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Hydrology and Earth System Sciences Discussions

Interactive comment on "Uncertainties associated with digital elevation models for hydrologic applications: a review" *by* S. Wechsler

Anonymous Referee #1

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This paper addresses a timely and interesting subject, which deserves an in-depth review. The paper is fairly well-written (in particular the Introduction), although at times too much text is used to make a case. For instance, the Conclusion section may be condensed without losing any of the message it aims to get across.

Sections 3 to 7 are perhaps too long. They address issues that have little to do with DEM uncertainty as such. For example, section 3 pays too much attention to computation of topographic parameters. It should not be the aim of this paper to review methods for deriving topographic parameters. In fact, sections 1 to 7 read like an introduction in which DEM errors and the use of DEMs in hydrological analyses are introduced. The real content of the paper then starts with section 8. However, it must be said that this section is rather poor and suffers from several problems. For example, the starting points and assumptions behind spatial stochastic simulation are not thoroughly explained. No mention is made of the stationarity assumption, which is crucial to be able to estimate the parameters of the random fields, but which may not be reasonable assumption for modelling DEM errors. Also the Monte Carlo method is not properly explained. Further, the distinction between heuristic and empirical random fields does not make much sense. I do not agree that the heuristic and empirical approach “reflect two different philosophies about the nature of DEM error” (page 16). The first line of section 8.2.2. suggests that the heuristic approach assumes that the RMSE alone is enough for DEM error assessment but section 8.2.1 clearly states that spatial autocorrelation of DEM error must be incorporated as well (judging from the last sentence of the first paragraph of section 8.2.1). The only difference that I can see between the two approaches is that in the empirical case one has a higher accuracy data source from which the parameters of the random field representing DEM error can be estimated, whereas in the heuristic approach one has to make do ‘expert judgement’ or ‘educated guesses’.

Section 8, which is the core of the paper because it explains how DEM errors can be represented and how error propagation can be traced, needs much improvement and restructuring. The current headings are ‘DEM uncertainty simulation’, ‘Stochastic simulation’ and ‘DEM error simulation: case studies’. These should be replaced by ‘Representing DEM errors by random fields’, ‘estimating the parameters of random fields’ (in which the empirical and heuristic methods may be described), ‘error propagation methods’ (also discussing the Monte Carlo method) and ‘stochastic simulation from random fields’, or something along these lines. Examples and case studies should be integrated in these sections (it is a review paper, after all).

The paper is also somewhat disappointing in that it gives no answers to the questions that really matter. For example, the questions (in italics) on page 6 and on pages 11-12 are not really answered. Conclusions are somewhat obvious and facile, such as the last paragraph of section 3.0, the last two paragraphs of section 4.0, last paragraphs of sections 5.0, 7.0, 8.2.2 and 9.2. I would have liked if the author had stimulated readers more by coming up with more challenging and original conclusions and statements.

I feel also that the author has missed some key references that should be included in this review, in particular the work by Keith Beven and co-workers (see below).

In all, I think this paper should be supported but needs substantial improvement, particularly section 8. I therefore advise major revision.

DETAILED COMMENTS

(page 2) I do think errors are 'bad' in the sense that one would always rather not have them. It is just that they often cannot be avoided or only at high costs. Thus, always a trade-off between accuracy and costs will be made and DEMs will always have some degree of error.

(page 3) RMSE is not based on the normal assumption, it is just that when RMSE is used as a measure of spread (standard deviation) then this only quantifies the second order moment and thus additional assumptions (such as assuming a normal distribution) are needed to characterise the full probability distribution.

(page 3) "The reality is..." That is too strong, we should urge DEM producers to start providing that information. In fact, if we have sufficient control points where the DEM error is observed then we can construct the spatial autocorrelation function of error ourselves. It should not be too difficult for DEM producers or users to collect independent and accurate observations at control points.

(page 6) I do not agree that accuracy decreases with increased support. For example, it is well known from geostatistics that block kriging variances are smaller than point kriging variances, and that these get smaller when the block size increases. The author

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should distinguish between ‘support’ and ‘resolution’, because I can agree that accuracy decreases when resolution becomes coarser (while still predicting the elevation at point support, i.e. the centre of grid cells).

(page 6) I do not entirely agree that greater slope angles are obtained when using finer resolution because of increased ‘topographic complexity’. The real reason is that there exists no single unique ‘slope’ but that it must always be defined relative to the size of the ‘yardstick’ used. The smaller the yardstick, the smaller the terrain features that are included, the greater the variability in slopes. This has nothing to do with the complexity of the terrain, but rather with the size of features that one wants to include in the analysis. In fact, on page 7 (second paragraph) the author revisits this issue and makes statements that are more in line with my view. For clarification, see also http://en.wikipedia.org/wiki/How_Long_Is_the_Coast_of_Britain%3F_Statistical_Self-Similarity_and_Fractional_Dimension.

(page 6) In the bottom paragraph, it may be worthwhile to also include a reference to ‘Claessens et al. (2005), Earth Surface Processes & Landforms 30, 461-477’.

(page 12) I am not happy with the breakdown in four components as ‘approaches to addressing DEM uncertainty’. I do not see how visualization techniques can be a stand-alone technique for that, rather they are used to visualize results from the other approaches. Also, creation of (deterministic) error maps makes no sense because one would immediately use such maps to correct and improve the original DEM. Rather, one should create stochastic error maps (or map parameters of the probability distribution of the DEM error), but this essentially is the same as using simulation techniques to model DEM error.

(page 13) I would not say that one choose a “good” answer from potential answers: all answers are equally good, because of uncertainty one does not know which is the “good one”.

(page 13) section 8.1 mixes up the process of representing DEM errors by random

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functions and simulating from the probability distributions of these random functions with the process of propagation of DEM errors to terrain parameters or other DEM-derived variables. It would be better to separate these two issues. First explain that DEM errors can be modelled statistically using pdfs. Next address methods to analyse how errors propagate (mention Taylor series expansion and Monte Carlo), next focus on and explain briefly the Monte Carlo method and observe that in order to use it one needs to simulate from the pdfs of the DEM error, and conclude with a description of methods how to simulate from pdfs.

(page 15) Mention is made of the iterative swapping method, but this (obscure?) method is much less familiar than mainstream and well-accepted sequential simulation geostatistical methods. These were also applied in DEM error studies, see for example Aerts et al. (2003).

(page 16) I find it hard to believe that increasing the error leads to a decrease of model uncertainty. The reason given ("the nature of the normal distribution of the error fields used" makes me feel even more uncomfortable. I tried to check the Cowell and Zeng (2003) reference but the reference list only gives author names and title.

(page 16) Te second paragraph states that (spatially) uncorrelated random fields may be a valid mechanism. This contradicts an earlier statement (top paragraph page 15) that spatial dependence of error must be included.

(page 16) There must be a lot of literature in which DEM uncertainty propagation methodology is integrated with hydrological models. For example, what about the GLUE work by Beven and co-workers?

(page 18) Perhaps refer to Karssenberg. D. and De Jong, K., 2005, Dynamic environmental modelling in GIS: 2. Modelling error propagation. *International Journal of Geographical Information Science*, **19**, pp. 623–637; and to Heuvelink, G.B.M. and J.D. Brown (2005), Handling spatial uncertainty in GIS: development of the Data Uncertainty Engine. In: *Proceedings GIS Planet 2005*. Some progress is made to incorporate error assessment functionality in GIS.

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