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Earth Surface Dynamics Discussions

Interactive comment on "Computing water flow through complex landscapes, part 1: Incorporating depressions in flow routing using FlowFill" *by* Kerry L. Callaghan and Andrew D. Wickert

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Firstly I would like to thank the authors for this contribution, in my opinion this is an issue which has urgently required attention for some time and so I am pleased to see it is being worked on. This manuscript presents a new method for the hydrologic correction of DEMs, based on the distribution of hydrologically meaningful surface runoff across the landscape. This new algorithm allows for the preservation of pits or depressions in the landscape which the authors argue are likely to be natural features, rather than processing artifacts as often assumed. In doing this, the amount

of hydrologic connectivity of a landscape can be estimated for a range of runoff regimes. The effectiveness of this algorithm is demonstrated using DEMs of two distinct landscapes, both known to contain natural depressions. This manuscript and the algorithm it presents should be of broad interest to the surface process community as it begins to integrate hydrologically meaningful information to a previously abstract process.

General comments

Reading through this work, I often found that my questions were answered in a later section of the manuscript, and the inclusion of the Limitations section has neatly outlined many of the issues I would otherwise have raised. The first issue I have with this manuscript is the two test DEMs which have been selected. A prime motivation for this work, one that is discussed in detail in the introduction, is that as we move from lower resolution DEM products (e.g. SRTM, ASTER) to higher resolution LiDAR derived products, the depressions we identify and traditionally fill are more likely to be natural features. So it was disappointing to see the two datasets being employed having a grid cell size of 15 and 120 meters. It is unclear as to why this algorithm was not tested on higher resolution data, if there is a valid reason for this it should be communicated to the reader, otherwise, I suggest using some higher resolution data for the evaluation which is more typical of the work being done with high resolution topographic datasets in our community.

Following from this, there is no detail provided on the resampling method employed for either of the datasets. Depending on the technique used this could introduce noise into the topographic data, which may cause bias in the results by either creating new depressions or removing existing ones. I am assuming that the original data was also in gridded form, and so this effectively means that the data you are using has been resampled twice. I would like to see a clear justification of this double resampling and also some confirmation that it id not biasing the results.

It is not clear to me how easy it will be to use the output of this code in existing topographic analysis packages. If we do not use a maximum runoff depth, some depressions will be preserved in the data, which will cause many popular flow routing algorithms to fail (eg Braun and Willett, 2013). If I am correct in this assumption, some more detail in the manuscript drawing out this distinction would be helpful. I think the power of this technique comes not as a replacement to existing hydrologic correction, but as a tool to identify connectivity under different runoff regimes. For example, would it be possible to couple this code with a rainfall generator such as STORM (Singer et al., 2018) to develop better understanding of landscape connectivity and surface runoff under variable rainfall conditions?

There is no discussion of the suitability of a D8 flow routing scheme as a representation of real runoff behavior. Doing a full analysis of how differing schemes (D-infinity, MFD) may impact the results would be well beyond the scope of this contribution, but some discussion around these issues would be a valuable addition. I think this is particularly important as the basis of this algorithm is that it is physically-based and so it is important to justify the abstraction of using D8 rather than a more "correct" routing scheme.

Line by line comments

In addition to the issues mentioned above, I have some more general minor line by line comments:

Page 1, Line 13 - The discussion of time here is subjective, and dependent on dataset size, compute speed and landscape complexity. I would reframe this sentence around efficiency rather than time.

Page 2, Line 5 - Suggest changing ‘of the landscape, the climate’ to ‘of landscapes, climates’.

Page 2, Line 7 - Replace ‘equations’ with ‘equation’.

Page 2, Line 9 - Cite Braun and Willett (2013) here as well.

Page 3, Line 13 - Fix brackets around citation at end of line.

Page 4, Line 15 - Figure 3 is referenced before figure 2 here.

Page 6, Line 3 - What about integer DEM datasets? In that case ties could be very common, is there a better way to address this?

Page 14, Line 3 - I am asking a lot here, but is there any way to differentiate between true and spurious depressions?

Figures and Tables

Figure 2 - Is it possible to add labels to the cells? In the caption you refer to cells by their number and it would be nice to see those in the figure.

Figure 5, 6, 8 - The captions do not mention which dataset is being shown, its clear from the text, but would be helpful when skimming the paper to have this information to hand.

Figure 10 - I don't think you explain anywhere how you have extracted your channels, it looks like you have set an area threshold, and as long as this is constant within each dataset this won't matter much but it would be nice to see it stated explicitly.

Code

I am happy that the algorithm being described in the paper is what is being implemented in the cited github repo, and would like to commend the authors for the provision of open source, licensed code with detailed documentation.

– Stuart Grieve

References

Braun, J., Willett, S. D. (2013). A very efficient O (n), implicit and parallel method to solve the stream power equation governing fluvial incision and landscape evolution. Geomorphology, 180, 170-179.

Singer, M. B., Michaelides, K., Hobley, D. E. (2018). STORM 1.0: a simple, flexible, and parsimonious stochastic rainfall generator for simulating climate and climate change. Geoscientific Model Development, 11(9), 3713-3726.

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