Hydrology and Earth System Sciences Discussions

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

S. Wechsler

Received and published: 21 May 2007

I would like to express appreciation to Reviewer #1 for your thoughtful and detailed comments.

Anonymous Referee #1 Received and published: 29 September 2006

Comment: 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.

Response: The conclusions were edited and condensed. The section on interpolation was removed and partially integrated with the discussion of DEM error in section 2.

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Comment: 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.

Response: For the purposes of a review article on this topic, I believe that the information contained in the sections prior to Section 8 provide the context and background required by a novice reader to understand the content. In determining what approach should be taken in laying out the content of this article, I considered concepts from Bem (1995) "Writing a Review Article for Psychological Bulletin" and the stated aim of the HESS Journal. The Psychological Bulletin ". . . publishes evaluative and integrative research reviews and interpretations of issues in scientific psychology" for the American Psychological Association (http://www.apa.org/journals/bul/). Bem writes ". . . your review should be accessible for students in psychology 101, your colleagues in the Art History Department and your grandmother". Granted, the HESS audience may not be as broad as readers of the Psychological Bulletin. However, HESS's stated aim is to ". . . serve not only the community of hydrologists, but all earth and life scientists, water engineers and water managers. . . and the utilization of this holistic understanding towards sustainable management of water resources, water quality and water-related natural hazards". To that end I felt it important to contain information that may be considered introductory and seem basic to the knowledgeable reader.

Bem, D., 1995, Writing a Review Article for Psychological Bulletin. *Psychological Bulletin,* **118**, pp. 172-177.

Comment: 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. . . **Comment:** 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

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restructuring.

Response: As originally presented, this section was poorly organized. I appreciate and have integrated the Reviewers' comments and suggestions. I hope that the section, as rewritten, does a better job of representing the contributions to date in this area.

Comment: For example, the starting points and assumptions behind spatial stochastic simulation are not thoroughly explained. . . Also the Monte Carlo method is not properly explained.

Response: A section that specifically describes stochastic simulation and Monte Carlo simulation was added (and is included in the reformatted sections 7.1 Stochastic Simulation and 7.2 Representing DEM Errors by Random Fields).

Comment: 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.

Response: In response to Reviewers' comments the original section on simulation was significantly reformatted and reworked. A separate section discussing random field was added. In this section a reference to the stationarity assumption was provided in a footnote as follows:

". . . The random function model for estimating uncertainty is rooted in the field of geostatistics and is based on an assumption of local stationarity which assumes that spatial properties are independent of location. Error is complex and is likely non-stationary, and spatially autocorrelated. The assumption of stationarity, however, applies to the search neighborhood, not the entire data set and as such is a "viable assumption even in data sets for which global stationarity is clearly inappropriate" (Isaaks and Srivasta, 1989, p. 532)..."

Comment: Further, the distinction between heuristic and empirical random fields does

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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'.

Response: The reference to the distinction between these approaches as either "heuristic" or "empirical" was removed. The section now discusses the topic in reference to whether higher accuracy data is available. .

Comment: 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).

Response: This section was reformatted and restructured as follows. Section 7.1 Stochastic Simulation provides an overview of simulation. Section 7.2 Representing DEM Errors by Random Fields provides a discussion of Monte Carlo simulation. Section 7.3 Estimating the parameters of random fields, provides a discussion of methods that have been applied to generate random fields for Monte Carlo simulations of DEM uncertainty. Section 7.4 DEM Error Simulation: Case Studies reviews the applications to which these methods were applied in the literature.

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Comment: 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.

The questions: Is higher resolution necessarily better. . . to what extent is the grid cell resolution a factor in the propagation of errors from DEMs to derived terrain parameters" . . . what is the appropriate grid cell resolution for a hydrologic analysis. . . how does uncertainty propagate from the DEM to input parameters and through the models. . . have been addressed by studies and these studies were mentioned in this review. The answers to these questions are, in my opinion, still far from being definitively answered.

Comment: Conclusions are somewhat obvious and facile, such as the last paragraph of section 3.0.

Response: The statements in this paragraph aim to challenge the research communities to provide more appropriate methods for deriving data from DEMs. This research is currently not available, probably because the user community has not demanded it. The purpose of this paragraph, then, is to send a call to that community to initiate R&D of these "smarter GISs" so that practitioners can have more confidence in the results of application of DEMs to hydrologic analyses.

The conclusions section was rewritten in an attempt to identify key areas where progress can be achieved. This paper aims to review the current state of research. Conclusions are aimed at researchers, practitioners and educators and aim to address what is relevant regarding issues associated with DEM uncertainty. Yes, I concede that overall the conclusions are obvious and facile. But the attempt here is not to come up with a new scientific approach or answer to address uncertainty, but to suggest ways in which the GIS community can move to achieve this goal.

Comment: . . . the last two paragraphs of section 4.0,

Response: *While many DEM practitioners are aware of the issue of DEM resolution and subgrid variability, by reiterating this issue here I hope to call greater attention to it as an unresolved dilemma.*

Comment: ... last paragraphs of sections 5.0, last paragraphs of sections 7.0, 8.2.2 and 9.2.

Response: *Section 5 was removed and integrated with section 2 in response to comments from Reviewer #2. The other sections were edited. The conclusions were reformatted in an attempt to be more direct in the conclusions.*

Comment: I would have liked if the author had stimulated readers more by coming up with more challenging and original conclusions and statements.

Response: *In this paper I tried to identify areas where progress is needed. I tried to provide suggestions for how this can be achieved through collaboration among researchers, software developers and educators. This is what I see as the challenge. I would welcome additional suggestions.*

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

Response: *I appreciate the Reviewers' detailed and constructive comments and suggestions. I have attempted to improve and revise the manuscript in accordance with many of these comments.*

DETAILED COMMENTS

Comment: (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.

Response: *The term 'bad' was removed. The paragraph refers to spatial data un-*

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certainty in general, so the reviewers' comments related to DEMS specifically were not integrated. The discussion was broadly aimed at rethinking the term "error" and embracing it as a component of spatial data. I hope the following modification to the sentence helps in this clarification.

". . . In colloquial terms, the word error has a negative connotation, indicating a mistake that could have been avoided if enough caution had been taken ..(Taylor, 1997). However, errors are a fact of spatial data and often cannot be avoided. In the context of spatial data, errors are often unavoidable and therefore must be understood and accounted for. . . ."

Comment: (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 quantities the second order moment and thus additional assumptions (such as assuming a normal distribution) are needed to characterise the full probability distribution.

Response: Just as the standard deviation, as a parametric statistic, is based on the assumption of normality, so too is the RMSE. The sentences were modified as follows:

". . . DEM accuracy is usually quantified using the Root Mean Square Error (RMSE) statistic. While a valuable quality control statistic, the RMSE does not provide an accurate assessment of how well each cell in a DEM represents a true elevation. Furthermore, because the RMSE is used as a measure of spread, it requires the assumption of normality ...(Monckton, 1994), which is often violated in the case of the DEM. . . ."

Comment: (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.

Response: I agree that we should urge vendors to provide this and have argued that

in this paper. The paragraph was altered as follows:

". . . To date, information beyond the RMSE is not readily provided to DEM users. Most DEM users will not take the time or spend the money to obtain such data sets in order to conduct DEM error assessment ..(Wechsler, 2003). Because information on sources of error are not readily available, it is currently often difficult, if not impossible, to recreate the spatial structure of error for a particular DEM. Knowledge about the spatial structure of error is an important component for gaining an understanding of where errors arise and uncertainty is propagated. DEM vendors should be urged to provide this information. DEM users must be able to easily apply such information for it to be of use. Therefore the research and software communities must develop DEM assessment methods that accommodate detailed DEM error information when available, yet provide mechanisms for addressing uncertainty in the absence of this information. . . ."

Comment: (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 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).

Response: The following sentence was removed: ". . . The grid cell size imposes a scale on raster GIS analyses. It is also a representation of the spatial support which in geostatistics refers to the area over which variables are measured ....(Dungan, 2002; Heuvelink, 1998). . . ."

In removing this reference to "support" the confusion is hopefully avoided.

Comment: (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

Response: I agree with the reviewer. The reference to topographic complexity was the conclusion of some of the literature cited in the previous sentence. The ability to capture topographic complexity is related to grid cell resolution. And the ability to adequately reflect slope in complex topography is related to the slope algorithm used.

I removed that statement and left it to the reader to revisit the literature on this topic. The difference appears to be due to the algorithm used to compute slope, that is, as the reviewer mentions, the "yardstick' or denominator in the slope algorithm.

Comment: (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'.

Response: This reference was added as suggested.

Comment: (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.

Response: In the process of restructuring this section, I removed the paragraph that

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contains the content to which the Reviewer objects.

Comment: (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".

Response: The word "good" was not the best choice of words. The sentence was attributed to Journel, 1996 who states that ". . . one may choose from that distribution a "best" answer according to some goodness criterion. . ." (p. 518). I removed the statement in question as part of the reworking of this section.

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

Response: As mentioned above, the section on DEM Uncertainty Simulation (originally Section 8) was reformatted and restructured. The sections now appear as follows: Section <u>7.1 Stochastic Simulation</u> provides an overview of simulation. Section <u>7.2 Representing DEM Errors by Random Fields</u> provides a discussion of Monte Carlo simulation. Section <u>7.3 Estimating the parameters of random fields</u>, provides a discussion of methods that have been applied to generate random fields for Monte Carlo simulations of DEM uncertainty. <u>Section 7.4 DEM Error Simulation: Case Studies</u> reviews the applications to which these methods were applied in the literature.

Comment: (page 15) Mention is made of the iterative swapping method, but this (obscure?) method is much less familiar than mainstream and well-accepted sequential

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simulation geostatistical methods. These were also applied in DEM error studies, see for example Aerts et al. (2003).

Response: A discussion of the various methods that have been used to generate random fields was added. In this section reference to sGs was made.

Comment: (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.

Response: The reference information was fixed. Cowell, P., and T. Zeng, 2003, Integrating uncertainty theories with GIS for modeling coastal hazards of climate change. pp. 5-18. The following is quoted from their abstract on page 5 ". . . Paradoxically, output-uncertainty reduces slightly with simulated increase in random error in the digital elevation model (DEM). This trend implies that the magnitude of modeled uncertainty is not necessarily increased with the uncertainties in the input parameters.. . ."

The sentence in question was removed and replaced with the following:

"Cowell and Zeng, ..(2003) assessed uncertainty in the prediction of coastal hazards due to climate change. Uncertainty in the DEM was represented by random, normally distributed error fields. Ironically, as error was increased, model output uncertainty decreased. The authors attributed this to a "compensation effect" (p. 15) whereby random normally distributed error values, which range from positive to negative, cancel each other out."

Comment: (page 16) The 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.

Response: This following paragraph is the paragraph referred to by this Reviewer.

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This paragraph contains an erroneous statement that has been corrected in revisions. The statement as follows is incorrect. The words "may be" should be replaced with "are not". Completely random uncorrelated random error fields were shown to not be a valid mechanism for representing DEM error :

More recently, a "process convolution" or spatial moving averages approach to the generation of random error fields was used to evaluate the delineation of drainage basins that were found to be very sensitive to DEM uncertainty ....(Oksanen and Sarjakoski, 2005a). The approach was applied to both slope and aspect derivatives and demonstrated that completely random uncorrelated random error fields may be are not a valid mechanism for representing DEM error ....(Oksanen and Sarjakoski, 2005b).

Comment: (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?

Response: *According to Anonymous Reviewer #2: ". . . .Obviously errors in the DEM affect distributed hydrological models (part 7). For this article, I guess one should leave it at this statement, getting more into modelling would probably require a review article on its own. . . ." The GLUE method and its application to assessing uncertainty in distributed parameter models could be the topic of an entire review article. Its contribution to this topic is noteworthy and references have been added.*

Comment: (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.

Response: *This reference was integrated.*

Comment: 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).

Response: *Please see response above.*

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.

Response: *I was unfortunately unable to obtain the Heuvelink and Brown (2005) reference. The following message was received by my Interlibrary Loan service. I was disappointed to not have had an opportunity to read this reference. I will continue to search for this reference and hope that it will become available in US libraries.*

——Original Message——
From: Interlibrary Services [mailto:lib-illiad@csulb.edu]
Sent: Friday, April 20, 2007 2:42 PM
To: wechsler@csulb.edu
Subject: Automated Interlibrary Loan Notification

The request you placed: GIS Planet 2005 May 2005

Title: Handling spatial uncertainty in GIS: development of the Data Uncertainty Engine
Author: Heuvelink, G.B.M. and J.D. Brown

Transaction Number: 154137 has been cancelled by the Interlibrary Services staff for the following reason: We have exhausted all possible sources. No library is able to supply this item. Not yet available in US libraries

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