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Interactive comment on "Estimating Sand Bedload in Rivers by Tracking Dunes: a comparison of methods based on bed elevation time-series" *by* Kate C. P. Leary and Daniel Buscombe

Robert Mahon (Referee)

rcmahon@uno.edu

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The authors present a systematic comparison of bedform bedload measurement techniques using a unique dataset. Using field data, as opposed to flume data as is often the case, the authors are able to investigate some of the complexities associated with systems evolving under unsteady flow conditions. The ultimate outcome of this paper can inform decisions on both multibeam sampling and processing strategies as well as the placement of singlebeam echosounder instrumentation on rivers to monitor bedload flux. Thus the results of this paper are broadly relevant to river managers as well as to academic geomorphologists.

The overall flow and structure of the manuscript are quite clear. Figures are well placed into the manuscript context and are appropriate for fully describing the nature of the work. While I have no concerns that fundamentally call into question the nature of the science being done, there are a number of points which the authors could clarify or analyses that could be bolstered by more complete discussion. These comments are below:

I would like to see a description of the methods used to extract height and wavelength data from the BTT toolbox as it is a fundamental operation to the analysis in the paper. There are several methods for calculating these parameters, each of which have their respective advantages and disadvantages so it would be good for the authors to describe why the calculations employed in this toolkit are appropriate to their system.

Were bed elevation surveys corrected for apparent dilation as a function of the time between start and end of each multibeam survey? If not, was this considered and determined to be a negligible effect? See McElroy dissertation 2009, p. 44 (URI: http://hdl.handle.net/2152/15117).

A figure demonstrating the cross-correlation results would be good to show, as a lot of discussion is based on issues resulting from velocity calculations.

In Page 5 Line 6 the method for estimating wavelengths for the singlebeam experiment is described as the daily average from the repeat multibeam. I wonder if this introduces potential for extra accuracy for this method that may not be possible in a situation in which a single beam fixed echosounder would be employed. I would suggest more discussion of when a situation would arise where you have a measurement or a daily average of bedform wavelengths but only a single beam profile to estimate flux from. An alternative formulation might be to estimate wavelength using a height-wavelength relationship such as Bradley and Venditti, 2017 as this might be a more realistic representation of a likely application (i.e. a deployed single beam sensor established for continuous monitoring).

What did the manual process entail for determining bedform velocity? Were you picking crests and tracking them? Looking at the slopes of the forms in the η(x,t) field (e.g. in Figure 2D)? It would be critical to determine whether the manual method itself includes any potential sources of bias in order to interpret its relation to the cross-correlation results.

I wonder if other methods for calculating bed velocity might be more appropriate than the cross-correlation method for this application, particularly given the unsteady flow conditions investigated. One example from Ganti et al., 2013 (doi:10.1002/jgrf.20094), their eq. 5 to compute the local velocity based on dividing the temporal change in local elevation by local slope at all points on the bed.

Were any physical bedload samples collected during the multibeam campaigns to compare with the ranges of flux measurements?

Some discussion is warranted of whether the bedform bedload equation of Simons et al., is even geometrically appropriate in situations where bedform growth/decay is occurring. I don't believe they considered this in their original work, and I am not aware of any later publications that show the validity of this method for non-steady bedform fields.

Along similar lines I would encourage the authors to consider incorporating, or at least explaining the inappropriateness for their application, the insights from Guala et al. (2014, their Section 4 paragraph 2 in particular; doi: 10.1002/2013JF002759) in joint averaging of the elevation and velocity values.

While somewhat outside the scope of the review of the paper itself, I should note that the license type given to the dataset and code hosted in the SEAD repository is potentially quite restrictive to some river management uses and researchers, given that it does not allow commercial use or any derivatives. This may be less important for the data itself, but it may heavily limit the use of this work to have code that cannot be modified. A share alike restriction, for example, would make this more accessible.

Line Comments: The following line specific comments are non-critical to the science of the manuscript and are meant to help improve readability or clarity. Page 1 Line 2: "remains elusive" is relatively non-concrete and feels dismissive of the wealth of literature and practice on field-scale bedload measurement techniques spanning half a century or more. Page 1 Line 14: references are missing at "(e.g. ?)" Page 1 Line 20: References such as Simons et al., 1965 and others don't explicitly derive from the exner equation, per se. They are derivations of mass conservation but not necessarily predicated on Exner's formulations.

Page 2 Line 1: Simons wasn't the first to show this, as written. For example, Bagnold 1941, Chapter 13 derives a similar formulation, albeit with some geometric inaccuracies. I suggest simply removing the word "first" from the sentence. Page 2 Line 9: remove comma after "...discharge conditions," Page 2 Line 12: is there a reference for "...bedload flux estimated from translating dunes remains one of the most accurate..."?

Page 4 Line 14: ISDOTTv2 is not a familiar/common tool since it is not public. If you wish to include this statement, it would be good to describe what that tool is and why it would be useful here. Otherwise I would suggest removing it. Page 4 Line 16: please describe the "missing triangles" correction. Page 4 Line 22: consider rewording as it states a 1965 reference is based on a 2005 reference.

Page 7 Line 14: "...for growing (shrinking) dunes is 1.2 (0.75)." I suggest rewording to "...for growing and shrinking dunes is 1.2 and 0.75, respectively."

Figures: for figures 1, 2 and 4 there are abbreviations used which would be helpful to have defined in figure captions so the reader doesn't have to remember or find from the text. BEP, RMB, SB and MSB are all used. Additionally, Xcorr and RMSE are used but not defined in captions or in the text body.

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