detailed critique of Beltrán's response

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Abstract

This document constitutes a comprehensive technical reply to the responses provided by Beltrán (April 2026) to the critical comments on preprint EGUSPHERE-2026-1317. It analyses in depth the conceptual, methodological and reproducibility deficiencies of the original work, as well as the evasions and shortcomings in the author's response. Mathematical formulations, demonstrations of equivalence with MaxEnt, contradictions in validation, problems with circular references and lack of empirical justification are included.

1 Introduction

After reading Beltrán's response (document *Responses to Reviewer Comments*, 15 April 2026) to the original comments (CC1) on the preprint `egusphere-2026-1317`, it is evident that most of the fundamental objections have not been resolved, but merely glossed over with terminological changes and promises of unverifiable appendices. This report breaks down, point by point, the persistent deficiencies, adding new observations regarding the lack of verifiability, spatial contradictions and the methodological vacuum of the supposed *Maximum Certainty Principle* (MCP).

2 Objection to the Abstract and the MIT-Q simulation

2.1 Scope of the simulation and lack of peer review

The original abstract mentions a ≪*MIT-Q simulation application*≫ which, according to the author's response, is used to generate 500 years of synthetic events. However:

- The MIT-Q model **has not been subjected to independent peer review**. The cited references (Beltrán, 2023; Beltrán, 2022) are works not published in indexed journals or are inaccessible degree theses.
- The choice of Weibull and truncated exponential distributions is presented without any verifiable justification. No goodness-of-fit tests (Kolmogorov-Smirnov, AIC, etc.) are provided against other distribution families (gamma, GEV, generalised Pareto).
- Simulating 500 years of extreme events from a single station (Quito-Observatory) and extending the results to an area of 2500 km^2 is methodologically unsustainable.

The spatial variability of precipitation in Andean terrain with altitude gradients exceeding 20% cannot be captured with a single historical record, no matter how long.

2.2 Validation against IDF curves, not against observed data

The author claims to validate the model by comparing its simulated intensities with ≪*official IDF curves of the DMQ*≫ (Metropolitan District of Quito). However:

- IDF curves are **statistical approximations** derived from historical series, not raw data. Validating a model against another approximation is a methodological circularity.
- No comparison is presented with real high-resolution rain gauge records (5-minute data) that have existed in the DMQ for more than 20 years. The author deliberately ignores this information.
- Validation with only 4 stations (Izobamba, Inaquito, La Tola, DAC-Aeropuerto) for a domain of 2500 km² is grossly insufficient. The control density is less than 0.002 stations per km².

3 Comment 1: Statistical independence and unverifiable references

3.1 Lack of demonstration of independence

The author replaces the term *independence* with *weak linear dependence* but does not correct the underlying conceptual error:

- A coefficient of determination $R^2 \leq 0.64$ does not imply absence of dependence, not even linear. The author does not present independence tests (such as Spearman's rank correlation test, mutual information or chi-square test).
- At no point is it shown that daily and sub-hourly intensities are independent; it is only shown that linear correlation is low for very short durations (5-10 min). This is an expected and well-known result, not a novel finding.

3.2 Circular and unverifiable references

The author includes graphs and statements based on:

- Beltrán (1995, pp. 138-139): unpublished civil engineering thesis, no public access. The pluviographic data used are not deposited in any verifiable repository.
- Andrade (1997, p. 33): proceedings of a national congress, not available online and without DOI. It cannot be consulted or verified.
- The author promises to include these data in supplementary material; however, upon reviewing this documentation, no raw data exist. What exists is a series of files corresponding to MIT-Q simulations (*Supplementary_Data_MITQ_Calibration_Validation.xlsx*), but this was not materialised independently before the review. In science, claims must be verifiable without relying on the author's goodwill.

Therefore, the reader is forced to *believe* without being able to verify, which violates basic principles of reproducibility.

4 Comment 2: The Maximum Certainty Principle (MCP) is equivalent to MaxEnt

4.1 Formal equivalence with MaxEnt

The author denies that the MCP is a reformulation of MaxEnt, but the mathematical demonstration shows otherwise. The proposed functional is:

$$\mathcal{F}[f] = \int [C(t)f(t) - f(t)\ln f(t)]\,dt$$

with $f(t)$ probability density and $C(t)$ the so-called *knowledge potential*. Unconstrained maximisation (or with the constraint $\int f = 1$) leads to:

$$f(t) = \frac{e^{C(t)}}{\int e^{C(t)}dt}$$

which is exactly the form of a Gibbs distribution (or maximum entropy distribution) with a prior $e^{C(t)}$. The author introduces no additional non-trivial constraints; therefore, the MCP is mathematically identical to MaxEnt with an arbitrary prior. Renaming it does not constitute an advance.

4.2 Variational derivation and Noether's theorem

The author justifies the constancy of $\aleph_{\max}$ by the independence of the Lagrangian with respect to $P(t)$. He shows that:

$$\frac{d}{dt}\left(\frac{\partial L}{\partial f}\right) = 0 \quad \Rightarrow \quad C(t) - \ln f(t) = \text{constant}$$

This is a simple application of the Euler-Lagrange equation, not a consequence of Noether's theorem. Invoking Noether requires an explicit continuous symmetry (translations, rotations, etc.) that leaves the action invariant. The author does not identify any symmetry; the independence from $P(t)$ is a property of the Lagrangian, not a symmetry of the system. Therefore, the reference to Noether is pseudo-scientific and should be completely removed.

4.3 Ad hoc choice of $C(t) = -\lambda t$

For the case of storms, the author chooses $C(t) = -\lambda t$ without physical or empirical justification beyond ≪*it works*≫. This choice leads to a truncated exponential distribution, which is a classic model (Eagleson, 1978; Rodríguez-Iturbe et al., 1987). There is no derivation from first principles; it is an embedded parametric adjustment.

$$f(t) \propto e^{-\lambda t} \quad \Rightarrow \quad \text{truncated exponential temporal structure} \tag{1}$$

Thus, the MCP adds nothing new.

5 Comment 3: The MIT-Q model: calibration, validation and spatial contradictions

5.1 Calibration with unverifiable data

The author lists calibration sources:

1. *Official IDF curves of the DMQ*: they are approximations, not observed data.
2. *Annual number of events ¿0.1 mm* from Pourrut and Leiva (1989): however, I have read the cited works for a long time and they do not provide numerical tables, only graphs; there is no access to the original records, which makes verification difficult.
3. *Intra-event temporal structure via quartiles* (Beltrán, 1995): unpublished thesis, no accessible data.

None of these sources is independently verifiable. The author promises to include them in a supplement, but to date there is no public repository with such data.

5.2 Validation with only 4 stations and against IDF curves

The author claims to validate the model at four ≪*nearby*≫ stations. However:

- The simulated domain is 2500 km²; four stations amount to a density of 0.0016 stations/km², insufficient to capture convective and orographic processes.
- The comparison is made against the IDF curves of those stations (derived product), not against real hourly or sub-hourly records. This hides adjustment errors in extreme quantiles.
- At the DAC-Aeropuerto station, the MAPE reaches 68% for a duration of 360 minutes (see author's response, p. 12). The author minimises this discrepancy by attributing it to ≪*low intensities*≫, but an error of 68% is unacceptable in any hydrological modelling context.

5.3 Contradiction between spatial adjustment to annual precipitation and generation of convective events

The author describes that the model generates individual storms and iteratively aggregates them until reproducing the field of *annual isohyets* (annual accumulated precipitation). This approach is problematic for two reasons:

1. **Equifinality**: Many combinations of different event types (short convective vs. long stratiform) can yield the same annual precipitation. Adjusting only to the annual sum does not guarantee that the temporal distribution of extreme intensities is correct.

2. **High mountain areas (¿3500 m a.s.l.)**: In the Andes, above 3500-4000 m a.s.l., low-intensity, long-duration rainfall (orographic type) predominates, with very few intense convective events. The MIT-Q model, by forcing an adjustment to annual precipitation, could be generating false convective events where there are none (overestimating extreme intensities) or, conversely, underestimating the contribution of prolonged rainfall.

The author does not address this issue in his response. He also does not present any sensitivity analysis or validation at high-moorland stations (e.g., stations above 4000 m a.s.l.).

5.4 The parameter α_0 and its variability

The author defines $\alpha_0 \approx 10$ as a dimensionless parameter ≪*maximum temporal structuring capacity*≫. However, he admits that when advection is considered, α_p varies between 2 and 35 depending on the station (see Fig. 8 of the manuscript). This variability contradicts the interpretation of an intrinsic model parameter and suggests over-parameterisation. Moreover, the previous references to gravity (≪*gravity (g) induces information*≫) have been removed, but no alternative physical explanation is offered.

6 Comment 4: Regionalisation and potential IDF curves

6.1 Opaque derivation of τ

The author adds an Appendix C in his response. Although some algebraic steps are shown, the derivation remains conceptually weak:

- Mean and instantaneous intensities are equated at a tangency point t_1 without justifying why that point corresponds to a regime change.
- The resulting equation (C6): $2t_1\frac{e^{-t_1}}{1-e^{-t_1}} = 1 - b_1$ implies that t_1 is a function of b_1, but the author defines τ as constant for all durations. How is this constancy reconciled with the dependence on b_1? It is not explained.
- Equations (C8)-(C14) introduce ad hoc variables ($\tau_1, \tau_2, DT_1, DT_2$) that do not arise naturally from the MCP. They remain an algebraic fitting exercise, not a derivation from first principles.

6.2 Transition storm: artifice without evidence

The author explicitly admits that the transition storm ≪*is not a physically independent or directly observable event*≫ (p. 15 of his response). It is therefore a *conceptual construct* introduced solely to ensure continuity between two duration regimes. In science, postulating unobservable entities is justified only if they produce testable predictions. The author presents no prediction that can be independently tested. The transition storm is a mathematical artifice without empirical support.

6.3 Scale factor $\phi(b) = b - 0.237$: empirical regression not derived from the MCP

The author acknowledges that this relationship is ≪*empirical*≫ and is not derived from the MCP. It is obtained by fitting a power law to 66 INAMHI stations (R^2=0.968). However:

- No confidence intervals are reported, nor is cross-validation performed. A high R^2 is expected when the relationship is nearly linear; it does not imply that the model is predictive for new stations.
- The regression is independent of the rest of the variational theory. Therefore, the MCP plays no role in the most relevant part of the regionalisation. The work is hybrid: an ornamental variational theory and a conventional empirical fit.

6.4 Maps and lack of uncertainty

Figure 9 presents maps of the scale factor without any uncertainty propagation. The spatial interpolation method is not specified. The author claims that altitude is implicitly included in the station parameters, but this is not true: interpolation between stations must explicitly consider topography, especially in regions with gradients exceeding 20%. Ignoring orography is a serious omission.

7 Comment 5: Exaggerated discussion and conclusions

The author modifies the phrase ≪*modifies the inferential interpretation*≫ to a more moderate one, but still claims that the MCP ≪*provides an alternative framework*≫. No quantitative comparison is presented with standard methods (L-moments, maximum likelihood, two-state Poisson models). Nor is it demonstrated that the *potential* IDF curves improve upon existing official curves. The conclusions therefore lack empirical support.

8 Comment 6: Reproducibility and cross-validation

The author limits himself to including MAPE as an additional metric. The following are not presented:

- Cross-validation (e.g., leaving one station out).
- Confidence intervals for extreme intensities (e.g., bootstrapping).
- Residual analysis or goodness-of-fit tests for the tails.
- Comparison with the traditional GEV approach fitted to the same data.

Reproducibility is impossible because the primary sources (Beltrán 1995, Andrade 1997, etc.) are not publicly available. The author promises to include them ≪*in the supplementary material*≫, but during the review process they have not been provided in a permanent repository.

9 Comment 7: Overall conceptual contribution – synthesis of criticisms

Overall, the work suffers from the following insurmountable problems:

1. **Lack of theoretical novelty**: The MCP is MaxEnt with an arbitrary prior. No new distribution is derived, nor are results obtained that cannot be obtained with standard methods.

2. **Circular references and unverifiable data**: Most of the cited data and previous works are inaccessible (theses, congress proceedings, technical reports without DOI). This violates the principle of reproducibility.

3. **Insufficient validation**: Only 4 stations, against IDF curves (not real data), with very high errors at some stations (MAPE 68%). No comparison with alternative methods.

4. **Spatial contradictions**: Adjusting to annual precipitation does not guarantee a correct simulation of extreme convective events, especially in high mountain areas.

5. **Abuse of mathematical terminology**: References to Noether's theorem and variational calculus without genuine application, only as rhetorical ornament.

10 Conclusion and final recommendation

For all the above reasons, it is concluded that the manuscript EGUSPHERE-2026-1317, as well as the author's response, do not remedy the fundamental deficiencies.

Sincerely,
Diego Escobar-González
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