*Response to Anonymous Referee #3*

*We would like to thank referee #3 for his valuable comments on our manuscript. We used italics to mark our answers to his comments.*

General

The paper tackles a topical issue related to the impact of land use changes on hydrological systems. This is an important area of research as many parts of the world are going through changes in land use. The drivers for land use changes are many and their impacts are more or less site specific hence the need for a wide range of research to better understands these processes.

The paper is generally well written and is well backed by references. However, the methods used and the scientific arguments presented leave questions on the part of the reader and, although the authors attempt to justify some of these shortcomings I am inclined to believe that with better methods, those weaknesses could have been avoided.

I would also have expected a discussion on the effect of climate change in the whole process as the paper is very silent on this yet climate change and/or variability can also affect the runoff from the studied catchment.

*Answer: The model was run with two different land use classifications, but with the same weather input. The results were compared using average values of water balance components for the 20-year model runs. Thus, effects of climate variability do not affect our analysis, which is focused on differences of the average water balance components in the two model runs. We will underline this fact in the revised version of the manuscript.*

*Insights on the effects of climate change on the water resources in this study area are presented in a companion paper, which is currently under review for Regional Environmental Change.*

Specific comments

1. Section 2.2 describes three different methods used for classification based on satellite data. One wonders why three different approaches had to be used it instead of one. Furthermore, it is not explained if the satellite pictures were taken during the same period or at different times of the year and what implications this would have on the final analysis. This seems to have been addressed further on on page 1951 (last paragraph) when it should have been presented earlier.

*Two different approaches were used for the past and the current land use classifications. This is necessary, as mapped ground truth data was not available for the past (see p.1949, l.1*: "Due to the absence of mapped ground truth data for the past, *two* different methodologies were applied for the current and the two historic land use classifications, *respectively*." *We will add "two" and "respectively" to make this statement clearer in the revised version. The dates of the satellite images are given in tables 1 and 3. Our methodology aims at providing a reasonable representation of the cropping year. Thus, we used one satellite image for each of the two cropping seasons (2009/2010 classification). For the image that corresponds to the Kharif crop, which is grown in rainy season, the first available cloud-free image after rainy season was used. In case of the year 2009, this is a late November image, which is obviously not the optimal case as some crops have been harvested since the ground truth was mapped. However, an earlier image was not available.*

*Our accuracy assessment indicates an agreeable accuracy, which is further improved by not relying on the single November classification but by using a combination of two images.*

*For the past land use classifications mapped ground truth data was not available. To reduce uncertainty in our land use assessment that may result from missing mapped ground truth data, we used an additional, third image. Based on the availability of cloud free images, the images were taken at a similar time of the year (a one month lag seems acceptable, when relying on optical remotely sensed data). This reasoning for the applied methodology will be included in the revised manuscript.*

2. Similarly, on page 1950 (line8) classification of water bodies was conducted in November yet it is not clear which month was used for the other land use analyses. Line 23 indicates that in November fields would have been harvested which means that this is not the best time to classify vegetation in general.

*The water areas were always taken from the first image after rainy season corresponding to the maximum storage of the reservoirs. The information for the past land use classifications is given on p.1951, l.27:* "Water areas were taken from the October classifications, which correspond to the maximum water level in the reservoirs." *The November image was chosen since it was the first available cloud-free image after rainy season (see p.1949 l.11-12 and l.5-7). Please refer also to the more detailed answer to this topic in the previous comment.*

3. Page 1952 (i) assumes a linear growth of cropping. While this may be fine for the purposes of research, the authors should acknowledge that the selection of crops and possibly cropping areas is largely a farmer's choice and is generally driven by economics.

*While the selection of crops is a farmer's choice, the statistics indicate a linear increase of sugarcane and rice percentages of total agricultural area in Pune district. This increase can be approximated by a linear regression line. We will include this statement in the revised manuscript.*

4. Section 2.3: only one weather station at Pune was used for weather input parameters. What are the implications given that the catchment area is 2036 km2 with high variability yet Pune is on one end of the catchment? For the rainfall stations, an idea of the spread of the rainfall stations and its representativeness would be appreciated.

*The temperature and humidity data in Pune was adjusted to the spatial variability in the catchment as explained in:* "To account for temperature differences in the catchment, temperature values were adjusted using adiabatic temperature gradients. The spatially distributed temperature records and the specific humidity measured at the weather station in Pune were employed to calculate spatially distributed relative humidity (Wagner et al., 2011)." *Only solar radiation and wind speed data were taken directly from the data measured in Pune. In a situation of limited data availability this is reasonable and comprehensible approximation. As the interpolation of rainfall data is discussed in detail in a previous study, we would like to refer the reader to the paper by Wagner et al. (2012).*

5. Section 2.3: it would be appreciated if the authors described the degree of missing data and the process of data filling that was applied.

*The requested information has been published in detail in previous studies (Wagner et al. 2011; 2012). The focus of this study is on model application and the information on data preparation was therefore not included in detail in this paper. We will stress the references to* *previous research in the revised manuscript. Please refer to the mentioned studies for further information on data preparation.*

6. Page 1954, second paragraph: the input parameters were not based on observed data; values from literature and/or default values may allow the model to run but may not mimic reality as accurately. A sensitivity analysis of the default values used in the model should also be presented. Why did the authors choose not to include a figure to support the presented good performance of the model?

*Model performance was evaluated in previous studies. The detailed validation of the model with a presentation of hydrographs is given in previously published papers (Wagner et al., 2011; 2012). As the focus of this study is model application, we did not include a repetition of visual model validation. However, the key model efficiency indicators are provided, which indicate good model performance for the applied parameterization.*

7. Section 3.1: I do not fully agree with the interpretation of the results; from Fig 3, about a third of the study area has gone greener which suggests more cropland, forest cover or shrub land. However, when compared against Fig 2 this does not seem to tally as cropland has only marginally increased (about 3%) while semi-natural has lost 10% to cropland and urbanisation. This may also suggest that urbanisation is exaggerated in Fig 3 and has minimum effect on sub basins 8 and 9 as presented on page 1956 line 21.

*This is obviously a misunderstanding, which we would like to clarify as follows:*

*Figure 3 shows the areas that have changed between 1989/1990 and 2009/2010. Thus it indicates not "more" semi-natural area, but a change between classes. As can be deduced from Table 5 the mentioned greener, semi-natural land use areas (grassland, shrubland, forest) in Figure 3 were mainly semi-natural land use areas in 1989/90. See p. 1956, l.27 ff:* "The analysis of the changes by land use class between 1989/1990 and 2009/2010 shows that the land use changes of semi-natural classes are mainly intra-class changes (Table 5). The main percentage of the area that was changed to forest, shrubland, or grassland in 2009/2010 was under semi-natural land use in 1989/1990 (forest: 92.1 %, shrubland: 84.3 %, grassland: 83.5 %; Table 5)." *Figure 2 shows the percentages of land use classes for each year. Cropland indeed has increased in some areas (derivable from Figure 3), but decreased in others (not derivable from Figure 3). The net change for each class, as derivable from Figure 2, is also shown in table 5 (iii). An overestimation of urbanization is therefore not the case in Figure 3. Sub-basins 8 and 9 are obviously not affected by a pronounced urbanization. Please find the net change of cropland and urban area per sub-basin in Figures 4 and 5.*

8. Page 1956 last paragraph: why discuss about variations between classes when there is too much overlap to deduce anything meaningful?

*Due to the previous misunderstanding, this paragraph seems necessary to underline that i) the large percentage of green areas in Figure 3 is mainly due to changes in-between the semi-natural classes, and ii) the two major changes are the increase of urban area and cropland, which are hence analyzed in detail in the following section.*

9. Section 3.2: I think there is need to revisit the arguments presented by the authors. I argue that two-thirds or so of the study area has gone greener which should suggest more evaporation and transpiration in these parts and not necessarily sub-basins 2, 3 and 14 as

presented. Besides, urbanization has increased significantly at the lower end of the catchment only. How do the authors view and reconcile this? In addition what are the influences of temperature and moisture availability on this? How is yield defined in this study? Are the authors referring to reliable outflow from the catchment per given time or this is simply measured outflow from the catchment? The linear regression analyses presented are very weak and possibly emanate from the loose assumptions made and default model input parameters as presented under the methods section. Fig 6 and 7 do not present strong relationships as evidenced by the weak R2 values hence firm conclusions cannot be drawn from this. It would seem the authors forced straight line relationships where they are not necessarily evident.

*As discussed in our reply to comment 7, the green color should not be mistaken as an increase in vegetated area. It is instead a land use change within semi-natural classes. The model results showing large increases of evapotranspiration in sub-basins 2, 3 and 14 are therefore reasonable. Urbanization has increased in the eastern part of the catchment. As shown in Figure 4 and 5 this has a pronounced impact on the spatial pattern of evapotranspiration and water yield.*

*The impact of urbanization on temperature and moisture availability is not researched in this study, but it is inherent in the used measurements in Pune.*

*Water yield refers to the parameter WYLD in SWAT and the terminology is adopted from the terms used in SWAT. It is the net amount of water that is provided by the sub-basin and contributes to stream flow. It does not include the amount of water that enters the sub-basin from upstream basins. In our assessment we use the long-term 20 year average water yield for each sub-basin for comparison of the different land use setups. We will add the definition of water yield to the revised manuscript.*

*It is obviously true that the change in evapotranspiration or in water yield cannot be explained by the change in cropland and urban land alone. However, while figures 4 and 5 show the change in ET and water yield in the spatial domain, figures 6 and 7 are used to explain these patterns. Our regression analysis shows that between 46% and 64% of the variation of the change in ET and water yield can be explained by the change in cropland and urban land. Obviously, the change in ET or in water yield cannot be explained by the change in cropland and urban land alone. But the significant correlations presented in figures 6 and 7 indicate that the changes in ET and water yield can mainly be attributed to the major land use changes (i.e., to a change in cropland and urban area).*

10. I find the conclusion rather generalised and sometimes mixed with recommendations.

*The last paragraph of the conclusions holds more general conclusions as we have been encouraged by the editor to go beyond the local focus. We will restructure the conclusions for the revised manuscript, as follows: 1.General conclusions, 2. Main land use changes found and their implications for the future, 3. Main impacts on the water balance found and their implications on water users, 4. Outlook.*