

Interactive comment on “Stochastic reconstruction of spatio-temporal rainfall pattern by inverse hydrologic modelling” by Jens Grundmann et al.

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Received and published: 24 September 2018

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Authors final comments to referee 2

September 24, 2018

Journal: HESS

Title: Stochastic reconstruction of spatio-temporal rainfall pattern by inverse hydrologic modelling

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MS No.: hess-2018-350

We would like to thank the referee for his/her time to review the manuscript. Our reply is organized as follows: (1) comments from Referee are in black color, (2) author's response is marked in blue color and placed within referee comments whenever it's needed, (3) author's changes in manuscript based on comments of both referees are summarized at the end of this document.

REFeree 2:

The paper 'Stochastic reconstruction of spatio-temporal rainfall pattern by inverse hydrological modelling' by Grundmann J., Hörning, S. and Bárdossy, A. proposes a

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method to estimate high resolution space-time rain fields from sparse rain gauges observations complemented by streamflow measurements. I find the idea of incorporating streamflow measurements and inverse hydrological modelling to reconstruct rain fields very interesting. And to my knowledge it is the first time that it is proposed to apply this idea to the reconstruction of high resolution space-time rain fields. In that respect I find this paper original. In addition the topic is relevant for the readers of HESS.

However, I feel that in the present version of the manuscript, the authors do not provide enough information (and of sufficient quality) to be able to assess the proposed framework. In addition I have the impression that even if interesting, the proposed approach cannot reach all the targets stated by the authors.

To sum up, I have the feeling that this paper addresses an interesting idea, but the current version is very preliminary (too much in my opinion) and does not allow to capitalize on the framework developed by the authors. I start by listing the points I would need to know in order to fully understand and assess the proposed method. After that, I will detail some concerns I have about the method itself. Afterwards I finish my review by few minor comments.

Possible improvements to better explain the method:

First of all, the written English must be improved. The present version of the manuscript is full of errors that shocked me even though I am not a native English speaker. At a minimum, a spell checker must be used. When I applied mine to the present manuscript I obtained dozens of errors and typos... In addition, some sentences are grammatically incorrect or difficult to understand. For instance: p1L20-24, p3L3-4, p11L5-8.

We are going to fix all typos and improve the grammar in the revised manuscript. The revised manuscript will be double checked by a native speaker.

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Regarding the introduction and the context of this study, I acknowledge that the application of inverse modelling to the reconstruction of space-time rain fields is new. However the idea of inverse hydrology in general (i.e. without space-time application) has already been proposed by several authors, as well as the idea of using streamflow data to improve rainfall input estimation. Unfortunately, none of these works are mentioned in the introduction. I find it quite unfair. I really would like to see more background about previous studies addressing similar ideas in order to better contextualize the present study. I can suggest for instance the following papers (I didn't participate to these works):

- Kirchner J.W. (2009): Catchments as simple dynamical systems: Catchment characterization, rainfall-runoff modeling, and doing hydrology backward, Water Resources Research, 45, W02429, doi:10.1029/2008WR006912.
- Kretzschmar A. et al (2014): Reversing hydrology: Estimation of sub-hourly rainfall time-series from streamflow, Environmental Modelling and software, 60, 291-301.
- Del Giudice, D. et al (2016): Describing the catchment-averaged precipitation as a stochastic process improves parameter and input estimation, Water Resources Research, 52, 3162-3186, doi:10.1002/2015WR017871.

Thank you very much for these references. We will improve the introduction and broaden the literature review and discussion.

Regarding the description of the rainfall-runoff model, very few information is provided. What is specified is basically that it is a distributed model, no more. For instance I don't know the name of the model, there is no reference about this model, and no equation to explain how it works. However I am sure that the hydrological model used for the inversion of the streamflow to reconstruct rainfall has a significant impact on the final result. By the way, the impact of the choice of the hydrological model (e.g. distributed vs semi-lumped) should be discussed somewhere in the paper.

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We will add additional information in the revised manuscript. Up to now, the model has no name. It uses only simple approaches known from hydrologic textbooks for the simulation of single events (no long-term water balance). It focuses on Hortonian runoff and considers spatial distributed travel times for surface runoff. You are right, the choice of the model has impact on its results. We will enhance the discussion of this issue in the last section.

Regarding the description of the Random Mixing approach, I really lack information about the underlying statistical model and the inference of its parameters. To be honest I had to read the paper of Haese et al (2017) to be able to understand the application of Random Mixing to rainfall modelling. Therefore I think that not enough efforts have been made to explain the Random Mixing method in the present paper. In particular I would be interested to know:

- Which spatio-temporal copula is used? Does it need to be a valid covariance function (or is it irrelevant in the context of copulas)?

We have used a Gaussian copula. And yes it needs to be a valid covariance function. We are going to add more information on copulas in general and the Gaussian copula in the revised manuscript.

- How are the parameters of the model (i.e. the marginal transform function and the copulas) inferred in practice? In particular how do you deal with dry measurements (i.e. rain intensity=0) in the inference process? (I think it is important here since rain intermittency can be significant in semi-arid and arid regions). Ok there is a reference to Li (2010), but more information within this paper would be a plus for the reader.

We are going to add a bit more information (and more references) on the inference process however we do not want to go into great detail as this is not the main focus of this work.

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- Which simulation method is used in practice to generate the unconditional simulations? You cite several methods but I would like to know the one you are actually using.

We have actually used the spectral representation method: Shinozuka, M., and G. Deodatis (1996), Simulation of multi-dimensional gaussian stochastic fields by spectral representation, Appl Mech Rev, 49(1). It is not in the references list yet so we are going to add it and mention it in the revised manuscript.

Regarding the synthetic case study, it is not clear to me if the parameters of the statistical rainfall model used in the random mixing are inferred from the synthetic data. I suppose that it is the case, but it should be clearly mentioned. If it is the case, it would be interesting to show the results of the fitting procedure. For instance: which copula (with which parameters) has been fitted? And also which marginal distribution? And how do the estimated values of the model parameters compare with the true ones (in this case you know the true values because it is a synthetic case)? In fact I suspect that the inferred statistical rainfall model cannot capture properly the true statistics of rainfall because the center of the rain cell is not observed. This can explain why conditional simulations (without streamflow constraints) cannot reproduce the observed hydrograph. I will come back to this point in my concerns about the method.

Yes the parameters are inferred from the synthetic data (only from the 'observations' though). Thus you are right, the inferred statistical model cannot capture properly the true statistics as for example the center of the rain cell is not observed. And this of course also leads to the fact that conditional simulations (without conditioning on runoff data) are not able to reproduce the observed hydrograph (but that is a general problem of course). The marginal distribution throughout the whole paper is the mixed distribution described in Eq.3 with a discrete probability of zeros (p_0) and an

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exponential distribution for all values > 0 . Based on the available observations the fitted parameters are: $p_0 = 0.36$ and $\lambda = 0.48$. The fitted copula is a Gaussian copula with an exponential correlation function with a range of 2.5 km in space and a range of 1.5 hours in time.

Regarding the real world application. I would have been more convinced if you have shown an example with cross-validation. For instance the reconstruction of space-time rainfall for a well instrumented catchment (with many rain gauges). In this case you can select some stations for the inference of the mixing model parameters and the estimation of rain fields, and keep other stations to cross-validate the rain estimations. In addition, in the real world application, the altitude of the catchment ranges from 600m to 2500m; in this case one can expect some non-stationarities in rainfall statistics. Could you please discuss a bit this potential issue?

The presented real world application in this manuscript is more or less the initiator for this research. It is based on our multiyear research on hydrologic processes in this arid region under data scarcity and small scale rainstorms. We understand your wish for "... an example with cross-validation." and we acknowledge this idea. However, in this case data quality and situation is bad and scarce. The walnut gulch catchment in US might be more appropriate for an investigation with cross-validation, but not manageable now. We will consider this in our future research. Thank you for this hint. Regarding the non-stationarities in rainfall, in this case the application shows a reconstruction of a single rainstorm which doesn't consider rainfall non-stationarities. The Figure 1 below shows the measurements of the rainfall gauging stations for this event and their altitudes. Most of the rain is recorded on stations with lower altitudes located in the north-west and south-eastern part of the catchment. We will add this figure and information in the revised manuscript. Obviously much more research is needed to fully exploit the advantages and limits of this procedure but we thought that we are at a level so that results can be communicated to the advantage of the possible readers of the journal.

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Concerns about the method itself:

In the proposed method, the parameters of the hydrological model are supposed to be known and fixed. But at the same time the goal is to infer high resolution space-time rain fields to... improve hydrological modelling. This seems a bit circular reasoning. I see two options to break the circle:

- Either clearly acknowledge that the proposed framework is a first step that only aims at reconstructing space-time rain fields from rain gauge and streamflow measurements. Basically a proof of concept with strong assumptions (incl. known hydrological model), that will be relaxed only in future work. And in this case do not claim that the goal is to improve hydrological modelling, but just to show that doing reverse hydrology to reconstruct space-time rain fields is somehow feasible. In my opinion this is already a very nice contribution.
- Or improve the proposed framework to jointly reconstruct space-time rain fields and calibrate the hydrological model. This can be seen as the extension of the work of Del Giudice et al (2016) (see ref above) to the case of space-time rain fields. But I suspect that this will require a lot of developments... and I am not sure it will work in many configurations... But if it works it would be an even nicer contribution.

It is definitely option one and as you argued correctly, option two would require lots of developments and is not manageable within this manuscript. We had in mind that an improved estimate of the model input also improves the hydrologic modeling results. But you are right, this can be misunderstood and we will formulate our arguments more carefully in the revised manuscript.

In the synthetic case study, runoff simulations based on simulated rain fields conditioned to point observation only do not encompass the ones based on conditioning to rain gauge observations and streamflow observations. This is clearly visible by comparing figures 6 and 8. I am very surprised about it. Indeed, in my understanding,

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the second case (adding conditioning to streamflow) should just add constraints to the first case. Therefore it should only select the rain fields obtained by simulation conditional to rain gauge only that are compatible with streamflow observations. But it is clearly not the case here... Therefore either I am missing something, and in this case I believe the reasons why this result arises must be explained in much more details by the authors; or there is some issue. One possible reason I could suggest (but I am not sure) is that the actual rain field that generates the observed hydrograph is kind of an extreme of the multivariate statistical distribution that underlies the random mixing model (after fitting model parameters). And therefore this extreme is not sampled by the 200 realizations performed in Figure 6.

Yes, your assumption is right and we are going to improve the discussion in the revised manuscript. Most probably, if we would sample more than 1000000 conditioned rainfall fields we would find a realisation which matches the runoff observation too, since the amount of possible conditioned rainfall fields is very much higher than the amount of rainfall fields matching point observation and runoff. Due to additional conditioning we find these realisation faster.

Regarding the assessment of uncertainty, I would be more cautious before stating "This ensemble can be used to describe the uncertainty in estimating spatio-temporal rainfall patterns" (p11, L9). In my opinion, the ensemble of realizations that is obtained is only a very partial descriptor of the total uncertainty. Indeed, in the proposed framework, both the statistical rainfall model and the runoff generation model have fixed parameters. Therefore the uncertainty originating from these two components is neglected. In the end, only the uncertainty related to the scarcity of the rain gauge measurement network is accounted for. I think this should be more clearly explained to the reader.

You are right. It is a partial descriptor of the total uncertainty. It describes the remaining uncertainty of spatio-temporal rainfall fields if all available data are exploited (under the assumption of known hydrologic model parameter, error-free measurements, and

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reliable statistical rainfall models). We believe that we can reduce the uncertainty of precipitation this way. We will formulate our arguments more carefully in the revised manuscript.

Minor comments:

-P2L22: In my knowledge, the turning band method is linked to the theory of random fields rather than to point processes.

To our knowledge, the turning band method has been introduced in its general form by Matheron (1973) and popularized for 2-D applications in hydrology by Mantoglou and Wilson (1982). So, it starts from 1-D point processes and was generalized to generate random fields.

-P2L34: "with respect to the outlined problem in the second paragraph above" - not clear what you are referring to.

We will improve this.

-Modeling vs modelling: you have to choose one spelling.

Thanks we will go with modeling in the revised manuscript.

-Eq 2: why q_n and not $q(t)$?

You are right, $q(t)$ would make more sense. We'll change this in the revised manuscript.

-P4L20: It would be more clear if you say that $P(x,t)$ is precipitation instead of rainfall. Or maybe call the variable R ?

That's also right, we will change it in the revised manuscript.

-Eq 3: Don't mix P and p.

This is actually correct but I admit that it is rather confusing. P represents a field, while p is a single value at a specific location within that field. However, the equations will be improved in the revised manuscript.

-Eq 4: Don't mix W and w.

Same as for P and p.

-Eq 6: You should mention that conditioning is made at this step.

Yes you are right we should point this out more clearly in the revised manuscript.

-Figure 3, 5, 7, 9: Please add units to the X and Y axes as well as to the color bar. You could also add the limits of the watershed.

You are right. We will improve the figures.

-Real case study: you should show the observation dataset.

We will add the figure shown here in the authors final comments.

author's changes in manuscript

Some hints regarding author's changes in manuscript have been already given in the comments section. Here, a summary of author's changes in manuscript based on comments of both referees is given.

- chapter 1 "Motivation": adding and discussion of literature regarding inverse hy-

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drologic modeling, improved reasoning

- chapter 2.2 "Rainfall runoff model": description will be improved
- chapter 2.3 "Random Mixing for inverse hydrologic modeling": description will be improved
- chapter 3.2 "Results and discussion": discussion of the synthetic example will be enhanced and performed more precise.
- chapter 4.1 "Arid catchment test site": figure of rain gauge measurements and additional information will be added
- chapter 4.2 "Results and discussion" discussion of the real case study will be enhanced and performed more precise
- chapter 5 "Summary and conclusion" reasoning will be enhanced and performed more precise
- revision of Figures 3,5,7,9
- all typos will be fixed and grammar will be improved

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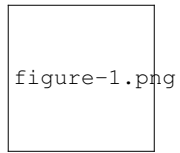


Fig. 1. Rainfall amounts and altitudes of rainfall gauging stations for the case study area (or see supplement)