Dear Reviewer#1,

Thank you for your detailed comments about our manuscript. All your suggestions have been considered, and the following changes have been implemented to address the questions you raised in your review.

In the following point-by-point responses your comments are in normal font and our responses are in *italic*. The line and figure numbers refer to the revised version of the manuscript. In the marked revised manuscript the sections with changes are marked in blue.

Hoping that these improvements will fulfill your expectations,
Best regards,

Lionel Benoit, on behalf of the authors.

Major comments:

I/- There are other ways to introduce non-stationarity in a Gaussian process. It would be nice to have some brief insight into different ways to introduce non-stationarity and provide some argument why your choice of using this parametrised covariance better suits your target.

We agree that this information was missing in the initial manuscript. The following paragraph has been added (l 154-160) to give a brief summary of the different ways to introduce non-stationarity in the covariance function, and to better motivate our choice of parametrization:
"Different approaches have been proposed to obtain valid non-stationary covariance models, for instance the convolution of locally stationary covariance kernels (Paciorek and Schervish, 2006; Fouedjio et al., 2016), the space deformation of the modeling domain (Sampson and Guttorp, 1992; Fouedjio et al., 2015), and the use of a locally varying diffusion operator in stochastic partial differential equations (Fuglstad et al., 2015; Pereira et al., 2022). Here we follow the convolution approach because it provides a direct extension of the Matérn covariance function defined in equation 1 to the non-stationary case, and because the use of locally stationary covariance kernels simplifies the calibration of the model based on sparse rain gauge observations (cf. Sect. 3.3)."

II/- To make the paper easier to read, it would be better to introduce the data, the climatic regions and the rain clusters before describing the components of the models. Indeed, there are instances where rain climatology, climatic regions and clusters are mentioned before being introduced and understanding that there is one model per cluster/region earlier would be nice.

This is a good idea. A new section entitled "2. Example dataset: orographic precipitation on the Island of Hawai'i" has been added just after the introduction (l 75-100). This new section introduces the climate of Hawai'i and the climate divisions early in the paper. This section includes the material of the former section "3.1 Orographic precipitation on the Island of Hawai'i" as well as the new figure 2 (and associated description in the main text) which illustrates the rainfall climate of each climate division, with a focus on seasonality and inter-annual variability.

III/- What are the modelling differences between the model developed in this paper and the benchmark model? It can be presented as a table in the paper (Appendix).

This is a good idea. This comparison has been added in the new Appendix C: "Comparison between the BSNLG2022 benchmark model and the non-stationary trans-Gaussian model of this study" (l 620-623).

IV/- Before assessing the spatial pattern, is it possible to look at the seasonal and inter-annual variability for a sub-sample of stations (section 3.3)?

Yes, this information has been added in the new figure 2.b-c (l 97).

V/- Can we see some evaluation of correlation /auto-correlation for all pairs of stations in addition to the correlation spatial pattern, conditionally to one location given in Figure 4.

This is a good idea. Such plots have been added in Figure 5 – panels e-h (l 408).

VI/- It seems that there are non-negligible correlations (>0.5) in the observation between different climatic regions for the three examples given in Figure 4(c) between regions 12/11 (top), 9/11 (middle) and 8/12 (bottom). Limiting the covariance estimation to climatic regions seems to cause this. Why not estimate one non-stationary covariance function for the whole island (at least to compare)?

This is a good point. The following paragraph has been added to section 4.2 in order to better explain the imperfect modeling of some long-distance correlations, and suggest possible ways of improvement (l 391-400):
"This imperfect modeling of some long range correlations is likely due to the use of relatively large climate divisions when estimating the parameters of the covariance function at the local scale, which is necessary in the present setting to ensure that enough observations are available for a robust estimation of the covariance parameters (cf. Sect. 3.3). The interpolation of these parameters to build an island-scale non-stationary covariance function can misrepresent the actual values in areas where the covariance parameters vary strongly in space as well as at the edge of the climate divisions, leading to an imperfect modeling of the spatial dependencies in these areas. The spatial interpolation of the covariance parameters could be improved by extending the rain gauge network in poorly gauged locations, and in turn defining smaller climate divisions. The densification of the rain gauge network may also provide more detailed insights on the patterns of spatial dependencies, which could call for the use of a more refined covariance model accounting for instance for barrier effects (Bakka et al., 2019) that may be induced by the drying effect of the TWI in the center of the island."

VII/- I also suspect the results would be very similar with an anisotropic stationary covariance function estimated for each climatic region. I would like to see the result with the stationary version of your covariance.

We agree that within each climate division the results would be relatively similar with an anisotropic but stationary covariance function estimated for the climatic region at hand. However, the non-stationary covariance model leads to correlation patterns with more diverse isolines than the "simple ellipses" that would occur if anisotropic but locally stationary models were used.
In addition, the use of anisotropic stationary covariance functions estimated for each climatic region would hinder the simulation of rainfall fields covering the whole island, which is the main goal of the paper. The result would be spatially disconnected maps for each climate division, and not rainfall maps (or synthetic rainfall fields) covering the whole island. Since such discontinuities would create unrealistic rainfall maps we decided to not further explore this option.

VIII/- What is the purpose of performing conditional simulations? Are the unconditional simulations not enough? How would it be necessary for hydrological simulations?

The purpose of performing conditional simulation is the estimation of the uncertainty of spatial rainfall estimates derived from sparse rain gauge observations. The following sentence has been added to the introduction to better explain the interest of performing conditional simulations (l35-39):
"Trans-Gaussian models can be used to simulate rainfall in two main settings: unconditional simulations allow

for the generation of synthetic rainfall fields (Paschalis et al., 2013; Peleg et al., 2017; Vaittinada Ayar et al., 2020; Wilcox et al., 2021) while conditional simulations are used for mapping rainfall from sparse rain gauge observations (Creutin and Obled, 1982; Lanza, 2000; Kyriakidis et al., 2001) and for radar-rain gauge data fusion (Creutin et al., 1988; Foehn et al., 2018).”

Minor comments:

• In the introduction, it is not fully clear that the paper only tackles spatial non-stationarity; please clarify this.

The introduction has been largely re-written to better contextualize our work and be more specific on our goals. Regarding the fact that the paper only tackles spatial non-stationarity we added the following sentences:
(l 51-52) “In this context, the present study intends to develop a trans-Gaussian model for daily rainfall over complex topography, which requires to adapt the trans-Gaussian framework to the spatial variation of rainfall statistics caused by orographic effects”.
(l 58-59) “In this paper we aim to combine these two ways of modeling spatial non-stationarity, which leads to what we call a fully non-stationary trans-Gaussian model, i.e., a trans-Gaussian model with all parameters varying in space.”
In addition we added a sentence in the beginning of Section 3 when we introduce our model to remind the reader that we focus on the spatial modeling of rainfall (l 103-104): “As the focus of the study is on the spatial modeling of rainfall, we model the temporal variability separately (and beforehand) following a rain-typing approach (Ailliot et al., 2015; Blanchet et al., 2019; Vaittinada Ayar et al., 2020).”

• I do not see the contrast between paragraph lines 45 and 50. Indeed, the authors mention in lines 47-49: “non-stationary geostatistical models tend to have a large number of parameters [...] focus on one aspect of non-stationarity, the choice of which is driven by the problem at hand.”. How are you handling the high number of parameters in your “fully non-stationary trans-Gaussian geostatistical model?”

We agree that this part of the introduction was confusing, and it has been re-written. What we meant is that our fully non-stationary model has many parameters, which offers great flexibility in spatial rainfall modeling but requires a good quality and extensive dataset for calibration. We added the following paragraph in the introduction to make it clear (l 58-64):
“In this paper we aim to combine these two ways of modeling spatial non-stationarity, which leads to what we call a fully non-stationary trans-Gaussian model, i.e., a trans-Gaussian model with all parameters varying in space. This provides a very flexible spatial model for daily rainfall, in which the statistics characterizing (1) rainfall occurrence, (2) the marginal distribution of rainfall intensity and (3) the spatial dependencies within rainfall fields are location-dependent. The performance of the model is illustrated for the Island of Hawai‘i (State of Hawaii, USA) where orographic effects are very strong (Giambelluca et al., 2013), and for which an extensive and high-quality rain gauge dataset enables the calibration of a highly parametrized model.”

• line 145: spectral simulation? Is that what the authors do?

Both the Cholesky decomposition and the spectral simulation approaches have been tested and give similar results. The description of unconditional simulation has been re-written to avoid confusion (l 207-217).

• Can you give the total surface of Hawai‘i in section 3.1

The area of the Island of Hawai‘i is 10432 km^2. This information has be added in the new section 2 (l 75).

• Appendix A: Since α iteratively determined for each dry station, how big *Niter* needs to be?

Usually few thousand iterations are enough to ensure convergence. This information has been added in the *description of the inputs of the algorithm (l 564) (and similarly in Appendix B – l 593).*

• Appendix B: Can you precise how $Y_{f,sim}$ is obtained, in particular how is it initialized?

This information has been added in Appendix B (l 599-601), as well as in the new description of the Metropolis within Gibbs algorithm in the main text (l 264-270).

• In Figure 4: Are the simulations with the non-stationary model unconditional? Can you clarify this?

Yes, the simulations with the non-stationary model in figure 4 are unconditional simulations. This has been clarified in the caption of the figure (l 359).

• line 502 & 511: Appendix A (not 1).

Thank you for the comment. This mistake has been corrected.

Dear Reviewer#2,

Thank you for your detailed comments about our manuscript. All your suggestions have been considered, and the following changes have been implemented to address the questions you raised in your review.

In the following point-by-point responses your comments are in normal font and our responses are in *italic*. The line and figure numbers refer to the revised version of the manuscript. In the marked revised manuscript the sections with changes are marked in blue.

Hoping that these improvements will fulfill your expectations,
Best regards,

Lionel Benoit, on behalf of the authors.

Main comments :
The article is well written and very clear. The topic is clearly worth for HESS journal. However,

- Although the results are clearly convincing on the ability of the model to reproduce strong spatial variability, it's not completely clear to me what part of the model makes it suitable for modeling such orographic gradients : is it the use of a non stationary covariance function (however is it new in the context of SWG?) ? And/or the use of climate zones ?

The good performance of the model to reproduce the strong spatial variability of orographic rainfall comes from both the non-stationary transform function AND the non-stationary covariance function. This is the combination of these two components coupled with a robust strategy for model calibration that makes the novelty of the proposed approach. The following paragraph has been added to the introduction to better explain this point (l 51-61):
"In this context, the present study intends to develop a trans-Gaussian model for daily rainfall over complex topography, which requires to adapt the trans-Gaussian framework to the spatial variation of rainfall statistics caused by orographic effects. This implies making some parameters of the model location-dependent, a condition referred to as spatial non-stationarity (Sampson and Guttorp, 1992). Two approaches have been proposed so far to make trans-Gaussian rainfall models non-stationary: either the marginal distribution (i.e., the parameters modeling rainfall occurrence and intensity) is made non-stationary (Frazier et al., 2016; Benoit et al., 2021; Lucas et al., 2022), or the non-stationarity is modeled in the covariance function (i.e., the parameters modeling the spatial dependencies within rainfall fields) (Paciorek and Schervish, 2006; Fuglstad et al., 2015; Fouedjio et al., 2016). In this paper we aim to combine these two ways of modeling spatial non-stationarity, which leads to what we call a fully non-stationary trans-Gaussian model, i.e., a trans-Gaussian model with all parameters varying in space. This provides a very flexible spatial model for daily rainfall, in which the statistics characterizing (1) rainfall occurrence, (2) the marginal distribution of rainfall intensity and (3) the spatial dependencies within rainfall fields are location-dependent."

By the way, I think the climate zones should be introduced before, and their use should be motivated. Here they appear for the first time l. 100 and we don't really understand what they are, and why.

This is a good idea. A new section entitled "2. Example dataset: orographic precipitation on the Island of Hawai'i" has been added just after the introduction (l 75-100). This new section introduces the climate of Hawai'i and the climate divisions early in the paper. This section includes the material of the former section "3.1 Orographic precipitation on the Island of Hawai'i" as well as the new figure 2 (and associated description in the main text) which illustrates the rainfall climate of each climate division, with a focus on seasonality and *inter-annual variability. We also added the following paragraph to better explain what climate divisions are, and why they are useful for our study (l 90-96): "To account for the diversity of climate patterns in the State of Hawaii, Luo et al. (2024) proposed a partitioning of the archipelago into twelve climate divisions (Fig 2). Climate divisions constitute the basic geographic unit used by the National Oceanic and Atmospheric Administration's National Centers for Environmental Information (NCEI-NOAA) for climate analyses at the scale of the United States of America (Guttman and Quayle, 2005). The Hawai'i climate divisions have been defined based on a cluster analysis on monthly rainfall maps and local expert knowledge (Luo et al., 2024), and will be used in the present study to delineate six sub-domains of the island of Hawai'i within which the rainfall climatology is assumed to be relatively homogeneous (Fig 2.a)."*

- I got pretty much confused with the covariance function. Reading Sections 2.2 and 2.3, I understood that the covariance function was non stationary within the climate zone (given by eq 2), then its location-dependent parameters were interpolated in space to produce maps. But then it is written in the conclusion that the covariance function is first assumed to be stationary within the climate zones and then a non-stationary covariance function is subsequently obtained by convolution (??) of these stationary covariance. So I probably strongly misunderstood this part of modeling. Please consider clarifying.

We agree that the estimation of the parameters of the covariance function and the construction of a non-stationary covariance model were not clearly explained in the initial version of the manuscript. The following changes have been made to improve the description of these two aspects:
- An introductory sentence has been added to "Section 3.3 Model calibration" in order to clearly specify the two steps of model calibration (167-168): "Model calibration starts with the estimation of model parameters at the local scale assuming local stationarity, and continues with the interpolation of these parameters to create a non-stationary model."
- The description of the estimation of the covariance parameters has been re-written to better explain what we call "local stationarity" in this context (l 176-185): "The estimation of covariance parameters requires observations from several rain gauges simultaneously to capture spatial dependence, and therefore the basic unit used for the estimation of locally stationary parameters of the covariance function is the climate division. Climate divisions form sub-domains of the target area within which the climatology of rainfall is deemed relatively homogeneous (cf. Sect. 2), which allows us to make an assumption of local stationarity for the dependence structure. The size of the climate divisions used as sub-domains where local stationarity is postulated derives from the tradeoff between (i) the number of rain gauges within each division that should be maximized to improve the estimation of the covariance parameters and (ii) the distance between climate divisions that should be minimized to improve the performance of the interpolation used to build the non-stationary covariance model. After the delineation of climate divisions, the parameters of the covariance function CY of the latent field are estimated for each division separately using pairwise likelihood maximization, which has been proved to be an efficient and unbiased method for the estimation of covariance parameters (Bevilacqua et al., 2012)".
- The conclusion has been revised to avoid confusion (l 529-532): "Similarly, the covariance function of the latent field is first assumed to be stationary within relatively small climate divisions during the estimation of the covariance parameters, then the covariance parameters are assigned to the barycenter of each climate division, and finally these parameters are interpolated through space and incorporated in a model of non-stationary covariance."

- I found Section 2.6 difficult to understand because all the steps of the procedure are actually described later in Section 3.

We agree that the description of the steps of the procedure was not relevant in the former Section 2.6. It has been simplified and moved to the beginning of Section 4 (l 295-298).

More importantly, I haven't understood what monthly total you use to condition at the virtual stations since there is no data there ? Is is with HCDP ? In which case, I find the comparison in Fig 6 unfair because of course we expect at the end to retrieve the right monthly totals since we used them to condition.

The monthly total used to condition at the virtual stations indeed derives from the HCDP dataset. However we don't think that Figure 7 (former Fig. 6) is unfair since this figure is not made to compare the conditioned monthly total with the HCDP, but rather to (1) show the need for conditioning to monthly totals (i.e., show that the rain-gauge-only monthly total has significant differences with HCDP) and (2) show that the conditioning algorithm based on Gibbs within Metropolis is effective. We reformulated the description of figure 7 to avoid misunderstanding, and the improved description now reads as (l 504-506): "Results in Figure 7 and in supplementary material show that conditioning daily rainfall maps to monthly totals enables to transpose the features of the reference HCDP maps into the daily rainfall maps derived from rain gauge observations, and in their monthly sum displayed in figure 7-bottom row."

- The temporal dependence seems not to be modeled at all. Are daily rainfall fields in this region almost independent ? Please justify this assumption.

This is true that this paper focuses on the spatial modeling of rainfall, and that only the spatial dependencies are explicitly modeled. However, the temporal variability of rainfall is not overlooked since it is accounted for by the rain types. Daily rainfall fields are therefore not fully independent, but are independent conditionally to the rain types, which explains why a purely spatial model (i.e., without temporal dependence) is calibrated for each rain type.
The beginning of Section 3 has been re-written to make it clear, and justify why this is a reasonable assumption for our case study (l 102-109): "As the focus of the study is on the spatial modeling of rainfall, we model the temporal variability separately (and beforehand) following a rain-typing approach (Ailliot et al., 2015; Blanchet et al., 2019; Vaittinada Ayar et al., 2020). We therefore assume that days can preliminarily be pooled into Nclust clusters referred to hereafter as rain types, within which rainfall fields are statistically similar to each other. All the information about the temporal variability of rainfall is therefore encoded into the sequence of rain types, which has been shown to be a reasonable assumption for daily rainfall in tropical islands, and in particular in Hawai'i (Benoit et al., 2022). As a result, daily rainfall fields are assumed independent to each other conditionally to rain types."

Minor comments :
- eq 2 : can the parameters (gamma, nu, rho) take any value ?

No, there are some constraints on these parameters. This is now clearly stated l 140-147.

- eq 4 : shouldn't the parameters be \nu_i, \nu_j, \gamma_{1,i}, \gamma_{2,i}, \gamma_{1,i}, \gamma_{2,i}, \rho_{2,i}, \rho_{2,j} as in eq. 1 ? I guess your notations are that \nu=(\nu_i,\nu_j) but that's not clear.

No, for model calibration the parameters of the model are first estimated assuming local stationarity. This is now mentioned l 174-179. We think that this misunderstanding can also arise from the fact that in the initial manuscript, the anisotropy was introduced at the same moment as the non-stationarity, which was misleading. We therefore decided to move the introduction of anisotropy to Eq 1 in order to focus Eq 2 on the description of *non-stationarity only. We hope that this new formulation of the model improves the understanding of the different steps of our modeling framework.*

- in the interpolation process, how do you constrain the positive parameters (e.g. k, theta) to be >0 ?

The parameters are constrained during the estimation step, but not during the interpolation step. In practice, in our case study we did not observe instances of the interpolation process leading to negative parameters.

- Section 2.6 : In step 4, «assess the uncertainty of rainfall interpolation (described in Section 3.4) » : I don't see any uncertainty in Section 3.4.

This sentence has been re-formulated to avoid misunderstanding (l 297): "Condition the synthetic rainfall fields to rain gauge observations in order to generate daily rainfall maps."

- In step 5 « Identify areas where rainfall maps are highly uncertain» : actually in Section 3.5, the virtual stations cover all the island, not only the most uncertain parts.

This is correct, this sentence has been reformulated to avoid misunderstanding (l 298): "Condition daily rainfall maps to monthly totals to improve daily rainfall estimation in poorly gauged areas."

- Fig 2 : the number on the color scales are to small

We agree. These numbers have been enlarged (Fig 3, l 326).

- l 265 : please specify what are the main differences with the benchmark BSNLG2022 ? Is it only the covariance non stationarity ?

The main differences with the benchmark BSNLG2022 are now introduced l 341, and detailed in the new Appendix C: "BSNLG2022 is a multi-site stochastic rainfall generator that has been designed specifically for tropical islands with complex topography. It draws on rain types to model the temporal evolution of rainfall statistics and on a combination of empirical copulas and Gamma distribution to model the multi-site distribution of rainfall conditional to rain types (see Appendix C for a comparison between BSNLG2022 and the trans-Gaussian model of this study)".

- Section 3.4 : I guess that BSNLG2022 could not be used as benchmark because it's multisite and not spatial ?

Yes this is correct. This is now clearly stated l 451-452: "CAI is used in place of BSNLG2022 to benchmark rainfall mapping because the latter model only allows for multi-site modeling and therefore lacks the interpolation skills necessary for rainfall mapping."

- Fig 5 : it's confusing that the same color scale is used for positive and negative criteria (MB ; best color=green) and positive criteria (MAE, best color=blue).

This is a good point. The color scale of the mean bias has been changed to avoid this problem (Fig. 6, l 487).

- Fig. 6 : please consider adding the raingauge locations in the middle row

We agree and we added the rain gauge locations in the middle row (Fig. 7, l 502).

Dear Reviewer#3,

Thank you for your detailed comments about our manuscript. All your suggestions have been considered, and the following changes have been implemented to address the questions you raised in your review.

In the following point-by-point responses your comments are in normal font and our responses are in *italic*. The line and figure numbers refer to the revised version of the manuscript. In the marked revised manuscript the sections with changes are marked in blue.

Hoping that these improvements will fulfill your expectations,
Best regards,

Lionel Benoit, on behalf of the authors.

Overall assessment

The abstract could be more concise.

The abstract has been revised and made more concise. It now reads as (l 1-9): "In mountainous regions orographic effects create strong horizontal gradients of various rainfall statistics such as the frequency of occurrence, the distribution of intensity and the structure of spatial correlation. However, most statistical models of daily rainfall assume spatial stationarity (i.e., the spatial homogeneity of rainfall statistics) and are therefore not well suited for studying the highly non-homogeneous characteristics of orographic rainfall. To overcome this limitation, we design a non-stationary trans-Gaussian geostatistical model for the analysis of daily rainfall fields over complex topography. This framework infers rainfall statistics from sparse rain gauge observations, simulates realistic rainfall fields after calibration and stochastically interpolates rain gauge observations to create rainfall maps. The performance of the model is assessed with data from the Island of Hawai'i where extreme spatial gradients in rainfall are observed. Results demonstrate that the non-stationary trans-Gaussian model can skillfully reproduce orographic rainfall statistics as well as their variations in space."

The literature review on rainfall models appears overly centered on the authors' own prior work; the opening could be broadened to include a wider range of related approaches.

The literature review in the introduction has been broaden to include a wider range of approaches allowing for stochastic rainfall modeling (l 20-30): "A large variety of stochastic models have been applied to rainfall modeling, the choice of which depends on the features of rainfall to reproduce in priority. For instance, point process models have been used to simulate the occurrence of the main meteorological processes responsible for rainfall generation (e.g., rain cells, rain bands, storms) (Onof et al., 2000; Cowpertwait, 2010), models combining Markov chains and parametric distributions of rain intensity (Richardson, 1981; Ailliot et al., 2009) as well as resampling approaches (Rajagopalan and Lall, 1999; Oriani et al., 2014) have been applied to time-series simulation, generalized linear models have been used to capture the relationships between atmospheric conditions and rainfall (Chandler and Wheater, 2002; Chandler, 2020), fractal approaches have shown good performance in simulating rainfall scaling properties (Molnar and Burlando, 2005; Ramanathan et al., 2022; Maloku et al., 2025), and generative AI recently demonstrated promising skills at downscaling and forecasting complex rainfall features (Leinonen et al., 2020; Ravuri et al., 2021; Harris et al., 2022; Srivastava et al., 2024)."

It is not yet clear, from the abstract and introduction, what specific objectives the generator targets and which statistics are used to evaluate performance.

In the revised introduction we now explicitly state:
- What specific objectives the proposed stochastic rainfall model targets (l 64-67): "The model is designed and tested with two specific applications in mind: the interpolation of rain gauge data to generate daily rainfall maps for the Hawai'i Climate Data Portal (HCDP) (Longman et al., 2024), and the stochastic generation of spatially explicit rainfall fields to foster hydrological modeling in Hawai'i (Huang et al., 2021; Strauch et al., 2024)"
- What features/statistics of rainfall the model aims to reproduce (l 60-61): "This provides a very flexible spatial model for daily rainfall, in which the statistics characterizing (1) rainfall occurrence, (2) the marginal distribution of rainfall intensity and (3) the spatial dependencies within rainfall fields are location-dependent."

Major comments
1. Novelty and positioning
- What is the genuinely new feature developed here? Gaussian models (including some forms of nonstationarity) have been studied for some time (e.g., Alliot 2009). The comparison to prior literature should be expanded to better highlight the novelty.
- l50: "fully non-stationary" — in what sense? Space only, or also time?

The main novelty of this paper is to develop a trans-Gaussian model that is fully non-stationary, in the sense that both the transform function AND the covariance function are non-stationary in space.
The introduction has been largely rewritten to better highlight the novelty of the paper, and in particular we added the following paragraph (l 51-67): "In this context, the present study intends to develop a trans-Gaussian model for daily rainfall over complex topography, which requires to adapt the trans-Gaussian framework to the spatial variation of rainfall statistics caused by orographic effects. This implies making some parameters of the model location-dependent, a condition referred to as spatial non-stationarity (Sampson and Guttorp, 1992). Two approaches have been proposed so far to make trans-Gaussian rainfall models non-stationary: either the marginal distribution (i.e., the parameters modeling rainfall occurrence and intensity) is made non-stationary (Frazier et al., 2016; Benoit et al., 2021; Lucas et al., 2022), or the non-stationarity is modeled in the covariance function (i.e., the parameters modeling the spatial dependencies within rainfall fields) (Paciorek and Schervish, 2006; Fuglstad et al., 2015; Fouedjio et al., 2016). In this paper we aim to combine these two ways of modeling spatial non-stationarity, which leads to what we call a fully non-stationary trans-Gaussian model, i.e., a trans-Gaussian model with all parameters varying in space. This provides a very flexible spatial model for daily rainfall, in which the statistics characterizing (1) rainfall occurrence, (2) the marginal distribution of rainfall intensity and (3) the spatial dependencies within rainfall fields are location-dependent. The performance of the model is illustrated for the Island of Hawai'i (State of Hawaii, USA) where orographic effects are very strong (Giambelluca et al., 2013), and for which an extensive and high-quality rain gauge dataset enables the calibration of a highly parametrized model. The model is designed and tested with two specific applications in mind: the interpolation of rain gauge data to generate daily rainfall maps for the Hawai'i Climate Data Portal (HCDP) (Longman et al., 2024), and the stochastic generation of spatially explicit rainfall fields to foster hydrological modeling in Hawai'i (Huang et al., 2021; Strauch et al., 2024)."

2. Modeling assumptions (end of p. 3)
- The choices of Gamma and Matérn are not justified. Why Gamma rather than, say, double exponential or heavier-tailed alternatives? Why Matérn rather than exponential, Gneiting Matérn, or other forms? Please discuss strengths/weaknesses of these choices.

We agree that this justification was missing. The choices of the Gamma and Matérn are now justified l 127-132: "We assume a Gamma distribution for single-site rain intensity (Wilks and Wilby, 1999; Berrocal et al., 2008) *because this distribution has easy-to-interpolate parameters (i.e., with high auto-correlation and low cross-correlation) and because when combined with rain types the Gamma distribution has been shown to accurately model daily rainfall intensity (Blanchet et al., 2019). In addition we assume a Matérn covariance function for the latent Gaussian random field because this model of covariance is very flexible (Porcu et al., 2024) and has shown good skills in modeling the spatial dependencies of most atmospheric variables, including rainfall, at daily resolution (Obakrim et al., 2025).”*

3. Definition and use of “trans-Gaussian” (l91)
- The term sounds new here, but the description resembles what several cited works already do (sometimes under labels like “censored Gaussian”). Clarify what is new in your definition/usage.

The introduction has been re-written to better introduce trans-Gaussian models (l 30-42): “When the focus is on spatial patterns and spatial dependencies, models based on Gaussian random fields (GRFs) – also known as geostatistical models - are often chosen because they enable modeling rainfall at any point of the spatial domain while accounting for spatial dependencies (Chilès and Delfiner, 2012). However, the distribution of rainfall intensity is rarely Gaussian at daily to sub-daily resolution, and a parametric transform function is often combined with a GRF to model rainfall at these time scales resulting in so-called trans-Gaussian (or meta-Gaussian) approaches (Leblois and Creutin, 2013; Benoit et al., 2018; Papalexiou and Serinaldi, 2020). Trans-Gaussian models can be used to simulate rainfall in two main settings: unconditional simulations allow for the generation of synthetic rainfall fields (Paschalis et al., 2013; Peleg et al., 2017; Vaittinada Ayar et al., 2020; Wilcox et al., 2021) while conditional simulations are used for mapping rainfall from sparse rain gauge observations (Creutin and Obled, 1982; Lanza, 2000; Kyriakidis et al., 2001) and for radar-rain gauge data fusion (Creutin et al., 1988; Foehn et al., 2018). In both cases the resulting rainfall datasets can be used to assess input errors in distributed hydrological models (Vischel et al., 2009; Renard et al., 2011) or to conduct sensitivity analysis of spatially explicit hydro-meteorological modeling chains (Moraga et al., 2022; Liu et al., 2024).”

- The function Ψ is introduced in Section 2.1 but does not reappear until the start of Section 2.2. Consider re-using or referencing it explicitly at the end of Section 2.1 for continuity.

This is a good idea, we referenced it explicitly in equation 1 (l 135).

- l93: Define the separation vector and the norm used. If altitude/mountain effects are important, indicate where they enter the model.

The separation vector is now defined (l 141) and we removed the reference to the norm because anisotropy is now introduced directly in equation 1.

- As I understand it, there are two kinds of non-stationarity: (i) parameter variation by location s and (ii) possible non-stationarity in the covariance. Should the covariance be non-isotropic?

Yes, this understanding of the two sources of non-stationarity is correct. And the covariance is already anisotropic in the current model (and the anisotropy is also non-stationary).
We reformulated equations 1 and 2 to avoid confusion, and make it clear that the final model is both non-stationary and anisotropic.

- Eq. (2) lacks sufficient explanation; please expand.

We reformulated equations 1 and 2 and added explanations of these two equations in the revised manuscript (l 140-147, and l 152-162).

- Eq. (3): I am not sure N_s was defined. What optimizer is used for l in Eq. (3)?

N_s has been changed to N_obs to avoid confusion and is now defined l 173.
Regarding the optimizer we used the default algorithm of the fmincon function in Matlab, which correspond to the interior-point method. This information has been added in the revised manuscript (l 195-196): "The log-likelihoods of the marginal distribution and covariance function are maximized sequentially using the fmincon function in Matlab (using the default interior point algorithm (Byrd et al., 1999))."

- Estimation strategy: Is there a risk in estimating the Gamma parameters first and then C? Should these be estimated jointly? Why use pairwise likelihood, and what guarantees does it provide in this context?

We added the following sentence and references to justify our choice (l 196-198): "This sequential estimation scheme has shown good performance in estimating the parameters of trans-Gaussian models of rainfall (Leblois and Creutin, 2013; Caseri et al., 2016; Papalexiou and Serinaldi, 2020)."

4. Structure of presentation (l135 and Section 2.4–2.6)
- This is the second mention of "climate zones," but they have not been presented. Without this context, it is difficult to follow. Either present the model fully and then describe the application (e.g., fitting within each zone), or introduce the application earlier so the assumptions are easier to track.

This is a good idea. A new section entitled "2. Example dataset: orographic precipitation on the Island of Hawai'i" has been added just after the introduction (l 75-100). This new section introduces the climate of Hawai'i and the climate divisions early in the paper. This section includes the material of the former section "3.1 Orographic precipitation on the Island of Hawai'i" as well as the new figure 2 (and associated description in the main text) which illustrates the rainfall climate of each climate division, with a focus on seasonality and inter-annual variability.

- The paragraph before Section 2.4 lacks mathematical detail, which makes it harder to follow; the last sentence is particularly unclear.

This paragraph has been re-written to add details and improve readability (l 208-222).

- Section 2.6 mentions MCMC, but its role is not clearly explained. A brief conceptual explanation would help (and the Appendix would benefit from a few guiding sentences). Clarify the list of steps as well.

This is a good idea. We added a brief conceptual explanation of the MCMC algoritm and its main steps l 259-285. We also added some guiding sentences in Appendix B (l 585-620).

5. Evaluation metrics and visualization

- In Section 3.3, Comparing to another model is a good idea, but including a comparison with a model not authored by the same team would strengthen the case. Also, briefly recap the key differences that make BSNLG2022 distinct.

The differences between the two models are now presented in Appendix C (l 620) and the main features of BSNLG2022 are introduced in the main text l 341-344.

- Figure 3 is promising; the Wasserstein distance is an interesting choice, has it been used elsewhere in the literature of weather generators?

We are not aware of the use of the Wasserstein distance in the literature of weather generators strictly speaking but it has been proposed to apply it to the comparison of climate models (Vissio et al., 2020), which motivated our choice for this metric. We added this reference l 337.

- I would also like to see an observed vs. simulated scatter plot, or an overall RMSE for the chosen statistics. Visual comparison across multiple maps is difficult and the lack of quantitative comparison is a limitation.

This is a good idea. These scatter-plots and RMSE have been added in Fig. 4 (l 359).

Minor comments
- Clarify the meaning of “local scale” vs. “rain gauge” (l114 and l132).

The meaning of “local scale” is now clearly defined l 169-170 “The basic unit for the estimation of locally stationary parameters of the transform function is the rain gauge” and l 174-179: “The estimation of covariance parameters requires observations from several rain gauges simultaneously to capture spatial dependence, and therefore the basic unit used for the estimation of locally stationary parameters of the covariance function is the climate division. Climate divisions form sub-domains of the target area within which the climatology of rainfall is deemed relatively homogeneous (cf. Sect. 2), which allows us to make an assumption of local stationarity for the dependence structure.”

- l146: “more scalable” — please quantify (e.g., $\mathcal{O}(N)$ vs. $\mathcal{O}(N^2)$).

“More scalable” is now quantified l 217-218: “[…] for instance based on a spectral approach that scales linearly with the number of simulation points (Emery and Arroyo, 2018; Allard et al., 2025).”