We warmly thank the Reviewer for his/her valuable comments. We provide below a detailed point-to-point reply to these comments. The proposed changes for the next version of the article are indicated in red.

The paper describes a method for mapping distribution parameters from single rain gauge record across a domain, but lacks a proper discussion of the role/significance of the proposed framework in the landscape of rainfall hazard mapping: in that respect, if the proposed framework is really a (significant) step forward then the authors should demonstrate it by comparing it to the state-of-the-art in rainfall hazard mapping, which they as such also describe in the introduction (page 2, line 4-14). I am left with the impression of reading a technical report rather than a paper significantly advancing the field (which does not mean there is no advancement per se, but it is difficult to judge at this stage).

⇒ We think there is a misunderstanding here. First, the idea of mapping rainfall hazard by mapping the parameters of rainfall CDF is not new. See for example Beguería and Vicente-Serrano (2006); Beguería et al. (2009); Szolgay et al. (2009); Blanchet and Lehning (2010); Ceresetti et al. (2012), which are cited p. 2 line 25 of the article. Second, the goal of this paper is not to propose a new way of mapping rainfall hazard, but to propose an objective framework, based on cross-validation criteria, in order to assess goodness-of-fit of the full procedure of hazard mapping. This comprises the selection of both the best marginal distribution and the best mapping method *among those tested*. Thus, we do not claim to propose a new and better way of mapping rainfall hazard, but we propose a framework for selecting objectively among a bench of methods/models. To the best of our knowledge, such a framework has never been proposed so far. As explained p. 2 lines 15-27, usually either a given rainfall CDF is assumed and different mapping methods are compared (often not in a cross-validation framework), or different CDFs are compared but the mapping step is not considered. Here we do both and for this we propose a new cross-validation framework based on objective criteria.

Regarding the recommendation of comparing our results with a method based on interpolating rainfall, as described page 2 line 4-14, this would indeed be interesting but we think this is out of the scope of the paper. As already said, our goal is to develop an objective cross-validation framework for mapping rainfall hazard, not to develop a better way of mapping hazard than state-of-the-art methods. Second, there is not a unique way of spatially interpolating rainfall, and this issue can be the subject of an article on its own (see e.g. Camera et al., 2014; Creutin and Obled, 1982; Goovaerts, 2000; Ly et al., 2011; Rogelis and Werner, 2013, , which are cited p. 2 lines 7-8).

I do not understand why classic interpolation of rainfall of a certain frequency comes along with issues of zero values (even more obvious when mapping amounts and their exceedance probabilities), see page 2, line 10-11. A certain exceedance probability of rainfall is >0 by definition, and we are talking about regional hazard maps and not about scenarios (i.e. rainstorms). Or am I getting something wrong? Also, provided the *issue exists, the authors address a solution themselves (which is the analytical transformation), which brings me back to the issue of ideally comparing the proposed framework to the state-of-the-art.*

⇒ By "spatial interpolation of rainfall", we mean here interpolating every daily raingage value within the region, in order to have daily time series at every grid point of the region. Then a CDF could be estimated at every grid point based on the interpolated time series. Maps such as those of Figures 10 and 11 of the article could directly be produced based on these estimated CDFs. The issue in doing this is that 65% of the daily values are zeros. Interpolating methods work usually well for smoothly varying unbounded values (e.g. temperature), but it performs much more poorly for handling spatial intermittency.

The reviewer mentions the possibility of interpolating probabilities rather than daily rainfall values. This would indeed be an alternative way of doing if a single probability were of interest, for example the 100-year return level. However this is not possible if one aims to be able to produce any probability map. This is for example the case in rainfall simulation frameworks, e.g. when rainfall are input of spatially distributed hydrological models: one needs to be able to simulate any rainfall with the right frequency. In such a case, one needs to know the rainfall CDF at any grid point. To do that, as explained in the article p. 2 lines 3-26, two methods are the great majority of the time followed, either by interpolating rainfall values, or by estimating the CDF at every raingage and then interpolating the parameters of these CDFs. This work follows the latter alternative.

I also do not understand why one would feed a distributed rainfall-runoff-model with a rainfall-frequency map (page 1, line 17-18). The resulting rainfall-runoff-model output is highly artificial, not much telling about a realistic hydrological scenario. In hydrological hazard/risk assessment, one would probably conduct scenario-based analyses based on potential (realistic) rainstorms or continuously simulate rainfall time series to feed an RR-model, to get insights about (extreme) runoff events. But maybe I did not understand what the authors intend to say here; citations of related work would maybe clarify.

⇒ Of course we do not intend to feed a distributed rainfall-runoff-model with a rainfall-frequency map. We mean page 1 lines 17-18 that when rainfall-runoff-model are feeded with rainfall simulations, one needs to be able to simulate possible rainfall fields. For this two ingredients are necessary: first to know the rainfall CDF at any grid point, i.e. the marginal distribution. Second to know the spatial correlation of rainfall, i.e. the spatial distribution. This article intends to contribute to the first point, the marginal distribution.

In order to make this clearer, we propose to replace p. 1 lines 18-19 by: spatially distributed hydrological models. In such a case one needs to be able to simulate any possible rainfall fields. This implies knowing both the local occurrence of any rainfall value with the right frequency, and not only the largest ones, and their spatial co-occurrence.

Modelling only two seasons is a clear limitation, and so is the assumption of stationarity. The same *applies to the three weather patterns, which is another constrain. I am also in doubt that the Gamma (or the mixture of a Gamma) is suitable in other regions, especially in the tail. That is, it remains open whether the framework is really applicable to other regions. The authors put that into question themselves (e.g. page 24, line 30, among others). So besides comparing the proposed method to the state-of-theart, a second study area (other climate, more seasons) would be – in my eyes - very important.*

⇒ We would like to stress again that the goal of this article is *not* to propose a new and better way of mapping rainfall hazard. Our goal is to propose an objective framework for selecting among a bench of methods/models in order to pass from isolated raingage records to CDFs maps in a region. Therefore we do not claim that the considered mixture of Gamma distribution is the best model for the region, and even less for other regions of the world. What is claimed is that, based on the proposed cross-validation framework, the mixture of Gamma is the best model *among those tested*. Therefore, although testing the generality of the mixture of Gamma to fit rainfall in other regions of the world would be very interesting, we think this is out of the scope of this paper.

What is also missing is a proper discussion of the uncertainties of the rainfall records in mountain regions. It would be important to consider these observation errors in the framework, again for the proposed model and the state-of-the-art in comparison, to really understand all implications.

⇒ This is a very interesting point. Every measurement comes with some uncertainty - whatever the variable of interest. Uncertainty in precipitation measurement is usually larger for snow events, and particularly when wind blows. However the region of interest, although mountainous, is located in a relatively warm area where snowing days are very rare (maximum a few days per year but usually zero). Therefore we believe that data uncertainty in our case can be considered as reasonably small. How small it is and what is its impact on the estimated rainfall distribution? We are sorry to admit we lack information to document this.

A figure describing the entire framework would be important, it is difficult to follow all steps and practitioners will for sure appreciate a presentation that is a tad more "handson".

⇒ We thank the reviewer for this nice suggestion. The schematic summary of the procedure is displayed in Figure 1. We propose to start Section 3.3.1 with the sentence: "The full cross-validation procedure for selecting both the marginal and mapping models is summarized in Figure 5.", where the new Figure 5 shows Figure 1 below.

I would recommend a final proofread by a native speaker, there is quite a number of minor language related mistakes throughout the manuscript, not a big deal but just a few examples: "One of the difficulty"; "models for nonzeros rainfall"; "Similar idea is used"; "in the same time"; "independently on each others"

⇒ We warmly thank the reviewer for pointing out these typos that will be corrected as follows: one of

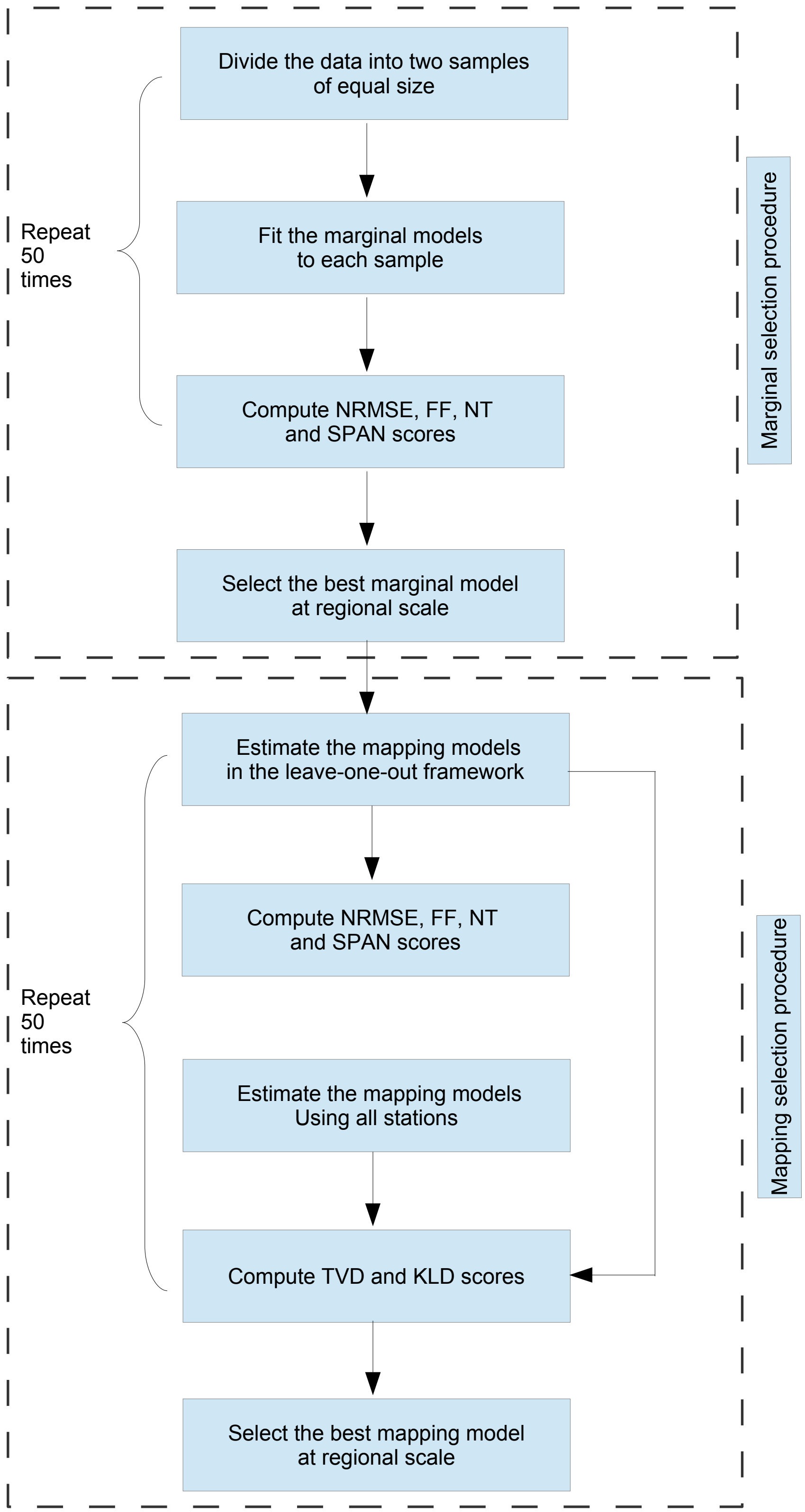

Figure 1: Schematic summary of the full cross-validation procedure for selecting both the marginal and mapping models.

the difficulties; models for nonzero rainfall; a similar idea is used; at the same time; independently of each others.

Sharing the original observation data (other journals even demand it) to allow for reproducing the results is recommended.

⇒ The rainfall data were provided by Electricité de France and Météo-France. The Météo-France data used in this publication are available through the Public Data portal of Météo-France available at https://publitheque.meteo.fr/okapi/accueil/okapiWebPubli/index.jsp and are free of use for research purpose. The people interested in the Electricité de France data for research purpose should contact Emmanuel Paquet, emmanuel.paquet@edf.fr.

In summary, at this stage, I am in doubt that the reader really understands the added value of the method, why and when established rainfall hazard mapping methods are competitive and if the method is applicable to another region/climate.

⇒ We hope we have clarified the added value of this article.

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