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Interactive comment on "Perspective – synthetic DEMs: a vital underpinning for the quantitative future of landform analysis?" *by* J. K. Hillier et al.

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This paper is a very thoughtful perspective piece on the efficacy of synthetic DEMs (e.g. idealized landscapes made from numerical landscape evolution models, analytic solutions to the fundamental PDEs, with and without the noise typical of nature) in geomorphic studies. The authors motivate their study by noting that landscape evolution studies often involve the inference of process from form, but making such inferences is rarely clear cut. Moreover, they note that new models and DEM analyses techniques are often demonstrated on real DEMs. Such validation exercises may not be as effective as using synthetic DEMs that have the advantage of user control on the morphology, degree of stochastic variability, etc. The authors provide a typology of synthetic DEMs that will be a useful guide to future researchers who wish to use them in their work.

I enjoyed reading the paper and think it makes some excellent points. One particular strength is that it draws from an impressively wide range of geomorphology (all subfields are represented) and even other subfields of Earth science (e.g. examples of data processing algorithms used in deep-Earth studies).

Points for the authors to consider as they revise their work:

I think the key message in this paper can be made more succinctly: the results of any proposed numerical landscape evolution model or DEM-analysis algorithm must return the exact answer for at least one case (similar to the intended application of the model) in which an analytic or exact solution is available. I agree with the basic message of the authors that this should always be done but often is not (including in my own work). One consequence of this point is that some synthetic DEMs (type 2, i.e. those created from landscape evolution models) actually require other synthetic DEMs (type 1, i.e. analytic solutions of the governing equations for simple geometries and forcings) to be established as proper synthetic DEMs. To make this validation-related point stronger, the authors could consider connecting to the large literature on the necessary conditions of model validation (or, more generally, on the kinds of confidence building that should be performed on a model to check whether it meets minimum standards of quality for its intended purpose).

The paper raises some interesting issues that I would liked to have seen explored more deeply. For example, it is not clear to me how synthetic DEMs can solve the equifinaility problem (i.e. similar topography resulting from different processes or forcing histories). At several junctures (including the first bullet point in the conclusions) the paper suggest that the use of synthetic DEMS can mitigate this problem, but precisely how they can was not clear to me.

The author's criticism of morphological data as typically of a quality that is weakly constrained seems outdated. We can now create bare-earth point clouds/DEMs of unvegetated landscapes on demand with $\sim$1 mm accuracy and comparable resolution using terrestrial laser scanning (Hodge, 2010). This is remarkable by any measure.

As someone who has published on the drumlin problem, I was intrigued by the statement (p. 610) that no process-based model of drumlin formation exists. In Pelletier, Quantitative Modeling of Earth Surface Processes (2008), I proposed a model of drumlin formation by deformation of subglacial till modeled as a deformable porous medium. I think there are process-based models of drumlins that are based on reasonable physics (in the case of my model on the stresses developed during till compaction and dewatering) that match observed relationships between drumlin aspect ratio and average till thickness (this relationship was shown for two US drumlin fields in Pelletier (2008)).

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