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Geoscientific Model Development Discussions

Interactive comment on "A high-fidelity multiresolution DEM model for Earth systems" *by* Xinqiao Duan et al.

Anonymous Referee #1

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This paper present a method for generating DEM's of differing resolution from point cloud data such as that obtained from LiDAR. Such a process is widely undertaken across the geosciences and therefore should be of interest to a range of researchers who require topography. The authors are clearly experts in their field and make a number of really nice augments for their method, however as geoscientist I found some of the introductory section difficult to follow. I'd strongly recommend the authors give substantial consideration to making the introduction more accessible to a general audience and in the process explaining or removing much of the technical jargon. For example, I found this sentence really confusing, after several attempts to read it I'm still only partially understand what is being conveyed. "With the success of Earth and environment systems in conforming theories, explaining observations with these scale diversity processes, there exists persistent demands for extending their utility into new

and expanding scopes" Perhaps you are trying to say too much in one sentence. also "to simulate land-atmosphere, land-ocean, or land-hydrology interactions, or how to perform commensurate scale transformation to the topography itself taking care of the coupling endogenous features have proven to be a quite difficult task". For me this is unnecessarily complicated way of saying this and I don't know what you mean by coupling endogenous features. I do not doubt that the argument is correct but I just feel you will alienate some readers by presenting what are quite simple concepts in a difficult to follow manner. Most importantly the validation of the technique has some import limitations that could be easily overcome. The article strongly advocates the method for the generation of multiresolution DEM's. But this aspect is never tested in relation to the HFPR with only one number of points tested. Assuming its computationally feasible I would compute the surface accuracy statistics across a wide range of resolutions and point densities. These could be added to Table 1 or as a plot. Finally, I have three questions: 1) How does the selection of feature points effect the results (e.g. what if this was done poorly) 2) What can cCVT not be compared to previous CVT methods such as that in Du et al., 2010? 3) How would the method compare to an optimal estimator such as kriging? Overall I think the paper is very interesting, however it's very difficult to follow in places and the validation of the method is quite limited.

Specific points: Introduction increasingly improved resolution –> finer resolution P3 L 13 "for clearance of confusions and distractions with terrain generalisation" is this bit necessary? I'm not sure what it means or adds to this sentence. Section 1.3: For a general geoscience audience I think there is quite a lot of jargon here. For example, what are field control points, ground checking points? What is "an exact geometry clipping-based energy estimation"? As section two goes into much detail perhaps section 1 could be written for a general audience and no presuppose a detailed understanding of CVT. Finally, this section should clearly point out what component of the proposed method is novel, it not clear if the cCVT is novel or the application to a DEM is novel or just the software is new. Section 2.3: "For example, P. J. Kennelly pointed out that,

compared to hydrologic model, curvature-generated drainage networks has more nature performance, has not limit to one single pixel thickness, has no requirement on flat filling (depressions are rather useful sometime such as flash flood modelling), and capable of delineating both convergent flow and divergent flow (Kennelly, 2008)." I don't understand this sentence, what is a hydrologic model in this context? Do you mean that for the purpose of hydrological modelling curvature-generated drainage networks are better than some other methods. Also what does more natural performance mean in this context, could you not just say it is not limited to one single pixel thickness and has no requirement on flat filling. "The localization makes geometrical operation costs minimized. . ." -> The localization reduces geometrical operation costs such that the efficiency of the cCVT approximation as a whole is comparable to that of the elegant clustering approach Section 3.3: To what extent is the accuracy effected by the scale transformation ratio of the HFPR method. Am I correct in thinking the number of point used by the mesh is determined by this HFPR criteria and thus would you not want to prove that the cCVT is superior across multiple resolutions (e.g. 0.5% point left, 10 % point left etc.) P11 L 5: "more natural transition effect" I'm still not sure what more natural means in this context, A different discussion is needed here. P11 L 9: Do you mean precision or accuracy of the general approximation?

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