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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.

D. D. Alexakis et al.

dimitrios.alexakis@cut.ac.cy

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REPLY TO THE REVIEWER 2

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 a 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 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.

- We thank the reviewer for the very useful and constructive suggestions on our submitted manuscript. As suggested, we revised our manuscript. Your concerns were carefully taken into consideration for the manuscript revision, leading to a more consistent and strengthened manuscript. We are providing clarifications to the comments below, point by point. Moreover, a number of small scale additions were made in the manuscript (please find the revised manuscript in the supplementary file).

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.

1. Concerning the remote sensing techniques/processing we believe that the implementation of Markov model for predicting LULC is an innovative contribution of this certain manuscript (not presented in Alexakis et al 2012a, b) providing hydrological analysis with new data and perspective. Moreover, authors feel the imbalance in manuscript's content, thus we elaborated further with the hydrological mod-

eling/analysis according to your recommendations. The following paragraphs were added at the following sections.

In section 6.1 Model setup: “Gialias basin was modeled using three subbasins following the available flow gauge locations within the basin. The outlets of the subbasins were set at Kotsiatis (75.15km2), Nisou (21.71km2) and Potamia (16.29Km2)”

A new section was added to describe the hydrological regime of the area, the selected data:

6.2 Data

Four precipitation events were selected for the calibration – validation of HEC – HMS hydrological model. The calibration events were the most intense that could be found in the recorded data. Three events were selected, to calibrate the model for the flood of 2003, which served as validation event.

The hydrological characteristics of each event is presented in Table 3. The three events (Events of 2000, 2001 and 2004) served for the calibration of the hydrological model. The calibrated model was then evaluated for its performance on the fourth event of 2003 that was a major flood event of the basin. The total precipitation heights (as estimated by the areal interpolation of the available rain gauge data for the entire period of the rainfall event is given in Table 3, along with the total duration of the event. It can be observed that the flood event of 2003 had the greatest precipitation height comparing to the calibration events. To identify the driving forces of the flood event, the return period of each maximum hourly rainfall rate was estimated for each rainfall station and event (Table 3). It can be observed that the flood event distinguishes from the rest of the rainfall – runoff events maily due to the relative high return period that it was occurred simultaneously in two stations (Leukara and Analiontas), comparing to the rest of the events.

Moreover, Table 3 was supplemented with information regarding the analyzed events'

return period.

Table 3: Selected rainfall – runoff events to calibrate/validate HEC-HMS hydrological model. Start date Finish date Total event P [mm] Return Period Lithrodontas Mantra tou Kampiou Leukara Analiontas Kionia Calibration Event 1 07 Dec 2000 18 Dec 2000 102.5 0.96 3.03 1.04 1.28 3.74 Event 2 06 Dec 2001 11 Dec 2001 82.2 0.91 3.96 1.33 4.25 21.64 Event 3 09 Jan 2004 15 Jan 2004 122.3 3.50 4.80 12.48 0.89 Validation Event 4 10 Feb 2003 17 Feb 2003 157.8 1.15 0.72 1.16 1.01

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.

The flash flood event of 2003 (peak flow at upper basin ∼120 m3/s) that we analyzed is the most intense flow event that has been recorded in the available data. In order to calibrate the model adequately to simulate this event, we used the most “extreme” available events that were available to calibrate the model. The next most “extreme” events that also offered adequate data are these tree events that we selected (2000, 2001, and 2004). The peak discharge however at the Nisou (mid subbasin) was still of significantly lower peak discharge (∼35 m3/s, ∼25 m3/s ∼15 m3/s). The rest events were of even lower peak flow (under 10m3/s), thus were considered inadequate for the calibration of the model. Appropriate text was added at section 6.2 Data. Moreover a clarification remark was added in section 6.2.

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?

Following the major comment 1, we expanded the analysis of the hydrologic simulation part of the manuscript. Regarding the observations of the examined event, a paragraph is added to the manuscript and the discussion section:

The high flow events are mainly created from the intense precipitation that occurs at the upper parts of the basin, as it can be seen in the observed hydrographs of Kotsiatis. The relief of that part of the basin is characterized by steep slopes, which in addition to the medium permeability geology, potentially can create intense runoff. In the lower parts of the basin, the river crosses the alluvial deposits of the valley between Kotsiatis and Potamia. It can be observed that the peak discharges are retained from the Kotsiatis, to Nisou flow gauges, but interestingly it does not increase. This is mainly attributed to the gentler slopes that characterize the mid subbasin, leading to increased lag times, but also to the alluvial channel bottom that permit high transmission losses. This is obvious in the observed hydrographs (Fig 11).

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.

The discussion of the land use effect on the peak discharge is expanted in the discus-

sion section. The paragraphs bellow here added to the discussion:

“In this study the multi-temporal land use regime of Yialias watershed in Cyprus was thoroughly searched with the use of object oriented classification technique and application of CA- Markov model. The specific model appears to have certain advantages as well as specific disadvantages in its application. Initially, it does not require deep insight into the mechanisms of dynamic change, but it can help to indicate areas where such insight would be valuable and hence act as both a guide and stimulant to further research. On the other hand, Markov analysis ignores the forces and processes that produced the initial land use patterns and also it assumes that changes will continue to do so in the future by sometimes ignoring social, human and economic dynamics. However, in order to give a spatial dimension to the Markov model we applied Cellular Automata Markov model. Through the 2000 – 2010 decade’s analysis, results denote an increase in Agricultural Generic, Olive tree cultivation and herbaceous areas, putting stress into the Close Growth Agricultural Land which is the main decreasing land use category. The forested is shown to roughly occupy the same land portion. The same tendency seems to be for the next decade affecting the potential hydrological response of the basin. Specifically, the simultaneous increase of residential areas and the decrease of agricultural close grown cover throughout the basin is expected to enhance the potential devastating surface run off processes.”

“The changes indicate that between 2000 and 2010, 2020, the area weighted curve number for all the land use categories except the urban areas, retains a relatively constant value around 53. (From CN = 52.9 for year 2000, to 52.7 and 53.3 for 2010s and 2020s respectively). In contrast, the areal weighted CN for all the land use categories retain a more robust increasing trend from 53.8 to 55 and then to 56.2. It is shown here that the increase in the urban land use in 2010 (from 1.85% to 5% of area) outweighs the slight decrease in the CN in the rest of the basins’ land use classes. Accordingly, the 2020 projected land use shows that the CN is projected to increase from 55.0 in 2010 to 56.2. This increase by 1.2 units is both attributed to the change to non-urban land uses and the further urbanization of the basin (from 5% to 6.5%)."

"The changes in the simulated peak discharges can be explained by the overall increase in the curve number of the basin. In both 2010 and 2020 simulations. Moreover the pattern of the CN increase between the urban and non-urban land use classes can stand as a positive proof that the change in 2010 peak discharge is wholly attributed to the urban areas increase, while the 2020 further increase is merely attributed to urban areas increase but also in the trade off of non-urban land uses. It has to be noted that the above rationale explains in general the mechanism of the land use change effect on the peak runoff, but it does not account neither for the spatial distribution of the land use changes, nor the distribution of the precipitation."

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.

Dear reviewer, as you mention the trend derived between 2000-2010 changes, is applied on the 2010-2020 step. Concerning the conversion of the derived probabilities to spatial output this is excused a new sentence added to the text: "this is relevant to underlie dynamics of the change events based on proximity concept so that the regions closer to existing areas of the same class are more probable to change to a different class".

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.

The limitations of the approach (both in remote sensing and hydrological part) are

discussed in the "Discussion –Results" chapter.

7) Improve introduction and bibliographic references on the subject. All the suggested references were added in the text.

Minor Comments

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. Ans: The flow was routed only using the routing procedures of HEC HMS hydrological model, there was not any hydraulic model coupling. As suggested, the word "hydraulic" was deleted.

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. Ans: It has been rephrased.

3. Page 4834, line 24: correct "exposurevulnerability" Ans: The phrase was corrected.

4. Methodology page 4838, line 6: Provide reference for CA-Marcov algorithm Ans: CA was changed to "Cellular–Automata"

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

Ans: The soil classes are now described in detail within the text: "Specifically, Vergennes is a very deep, moderately well drained soil of sandy loam composition concerning the specific area. Windsor consists of very deep, excessively drained soil which for the specific area is of coarse sandy loam composition. Covigton consists of very deep, poorly drained soil that is formed in calcareous glaciolacustrine and estuarine clays mainly found in the northeastern part of the basin."

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

Cellular Automata. It is corrected.

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

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. Ans: A better description was added to comment about the model efficiency according to your reccomentations.

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. Ans: We thank the reviewer for the comment. The phrase was changed with the suggested one.

10. Consider merging Fig1 and Fig2. Ans: As suggested, the two figures were merged

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. Ans: The stream lines were corrected in the new version of Figure 2.

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). Ans: The flowchart has been corrected as suggested by the reviewers.

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 (%). Ans: As suggested the figures were corrected.

14. Fig.10. Consider using hot/cold colors for positive/negative differences to assist interpretation of the graphs. Ans: The figure colormap was changed to the hot/cold scale. The changes are more recognizable now.

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. Ans: Each subbasin area was added in section 6.1 of the text.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C2116/2013/nhessd-1-C2116-2013-supplement.pdf

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