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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Spatial sensitivity analysis of snow cover data in a distributed rainfall–runoff model" *by* T. Berezowski et al.

**T. Berezowski et al.**

t.berezowski@levis.sggw.pl

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Response to review

Anonymous Referee #1

General comment: The paper contains numerous abbreviations and is therefore more difficult to read.

Response: The abbreviations are now replaced with full names except SCF – snow cover fraction – which is used unabbreviated in many papers.

Comment 1: Figure 3 shows as well numerous lakes, which are present in the basin. In the paper there is no information how these lakes influence the hydrology in the basin.



Are the lake levels and discharges regulated in reality and how is this modelled using WetSpa.

Response to 1: The lakes in WetSpa are modelled by incorporating appropriate values of hydraulic parameters. We now detail that in the section "2.2 Study area":

Several lakes in the northern part of the catchment are controlled by management schemes, which usually discharge into Biebrza tributaries after accumulation period. Lakes in WetSpa are modelled by setting appropriate values of the hydraulic parameters in the model e.g. by a high runoff coefficient and a low friction. The simulation of water management schemes in the controlled lakes is, however, not implemented.

Comment 2: Snow accumulation was not calculated, but replaced with the input SCF. SCF is just a ratio of the area, but it's for me unclear what the magnitude is of ksnow and krain.

Response to 2: The values of ksnow and krain are obtained during the calibration of the model. We now provide the values of ksnow and krain in the results section 3.1:

The snow related global WetSpa parameters were estimated during the calibration as: ksnow = 5.03 mm *C-1, krain = 0.02 mm mm-1 *C-1.

Comment 3: In Fig. 6 the comparison of measured and calculated discharge is shown. It looks that the calculated discharge is underestimated. Perhaps discuss this and give reason: accuracy of Gauging station, too high actual evpotranspiration, or other ..

Response to 3: The reason is most likely indeed the accuracy of the gauging stations during the highest peaks, as during these events water flows on a densely vegetated floodplain, where a correct measurement of water velocity is very difficult. We now discuss this issue in the paper:

The peak discharges underestimation are possibly determined by the uncertainty of the rating curve. During the yearly spring floods, the measurement profile near the gauging station widens outside the riverbed and extends into the densely vegetated floodplain, where proper hydraulic measurements are very difficult.

Comment 4: Fig. 7 should be described in the text a bit more, what are the highlights and why do I see a pattern for the mean simulated snowmelt.

Response to 4: In the introductory paragraph to the section 3.2 Figure 7 is only generally discussed, much broader discussion is provided in sections 3.2.2, 3.2.3 and 3.2.4. Reviewer #2 also commented on this issue, which lead us to separate section 3 into two sections. This results in a discussion on Figure 7 (Figure 8 in the new manuscript), which is much easier to follow for a reader.

Comment 5: Figure 9 should be described in the section on the study area.

Response to 5: Figure 9 is now moved into the "Study area" section with appropriate explanation:

The dominating landscape features, that certainly have influence on the functioning of the Biebrza hydrological system are the river valley and the large forest complex located in the north-eastern part of the catchment (Fig. 5). âĂČ

Anonymous Referee #2

Comment 1: The introduction refers to some interesting publications, however the content is not always logically structured which hampers the readability of the introduction.

Response to 1: We have edited introduction, in order to improve the readability.

Comment 1a: A better differentiation should be made between previous methods applied to quantify the spatial sensitivity of parameters in hydrological models and the spatial data that could be applied for spatial sensitivity analysis (rainfall, Land surface temperature, impervious surfaces, et...).

Response to 1a: We gave a better structure to the first four paragraphs of the introduction, so the evolution of the spatial sensitivity aspects from very general studies, into more complex approaches is now clearly visible. There is now a clear differentiation



between aspects of various data used and spatial sensitivity in general. For changes, see the reviewed manuscript.

Comment 1b: Regarding the spatial sensitivity analysis methods the advantage of the presented LH-OAT compared to the methods used in the referenced studies should be addressed more clearly.

Response to 1b. We tried to address these issues already in the first version of the manuscript in p.119889, l.13-17. In the new version we emphasise how the proposed method extends the former studies:

In this study, the various approaches of spatial sensitivity (or uncertainty) analysis presented above are compiled and extended in order to propose a method that would be generally applicable and thus would give a framework for inter-comparison of different models. Such a method would use a regular grid to quantify the spatial pattern of sensitivity as in Stisen et al. (2011), hence it differs from the irregular zonation in Younger et al. (2009). Furthermore, the perturbation of spatial input data in a general framework should be realized using a well-established algorithm, e.g. Latin-Hypercube One-factor-At-a-Time (LH-OAT) (van Griensven et al., 2006), instead of predefined factors (Younger et al., 2009). This change would give a straightforward interpretation of the sensitivity. Similarly, Hostache et al. (2010) used a well-established gradient method for spatial sensitivity analysis. However, unlike the gradient method, LH-OAT provides global insight into sensitivity. Such a method would also allow to quantify the sensitivity of spatial data with respect to the output and be able to explain the causes for the sensitivity patterns.

Comment 1c: The authors should more clearly state for which purposes the spatial sensitivity analysis can/should be applied. Is the method for example suitable to locate areas which are, form a hydrological point of view, most or least suitable for deforestation, urbanization,..? This topic is now briefly addressed in section 3.3 but should also be addressed in the introduction.

Response to 1c: We have slightly modified section 4.4 (see the reviewed manuscript) and signalled the purpose also in the introduction:

Main purpose of the application of spatial sensitivity analysis proposed in this study would be, after the Saltelli (2002) definition of sensitivity analysis, to quantify spatially the vulnerability of the model output to uncertainty of spatial input. Thus a result of this analysis would provide feedback e.g. where in a model domain a modeller should focus more on the quality of input data and parameters. However, the same method can be used for comprehensive spatial change (e.g. land-use change) analyses to show where the change (e.g. urbanization) would be least or most influencing the model output.

Comment 1d: The sentence: ".. i.e.: is the uncertainty in different zones of the model dependent on the spatial patterns in the SCF" is unclear to me.

Response to 1d: The sentence is now edited:

Purpose of this analysis is to show if the WetSpa model is spatially sensitive to SCF i.e.: identify zones where the model output is most vulnerable to input uncertainty.

Comment 2a: The LH-OAT method is explained in section 2.4.1 while the spatial approach of the sensitivity is explained in section 2.4.3. I find this division somewhat confusing. It would be interesting to know what the ei, fi, j values used in this paper are while reading 2.4.1. The p number of parameters in section 2.4.1 is for the spatial sensitivity analysis the number of snow zones? The authors might consider combining 2.4.1 and 2 .4.3.

Response to 2a: The section 2.4.1 and 2.4.3 are now combined into "2.4.1 Spatial sensitivity analysis with Latin-Hypercube One-factor-At-a-Time algorithm". This combined section gives now a clear overview and presentation of the parameters in the spatial approach.

Comment 2b: Do I understand correct that for each LH sampling the SCF is calculated



by multiplying the SCF derived from the MODIS SCF by the ei? This would mean that for the sensitivity analysis the MODIS SCF is used only for the temporal dynamics? The SCF magnitude is sampled random between 0 and 1? If this is true this should be explained better in the data section about the MODIS SCF and/or around equation 1.

Response to 2b: Yes, SCF is calculated by multiplying the SCF derived from the MODIS SCF by the ei. Indeed, the MODIS SCF is used only for the temporal dynamics. We now clarify this in the section "2.3 Data":

The in snow zones aggregated MOD10A1 SCF data was used to calibrate the WetSpa model. For the spatial sensitivity analysis, however, the daily time series of catchment averages of MOD10A1 SCF's were used, i.e.: the spatial pattern of SCF in snow zones was obtained by perturbing the catchment averages by random factors (Sect. 2.4.1).

In the section 2.4.1 we also address these issues:

Since ei are randomly sampled the MOD10A1 data constrains only the temporal dynamic of SCF. Hence, results of the sensitivity analysis are interpretable in terms of SCF as input data in general, rather than in terms of MOD10A1 in particular.

In the first version of the manuscript the questions concerning SCF magnitude sampling were already addressed in p.11998 l.22-24.

Comment 2c: Figure 5 contains some interesting information regarding the methodology however some aspects remain unclear to me:

2c i. In the caption, check the references to left, right, left column, central column, right column? Is this correct?

Response to 2c i: We have now corrected the referencing in the caption.

2c ii. Do the j snow zones refer to the 524 snow zones in the catchment? This is not clear

Response to 2c ii: There was a typo in Figure 5, subscripts i and j were switched. The typo is now corrected. We also add clarification in the Figure 5 (now Figure 6) caption:

[. . .]ei and ei+1 represent a fraction of the SCF in the snow zones i and i+1, [. . .].

2c iii. Do the 3 first rows refer to the simulations for the 524+1 for the first LH sample? Does the last row in the figure shows the first calculation of the second LH which also contains 525 calculations? This is not clear

Response to 2c iii: As in the previous point the typo was corrected. Also we add information in the section 2.4.1. We change p.11997 l.4 from "[. . .] j is the current LH sample [. . .] to:

"[. . .] j is the current LH sample ranging between 1 and n [. . .]

2c iv. From section 2.4.3 I understood the perturbation factor fi was 1%, in the third row, the figure in the second row shows a perturbation factor of -1%? Why is this factor negative in this case?

Response to 2c iv: According to the LH-OAT paper (van Griensven et al., 2006) fi has a predefined magnitude (1% in our case) but its sign is random at each loop as the value can increase or decrease. We explain this in section 2.4.1 now:

[. . .] fi is the fraction by which ei was changed during the OAT perturbation, the sign of fi is random at each loop as the value can increase or decrease. [. . .]

Comment 3a: Separating this section in a results part and discussion part could improve the readability of this section. Consider this option.

Response to 3a: The separation in two sections indeed makes the manuscript more clear, as the evaluation of the proposed method is now separated from the discussion about the functioning our study catchment. Thus we decided to apply this suggestion into the manuscript.

Comment 3b: I understand you want to focus the sensitivity analysis on the spatial aspect but I would find it interesting to add a t-Q graph with the bounds from the SCF



sensitivity analysis.

Response to 3b: Such a hydrograph could be interesting for readers if one wants to see the overall effect on discharge at the catchment outlet only. We, however, did not indicate the necessity of such a plot throughout the methods section, as the main aspect of this work is the spatial sensitivity. In our opinion, such a plot would distract a reader from the main idea of this research. In fact the multitude of response functions presented in Figure 8 (former Figure 7) tells in details and quantifies with numbers the discharge response, while a hydrograph with sensitivity analysis bounds would give just a general view and thus is less meaningful. As we understand this comment as a suggestion, we left the plot out of the manuscript.

Comment 3c: Fig 6. the dates in the x-ax is not clear. Does the series start at the first of November, what is the time between the stripes?

Response to 3c: Series starts at the first of November, this is an arbitrary beginning of the hydrological cycle in the study area (start of snow accumulation), see p.11998 l.25-27. Markers indicates beginning of a new month, however, the discharge is simulated daily. We changed the Figure 6 (now Figure 7) caption:

Observed and simulated daily discharge from the calibrated WetSpa for the period in which the sensitivity analysis was conducted (upper panel). Also presented is WetSpa simulated groundwater and interflow discharge as well as only groundwater discharge. Catchment average daily temperature and SCF in the same period is presented in the lower panel. The ticks on the time axis indicate the 1st day of a month.

Comment 3d: Discuss more in detail the maps representing the model output sensitivity to SCF presented in Figure 7. e.g. What does a high or low si* value indicate? Why is the legend different for the figures in the last row?..

Response to 3d: The Figure 7 details are provided in the sections 3.2.2, 3.2.3 and 3.2.4 and in the discussion section, the initial paragraph of section 3.2 in the former manuscript gave just the introduction to the discussion. However, we now address the mentioned issues in section 2.4.1:

si* should be interpreted as a response measure of the changes in SCF in the snow zones to the value of F (.), a higher sensitivity stands for a stronger response and means that the model output is more vulnerable to uncertainty in a particular snow zone.

And in section 3.2:

The minimum, maximum and mean values are indicated on each map (Fig. 8). If the minimum is equal to 0, the model is completely insensitive in at least one snow zone for this response function. The values presented in the first four rows can be compared within a row, however, comparison between the rows is more difficult as in different rows the response functions concern discharge components of different magnitude. Note that the grey scale is different for all maps in the lowest row. This is because, unlike in the upper rows, the si* calculated from these response functions are not intended to be compared within this row.

Comment 3e: Section 3.3 would fit better in a separate discussion section. Additionally, a discussion about the number of simulations required for a spatial sensitivity would be interesting. The number of simulations applied in this study (52500) is difficult to achieve for some models. How could the number of simulation be reduced: e.g. less LH samples, less zones, which would be the better option?...

Response to 3e: Section 3.3 is now in the discussion section. We now provide our opinion about the number of simulations in new section "4.3 Computational constraints":

The total computation time, a product of simulation time and number of required runs, is a limitation of the applicability of this method and is similar as in all methods requiring a large number of model runs to achieve the desired output. This was also the case in this study, as WetSpa required about 1 minute for a single run, the total time for



52500 simulations was about 36.5 days. The advantage of any random sampling based sensitivity analysis method (including LH-OAT) is that it is easily parallelized, i.e. the LH-OAT samples are obtained before the simulations and the model runs are divided over a number of processors or computers.

One could, however, consider decreasing the number of zones (n) in which the input data is perturbed or the number LH samples (p) to receive the results faster. The latter implies that the LHOAT method may not converge (Nossent and Bauwens, 2012). Thus, it seems more reasonable to decrease the number of zones and be satisfied with results at lower spatial resolution.

Technical corrections:

All technical corrections were applied.

References:

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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C6185/2015/hessd-11-C6185-2015-supplement.pdf

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