

Interactive comment on “GIS and remote sensing techniques for the assessment of land use changes impact on flood hydrology: the case study of Yialias Basin in Cyprus” by D. D. Alexakis et al.

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General comments and recommendation

The main objective of this paper is to present the value of coupling GIS and remote sensing with hydrological models in order to investigate land use/cover changes and their impact on basin flood response. The study is focused over an 110km² basin in the central part of the island of Cyprus. Although, the overall idea constitutes an interesting scientific topic, I think that the manuscript in its current form has several limitations in

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terms of analysis, discussion and conclusions that need to be addressed before the manuscript can be considered for publication. Below I provide a list with major and minor comments that hopefully can help the authors to improve their manuscript.

Major comments

1) There are two main elements in this work. The first related with the remote sensing data and processing techniques to retrieve/construct past and future land use/cover (LULC) maps. The second element involves examination of the hydrologic impact of different LULC. In its present form the work described in the manuscript is mainly weighted towards the first element while the hydrologic analysis is very short and exhibits several points of weakness. According to the title and the objectives stated, it is the second element that requires most attention. In addition, my feeling is that several elements of the work on remote sensing techniques/processing have already been reported in Alexakis et al. 2012 which reinforces the fact that the main novel elements of this work should spring out of the hydrologic analysis.

2) Why you limit your analysis on 4 events (3 for calibration and only 1 for validation) when you have 20 yrs of rainfall data? If you feel good about the model setup you can run the model for few years and analyze the various flood events. Reporting the impact of land use change as a function of flood severity would be a very interesting element. Expanding the analysis would make the results far more interesting and more statistically robust.

3) Most of the pages devoted on hydrologic analysis are used for the description of model and comparative metrics used, while the presentation and discussion of results are very limited. Discussion of results needs to be expanded in the revised version. For example, there is not discussion on the hydrologic observations for the events examined. One feature that I noticed is that flood peak at Potamia (which is the outlet of the overall basin according to Fig.2) is consistently lower than in the other subbasins. Are we dealing with loosing streams?

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4)Conclusions are not supported (or are not convincingly shown) by results. For example, authors state “as it is indicated in Table 2, the Forest mixed. . .have significant possibility to change to urban. . .”. This is not shown for Table 2 (in fact probability is low) and it is also not supported by Fig7a. More importantly, authors state repeatedly that the increase in simulated peak flows for the LULC 2020 is attributed to urban growth. However, from Fig. 7a we can see that other classes undergo significant change from 2000 to 2020 (e.g. AGRL, OLIV, HERB) which in addition are associated with greater areas than urban. So how the authors have isolated effect of urban in simulated peaks? I think that this is only possible if they carry out simulations with LULC involving only changes in urban areas.

5)Please clarify the part discussing the construction of future 2020 LULC. My understanding from reading the text is that the trend derived between 2000-2010 changes, is applied on the 2010-2020 step. Is this correct? It is not very clear how the derived probabilities are translated in spatial output (i.e. how the changes are mapped in space over the basin). The latter may have a significant impact on the hydrologic simulations.

6)I think it is important in the conclusions to discuss also the limitations of the approach, as for example the uncertainties associated with both CA-Marcov approach and hydrologic model used. The work provides a potential outcome of the future land use change on flood response but this cannot be treated as a certain deterministic finding.

7)Improve introduction and bibliographic references on the subject. Please consider the following papers and references therein: Ozturk et al. 2013 (Modeling the impact of land use change on the hydrology of a rural watershed). Roosmalen et al. 2009 (Impac of climate and land use change on the hydrology of a large-scale agricultural catchment) Fohrer et al. 2001 (Hydrologic response to land use changes on the catchment scale) Defries and Eshleman 2002 (Land-use change and hydrologic processes: a major focus for the future).

Minor comments

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1. Abstract line 5. You mention hydraulic models while in text only the hydrologic model HMS is discussed. Was the flow routing implemented by coupling a hydraulic model? Please revise.

2. Abstact line 6. You say “describe hydrological processes and internal basin dynamics”. Since the analysis is related only to hydrograph simulations at the basin’s outlet (without any analysis on processes involved) I would suggest to rephrase this sentence to better describe the work carried out.

3. Page 4834, line 24: correct “exposurevulnerability”

4. Methodology page 4838, line 6: Provide reference for CA-Marcov algorithm

5. Section on Soil map: Expand the section by including discussion/description of soil units and some basic characteristics.

6. Page 4843, line 4: The CA stands for what? Please define.

7. Page 4847 lines 14 and 21: “A list of rainfall runoff events. . .” this is not provided in Table 1 or 3. Please correct.

8. Page 4848, line 2: “model adequately describes”. The model validation with efficiency between 0.45-0.62 cannot be considered “adequate”. I would state that the model is able to reproduce quite well the timing and magnitude of peak flows but significantly underestimates total runoff volume, which in turn translates to low efficiency score.

9. Conclusions lines 10-12: “In addition, the implementation of CA-Marcov model gave the opportunity to predict the catchment area’s flood vulnerability in the near future”. I would feel more comfortable to state that the method used “provided indication of the potential impact of land use change on flood vulnerability in the area”. The truth is that there are a lot of uncertainties associated with both the LULC prediction provided by CA-Marcov approach and the hydrologic model used. Thus results can only be an indication or a sensitivity exercise.

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10. Consider merging Fig1 and Fig2

11. In Fig 2, some stream lines southern from the Nisou and Potamia outlet, look unnatural as they are disconnected from the main river system. Please check and revise accordingly.

12. Fig. 3. I think that flow chart should involve calibration and validation in sequence rather than in parallel (since calibration is performed first and after this comes the validation).

13. Fi7a. Improve y-axis label. Fig7b, Not sure how much (and how easily) info can retrieved from this plot. Consider to change this by pairing Fig7a with a graph showing differences in relative sense (%).

14. Fig.10. Consider using hot/cold colors for positive/negative differences to assist interpretation of the graphs.

15. Table 3&4. Consider including the basin area beside the name of the outlets. I think the area of subbasins is not mentioned in text. Please check.

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