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Hydrology and Earth System Sciences Discussions

Interactive comment on "Accurate stream extraction from large, radar-based elevation models" *by* M. Metz et al.

Anonymous Referee #1

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This paper discussed an interesting topics related to channel network extraction from radar derived (SRTM and IFSAR) DEMs. The authors redisigned the least-cost flow routing method implemented in GRASS in order to improve its speed, functionality, and memory requirementes. According to the authors this revisited method should be considered as an efficent tool for stream recognition from large DEMs when large depression are a critical issue. Three different methods were compared 1. traditional sink filling, 2. impact reduction approach, 3. least-cost path search, and two data set related to Central Panama were used (SRTM and IFSAR). The results suggested that this new version of the least-cost path is a) "significantly faster" than the original version, b) useful for large dataset, and c) it provides an accurate channel network recognition. This work is interesting, and it reaches some topics actually critical when remotely sensed

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tecnologies and large topographic datasets are used. Having said that the paper in this present form is really weak, and it should be better organized. Several points need to be clarified, and few sections improved. The three major findings addressed by the authors in my opinion should be better presented. One has some difficulties to have a clear idea about how the method is "significantly faster" than the original version without any consideration about the hardware and software used. To present this tricky point in a paper without a solid basis and specific background on topics about computational times and softwares used, leaves me rather puzzled. This interestin topic could be a paper in itsefl, and it may deserves publication in a more technical journal like Computer and Geoscience. Nevertheless this is not the most critical part of the work. The issue related to channel network extraction using an unique area trehsold of 100000 m2 for all the resolution used, and without any consideration related to channel heads is really critical. Firstly, one cannot use the same area treshold for channel network extraction for different DEM grid cell resolutions since such kind of approach is grid cell size dependent! A treshold value of drainge area at 10m DEM should be not the same at 90 DEM! This has to be related also to channel head locations surveyed in the field. Then several other moderate issues need to be clarified. My recommendation is that this paper undergo major revisions before it can be accepted for publication. The paper should be accepted only if these important issues are deeply addressed, and well explained.

Here the comments, and suggestions

Introduction: some relevant references related to remotely sensed DEMs and channel network extraction are missed. The authors should cosidered some of the several papers published in the last two decades starting from works of Tarboton et al. (1989, 1991), reaching Montgomery and Dietrich (1992), and Rodriguez-Iturbe and Rinaldo (1997) up to the last researches using SRTM Hancock et al. (2006) and high resolution topography derived by LiDAR (Lashermes et al., 2007; Tarolli and Dalla Fontana, 2009; Passalacqua et al., 2010; Pirotti and Tarolli, 2010, Passalacqua et al., in press) among

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others in order to enlarge the prespective of this paper. Why then the authors decided to omit the work of Grimaldi et al. (2007), and Santini et al. (2009) about a different and well supported method for removing pits? This method really improve the traditional sink filling apporach, and so the related DEMs used for channel network extraction.

Methods: here the authors should better explain the sentences related to computational time and the amount of memory available. What is the amount of memory available? Which hardware has been used? Maybe I missed some part, and I have not really understood, but all these considerations on computational time in my opinion are not well supported by a detailed analysis and description. The authors should better explain this critical point, or delete all the part on the paper related to computational time analysis, while focusing on the remaining major points of the work. The value paper surely would not be affected if the authors will decide to not consider this topic. The least-cost path methor should be better presented, and simplified in a flow diagram picture in order to help the readers to better understand how it works.

Data source and pre-processing: here some informations about vertical accuracy of datasets used are missed. What is the vertical accuracy of IFSAR and SRTM DEM respect to field GPS surveys? What do the authors mean for "processed by standard procedures. . ..."? What is a standard procedure? Is this the procedure related to the work of Kinner et al. (2005)? Why the authors did not consider in the analisis also the resolution of 2.5 m? I'm really courious to see how detailed is the channel network at this resolution, and so the performances of this revisited method using high resolution topography. This elaboration surely will improve the value of the work.

Hydrological analysis and accuracy evaluation: here there is the major issue of the paper. Why the authors decided to use 100000 m2 as threshold for channel initiation? Where are the channel heads? Upon which basis they decided to use such treshold for all the DEMs resolution? The threshold for channel initiation is grid cell size dependent, so using three different DEM resolution one should expect three different tresholds! Without any field analisys on channel heads one cannot use a random num-

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ber for channel initiation. Is the 100000 m2 supported by references? Without a clear explanation of this point, and a comparison with channel heads, supported also by a statistical analysis of the suitability of selected treshold, the paper should not be accepted. Here the authors have to spend much of their efforts in order to improve the work. The work of Hancock et al. (2006) could really help the authors in this. In this section the D8 reference (O'Challagan and Mark, 1984) is omitted. This section should also benefit of one picture taken in the field during gps survey.

Result: again, all the sentences related to the computational efficiency of the presented method leave me rather puzzled. The "x" times faster were recorder using which kind of processor and memory? Are there any informations or numbers about the "available system memory"? Here really I suggest to reconsider all these analysis on computational efficiency.

Comparison of extracted streams with reference points: this kind of comparison is so strange, and really weak. Why the authors did not consider the blue lines as reference feature to compare? And then a more robust test such for example Cohen's kappa accuracy (Cohen, 1960; Pirotti and Tarolli, 2010)? According to Heipke et al. (1997) it is possible to measure the "goodness" of the final extraction results through an index (quality index) that takes into account the percentage of the reference data which is explained by the extracted areas as well as the percentage of correctly extracted features.

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