Hydrology and Earth System Sciences Discussions

Interactive comment on "Geostatistical investigation into the temporal evolution of spatial structure in a shallow water table" *by* S. W. Lyon et al.

Anonymous Referee #2

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GENERAL COMMENTS

In the manuscript a relatively simple procedure is described to investigate the temporal-spatial groundwater table behaviour at short (15 min.) and long time intervals (month). In the geo-statistical investigation both hard and soft data are used and combined and the objective is to evaluate the dynamic behaviour of the saturation overland flow sources areas as triggered by single rain storm events and seasonal effects.

In the manuscript the theory underlying the saturation overland flow mechanism can be described much better and many terms that are used that are not defined or described. It makes the hydrology described suggestive and sometimes speculative. Terms like "interflow", "exfiltration", "event based processes that drive runoff", "old water", "new

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water", "saturation excess overland flow" are used throughout although the exact of role of groundwater flow in runoff generation by the saturation overland flow mechanism is only described poorly. Clearly such understanding should be at the core of any analyses and understanding the role and effect of topography in representing spatial patterns and spatial dependences of groundwater head distributions at saturation overland flow runoff source areas must be described too. In the manuscript terms like "non-physical water table behaviour", "non-linear variability", "physical hydrology", "non-linear increasing runoff"; are terms used but these terms have no realistic meaning unless clearly defined. Text therefore becomes suggestive. Also in the manuscript the term groundwater structure is repeatedly used but, to the opinion of the referee, such is inappropriate. Groundwater is commonly observed through piezometric head distributions that trigger groundwater flow and result in groundwater flow patterns and water table distributions. A structure suggests a fixed property while groundwater is a dynamic by nature.

Analyses in the study are based on relatively short time period and results carry many aspects of arbitrariness. Assuming that a soil is saturated in case the water table is 5 cm below the land surface is strong assumption and must be exercised with care. Results in Figure 4c for instance claim that over 65% of the hill slope area is saturated and such clearly is not realistic given the atmospheric forcing. Also the available data set should statistically be evaluated for consistency and anomalies and outliers in the runoff hydrographs (Fig. 3b) are not questioned.

The procedure to use hard data and soft data is appreciated although a clear argumentation why for instance not a spherical model (or any other model) in the experimental semi-variogram is used is not added. Also it is not clearly described what a semi-variogram tells us. Sometimes it is stated that it describes spatial patterns, sometimes the term structure is used, sometimes correlations over increasing distances (e.g. correlation length) are mentioned. A clear (geostatistical) understanding of a semi-variogram should be gained. It is unclear why Ordinary Kriging and Indicator

Kriging are used. It appears that Ordinary Kriging is used out of convenience since it is available in the Analyst extension in ESRI Arc Map. Kriging algorithms however are based on specific assumptions. For instance, Ordinary Kriging assumes that the phenomenon under study has a constant mean and also second order stationary is assumed. Whether such is true for the problem under study, however, is questionable. An argumentation why algorithms are selected must be added.

Some detailed comments Page 1684 Line 4: Little is known about Regarding 25 years of literature on the topic makes the statement unrealistic. Page 1684 Line 19: Hydrology does not change but the hydrologic behavior changes Page 1684 Line 8: Change spatial structure in spatial pattern. Page 1685 Line 7: The observation network in the study area cannot be called sparse. Page 1685 Line 18: Typo exfiltration. Page 1685 Line 25: Non-linear variability is undefined. Page 1686 Line 9: is still a topic. It is just a topic as many other topics Page 1688 Line 5: measurement techniques provide "new" information is clearly an overstatement since it is common practice to observed water tables by data loggers. Page 1687 Lines 13-16: Reconsider the applied definitions for the semi variogram parameters. For instance, the sill in the applied definition is undefined. Some definitions of the sill are that the sill value is the semi-variance as the lag (h) goes to infinity, or, the sill value is equal to the total variance of the data set. The nugget represents variations at a much smaller scale than any of the measured pair wise distances and represents unresolved, sub-grid scale variation or measurement error. Page 1687 Line 20: the comments why indicator semi-variograms are used to capture extreme values are irrelevant to this research. In stead a scientific sound argumentation must be added. Page 1689 Line 5: the event based processes could be identified. This is a claim but in the manuscript processes are not described nor identified. Page 1689 Line 13 : provides a manner to reinforce spatial observations. Reinforce is misused here. Page 1691 Line 25: was evaluated by dividing the 15-min data into monthly intervals. I think dividing should be combining. Page 1692 Line 8: The term indicator variables has been used now various times but it is still unclear which variables are used for what reason. Page 1692 Line 25: Why was a exponen-

tial semi-variogram model applied. The argument that many others use it as well is scientifically not sound. Please add proper argumentation. Page 1694 Line 19: just referring to a method of Goovaerts (1997) is inadequate but the approach should be elaborated on. Much information in Sections 4.1 and 4.2 is already explained and described. Page 1965 Line 18: According to Figure 3A high intensity storms occur more frequent. Page 1695 Line 20: The two largest runoff events occurred after .. According to Figure 3B this is not true. Page 1696 Line 5: The observed behavior in Figure 4A should be explained in the text; just describing what is observed is insufficient and has too limited added value to science. Page 1697 Line 17: R2 is not described and conclusions on values of R2 in Figure 6 are not explained. Page 1698 Line 23: June and August cannot be characterized as dry (see Figure 3A where significant rainfall events are observed with clear rainfall-piezometer responses). Page 1700 Line 8: It is discussed where saturation commonly occurs. This however is not shown in the study and also cannot be verified based on the manuscript.

In the Discussion and Conclusions sections much information is repeated from earlier Sections. Also it should be better discussed what can be observed / concluded from the results. The impression is left behind that too much is speculated on. Further detailed comments therefore are not presented in this review.

The authors are asked to revise the manuscript, to give clear descriptions of the role of groundwater in the saturation overland flow mechanism and then to draw conclusions based on simulation results. For instance, the fact that the semi-variogram range reduces when the median water table depth becomes smaller than 10 cm is an important observation but unfortunately not further elaborated on. Also the data set used for this study is relatively short (only 6 long term observations) but more important the set should statistically analyzed first and consistency between observation time series checked. The overall approach to apply hard and soft data that are combined in geo-statistical analyses is appreciated and carries a potential to successful applications.

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