Response to the comments of Reviewer#1

Reviewer comment (RC 1.1): Some of the ideas in the paper are potentially interesting. However, the existing literature on weather generators (particularly for daily precipitation) is vast: to justify yet another new approach therefore, it is necessary to demonstrate that it improves on existing methods in some way (it could be the performance of the method, its ease of implementation, its range of applicability, its computational feasibility etc.). The authors do not provide any such demonstration, perhaps because they don't seem aware of the state of the art in the area.

Authors' response (AR 1.1): In our opinion there are two main novelties in our study, which we briefly summarize hereafter and will discuss in detail throughout the present response to Reviewer#1 comments:

- Our model has been designed to account explicitly for rainfall patterns, i.e., not only inter-gauges correlation, as is often the case in multi-site stochastic weather generators (SWGs) (e.g., *[Mehrotra and Sharma, 2007]* [*Bennett et al.*, 2018]), but also rain location within a given target area (here a tropical island). In the present context, this allows us to infer which part of the island receives the most rainfall for a given time of the year, which is a very important feature of the climate of interest. It is indeed important to notice here that in tropical islands, rain location changes drastically with the course of the year (see Fig. 3a and Fig. SM2.1 for examples of spatial rainfall patterns in Hawai'i and French Polynesia respectively, and Fig. 4 and SM2.4 to see how these patterns influence site-specific rain seasonality).
- Our model accounts for the rain climatology of tropical islands. As stated in our introduction, we believe that stochastic rainfall generators (SRGs) should be tailored to accommodate the climate at hand, which is an avenue followed by several other researchers in the past (e.g., [*Peleg and Morin*, 2014] [*Wilcox et al.*, 2021]) and has led to valuable developments. We agree with Reviewer#1 that generic methodological developments are the foundation of SRGs, but we would add that the adaptation of pre-existing modelling blocks to different climates is a necessary extension of this theoretical effort. The present study follows this perspective, and tries to take advantage of the combination of the (pre-existing) concepts of rain typing (e.g., [*Oriani et al.*, 2017] [*Benoit et al.*, 2018a]), transformed Gaussian random fields (e.g., [*Baxevani and Lennartsson*, 2015] [*Allard and Bourotte*, 2015]) and analogs resampling (e.g., [*Gangopadhyay and Clark*, 2005] [*Yiou*, 2014]) to design a SRG adapted to the specific case of tropical islands. To the best of our knowledge, this topic has not yet been adequately addressed.

To emphasize this second point (the development of a new model dedicated to tropical islands), which was the motivation for our study, we decided to focus our introduction on the adaptation of SRGs to the climate of the area of interest. Unfortunately, this may have given the false impression that we neglected the many alternative modelling choices existing in the literature (e.g., Hidden Markov Models, Generalized Linear Models, and object based models). This false impression may have been reinforced by our initial choice to focus our discussion on the justification of our modelling choices instead of discussing how they fit within the broad literature of SWGs. We see now that it is not the best choice, and will improve our paper in this respect by substantially expanding the discussion section to include the points from this response.

RC 1.2: Literature that seems particularly relevant includes that on Hidden Markov Models (e.g. Hughes et al. 1999; Ailliot et al. 2009): this uses the same basic idea of classifying each day on the basis of the joint spatial distribution of precipitation, but does so in what seems to be a more principled way than the present paper.

AR 1.2: Hidden Markov Models are indeed broadly used in the SWGs literature, and we agree that they rely on the same idea as our method of defining rain types, namely the classification of each day based on rainfall (or more generally weather) statistics. The only difference we see between HMM and our method (where rain types transition according to a non-homogeneous Markov Chain) is that in one case the states of the Markov Chain are hidden (HMM), and in the other case they are explicit (the present approach). This second option is not new in the field of SWGs (see [*Ailliot et al.*, 2015] for a review of SWGs based on weather types, discussing both hidden and directly observed types), and has been shown to be valuable in several past studies (e.g., [*Bárdossy and Plate*, 1991] [*Bárdossy and Plate*, 1992] [*Wilby*, 1994] [*Vrac et al.*, 2007a], to cite but a few). We chose the explicit approach over the HMM in the present study because several steps of our modelling chain rely on non-parametric methods, which prevents the formulation of the likelihood of the full model and in turn the fit of an HMM.

RC 1.3: Moreover, approaches using generalised linear models with topographical indices as covariates (e.g. Ambrosino et al. 2014; Chandler 2020) address the issue of topographical variability directly: it's not obvious to me that such approaches would fail in a tropical island setting.

AR 1.3: We agree that Generalized Linear Models (GLMs) can address the issue of topographical variability in an elegant way in case of moderate and relatively steady orographic effects, but we see several obstacles to the application of GLMs in the present case where orographic rain enhancement is very strong and variable in space and time.

First, rain accumulation in tropical islands is not a simple function of altitude, but is strongly modulated by the exposure of slopes to prevailing winds. This aspect, common to temperate and tropical mountains, can be accounted for within the GLM framework, but requires the inclusion of information about slope orientation at a scale that properly represents rain-topography interactions (e.g., [*Chandler*, 2020]), which is not always easy to define in case of complex topography (e.g., [*Daly et al.*, 2017] [*Foresti and Pozdnoukhov*, 2012]) as encountered in tropical islands. This situation is further complicated by the possibility of successive mountain chains along the track of the moisture flux, with the consequence that the second chain is drier than the first one. This phenomenon can be seen on Oʻahu, where the summit of the Waianae range (Mt Kaʻala, 1227m, 2015 mm/year) is significantly drier than the summit of the Koʻolau range (Puʻu Kōnāhuanui, 960m, around 3000 mm/year). A possible predictor for this can be derived from the integral of positive height difference along the moisture flow, but since this flow is not constant through time it would involve complex pre-calculations. In addition, many local effects add to this already complex situation, in particular the presence of rain enhancement overshoot in many valleys (rain maximum is slightly leeward from the main crest, see e.g., [*Giambelluca et al.*, 2013] [*Benoit et al.*, 2021]), and the probable influence of the angle between the moisture flux and the mountain ridge, which makes the Northern Koʻolau (max annual rain >6000mm/year) significantly wetter than the Southern section of the range (max annual rain ~4000mm/year). These local effects are still only partially understood, which makes the selection of predictors and the setting of GLMs particularly difficult in such areas. Finally, the above features are drastically modulated by atmospheric conditions, which would require the inclusion of atmospheric covariates into the GLM, and most importantly the proper parametrization of their interaction with orographic effects. This has been shown to be possible in simple settings (as is the case for the

references proposed by Reviewer#1: [*Ambrosino et al.*, 2014] [*Chandler*, 2020]), but we are not aware of the application of such a framework in areas with complex patterns of orographic rain enhancement such as the ones described above, where a parsimonious parametrization of GLMs seems very challenging to obtain.
All in all, using GLMs in the context of tropical islands may be possible, but would certainly require a careful selection of atmospheric and topographic predictors, as well as the parametrization of their interactions and links with site-specific rainfall. This seemed like a very cumbersome process to us (partly because none of us is an expert in the field of GLMs), and as an alternative we chose a resampling approach focused on island scale rainfall patterns. Our approach has the advantage of directly and explicitly modelling: (1) rain location within the island, (2) island-scale rainfall pattern, and (3) the link between (1) and (2) and regional atmospheric conditions. This choice has been guided by a preliminary analysis of both island-scale and site-specific rain statistics, which has revealed the complex patterns of orographic rain enhancement we detailed above.

RC 1.4: I am also surprised that the paper doesn't cite Maraun et al. (2010) which has become almost the canonical reference for anyone working in this area.

AR 1.4: Maraun et al. (2010) is indeed a canonical reference in the field of climate projections downscaling, with a section dedicated to stochastic weather generators in this specific context. However, precipitation downscaling under climate change is only one possible application of our model, which can also be used for instance to study rainfall variability or to carry out impact studies under current climate. That is why we initially chose references more directly linked to stochastic weather generation [*Richardson*, 1981] [*Wilks and Wilby*, 1999] [*Ailliot et al.*, 2015], which we found more topical. But we agree that precipitation downscaling is a major application of our model, and we will add the proposed reference in the revised manuscript.

RC 1.5: In view of the concerns above, as well as some technical issues (see detailed comments below), I don't think the paper merits publication in its current form. To make the case, the authors need to demonstrate that their approach improves on existing methods in some way as described above. Ideally, this would be done by carrying out an informed and fair comparison with a leading alternative method: if this isn't possible then the authors should explain why, and should offer some informed discussion of how their approach might reasonably be expected to compare.

AR 1.5: As mentioned above (AR 1.1 to AR 1.3), we will address this comment by discussing in detail our modelling choices and by incorporating the related clarifications in the discussion section. This will allow for a better contextualization of our model within the broad literature of SWGs / SRGs.

RC 1.6: Line 31: this dismissal of "detailed physical modelling of rain generation processes" seems quite one-sided and poorly informed. It is true that stochastic models are computationally faster than physical ones, but physical models do have their own advantages which are not acknowledged here.

AR 1.6: We agree that our introduction of physical models was simplistic, which can be misleading. We will expand this description, and better acknowledge the pros and cons of both physical and stochastic models. This will include (but not be restricted to) the discussion of the reference [*Maraun et al.*, 2010] proposed by Reviewer#1.

RC 1.7: Line 79: what proportion of the data have been "gap-filled"? How is the gap-filling distributed across time and between stations? How was the filling done?

AR 1.7: Only stations with less than 5% gaps in raw data have been kept for processing. Gaps are usually spread across the whole duration of the dataset. The gap-filling has been performed using the normal ratio method [*Paulhus and Kohler*, 1952] for the Oʻahu dataset (in the main text), and using the vector sampling approach [*Oriani et al.*, 2020] for the Tahiti dataset (in supplementary materials).

RC 1.8: I will add that gap-filling in highly variable situations is, in my view, difficult and potentially dangerous because it will tend to underestimate variability. In my view therefore, for a stochastic rainfall model to be suitable for widespread use, it must be capable of handling incomplete datasets.

AR 1.8: We agree that incomplete datasets must be easily handled, that is why we selected (and implemented in our software) a pattern-preserving gap-filling method [*Oriani et al.*, 2020] for cases where no case-specific gap-filling has been performed (e.g., the Tahiti dataset, see supplementary material 2). This method has the advantage of being compatible with our modelling framework focusing on rainfall patterns. When a case-specific gap-filling has already been applied and validated (as is the case for the Oʻahu dataset [*Longman et al.*, 2018]), we chose to base our study on gap-filled datasets.

RC 1.9: (in particular, what would you do if you needed to generate precipitation at a site for which you have no data? This requirement is common in many realistic applications). Lines 216-217 suggest that the proposed methodology cannot handle incomplete datasets: this is a serious limitation that needs to be acknowledged clearly and openly.

AR 1.9: As suggested in lines 216-217 and with the terminology "site-specific" used throughout the paper, our model is indeed designed to simulate rainfall only at sites where data are available. It therefore belongs to the family of multi-sites stochastic rainfall generators (e.g., [*Breinl et al.*, 2017]) rather than the one of rain field generators (e.g., [*Paschalis et al.*, 2014]). In case precipitation needs to be generated at an ungauged location (i.e., where no data is available), we would first perform multi-site rainfall generation using the present model, and in a second step perform stochastic interpolation of these simulated values using a model that withstand non-stationarities in both space and time (e.g., [*Benoit et al.*, 2018a] [*Benoit et al.*, 2021]). However, the way uncertainties would propagate along the simulation chain remains to be investigated, and we therefore prefer to keep this interpolation step out of the scope of the present study. But Reviewer#1 is right to mention that it is a limitation of our model, and we will acknowledge it explicitly in the revised manuscript, in particular when summarizing the limitations of the model in the conclusion (lines 423-426 of the current numbering). The possible coupling with stochastic interpolation will be mentioned as well.

RC 1.10: Lines 101-105: although the spatial variation in precipitation statistics initially seems dramatic here, a more considered inspection reveals that the statistics are more or less constant along NW-SE transects and that the predominant variation is basically along the axis of the trade winds. If one were to scale the seasonal cycles at the specimen locations to a common scale (e.g. proportion of annual rainfall falling in each month), I doubt whether they would be dramatically different. I don't see *anything here that really challenges state-of-the-art multisite weather generators, therefore – and hence nothing that really necessitates the development of a new modelling framework.*

AR 1.10: We disagree with this statement. Although an inspection of rain statistics limited to the annual scale (i.e., based on Fig. 1a central panel only) can indeed give the impression that the predominant variation in precipitation is fully driven by trade winds, we would like to emphasize that a more careful inspection of rainfall features at the monthly to daily scale leads to very different results (see e.g., Fig. 3a and Fig. 4b). This is the motivation for section 3.1, which aims to show that spatial rainfall patterns drastically change with seasons and atmospheric conditions (this is even more obvious in supplementary material 2.2 and Fig. SM2.1, which focus on the more seasonal climate of Tahiti).
If one looks at the seasonal cycle at specimen locations as proposed by reviewer#1 (and illustrated in Fig 4b and Fig SM2.3), one can observe that seasonality actually differs between locations. For instance, in Ko'olau mountains, one can observe a weak seasonality with three distinct local maxima in April, November and July (this last one occurring during the dry season at the island scale). In contrast, at leeward stations, one can observe a single and marked rain maximum during the NDJFM (i.e., wet season). This difference of seasonality at specimen locations is well captured by our model (Fig 4b), and this good performance can be attributed to the careful modeling of the links existing between spatial rainfall patterns and atmospheric conditions in our model.

RC 1.11: Line 133: what's the justification for the formulation in equation (1)? It seems a bit ad hoc. Also, as defined, Z_i isn't latent because it's directly connected to observable quantities.

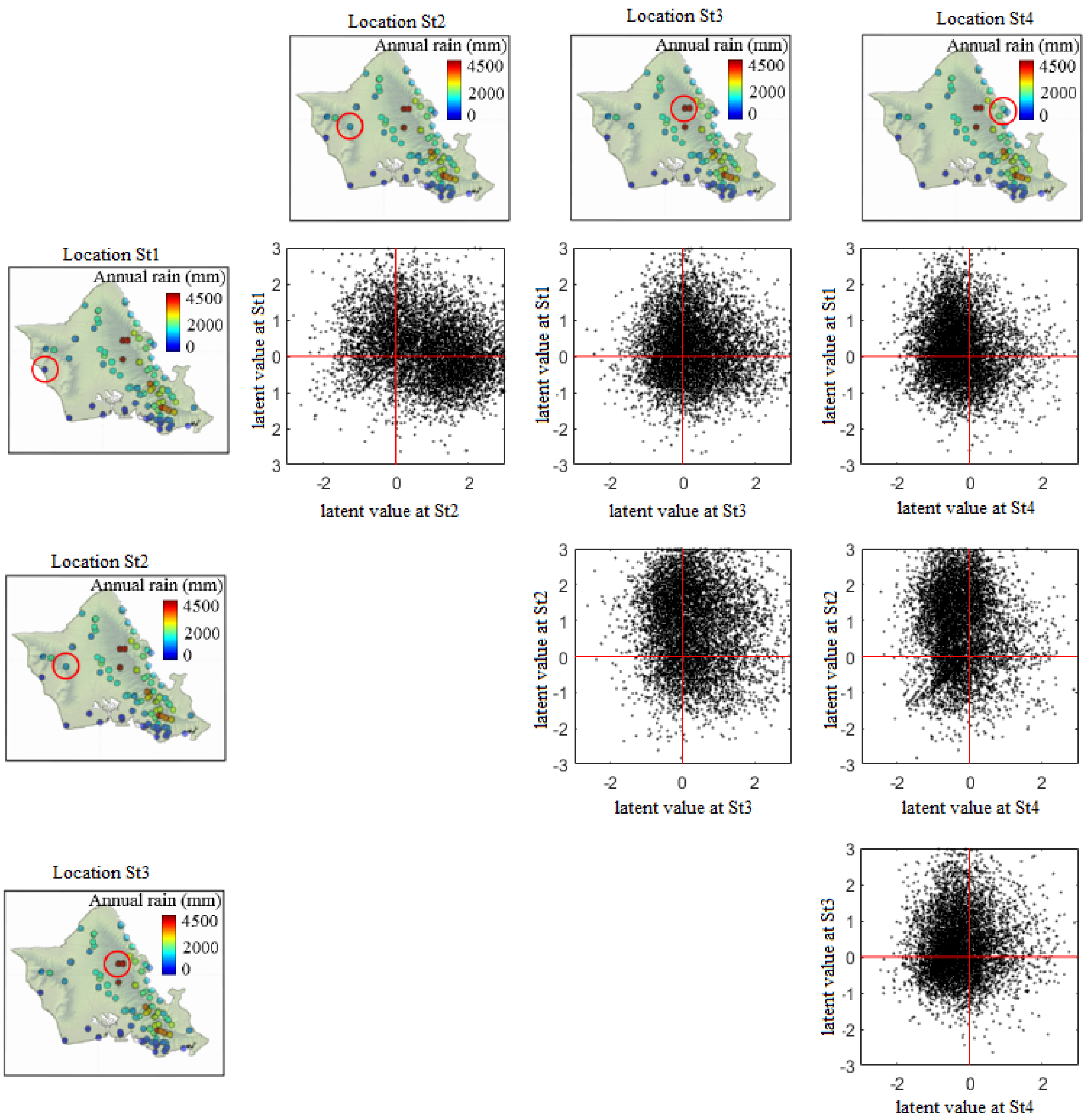
AR 1.11: Equation 1 simply means that we assume that a Gamma function is able to transform the observed rainfall (R) to a censored Gaussian variable (Z), such that when rain is observed the variable Z takes a value above a given threshold ($\Phi^{-1}\left(\frac{N_d}{N_t}\right)$), and when no rain is observed the variable Z takes a censored value below this threshold (the way we assign this censored value is given by Eq. (2)). The use of a parametric transform function coupled with censoring to relate observed intermittent rainfall to a latent Gaussian variable is common in stochastic rainfall models based on Gaussian processes (see e.g., [*Glasbey and Nevison*, 1997] [*Allcroft and Glasbey*, 2003] [*Allard and Bourotte*, 2015] [*Baxevani and Lennartsson*, 2015] to cite but a few). The choice of the Gamma function as a transform function may seem arbitrary, but has been selected here for the flexibility of this transform function to accommodate day-to-day variations of the distribution of non-zero rain accumulation across the island. In addition, it should be noticed that this choice is not disconnected from the literature on this topic, since a Gamma transform has already been used e.g., by [*Kleiber et al.*, 2012] in a different climate (Argentinian pampa).
Regarding the terminology, the term 'latent' is used here to describe the variable Z because Z is not directly observed. This is a common terminology in the literature related to stochastic rainfall models based on Gaussian processes (e.g., [*Glasbey and Nevison*, 1997] [*Allcroft and Glasbey*, 2003] [*Allard and Bourotte*, 2015] [*Baxevani and Lennartsson*, 2015]).

RC 1.12: Line 142: similarly, what's the justification for the distance-weighting in equation (2)? This again seems a bit arbitrary, although it's certainly true that what is sometimes called the "spatial intermittence" problem is hard to resolve satisfactorily. Nonetheless, in my view it's not clear that the authors' proposal improves on that by, say, Stehlík and Bárdossy (2002) – another example of literature that they seem unaware of, or at least haven't considered as carefully as they should have done. And once again, it's not a "latent" field if it's defined in terms of interpretable quantities.

AR 1.12: Equation 2 is indeed an attempt to deal with spatial intermittence. The idea behind our choice of distance-weighting is that a location far from any wet gauge should remain dry even after combining the copula at hand with slightly different parameters of the transform function during the simulation step. It is therefore application-specific and tightly linked to our other modelling choices. We agree with reviewer#1 that spatial (or temporal) intermittence is a problem that is hard to solve in a generic way in the framework of a transformed and censored Gaussian process. A large spectrum of methods have been proposed to solve it in case-specific applications. On one end of this spectrum, the option is to consider non-censored values as the main source of information about the whole process (including censored values). The spatial intermittence problem is then solved by simulating many times the full process conditional to non-censored values only, and selecting the simulations leading to patterns of spatial (and/or temporal) intermittency similar to the observed ones (e.g., [*Allard and Bourotte*, 2015] [*Papalexiou et al.*, 2018]). On the opposite end of the spectrum, more complex methods have been proposed to simulate the censored values conditional to (1) non-censored neighboring observations, (2) space (and/or time) patterns of rain intermittence, and (3) the fact that the latent variable is censored at the location of interest (e.g., [*Bárdossy and Pegram*, 2016] [*Benoit et al.*, 2018b]). Here we place ourselves half way between these two approaches, and we regard the latent variable as a rain potential that is decreasing when observed daily accumulation decreases (Eq. 1), reaches $\Phi^{-1}\left(\frac{N_d}{N_t}\right)$ at the wet/dry transition (Eq. 1 and 2), and keeps decreasing when the distance to wet gauges increases (Eq. 2).

RC 1.13: Lines 149-150: there's a claim here that the complex spatial distribution "prevents the use of a simple parametric form ... for the spatial copulas". It's not entirely clear to me what this means, but I assume it's something like: you can't find a "standard" spatial covariance model to represent the dependence structure between the Z_i. I'm prepared to believe this (although it could be partly an artefact of the artificial and deterministic distance-dependence structure of equation (2)), but it would be helpful to see some plots to justify it.

AR 1.13: Yes, we mean that the joint distribution of copulas between sites does not follow a stationary multi-variate Gaussian distribution, which implies that we cannot find easily a “standard” spatial covariance model to represent the dependence structure between the Zi. This is illustrated in the figure below, which displays bivariate scatterplots of the latent values at the 4 target stations used in Fig 1 and 4 for illustration purposes, and which shows that bi-variate spatial dependences are neither bi-variate Gaussian (i.e., scatterplots do not have an elliptical shape) nor stationary (i.e., scatterplots are centered on different means, and have different spreads). We will add this figure to the supplementary material section and discuss it to better justify our choice of non-parametric copulas to simulate the spatial dependence structure.



RC 1.14: Lines 158-159: the ability to calculate principal components of the "latent" field shows that it isn't latent. See earlier comments on this.

AR 1.14: Please refer to our response AR 1.11 above.

RC 1.15: Sec 2.2.3: I find this "rain typing" approach, which seems to be derived from techniques that are popular in the machine learning community, to be rather clumsy compared with the much more principled approach taken by other authors using HMMs. Even if one were to accept that the approach is worth considering, the classification in lines 172-174 is inappropriate because it fails to account for the uncertainty in each day's weather type (e.g. if you had three states then you could find that the probabilities are always (0.34, 0.33, 0.33) in which case you would assign every day to state 1 which is clearly nonsense – exactly like the electoral system in some recently failed democracies).

AR 1.15: As mentioned in AR 1.2, we adopted ‘explicit’ rain types (i.e., not hidden) to enable the non-parametric steps of our framework, which are justified by the complexity of rainfall patterns in tropical islands. This choice of explicit rain types requires in turn finding an alternative to HMM for rain typing, and for this study we chose a Gaussian Mixture Model (GMM) classification. This classification approach is widely used for clustering (e.g., [*Fraley and Raftery*, 2002]), and has been applied successfully in the context of weather and rain typing (e.g., [*Vrac et al.*, 2007b] [*Benoit et al.*, 2018a]). Regarding the classification (lines 172-174) we do not see anything wrong in our equations 4 and 5, which follow the usual GMM classification framework (e.g., [*Fraley and Raftery*, 2002]). We are not sure to understand the concern of reviewer#1. If we imagine that the probabilities of the three states (rain types) are 0.34, 0.33, 0.33 for all values of the feature-space, it means that the (multivariate Gaussian, in our case 6-dimentional) distributions of these three types are almost identical, in which case the model will automatically detect that only one single type exists ($N_C = 1$). And therefore this problematic case of rain type non-identifiability will not occur.

RC 1.16: Line 179: there seems to be something wrong with this equation. If I understand correctly, it is something like a kernel estimate of the transition probabilities based on the Mahalanobis distance in the space of meteorological covariates; but if so, then summing the right-hand side over j *should give 1 for any value of* i *or* MC_d *(it's supposed to define a conditional probability distribution). It doesn't, at least if* γ_{ij} *is a "baseline" transition probability (which isn't stated here, but seems to be the case based on a later description – if that's the case, then it's true that* $\sum_j \gamma_{ij} = 1$ *for all* i *but the exponential factor in line 179 messes things up).*

AR 1.16: Thanks a lot for spotting this typo in equation 6, the sign ‘$=$’ must indeed be replaced by ‘$\propto$’. By doing so we obtain the usual formulation of a non-homogeneous Markov Chain with transition probabilities based on the Mahalanobis distance in the space of meteorological covariates (.e.g., [*Hughes and Guttorp*, 1999] [*Vrac et al.*, 2007a]). We will correct this error in the revised manuscript. Regarding γ_{ij} it is indeed a baseline transition probability, as mentioned l 182.

RC 1.17: Line 184: how does the conditioning inform seasonality*? This is another slightly dangerous assertion: relationships between precipitation and meteorological covariates can themselves be seasonally varying, for good physical reasons (think precipitation and temperature in temperate latitudes - they are positively associated in winter but negatively associated in summer).*

AR 1.17: The conditioning actually informs the state of the atmosphere, which is strongly correlated with rainfall features at the island scale, as shown in Fig SM1.1 and SM2.2. At the same time, the state of the atmosphere defined from a set of meteorological covariates is known to have a strong seasonal signal (e.g., [*Vrac et al.*, 2014]). Hence, the link with seasonality is indirect, but is still very efficient in the present case to encode seasonal and inter-annual variations of rain accumulation at specimen locations as shown in Fig 4. Indeed, one should recall that meteorological covariates are the only part of the modelling chain that varies through time, and therefore the only source of information about rainfall seasonality.
We agree that the links between a single meteorological covariate and precipitation can change during the year for good physical reasons, but we argue that investigating the links between a set of several meteorological covariates and rainfall reduces the risk of misleading correlation. In practice, using this link between multiple meteorological covariates and rainfall features has been an effective way to encode rainfall seasonality in several past studies (e.g., [*Bárdossy and Plate*, 1992] [*Vrac et al.*, 2007a]

[*Benoit et al.*, 2020]). However, to avoid confusion, we will be more careful when describing the links between conditioning and seasonality in the revised manuscript.

RC 1.18: Lines 189-192: if the models are to be used for downscaling climate model projections, there are other requirements as well e.g. that the covariates are well represented by climate models and capture the climate change signal (see, for example, Maraun and Widmann 2018, Section 11.5).

AR 1.18: We fully agree with this comment, and we will add a paragraph in the discussion section to acknowledge it.

RC 1.19: Lines 224 and 226: "GMM model" is a tautology. See also my previous comment about the allocation to the "most probable" state.

AR 1.19: This is right, we will use GMM instead.

RC 1.20: Lines 232-233: I don't understand what you're doing here. In lines 218-220 you said that you already estimated the parameters of ψ: why are you doing it again, therefore? Also, any simplistic method of bandwidth selection such as equation (7) is almost guaranteed to fail in a large proportion of applications: the authors' use of such an approach suggests that they don't really understand the potential pitfalls of such an approach – or, indeed, the availability of alternatives.

AR 1.20: In lines 218-220 we estimate the parameters of ψ for each day of the training dataset, which allows us to derive the empirical joint pdf of p_0, k and θ from observations. In contrast, in lines 232-233, we propose a non-parametric approach to sample this joint pdf and therefore simulate $p_0(d)$, k(d) and θ(d) for a given target day d. In a very schematic way we could therefore say that lines 218-220 correspond to inference and lines 232-233 correspond to simulation.
Regarding the choice of the simulation method, we also tested a parametric approach in which the parameters p_0, k and θ followed independent parametric distributions (we picked Gamma distributions for flexibility) that we inferred using a maximum likelihood approach. However, p_0, k and θ are correlated, which made this parametric approach leading to not fully satisfactory results. Hence the choice of a joint inference and simulation. The selection and inference of a flexible tri-variate distribution being cumbersome, we have decided to adopt a non-parametric approach. Although simple, this approach gives good results in practice as shown in the sections 3.2 and 3.3 where we can see that the marginal distributions of site-specific and island scale daily rain (i.e., the summary statistics that are the most impacted by p_0, k and θ) are both very well simulated by our model (Fig 4d, 5c, SM2.3d, SM2.4c).

RC 1.21: Line 244: here, for the first time (I think) we discover that separate empirical copulas are being estimated for each day. What's the basis / justification for this? Are you not just resampling the original data, with a bit of smoothing? [actually this is mentioned on line 351, but I think it should be acknowledged upfront in the methodology].

AR 1.21: It is true that this information (resampling of analogs) arrives a bit late in our paper, and it would have a better place in the model description (section 2.2) rather than in the model

implementation (section 2.3) section. In the revised paper, we will therefore introduce the resampling of analogs at the end of section 2.2, in the paragraph lines 147-152 (current numbering).
The justification for the adopted method – namely the resampling of separate empirical copulas for each day coupled with gamma transform and censoring – is that despite the classification of days into many rain types (e.g., 22 for Oʻahu, which we justify hereafter), the intra-type variability of spatial patterns (and in turn of spatial copulas) remains significant. The best way to preserve this variability is therefore to resample daily analogs instead of using a single copula per type. As mentioned lines 350-353 and discussed lines 410-414, this approach indeed leads to results close to resampling the original data based on a preliminary classification into rain types, which we consider as a completely valid method for stochastic rainfall or weather generation in complex configurations (see e.g., [*Gangopadhyay and Clark*, 2005] [*Mezghani and Hingray*, 2009] [*Yiou*, 2014]), the innovation compared to previous studies being that our approach decouples spatial patterns and intensity in order to be able to create unobserved situations (which increases the variability rather than smooth the training dataset). We agree with reviewer#1 that this feature is an important part of our model, and we will mention it upfront in the methodology in the revised manuscript (i.e., by expanding the paragraph lines 147-152).

RC 1.22: Line 281: do you really believe there are 22 distinct rainfall regimes on the island? You've only got 86 stations, so this classification doesn't seem to be reducing the spatial dimension as much as one might hope. As a slightly peripheral (but important) comment: Figure 3(b) will be inaccessible to the ~5% of male readers who suffer from red-green colourblindness.

AR 1.22: Yes, we believe that 22 is a reasonable amount (even if not exact because of the many simplifications made during the classification process) for the number of distinct rainfall regimes on the island of Oʻahu. This may seem a lot for modelers used to mid-latitude and/or continental climates, but we would like to take the opportunity of this answer to emphasize one more time the tremendous variability of rainfall and rainfall regimes in tropical islands, which in our opinion largely justifies the development of a dedicated stochastic rainfall model. We would like to mention here that the Hawaiian language has several hundreds of names to describe rain (e.g. [*Akana and Gonzalez*, 2015]), which is at least one order of magnitude more than in European languages. In our opinion, this cultural difference indirectly illustrates the fact that far more distinct rainfall types exist in Hawaiʻi than for instance in Europe, and gives credibility to the high number of rain types identified in our study. A more quantitative justification for the proper dimension reduction of our classification is that the less complex (and smaller) dataset of Tahiti leads to an 11-type classification (supplementary material l41-44), which proves that our method can automatically adjust to the complexity of the dataset at hand (l381-384), and does not always produce very high numbers of rain types. We therefore believe that the 22 types obtained for Oʻahu accurately describe the complexity and variability of rainfall observed on this island.
This being said, one should keep in mind that the selection of the number of components of the GMM, and in turn the number of rain types, is derived from an automatic model selection procedure based on a compromise between model parsimony and goodness-of-fit. Here we chose the Bayesian Information Criterion (BIC, [*Schwartz*, 1978]) to guide model selection, but other choices are possible (e.g., Aikaike Information Criterion - AIC), each criterion leading to a different ratio parsimony/goodness-of-fit. BIC has been chosen here because of its widespread use, and because it emphasizes the model parsimony (and therefore reduces the number of rain types) more than AIC. If one had good reasons (based on actual observations or previous knowledge about local rainfall

variability) to believe that a smaller number of rain types must be selected, one could adopt an even more “parsimonious” selection criterion, but we don’t think this is required for the present dataset. Regarding the colors used in Fig 3b we agree with Reviewer#1, and we will therefore change the color scale for this figure and Fig SM2.1 to make them accessible to colorblind readers.

RC 1.23: Lines 321-326: although it's good to test the model using a variety of measures, I'm not always convinced by the way that this has been done here – and I don't fully understand all aspects of the plot. What are the grey bands in column (b)? What do you mean by "each statistic is estimated as the median across the 50 realizations"? [I ask this because you have dashed lines indicating the quantiles, suggesting that you're showing the distribution rather than the median – in any case, it isn't appropriate to compare a single observation to the median of 50 simulations because they will have different statistical properties even if the model is correct]. There are published papers that deal with this issue correctly: the authors need to familiarise themselves with the literature. The issue is particularly acute in the Q-Q plots of column (d): if you really understood what a Q-Q plot represents, you'd realise that you don't need to duplicate the observations 50 times (I hope you haven't used the median of the simulations here as well ...). Similar comments apply to Figure 5.

AR 1.23: There are several distinct concerns in this comment, and we will address them one at a time:
(16a) We share with Reviewer#1 the interest for multi-criteria model evaluation, and will try our best to make sections 3.2 and 3.3 easier to read and understand.
(16b) We made an error in the written caption of figure 3 (lines 321-326): in (a), (b), and (c), black refers to simulations and red refers to observations and not the opposite (the caption included in the figure was correct, but the description lines 321-324) was wrong. This probably caused confusion, and we would like to apologize for it.
(16c) We do not compare a single observation to the median of 50 simulations but rather compare distributions described by 3 quantiles: 10%, 50%, 90%. Hence the lower dashed red line is quantile 10% of observations, the continuous red line is quantile 50% of observations, and the upper dashed red line is quantile 90% of observations. If we would have simulated a single realization, the lower dotted black line (lower edge of the grey band) would have been the quantile 10% of this single realization, the continuous black line the quantile 50% of this realization, and the upper dotted black line its quantile 90%. However, we have simulated 50 realizations, and should therefore plot 50 lower dotted black lines, 50 central continuous black lines, and 50 upper dotted black lines, which would impede the readability of the figure. To improve on that point, we decided to summarize the information embedded into the 50 simulations of each quantile into a single black line, which we chose to be the median of each quantile evaluated through 50 realizations. This is what we meant by “*each statistic is estimated as the median across the 50 realizations*”, the word “statistic” referring here to quantile 10%, quantile 50% and quantile 90%. To avoid confusion, we propose the following phrasing for the revised version of the paper: “*(b) Observed (red) and simulated (black) monthly rain accumulation. Dashed lines denote quantiles 10% and 90%, and solid lines denote the quantile 50%. In the case of simulations (black), for readability, we report in the figure only the median of each quantile (10%, 50%, 90%) instead of 50 simulated quantiles.*” To conclude on this point we would like to mention that such representation is not new in the literature related to stochastic rainfall models, and has been used for instance in Fig 4 of [*Peleg et al.*, 2017].
(16d) For q-q plots we didn’t use the median of simulations but the 50 individual simulations as clearly indicated in the figure caption: “*Black dots line up vertically in q-q plots (d–e) because for each percentile, 50 simulations are compared to a single observation*.” (lines 325-326). We chose to duplicate observations in order to be able to draw one q-q plot for each realization, and therefore

show the variability between individual realizations. As for the previous point, we would like to mention here that such representation is not new in the literature related to stochastic rainfall models, and has been used for instance in Fig 1 of [*Allard and Bourotte*, 2015].

RC 1.24: Lines 340-341: I don't know what are the likely applications of precipitation modelling in this location, but if the potential stakeholders include farmers then I think they may be justifiably sceptical of your claim that underestimation of persistence can be tolerated for mathematical convenience. Precipitation modelling is not a mathematical exercise, it relates to lives and livelihoods.

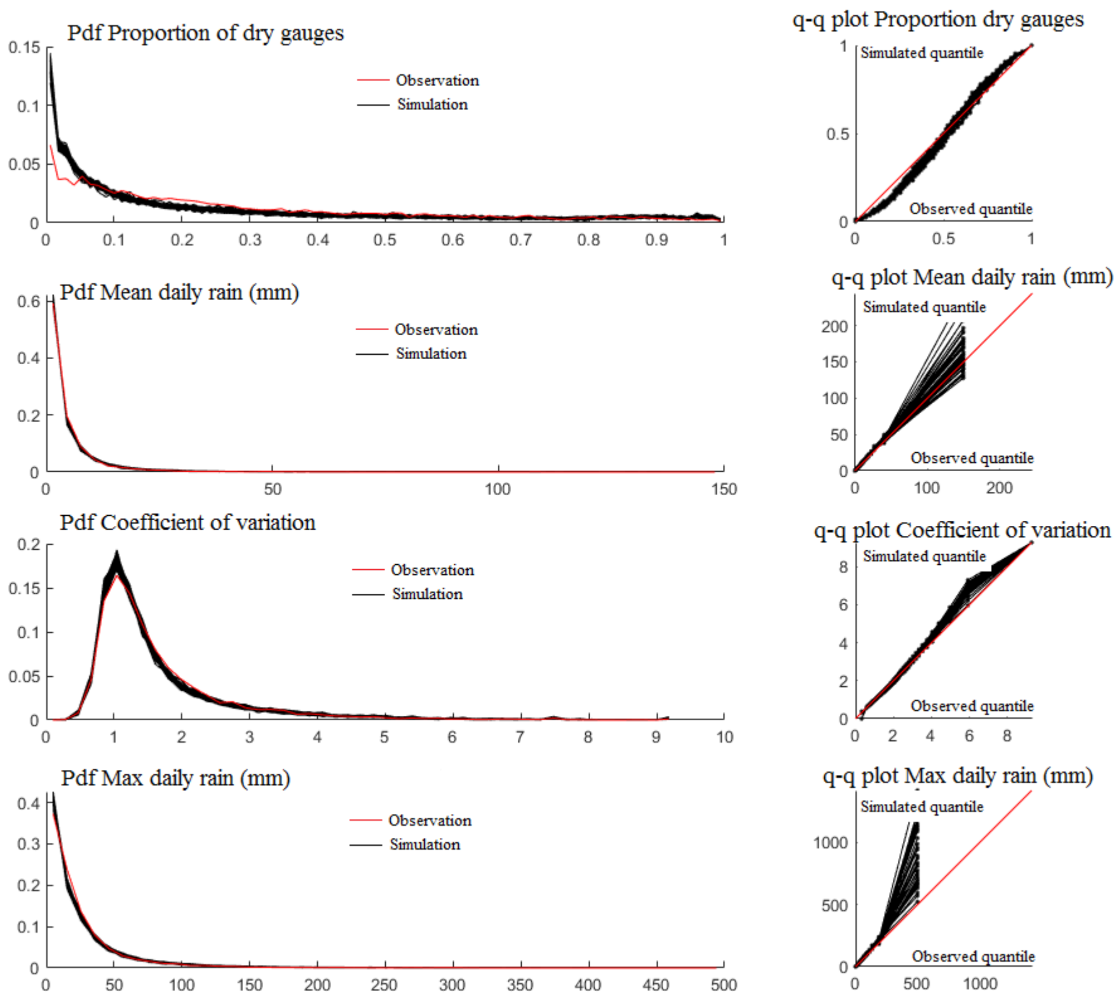
AR 1.24: The likely applications the authors had in mind when designing this study were water resources management "from Mauka to Makai" (i.e., from mountain to the coast), hydro-power assessment at the island scale, and eco-hydrology of tropical island watersheds. That said, we agree that we should be more careful in the way we underline the drawbacks of our model and the associated limitations in term of applications (although we think that nothing was hidden here, we acknowledge a tactless wording). We therefore propose the following rephrasing: "Although the resulting errors are of low amplitude, they should be kept in mind and regarded as a limitation of the present model for applications requiring a precise estimation of rain persistence, as is the case for instance for crops models in semi-arid environments that can be found in some leeward areas of the target islands."

RC 1.25: Lines 362-364: I'm not convinced by this dismissal of the curvature in the Q-Q plot of Figure 5(b). It is well-known that by compressing the tails of both observed and simulated distributions, Q-Q plots can make it hard to see discrepancies in the tails that may have substantial implications in applications. It would be helpful to consider alternative approaches to visualising this particular comparison (e.g. my guess is that if you were to plot the observed and simulated densities of proportions of dry gauges then you would be a bit more concerned about the simulation performance).

AR 1.25: To avoid a one-sided representation of the statistics of interest, we display in the following figure both (1) the observed and simulated distributions of the statistics of interest (left column, as proposed by Reviewer#1) and (2) the associated q-q plots (right column). One can notice that the representation of the results in the form of PDFs confirms the conclusions derived from the q-q plots (section 3.3, l348-376), namely: (i) the overestimation of the frequency of low proportion of dry gauges (leading to the underestimation of the low quantiles), (ii) the overestimation of the maximum daily rain at the island scale (not distinguishable in the left column of this figure, but visible when zooming on the tail of the pdf, and of course on the q-q plot), and (iii) the good simulation of all other aspects of the evaluation statistics.
Regarding the imperfect simulation of the proportion of dry gauges, the effect is indeed easier to interpret when looking at the pdf, but the conclusions we draw from this plot are rather comforting. Indeed, one can see that the main discrepancy between simulations and observations is the overestimation of the frequency of proportions of dry rain gauges below 5%, which correspond in the present setting to less than 4 gauges over 86. In other words, our model tends to simulate rain at all 86 gauges when a very small number of gauges (<4) actually records no rain, leading to a slight 'drizzle effect' in space where rain is simulated at more locations than it should during generally wet days (more than 95% gauges record some rain). A careful examination of the spatial patterns of daily rain percentiles (Fig 5a) shows that the patterns of spatial intermittency of the percentiles are properly simulated, which suggests that the drizzle effect is randomly distributed amongst locations, which reduces its potential impact for applications. This is supported by the proper simulation of the marginal

distribution of rain accumulation (incl. zeros) at site-specific locations (Fig 4), which confirms that the drizzle effect at the island scale does not cause biases at the local scale.

All in all, we acknowledge an imperfect simulation of the proportion of dry gauges causing a drizzle effect in space at the island scale (which was already pointed out in lines 362-364 of our original manuscript) but we believe that this weakness is not dramatic for applications. However, to avoid any misuse of our model, this point will be discussed in more details in the revised version of the manuscript, and the figure below will be added to supplementary material to provide a more exhaustive visualization of our results.



RC 1.26: Line 388: in what sense is the model structure "hierarchical"? It may be worth noting that "hierarchical modelling" has a precise technical meaning in statistics: this isn't what you are doing here. It's probably worth rephrasing for avoidance of ambiguity, therefore.

AR 1.26: To avoid ambiguity and confusion, we will use "two-step model based on rain typing" instead of "hierarchical modelling" in the revised manuscript.

RC 1.27: Line 392: here, the definition of weather types "based on rain features only" is offered as an apparent advantage of the proposed methodology. I would say that this is a distinct disadvantage because it ignores the physical processes that are operating. In my view, one of the key challenges in *stochastic weather generation is to find mathematically tractable ways of capturing the footprints of the fundamental physical processes: this remark from the authors suggests that they either haven't thought about this, or that they consciously disagree with me. At the very least, some more explanation is needed as to why this feature may be considered desirable and defensible.*

AR 1.27: We agree with Reviewer#1 that one of the key challenges in stochastic weather generation is to find statistical models able to capture the footprints of physical processes, but we don't see in which ways our rain-typing approach might be in contradiction with this aim. To go one step further, we would argue that if the main variable of interest is rainfall (which is the case of the present model), our approach allows a better identification of rainfall - climate interactions than a weather type approach, which would mix information about several meteorological variables during the classification step. In contrast, the choice of typing rainfall alone, and subsequently trying to relate rain type occurrence to meteorological covariates allows us to investigate (from a statistical point of view) the links that exist between the state of the atmosphere and the physical processes responsible for rain generation (incl. orographic effects). This is what we discuss in lines 395-401, and we think that this discussion is in line with the goal of statistically capturing physical processes stated by Reviewer#1.

RC 1.28: Line 424: I disagree that the authors' model represents "a new tool". I think it represents a first step towards reinventing some existing tools (e.g. HMMs), but without acknowledging their existence (or perhaps not understanding what they're capable of).

AR 1.28: We strongly believe that our model is “a new tool”. We hope that our thorough responses to the above comments will have convinced readers and Reviewer#1 that (1) tropical island rainfalls are complex and very variable in both space and time, (2) these rains are different from the ones for which most stochastic rainfall models have been designed, (3) direct transposition of existing models is not easy, and (4) our modelling choices are reasonable and properly capture the main features of rainfall in tropical islands. To conclude, we think that independent from the novelty of the model itself, having a complete processing chain able to properly simulate daily rainfall in tropical islands (which we showed in our model assessment section 2.4, and in supplementary material 2.3) is already a valuable contribution and fully justifies our work.

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