Hydrol. Earth Syst. Sci. Discuss., 12, C292–C297, 2015
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Hydrology and Earth System Sciences
Discussions

Interactive comment on "High-quality observation of surface imperviousness for urban runoff modelling using UAV imagery" *by* P. Tokarczyk et al.

Anonymous Referee #2

Received and published: 25 February 2015

The paper explores the potential of UAV imagery for urban run-off modelling. The paper focuses on two main issues: 1) remote sensing classification of surface imperviousness by the use of two different classification algorithms; 2) integration of the classified surface imperviousness maps into an urban drainage model to simulate a set of relevant hydraulic variables, such as peak flow, surface run-off. Results (classification performances and hydraulic model outputs) derived from the use of UAV imagery are compared with analogous estimates obtained from reference aerial image. The topic is interesting and suited for the journal; however I believe that the work proposed needs strong improvements, results and conclusions are strongly biased by

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some weaknesses of the method. Even if the authors clearly recognize the limitations of the approach, I think there are wide margins of improvements that could significantly strengthen the key message of the manuscript, the potential of UAV imagery for urban modelling.

I summarize some major issues in the following lines:

1) Surface imperviousness plays a key role in urban run-off processes. However from your modelling we get an opposite view. Different imperviousness maps (with quite large variations in terms of overall accuracy) when used as input into the urban drainage model result in negligible variations in the hydraulic output variables. The authors explain such results as a consequence of the auto-calibration procedure and spatial aggregation effects. I believe that most of the conclusions are based on effects of the drainage model, whose performances and limitations are not well explored. I believe that authors should assess the sensitivity of the model with respect to the spatial scale (different grid spatial resolution) and to degree of surface imperviousness (different percentage of surface imperviousness). Results of the sensitivity analysis will corroborate (or not) their conclusions and interpretations that at the moment seem to me mainly speculations. 2) The exploratory analysis performed in section 2.3.2. does not provide any relevant information. Results (3.2.) are not statistically significant and interpretations are mainly speculations. The exploratory analysis is wrongly designed and the authors should change methodological approach to properly assess the importance of input data for surface run-off. I suggest using the 36 independent rain events to test (ks test for instance) the variations between surface run-off produced by the different input data: surface runoff obtained using UAV-derived imperviousness vs. surface runoff obtained using ortho-derived imperviousness. 3) The state-of-the-art is very poorly reported. The authors should significantly improve this part - that cannot be left on supplementary material – and stress the novelty of their contribution. 4) Pre-processing steps of orthorectification of UAV imagery are crucial but there is no information about the procedures the authors have performed. The authors should pro-

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vide more details on these aspects (e.g., rolling problems). 5) There is no information on what are the features used to classify the two images. Spectral features? Texture features? This part needs strong improvement. 6) Authors claim very often along the manuscript that the use of UAV imagery is much cheaper compared to other remote sensing tools. Authors should clearly reported differences in costs of UAV with respect to standard high resolution imagery. 7) The contents of the work are not well presented. a. There are too many sections and nested sub-sections, even when unnecessary. A simple example: in section 2.2.2 you do not need to distinguish image data and height model headings. Similar cases are in many other parts of the manuscript. b. There is an excessive use of bullet points. c. There are too many scientific questions reported in the manuscript. Scientific questions should be reported at the beginning of the manuscript only. d. There are too many links to supplementary material, whose content in some cases should be included in the main body, in others do not add relevant information. This is not a writing style issue, but is more related to how conveying the key messages in a proper way.

Minor comments: Page 1205. “high-quality” referred to UAV imagery in title and rest of the manuscript. I suggest changing to “high-resolution”, more appropriate in this context. You did not demonstrate that UAV imagery is a higher quality product.

Page 1206 Abstract: I believe should be written in a more concise way, especially in relations to the first 13 lines. Too many information are reported that are not really relevant here. Line 10 “detailed image data is unavailable”, not truth. Thanks to repeated and global VHR satellite acquisitions any part of the globe is finely mapped. Line 16. Please add classification methods. Line 21. Take out swisstopo, not relevant here. Line 21. Change “correctness” to “overall accuracy” and report values.

Page 1207 Line 7-9. You did not verify this in your work. Please, take it out. Line 12. This is even more relevant because flood risk is dramatically increasing in many parts of the globe due to the combined effects of socio-economic developments and population growth in floodplains, and increases in hydrological extremes induced by

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climate change. I suggest to include the following references: Hirabayashi, Y., Mahendran, R., Koirala, S., Konoshima, L., Yamazaki, D., Watanabe, S., Kim, H. and Kanae, S. (2013). Global flood risk under climate change. Nature Clim. Change, 3,816-821. Hall, J., Arheimer, B., Borga, M. et al. (2014). Understanding flood regime changes in Europe: a state-of-the-art assessment, Hydrol. Earth Syst. Sci., 18, 2735–2772. Rojas, R., Feyen, L. and Watkiss, P. (2013). Climate change and river floods in the European Union: Socio-economic consequences and the costs and benefits of adaptation. Global Environ. Change, 23, 1737-1751. Line 19. I suggest to include the following reference: Arrighi et al. (2013). Urban micro-scale flood risk estimation with parsimonious hydraulic modelling and census data. NHESS.

Page 1208 Lines 7-8. Do not refer to manual techniques, they are well established mode modern techniques in research. Lines 14-17. References to coarse sensors are not appropriate here, I would focus more on works related to VHR data. Line 21. I suggest to include the following references related to multi-sensor approaches: Forzieri et al., ISPRS Journal of Photogrammetry and Remote Sensing 74 (2012) 175–184; Forzieri et al., Computers Geosciences 49 (2012) 72–80. Line 21. Given the central role you are giving to the classification method proposed in your work you, should also include in the state of the art appropriate references on the algorithms used for classification of surface imperviousness, with special focus on contextual techniques (Moser et al. Proceedings of the IEEE 2013, 101 (3), 6304904, pp. 631-651).

Page 1209 Line 11. You should better describe the potential criticalities due to the finer spatial resolution (e.g., shadow effects)

Page 1210 Lines 2-3. Not relevant information Line 8. Do not refer to figure here, not necessary. Lines 9-15. Please, take it out this paragraph. This is material for conclusions.

Page 1211 Line 10. Change “with an” to “by”.

Page 1212 Line 3. Images. Line 24. Not relevant information.

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Page 1213. Lines 1-11. This paragraph needs to be better explained. Line 4. Dense image matching? Please clarify. Line 9. “DTM provided by the swisstopo”, then the same provided by swissALTI3D. Line 16. “readings started . . . today” not relevant. Line 18. Quality checks, too vague. Please, clarify.

Page 1214 Line 8. Compartments, change to computing modules. Please 26. Please take out the term “standard”, it is only one of the available tools.

Page 1215 There are too many details in Section 2.3. Please synthesize. Line 10. “de-facto a standard” change to a largely used. Line 18. Consider also that this decaying behavior, known occurs when the number of classifier parameters (which generally increases, often super-linearly, with the number of features) becomes so large that the fixed training set is insufficient to accurately estimate all parameters. Landgrebe, D., 2003. Signal Theory Methods in Multispectral Remote Sensing. John Wiley and Sons, Hoboken, New Jersey, USA. Line 19. Spectrally consistent? Please clarify.

Page 1216. Line 19, “in our view. . .” this very subjective. Please corroborate properly your methodological choices. Usually testing set are selected randomly over the area to avoid subjectivity (Lillesand, Kieferm Chipman Remote sensing and image interpretation, Wiley; Richards and Jia, Remote sensing digital image analysis, Springer).

Page 1221. I suggest to merge Results and Discussion Sections, now your messages are too fragmented. Line 22. Pre-processing ans post processing, please specify to what you are referring.

Page 1222 Line 1. Not feasible. Why? Clarify in the text.

Page 1228 Section 4.2.3. This is not material of your work, but mainly speculation. You could synthesize this in one sentence only.

Page 1231. Many references are from conferences and grey literature not very relevant. I suggest to find more robust references.

Figures 5 and 6. Please, add legend the figures will be more self-explicative.

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Figure 10. Please, add goodness of fit values in the panels.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 1205, 2015.