

AUTHOR COMMENT AC6

Response to the main comments of referee report RC1

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Comment submitted by:

Anonymous Referee (9 September 2026)

Response prepared by:

Franklin Beltrán and Jhoan Beltrán (12 September 2026)

IDD Research

Address: Av. 6 de Diciembre N30-105 y Alpallana, Gayal 2
Building, Floor 3, Office 301, Quito, Ecuador.

Mobile: +593 99 710 3585

Website: <https://www.idd-research.org/idd-research>

CONTENTS

1	Introduction	3
2	Major Comments.....	3
2.1	Comment: PCM is the Gibbs construction, and the invariant is its defining identity..	3
2.1.1	Response to the comment “PCM is the Gibbs construction, and the invariant is its defining identity”	4
2.2	Comment: What is being modelled is never defined	7
2.2.1	Response to the comment “What is being modelled is never defined”	7
2.3	Comment: The model actually used does not follow from the principle	9
2.3.1	Response to the comment “The model actually used does not follow from the principle”	9
2.4	Comment: The IDFP curves are a rescaling of the information used to construct them 10	
2.4.1	Response to the comment “The IDFP curves are a rescaling of the information used to construct them”	11
2.5	Conclusions	14
3	References.....	15

1 Introduction

This author comment responds to referee report RC1, received on 9 September 2026, which raises four major comments and recommends rejection of the manuscript in its present form. We thank the referee for a report that separates mathematical results, model assumptions and evidence with precision, a distinction the manuscript did not draw clearly enough and which this response adopts. We answer the four comments in the order in which they were raised.

Each comment is treated in three parts. *What we accept* sets out the referee’s observations that we consider correct and concedes them without reservation. *What we maintain* sets out the points on which we disagree, together with the verification that supports them. *Committed changes to the revised manuscript* lists what will be incorporated.

The arguments are numbered consecutively from 1 to 26 and the committed changes from C1 to C19, so that they may be referred to without ambiguity. The text of the report is reproduced at the start of each section. Where this document cites earlier author comments it does so by their identifier, AC1 and AC3; where this response supersedes a position held in them, it says so explicitly.

We set out the balance in advance to aid reading. We accept the Gibbs identity and withdraw the claim to a distinct principle of inference and to a newly defined invariant. We retain the variational formulation as the specification of the storm model: it fixes the family of profiles, the space of arrangements and the ordering rule, and it will be presented as such. We correct two typographical errors in the preprint that affect reproducibility, we replace an empirical fit with an exact expression, and we present the IDFP curves as scenarios conditional on the model whose magnitude requires independent testing.

2 Major Comments

2.1 Comment: PCM is the Gibbs construction, and the invariant is its defining identity

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The certainty functional is, in effect, $\mathcal{N}[f] = \langle C \rangle_f + H[f] = \int f C dt - \int f \ln f dt$. Maximising this over normalised densities is the standard Gibbs variational problem. Introducing a multiplier for normalisation gives the stationarity condition $C(t) - \ln f(t) - 1 - \gamma = 0$ and hence $f = e^C/Z$ with $Z = \int_0^{DT} e^C dt$, which is Eq. (10). The cleanest way to see it is the identity $\mathcal{N}[f] = \ln Z - D_{\text{KL}}(f \| e^C/Z)$: since the Kullback–Leibler divergence is non-negative, the maximum is attained exactly at $f = e^C/Z$ and its value is exactly $\ln Z$. This is not a stationarity result that might hide further structure; it is a global identity.

This is the standard statement that a Gibbs measure has constant free energy. It is not a conservation law in Noether’s sense. A Noether construction requires an action, a continuous transformation leaving it invariant up to a divergence, and a derived conserved quantity. None is exhibited anywhere in the manuscript. The introduction’s discussion of proper and improper divergence relations, citing Kosmann-Schwarzbach (2011), is particularly unfortunate, since an improper conservation law is precisely one following from a differential identity rather than from dynamics, which is exactly what $\mathcal{N}_{\text{max}} = \ln Z$ is.

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2.1.1 Response to the comment “PCM is the Gibbs construction, and the invariant is its defining identity”

2.1.1.1 *What we accept*

1. We accept the identity the referee points to, and with it the conclusion he draws:

$$\aleph[f] = \ln Z - D_{KL}\left(f \parallel \frac{e^C}{Z}\right), \quad Z = \int_0^{DT} e^{C(t)} dt$$

This is an identity valid for every admissible f , with C fixed and $0 < Z < \infty$: the non-negativity of the Kullback–Leibler divergence places the maximum at $f = e^C/Z$ (except on sets of measure zero) and assigns it the value $\ln Z$. Euler–Lagrange stationarity yields the same maximiser; what the identity adds is that this maximum is global and that its value conceals no further structure. Consequently, $\aleph_{\max}$ is the logarithm of the partition function and its constancy is that identity, not a further result of the principle.

2. The referee cites both constructions, the Gibbs one and the maximum-entropy one; they share the mathematical structure he invokes: the exponential family, the partition function and the identity for $\ln Z$. We establish the correspondence with Jaynes (1957), which is the reference the manuscript already cites and the one that allows it to be fixed equation by equation. His Eq. (2-12) gives $\lambda_0 = \ln Z$ and his Eq. (2-13), $S_{\max} = \lambda_0 + \sum_r \lambda_r \langle f_r \rangle$; with $C(x) = -\sum_r \lambda_r f_r(x)$ that equation reads $\ln Z = S + \langle C \rangle = \aleph$, and the logarithm of his Eq. (2-10) gives $C(x) - \ln p(x) = \ln Z$ for every x . Our Eqs. (6), (8) and (9) are his Eqs. (2-10) to (2-13), and $\aleph_{\max}$ is his λ_0 .

3. The correspondence is not confined to densities over time. The identity of item 1 holds on any finite state space, including that of the $m!$ arrangements of a sequence, and entropic inference over trajectories has established antecedents (Pressé et al., 2013). We therefore do not maintain that Eq. (5), defined over the $m!$ trajectories, places the manuscript outside that framework, nor that the use of the space of arrangements constitutes in itself a distinct principle of inference. What the manuscript does on that space is described in items 7, 9 and 10; none of those steps is a new inferential result, and this will be stated.

4. We withdraw the reference to Noether, together with the passage in the introduction on proper and improper divergence relations, as was already committed in AC3 §4.2.1. The manuscript exhibits no action, no continuous transformation and no derived conserved quantity.

5. We withdraw the response given in AC1 §2.1.1 in view of the acceptance in item 1. We likewise withdraw the interpretation in AC3 §4.1.1, which presented the formulation over $PT(t)$ as a substantive difference from the formulation over the density (see item 8), and we qualify AC3 §4.3.1: that C does not act as a fitting parameter during simulation does not mean that it is not a postulated form whose parameter is calibrated (item 12).

6. We correct two aspects of the exposition. The first concerns the relation between the continuous and discrete treatments, presented in the manuscript as parallel developments. In fact the second is a finite-dimensional restriction of the first. Let $\Delta t = DT/m$ and restrict $P_T(t)$ to the set of piecewise linear functions on m uniform intervals, with increments $P_n = P_T(t_n) - P_T(t_{n-1})$; then $f_T = P_n/\Delta t$ on each interval. Substituting this representation into the continuous functional of Eq. (17) of Beltrán (2023), with $C(t) = -\lambda t$, gives the discrete functional of his Eq. (45) except for two additive terms independent of $\{P_n\}$ for fixed m : $\ln(DT/m)$, from the passage from differential to discrete entropy, and $\lambda DT/(2m)$, from integrating the linear term over each

interval. These terms do not affect the maximiser; hence $P_n \propto e^{-\lambda DT \cdot n/m}$, recovering his Eq. (13). The result is established for a linear potential and a uniform partition; for a general C the constants are replaced by the averages of C over each interval. The discrete treatment is therefore not a further postulate, but the restriction of the same continuous functional to a finite partition.

7. The second aspect concerns the ordering of the arrangements. Sections 2.1.5–2.1.7 construct the object and state in addition which arrangement realises Maximum Certainty. That statement follows as a corollary: the informational term is the same across trajectories sharing the values of $P'_T(t)$, and the coupling with $C(t)$ attains its maximum for the similarly ordered arrangement (rearrangement inequality, Theorem 368; Hardy et al., 1952). Section 2.1.7 already notes this when it observes that entropy has no order. This corollary orders the arrangements of an already fixed vector of increments; it does not determine the probability with which each one occurs, which is a different problem (item 9). We also correct, in Section 2.1.5, the statement that the Knowledge term $C(n)$ is kept “in a strictly increasing order”, which is inconsistent with $C(t) = -\lambda t$: the index n orders the bursts by rank, and the orientation must be stated.

8. The identity of item 1 is established over the density $f(t)$; the functional of the manuscript, over $P_T(t) = \int_0^t f$. Under the admissibility conditions (P_T absolutely continuous, $P_T(0) = 0$, $P_T(DT) = 1$) the two are equivalent: they describe the same object and the same variational problem, not merely the same maximum. We withdraw the reading under which the formulation over P_T adds structure that the formulation over f does not have. Why the manuscript is written over P_T is explained in item 11.

2.1.1.2 What we maintain

9. It is useful to separate three problems that the manuscript did not distinguish clearly. The first is maximisation over densities, settled by the identity of item 1. The second is which arrangement of a fixed set of increments is optimal, settled by the corollary of item 7. The third is the probability with which each arrangement occurs: that is a measure over the $m!$ permutations, it does not follow from an entropic criterion, it requires a further assumption, and it is what the model executes.

10. That measure is Eq. (14) of Beltrán (2023):

$$P_T = P_1 \cdot \frac{P_2}{1 - P_1} \cdot \frac{P_3}{1 - P_1 - P_2} \cdots \frac{P_m}{1 - \sum_{i=1}^{m-1} P_i}$$

The rule that generates it is explicit: the bursts occur one after another, none is repeated, and at each step a burst is selected with probability proportional to its weight among those not yet drawn. The denominators $1 - P_1$, $1 - P_1 - P_2$, ... are what remains to be allocated after each draw. This is the form of sequential weighted sampling without replacement (Plackett, 1975), and it is an assumption about the process, not a consequence of a principle of inference; it will be stated as such.

Over the set of $m!$ arrangements, unconstrained entropy maximisation yields the uniform distribution, placing the highest-ranked burst in each position with probability $1/m$; for $m = 4$, 25 % in each. Fig. 2 of Beltrán (2023) shows 46.3 / 31.7 / 16.8 / 5.2 %, and Eq. (14) of that work reproduces them as 46.34 / 31.69 / 16.84 / 5.13 % for $\alpha = 2.08$ and $m = 4$. This reproduction is a check of the calculation, not a comparison with observations.

We delimit its scope. For modelling purposes the manuscript does not use Eq. (14) of Beltrán (2023) in its pure form. Its Eq. (18) takes as weights the mean temporal pattern between the two states of the transition: the Lambda state, $\lambda_0 = \lambda$, and the final state, $\lambda_0 = 0$, in which one trajectory cannot be distinguished from another. With $\alpha_0 = 10$ and $m = 8$, which is the configuration of the model, the highest-ranked burst falls in the eight positions with 41.9 / 27.0 / 16.1 / 8.7 / 4.1 / 1.6 / 0.5 / 0.1 %, against 12.5 % for the uniform distribution. In the final state Certainty reduces to entropy, because $C(t) = 0$, and its maximum is the uniform distribution; that coincidence with the entropic result follows from the exhaustion of Knowledge, not from having invoked a principle of inference.

11. It remains to explain why the manuscript poses the problem over $P_T(t)$ and not over the density. The reason is not informational, and we withdraw what in the manuscript and in AC3 suggested otherwise. Two storms with the same depth increments in a different order share their set of increments and the entropy computed over them; they are distinguished both by their temporal density $f(t)$ and by their cumulative $P_T(t)$, which carry the same information. We choose the cumulative because it is the depth recorded by the rain gauge and because the boundary conditions $P_T(0) = 0$, $P_T(DT) = 1$ are imposed directly upon it. What the model does fix, and not the functional, is the space on which it acts: a storm is a sequence (Huff, 1967), the set of increments is determined by DT , PRE and α_0 , and what is drawn is their arrangement in time. The Certainty of Eq. (5) takes one value for each of the $m!$ ways of ordering the same set of magnitudes; that space and the rule of item 10 are assumptions of the model, and will be presented as such.

12. There remains, finally, a difference that is not one of result but of point of entry. Maximum-entropy inference starts from given constraints and obtains the multipliers from them. The manuscript starts from a Knowledge function postulated from the phenomenon, the Experience that accumulates over time, with the parameter to be calibrated. The resulting family is the same; what differs is which information is introduced, and in both cases that information enters from outside. This difference in point of entry does not constitute a distinct principle of inference and we do not claim it as one. The referee describes the situation by saying that the potential is fixed by assuming the answer. The description is correct: $C(t)$ is an assumption about the phenomenon, declared as such, with the parameter subject to calibration; it is not a result of the principle.

2.1.1.3 Committed changes to the revised manuscript

- C1. Explicit identification of $\aleph_{\max} = \ln Z$ with Ec. (2-13) of Jaynes (1957), in Sections 2.1.7 and 2.1.8 and in the discussion, citing also Cover and Thomas (2006): the invariance of $\aleph_{\max}$ with respect to temporal order is that identity, not a further result.
- C2. Withdrawal of the reference to Noether and of the passage on proper and improper divergence relations.
- C3. Rewriting of Sections 2.1.5 to 2.1.7: the ordering of arrangements is restated as a corollary of the invariance of the informational term under rearrangement and of the Hardy, Littlewood and Pólya inequality (Theorem 368), expressly distinguished from the probability law of the arrangements; correction of the orientation of the index n in Section 2.1.5; explicit connection with the temporal-pattern subroutine (Fig. A2).
- C4. Statement that the discrete functional is the restriction of the continuous one to piecewise linear functions, with the two constants that separate them and the hypotheses under which it holds (linear potential, uniform partition).

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- C5. Delimitation of Ec. (14) of Beltrán (2023) and of Ec. (18) as an assumption about the process: sequential weighted sampling without replacement with the weights of Ec. (18), citing Plackett (1975).
- C6. Rewriting of Section 2.1.8: the formulation over $P_T(t)$ is retained on account of the object the model constructs, and every claim of additional informational or variational structure relative to the formulation over f is withdrawn. Use of the dimensionless variable $u = t/DT$ and of a common reference in the informational terms.
- C7. Express statement, in Section 2.1.8, that $C(t)$ is an assumption about the phenomenon with the parameter subject to calibration, and not a result of the principle.
- C8. Withdrawal, in the abstract, the introduction, the discussion and the conclusions, of the presentation of Maximum Certainty as a newly defined probabilistic invariant and of every claim of superiority over maximum entropy. The Maximum Certainty Principle is retained as the name of the variational formulation that specifies the model; it is not presented as a principle of inference distinct from the Gibbs and maximum-entropy framework.

2.2 Comment: What is being modelled is never defined

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The manuscript is not consistent about what f is. In Sects. 2.1.8 and 2.3.1, T is a random variable, “the residence time in the development state, or the ability to remain in optimal conditions until reaching an unsustainable point”, and f_T is its density, so Eq. (13) is a probability statement about when a storm ceases developing. In Sect. 2.3, what is actually used is $\text{Pre}(t)/\text{PRE}$, the fraction of the event’s total depth accumulated by time t , which for a given storm is a deterministic mass profile and not a probability at all. Both are non-negative and integrate to one on $[0, DT]$, but they are different objects. Sharing an interval, monotonicity and endpoint values does not make them equivalent.

The ambiguity matters because the two halves of the paper need different readings. The apparatus of Sect. 2.1, comprising surprise, entropy, the unexpectedness of an event and the elimination of trajectories, requires a probability, hence the first reading. The application requires the second.

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2.2.1 Response to the comment “What is being modelled is never defined”

2.2.1.1 What we accept

13. We accept the observation in both its parts. The first is expository: the manuscript defines the random object in Section 2.1.2 and makes it concrete in Section 2.3.1, but does not set out in a single place the chain linking the two with what the simulation executes. The second is substantive: the identification of T as “the residence time in the development state” is an interpretative hypothesis. T was not measured as such, it has no observational definition distinguishing it from DT and from t_d , and its distribution was not tested. In the revised manuscript that reading is presented as a physical hypothesis and not as a result.

14. The referee is right that Eq. (14) does not define the link between a residence time and a rainfall mass; the square and the approximation sign relate two functions, not two objects. In the revised manuscript the objects are defined separately, with $u = t/DT$:

$$F_\alpha(u) = \frac{1-e^{-\alpha u}}{1-e^{-\alpha}} \text{ (Eq. 13, auxiliary distribution function)}$$
$$G_\alpha(u) = F_\alpha(u)^2 \text{ (Eq. 15, normalised mass profile)}$$

$$h_j = PRE \cdot [G_\alpha(j/m) - G_\alpha((j-1)/m)] \text{ (depth of burst } j)$$

$$w_j = \frac{1}{2} \cdot \left[F_\alpha\left(\frac{j}{m}\right) - F_\alpha\left(\frac{j-1}{m}\right) \right] + 1/(2m) \text{ (Eq. 18, selection weight)}$$

For an event with (PRE, DT, α_o , m) fixed, the effective random variable is the arrangement π of the m bursts, with the law of item 10 applied to the weights w_j ; the realised cumulative depth is $H_\pi(kDT/m) = \sum_{j \leq k} h_{\pi(j)}$. This is the object the simulation produces and the one compared with the records.

2.2.1.2 What we maintain

15. The random object is defined in two mutually corresponding places in the manuscript. Section 2.1.2 defines it in general terms: the temporal pattern is a discrete, finite and sequential process without replacement; the compound variable X takes values in the space of the m! trajectories, and probability is defined as a measure on that space. Section 2.3.1 makes it concrete in the phenomenon through the variable T, whose interpretation is qualified in item 13. The chain linking the two starts from λ_o . Eq. (13) gives F_α ; from there two branches follow: Eq. (15) fixes the depth of each burst, and Eq. (18) gives the weights governing the sequence of the conditional chain, that is, the arrangement in which the bursts occur. The joint realisation of both is the simulated hyetograph. The rule relating the two branches is a further assumption of the model and is declared as such.

16. The steps are not calibrated separately. A value of λ_o is imposed, the simulation is run over the 2500 km² of the study area, and the statistics of the simulated storms are compared with those of the recorded intense storms, in intensity, duration and temporal pattern; α_o follows from the sensitivity analysis over that set. The ordering of the bursts takes part in that comparison, and the manuscript documents it in four places: (a) Fig. A2 reproduces the ordering algorithm line by line; (b) Module 1 lists among its outputs the temporal rank order of the bursts; (c) Module 6 builds the database of the quartile of greatest precipitation of each event; (d) Fig. 2 compares the temporal distribution of 475 simulated first-quartile storms with the first-quartile curves of Huff (1990). We state what each of these tests: (d) is a descriptive comparison of the temporal shape conditional on the first quartile; it does not test the frequency with which each quartile occurs. The apparatus of arrangements is indeed used: without the ordering of the bursts there is no hyetograph to simulate and no quartile to compare. The revised manuscript states which data entered the selection of parameters and which were used only after they had been fixed.

2.2.1.3 Committed changes to the revised manuscript

- C9. Definition, in Section 2.3.1, of F_α , G_α , h_j , w_j , π and H_π , stating that Eq. (18) gives the weights of the conditional ordering chain and that the interpretation of T as a residence time is an untested physical hypothesis.
- C10. Correction of the description of Eq. (14): “proportional” is withdrawn and it is stated that the equation relates two distinct functions, the auxiliary distribution of Eq. (13) and the normalised mass profile.
- C11. Clarifications in Section 2.3.3. Terminology: the testing of MIT-Q against the IDF curves of the four additional stations is termed comparison and not validation, and it is stated that α_o and the rules of the model were fixed using the Quito-Observatory station and that the remaining stations entered only in the subsequent comparison. Nomenclature: m is the

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number of simulated bursts and α_0 the shape parameter of the truncated profile; the sentence defining α_0 as “the number of rainfall bursts in development within duration DT ” is withdrawn, and its reading as a potential number of bursts is retained as an interpretation and not as a count.

2.3 Comment: The model actually used does not follow from the principle

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Whatever one concludes about Sect. 2.1, the storm model does not inherit from it. The variational argument yields the truncated exponential cumulative form, Eq. (13). The model then discards it: Eq. (14) squares it, and the authors state plainly that “the squared form was adopted empirically because it provided the most consistent representation”. Every subsequent equation, (15), (19), (20), (23), (25), (34) and (35), uses the square.

The squaring is not a refinement but a repair, and this deserves to be stated quantitatively because it shows what the principle actually predicts. The unsquared law gives rainfall intensity proportional to $\lambda e^{-\lambda t}$, which is monotonically decreasing: the storm begins at its peak intensity and decays from $t = 0$, for every value of α_0 . This is physically impossible and is the opposite of the early-peaked but growing behaviour the paper attributes to storms. Squaring moves the peak to $t/DT = \ln 2/\alpha$. The initiation, development and dissipation structure that the paper presents as an outcome is supplied entirely by the empirical square.

Worse, the squared pattern lies outside the family PCM can generate. The implied density is proportional to PP' , so the corresponding potential is $C(t) = \ln[2P(t)P'(t)] + \text{const}$, which diverges to $-\infty$ at $t = 0$ and is non-monotone thereafter, and is therefore not of the postulated form $E + Cr$ with E decreasing and Cr increasing. The pattern used to fit the data is one the principle forbids.

The cleanest way to put the difficulty is as a question the authors should answer directly: which numerical result anywhere in the manuscript would change if PCM were deleted and Eq. (15) simply postulated as a two-parameter storm-accumulation model? As far as we can determine, none would. A reader may begin at Eq. (15) and reproduce everything that follows. The honest description of what was done is that a two-parameter shape was fitted to hyetographs and IDF curves.

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2.3.1 Response to the comment “The model actually used does not follow from the principle”

2.3.1.1 *What we accept*

17. The choice of the quadratic form was empirical: it was adopted because it better reproduced the normalised cumulative depth of the storms of the DMQ. This is a methodological fact and is declared as such. What we correct is not that declaration, but the referee’s assertion that the chosen form lies outside the family the formulation admits.

2.3.1.2 *What we maintain*

18. The quadratic form belongs to the family and its Knowledge is published. Table 2 of Beltrán (2023) records, for the quadratic truncated exponential, $E(t) = -\lambda t$ and $Cr(t) = \ln(1 - e^{-\lambda t})$, with Maximum Certainty $-\ln[2\lambda/(1 - e^{-\lambda DT})^2]$; its Eqs. (40) and (41) are the corresponding density and cumulative. The check is immediate: $e^C = e^{-\lambda t}(1 - e^{-\lambda t})$, $Z = (1 - e^{-\lambda DT})^2/(2\lambda)$, and $f = e^C/Z$ integrates to $P(t) = F(t)^2$, which is Eq. (15).

19. The referee obtains $C(t) = \ln[2P(t)P'(t)] + \text{constant}$ and observes that it diverges at the origin and is not monotonic. Both observations are correct and neither precludes the postulated form. The manuscript (Section 2.1.8) requires monotonicity of each term, E decreasing and Cr increasing or constant, not of the sum, and no boundedness, and the decomposition reads:

$$\ln[2P(t)P'(t)] = (-\lambda t) + \ln(1 - e^{-\lambda t}) + \text{constante}$$

The singularity at the origin is integrable: e^C , $f \cdot C$ and $f \cdot \ln f$ are integrable on $[0, DT]$, so that Z is finite and the identity of item 1 applies. As to the monotonicity of the sum: $C'(t) = -\lambda + \lambda/(e^{\lambda t} - 1)$ vanishes at $t = \ln 2/\lambda$, so that C increases up to that point and decreases thereafter; the interior descending phase exists when $\alpha_0 > \ln 2$, a condition met by every value of α used in the manuscript.

20. We concede the referee's reading of the origin of the temporal structure. The initiation stage comes from the term $Cr(t)$, that is, from the member of the family chosen empirically: specifying Cr or specifying F^2 are two ways of fixing the same profile, and the principle does not select that member on its own. The peak lies at $t/DT = \ln 2/\alpha_0$, as the referee himself computes. What the formulation contributes is the family within which the choice was made and the potential that characterises it.

21. The referee asks which numerical result in the manuscript would change if PCM were deleted and Eq. (15) were simply postulated as a two-parameter accumulation model. We answer directly: none. If the profile of Eq. (15), the weights of Eq. (18) and the ordering rule of item 10 are retained, withdrawing the variational interpretation alters no subsequent calculation. We add one qualification to his description of what was done: it is not merely a two-parameter profile fitted to hyetographs and IDF curves, but that profile together with a rule for ordering bursts without replacement; from that rule come the temporal patterns that Fig. 2 compares and the variability of short-duration intensities. What the formulation contributes is the specification: the family from which the profile is chosen, the space of arrangements and its optimal ordering. It is a contribution of formulation and organisation of the model, not a further empirical result, and it will be presented as such in the revised manuscript.

2.3.1.3 Committed changes to the revised manuscript

C12. Rewriting, in Section 2.3.1, of the sentence on the quadratic form: the quadratic truncated exponential is a member of the family the formulation admits, with $C(t) = E(t) + Cr(t)$, $E(\lambda t)$ and $Cr(t) = \ln(1 - e^{-\lambda t})$ (Beltrán, 2023, Table 2 and Eqs. 40–41), defined on the open interval with an integrable singularity at the origin; the choice of that member for the storms of the DMQ is empirical and the initiation, development and dissipation structure follows from that choice.

C13. Correction of the sign of the denominator of Eq. (16), $C'_o - C'_\lambda = +\lambda$, consistent with Eq. (17); and declaration of t_d as a closure relation of the model adopted with the references $C_\lambda(t) = -\lambda t$ and $C_o(t) = 0$, and not as a consequence of the identity of item 1.

2.4 Comment: The IDFP curves are a rescaling of the information used to construct them

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Stripped of the terminology, the operational outcome of the paper is $IDFP(t) = \varphi(b) IDF(t)$: an upward multiplicative tuning of the existing official curves, raising short-duration design intensities by 8 to 51%. The difficulty is where φ comes from. The baseline IDF information constrains MIT-Q. The same IDF shape parameter b is mapped into α_v by the imposed tangency. That reinterpretation generates α_p and then φ . And φ is applied back to the original curves. The chain runs from IDF to b to α_v to α_p to φ and back to IDF. The product being corrected is the principal source of the information used to generate the correction.

The “strong correlation” between φ and b in Fig. 8a is therefore not an empirical finding but a formula plotted against its own argument, and fitting $\varphi(b) = b^{-0.237}$ to it and reporting the fit quality is fitting a curve to an identity. The physical motivation, that a sparse network systematically misses the storm activity centre, is reasonable. But a motivation determines the sign of a correction, not its magnitude, and cannot validate it.

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2.4.1 Response to the comment “The IDFP curves are a rescaling of the information used to construct them”

2.4.1.1 *What we accept*

22. Fig. 8a is not an empirical test. The values of φ are computed station by station with Eq. (35), from the same official formulation that the IDFP curves modify, so that the statistic of Eq. (37) measures how closely a closed form reproduces that calculation, and not agreement with independent observation. The referee’s characterisation, that fitting a curve to an identity does not constitute a finding, is correct. We withdraw the description of it as a correlation.

23. The manuscript presents neither an uncertainty estimate nor an independent observational test for the IDFP curves. Replacing the fit of Eq. (37) by an exact expression (item 24) removes the error of that regression, but not the uncertainty of the official parameters a , b_1 , b_2 and ID, nor that of the original frequency curve, nor that of the assumption defining the IDFP curves (Eq. 33). That total uncertainty is not quantified in this revision, because we do not have the covariances of the official parameters that it would require; it is declared as a limitation and the scope of the IDFP curves is qualified accordingly (item 26).

2.4.1.2 *What we maintain*

24. The referee describes the chain accurately: b fixes α_v by tangency, Eq. (33) fixes α_p and Eq. (35) fixes ϕ . It is a deterministic chain with no fitted parameter. We do not dispute this. We dispute the consequence drawn from it, because from that same chain it follows that ϕ needs no fit at all. In verifying the derivation we also found a typographical error in the preprint that must be corrected for the calculation to be reproducible from the paper.

Erratum in Eq. (25). Tangency between the mean storm intensity, $\bar{I}(t) = K(1-e^{-ct})^2/t$, and the official curve $\bar{I}_{IDF} \propto t^{-b}$ is obtained by equating logarithmic slopes: $d \ln \bar{I}/d \ln t = 2\tau/(e^\tau - 1) - 1 = -b$, whence

$$\frac{2\tau}{e^\tau - 1} = 1 - b, \text{ that is; } \frac{2\tau e^{-\tau}}{1 - e^{-\tau}} = 1 - b$$

The preprint prints $1 - e^{-2\tau}$ in the denominator. The spreadsheet with which the published factors were obtained uses the correct form, so that the published values do not change; what changes is the printed equation. With the printed equation, the recomputed φ_1 factors differ from the published ones by 10 % on average; with the corrected one, by 0.02 %.

Closed form. Outside the transition interval $[t_1, t_2]$, Eq. (24) gives $\tau = \alpha_v t / DT$ with DT variable, so that t / DT equals τ_1 / α_v on the short-duration branch and τ_2 / α_v on the long-duration one. Substituting into Eq. (35), with $\alpha_p = \alpha_v \tau_2 / \tau_1$ from Eq. (33), the exponent of the numerator reduces to τ_2 on the first branch and to τ_2^2 / τ_1 on the second:

$$\varphi_1 = \left[\frac{(1 - e^{-\tau_2})(1 - e^{-\alpha_v})}{(1 - e^{-\tau_1})(1 - e^{-\alpha_p})} \right]^2$$

$$\varphi_2 = \left[\frac{(1 - e^{-\tau_2^2 / \tau_1})(1 - e^{-\alpha_v})}{(1 - e^{-\tau_2})(1 - e^{-\alpha_p})} \right]^2$$

where τ_1 is obtained from b_1 and τ_2 from b_2 by means of the corrected Eq. (25), α_v from τ_1 by means of Eq. (27), and α_p from Eq. (33). The factors are therefore functions of the pair (b_1, b_2) : $\varphi_1(b_1, b_2)$ and $\varphi_2(b_1, b_2)$. For $b_1 = 0.434$ fixed, φ_1 equals 1.163 / 1.236 / 1.268 according as b_2 takes the values 0.70 / 0.858 / 0.95. The scatter that Fig. 8a showed about the fitted curve was not noise: it was the dependence on the second exponent. It is also made explicit that α_v is inferred from the short branch and adopted as common to both branches; imposing Eq. (27) on both branches with the same α_v would give $\tau_1 = \tau_2$, which is incompatible with $b_1 \neq b_2$.

Cross-check. Recomputed with the corrected Eq. (25) and Eqs. (27), (33) and (35), the factors reproduce the published ones for the 66 INAMHI stations with a mean difference of 0.02 % (max. 0.08 %) in φ_1 and 0.004 % (max. 0.03 %) in φ_2 ; the residual corresponds to the tolerance with which τ and α_v are solved by scalar iteration. These differences measure the algebraic implementation, not an error against observations. There is no fit, no coefficient of determination and no free parameter.

Transition interval. Within $[t_1, t_2]$ the calculation keeps $DT = DT_{\text{transition}}$ fixed, so that t / DT varies and Eq. (35) yields a t -dependent factor joining φ_1 and φ_2 continuously; at Izobamba, with $t_1 = 32.2$ min and $t_2 = 60.9$ min, the factor equals 1.236 up to 30 min, 1.139 / 1.071 / 1.036 at 40 / 50 / 60 min and 1.034 from t_2 onwards. The piecewise formulation, with the base curve used in each interval, is documented in the revised manuscript.

25. The referee writes that fitting $\varphi(b) = b^{-0.237}$ and reporting the quality of the fit is fitting a curve to an identity. He is right, and the appropriate correction is not to improve the fit but to withdraw it. In the revised manuscript Eq. (37) is replaced by the expressions of item 24, and Fig. 8a ceases to be a scatter of points with a statistic: it becomes a plot of the computed factors against the published ones by station, or of curves of φ_1 conditional on b_2 . Eq. (38) is updated accordingly. The application requires no new data: b_1 and b_2 appear in the official formulation of every station.

26. We qualify what is asserted, separating three things that the manuscript presents together.

The construction of ϕ is arithmetic. Given the official formulation of a station, anyone can reproduce and verify it. It requires no observational test because it is not an assertion about observations.

The ability of MIT-Q to reproduce IDF curves has been compared, not uniformly validated: calibration at Quito-Observatory and comparison at four additional stations against reference curves from independent studies (Figs. 5a–5e), with errors by station and duration already

reported in AC1 §3.1.3 and §3.1.4, and in AC3 §5.2.1 for the DAC-Aeropuerto case, which include large deviations at some long durations. The revised manuscript states which data entered the selection of parameters and which did not (C11). That comparison does not validate the magnitude of the IDFP curves.

What has no independent support is the assumption from which the magnitude comes: Eq. (33). It is a rescaling rule for the temporal parameter at equal t/DT , $\tau_1/\tau_2 = \alpha_{v1}/\alpha_{v2}$, motivated by an earlier concentration of the precipitation; its physical reading, that the depth of the long-duration regime could develop within the time of the short-duration regime, is approximate and not exact, because the normalisers of the truncated profile differ by the factor $[(1-e^{-\alpha_v})/(1-e^{-\alpha_p})]^2$, which over the 66 stations departs from unity by 0.11 % at the median and 0.76 % at the 90th percentile, reaching 2.4 % at the station with the smallest α_v . The departure decreases with α_v : below 0.01 % for $\alpha_v \geq 10$. The referee is right to note that a physical motivation fixes the sign of a correction and not its magnitude. The magnitude here comes from that assumption, which the manuscript neither declares as such nor tests. We declare it expressly in the revised version.

The IDFP curves are therefore presented as intensification scenarios conditional on the model and on the official parameterisation, and not as a validated correction of the INAMHI curves nor as a physical upper bound. The return period they carry is the one inherited from the base curve; its correspondence with the exceedance frequency of the actual precipitation at the storm centre has not been established. Their use in design requires independent observational testing and an uncertainty analysis, and the revised manuscript says so.

2.4.1.3 Committed changes to the revised manuscript

- C14. Correction of the typographical error in Eq. (25): denominator $1-e^{-\tau}$, with the derivation by logarithmic slope.
- C15. Replacement of Eq. (37) by the exact closed form of $\varphi_1(b_1, b_2)$ and $\varphi_2(b_1, b_2)$, stating that α_v is inferred from the short branch and adopted as common; updating of Eq. (38) and of Fig. 8a, which becomes a plot of computed against published values without a goodness-of-fit statistic; withdrawal of the description as a correlation in Section 3.1.
- C16. Piecewise formulation of φ : φ_1 before t_1 , $\varphi(t; DT_{transition})$ between t_1 and t_2 , φ_2 after t_2 , with the base curve used in each interval and verification of continuity.
- C17. Short supplement with the derivation, the procedure for solving τ and α_v , and the cross-check table for the 66 stations.
- C18. Express statement of Eq. (33) as an assumption of the model: rescaling rule for α at equal t/DT , with its physical reading as an approximation and its untested character.
- C19. Reformulation of the IDFP curves, in Section 3.1, the abstract and the conclusions, as intensification scenarios conditional on the model, without any recommendation for use in design and with a statement that their total uncertainty and their effective return period have not been established.

2.5 Conclusions

The referee poses an alternative: either the theoretical novelty is reformulated from the ground up and rigorously distinguished from the Gibbs and maximum-entropy framework, or the manuscript is reduced to presenting MIT-Q as an empirical stochastic model without claiming a principle of inference. We state precisely where the revision stands with respect to that alternative.

We do not claim a distinct principle of inference. The constancy of Maximum Certainty is the Gibbs identity (item 1); Knowledge is an assumption about the phenomenon with the parameter subject to calibration (item 12); the formulation over the cumulative is equivalent to the formulation over the density (item 8); the optimal ordering of the arrangements is a classical corollary (item 7) and its probability law is a sampling rule adopted as an assumption (item 10). With those concessions, none of which is expository, the manuscript withdraws the presentation of Maximum Certainty as a newly defined invariant and the claim to a distinct principle of inference (C8).

Nor do we reduce the manuscript to a model without a formulation. The variational formulation is retained as the specification of the storm model: it fixes the family of profiles from which the quadratic truncated exponential was chosen, empirically (items 17–20); it fixes the space of arrangements on which the ordering rule acts (items 9–11); and it organises the relation between the exponent b of the official curves and the storm parameters (item 24). To the referee's question as to which numerical result would change without it we answer that none would (item 21); its contribution is one of specification and organisation, and it is presented as such.

The contribution claimed by the revised manuscript is therefore the applied one: a stochastic storm generator with an explicit ordering rule, compared against reference IDF curves with errors declared by station and duration; an exact relation, with no fitted parameter, between the official exponents (b_1 , b_2) and the factor ϕ , which replaces an empirical fit and corrects a typographical error in the preprint (items 24–25); and the IDFP curves as intensification scenarios conditional on the model, with the assumption that generates them declared and their scope qualified (items 23 and 26).

The nineteen committed changes, C1 to C19, are set out at the close of each section together with their location in the manuscript. The calibration of MIT-Q and the comparisons of Figs. 5a to 5e are retained with the nomenclature of item 26.

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Sincerely,
Franklin Beltrán
Jhoan Beltrán