Manuscript: Synthesis Report_ - Chapter 4

Author(s): Study Team

Name of Reviewer: Colin Rennie

1. Are the objectives of the work clearly stated? **1** 2 3 4 5

2. Are the methods employed valid, appropriate and sufficient to address the questions, hypotheses or the problem? 1 **2** 3 4 5

3. Are the observations, conclusions and recommendations supported by the material presented in the manuscript (e.g., data, model and analyses)? 1 **2** 3 4 5

4. Are the assumptions used valid and are the mathematics presented correct? 1 **2** 3 4 5

5. Is the manuscript well organized, material precise and to the point, and clearly written using correct grammar and syntax? 1 **2** 3 4 5

6. Are all of the figures and tables useful, clear, and necessary? 1 **2** 3 4 5

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Recommendation (please circle your response)

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Comments (limit responses to one paragraph for each question; reference pages, charts, and data. Please distinguish if responses are of major or minor concerns.)

A. What is the best/most unique part of the analysis?
B. What is the most critical aspect of the study/analysis? Why?
C. Which aspect of the analysis/modeling is weakest? Why? How can it be improved?
D. Are there any other suggestions that are related to how this analysis may be used more effectively or the results explicated in a more understandable manner?

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Signature: Colin Rennie_____ Date: _November 15 , 2009____

Comments for Transmission to Authors

It would be useful to have both general comments and specific comments for major and minor revision. Please use additional sheets should they be required.

Much of Chapter 4 is based on reports that I did not review, including Morris (2008), Foster and Denny (2009), Best (2009) and Krishnappen (2009). Thus, my review of Chapter 4 is based on what is presented in the Chapter itself. I assume that the methods and analyses of the individual reports were reviewed by others previously.

1) P58, Sec 4.2.1. It is stated that the RTK post-processing was applied to the survey, which yielded sub-m accuracy. Why only sub-m? RTK systems generally yield cm accuracy, particularly if dual frequency receivers are utilized.

2) P59. Refer to Figure 4-2d when discussing gravel-tongue features.

3) Throughout the chapter it is assumed that the reader is familiar with site locations on the river (e.g. Blue Water Bridge). Presumably, these are described and mapped in earlier chapters of the report.

4) P66.First paragraph. It is stated that the MINIMUM vertical error for the 2008 survey was 0.15 to 0.20 m. It is odd to state a minimum error. Some surveyed locations may have been exactly correct. Does this actually mean a minimum estimate for the mean error? Please clarify. In the subsequent paragraph it is stated that the maximum error in 2008 based on replicate surveying was 0.25 m. It would be useful to provide the distribution of errors, which could then be used to assess the previous statement of 0.15 to 0.20 m.

5) P66, last paragraph section 4.4.1. The Bennion (2008) report also included direct

comparison of elevation at surveyed points between survey years, not just comparison of interpolated maps from different survey years. This should be stated.

6) P67. It was useful to learn that the location(s) of continuing bed erosion (0.5m to 1.0 m between 2002 and 2007) correspond to locations of exposed till.

7) p68 and Section 4.4 in general. I think this section needs to be rewritten. The paragraph structure is disjointed and information is delivered in a disorderly fashion. More importantly, the intent of this section appears to be to discount the observed change in bed elevation between 1971 and 2007. This is done by asserting that the 1971 survey must have been biased. This argument is made without even stating the magnitude of the observed change in bed elevation (only figure 4-7 is provided, which shows only locations of erosion and deposition, without quantifying magnitudes). While it may be the conclusion/opinion of the report authors that the 1971 survey is unreliable, the argument is not presented well, and it has the appearance of a whitewash. This is unnecessary and could incite vigorous criticism. Instead, the Bennion (2008) report should be presented more thoroughly. To whit, the observed changes between 1971 and 2007 should be presented and quantified, using both raw data points (Bennion Figure 20) and interpolated maps (Bennion Figures 31-33). The lack of statistical significance of these changes should then be presented and explained, owing to uncertainty in the survey data (Note that I previously criticized the Bennion uncertainty model as excessively conservative. I do not know if the uncertainty model has been revised.) Only then should issues related to the 1971 survey and potential bias be raised, followed by discussion of the consequences for the Study (uncertainty of the results, etc).

8) As a general comment, figures should be cited to the original sources.
9) The river locations of Figures 4-12 to 4-14 should be specified. Also, local flow direction should be stated (the note “top to bottom” is insufficient, as the river does not flow exactly N-S at all locations along the reach).

10) Fig 4-15a. It would be helpful to use distinct colour schemes for elevations and morphological zones.

11) P74. The shear stress estimates employed the depth average Keulegan equation, which requires a ks estimate. It appears ks was assumed to be 0.05 m throughout the reach. However, the surface grain size in the reach is shown to vary from silty-clay to gravel/cobble (Fig 4-4). Clearly, ks is not constant in the reach. The authors could have varied ks based on location when estimating shear velocity. Alternatively, did the authors consider employing the log-law with a semilog-linear fit for vertical profiles of streamwise velocity? While this latter approach is sensitive to noise in the ADCP velocity data (thus averaging is required), it does not rely on a priori estimates of ks.

12) Fig 4-18. State which flow rate this figure is based on. In Figure 9 of the Liu and Parker (2009) report it is stated to be the “medium flow” condition.

13) p75. The last sentence of Section 4.6.1 suggests that observations at higher flows

may possibly generate sufficient shear to mobilize bed sediment. However, the subsequent section (4.6.2) shows that model results suggest flows within the range of observed flows are not competent. This should be clarified, and perhaps presaged at the end of section 4.6.1 .

14) p78, first paragraph. It is suggested that the median particle size (d50) is the maximum particle size that can be moved by a flow. This is incorrect. Rather, d50 is a good index value for the entrainability of a sediment mixture for a given flow (Wilcock 1992). In other words, you can use the Shileds curve with d50, and this should indicate when the bed will start to mobilize.

15) The chapter ends by acknowledging the apparent discrepancy between observed mobile dunes despite insufficient flow competence and lack of observed bedload transport. The suggested mechanism is ship wake. My immediate question is whether or not ship wake can generate coherent dune fields. In hindsight, it would have been useful for the Study to have evaluated the ship wake mechanism more specifically, as it remains an important unknown.