REPORT: SEDIMENT AUGMENTATION DATA SYNTHESIS COMPILATION, DRAFT, 27 NOV 2023
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1. SCOPE OF WORK

The PRRIP contracted this review to provide independent, critical but constructive feedback of this analysis of the Program's Sediment Augmentation experiment. The review is designed to examine specifically the scientific soundness of the work undertaken; the technical analyses performed; the conclusions drawn; and their presentation in this report.
Particular attention is drawn to five areas for general assessment (paraphrased below as):

(1) Does the Sediment Augmentation Data Synthesis Compilation use adequately multiple lines of evidence to assess the results of the augmentation experiment;

(2) Do the authors draw reasonable and scientifically sound conclusions;

(3) Are there seminal papers that have not been adequately considered and may have contributed to alternative conclusions;

(4) Are the statistical methods and modelling tools used appropriate and current; and

(5) Are potential biases, errors and uncertainties considered and discussed appropriately.

The reviewer's attention was also drawn to two further specific technical questions:

(6) LiDAR (sic.) data was processed to remove non-random error to the degree possible. Accordingly, two and three-dimensional change analyses did not utilize methods (e.g., thresholding) to quantify and/or account for error. Was that an appropriate approach? Would you suggest an alternative approach to quantifying and addressing error?; and

(7) Is the method used to separate lateral erosion from bed aggradation/degradation appropriate? Would you suggest any alternative method be utilized?

2. GENERAL COMMENTS

This report is an impressive and comprehensive examination of an ambitious field experiment. In recent decades, sediment augmentation has emerged a well-established intervention that aims to address the physical and ecological consequences of impounding and regulating natural river flow regimes. Few attempts to achieve this, however, have been designed as effectively and scrutinized as robustly as detailed here.

The approach taken is distinctive, and I would argue world-leading, for its application of powerful emerging technologies. The acquisition and interrogation of dense topobathymetric lidar covering a 6-year period provides an unparalleled, data rich lens through which to assess this intervention. While I have some technical questions over the precise approach to the volumetric change analysis and uncertainty estimation, I find the broad geomorphological interpretation astute and compelling.

Ultimately, the study demonstrates the complexity in insolating and explaining system-scale responses in a natural, open system over (what are logistically impressive, but nonetheless) short environmental timescales.

3. OVERVIEW AND STRUCTURE OF THE REPORT

The report is structured into four chapters, preceded by an Executive Summary. This opening summary synthesizes the findings of the study though a series of fundamental questions (E.1 – E.5, p. 8-10) that address directly the principal information needed to guide future decision-making.

Following this, the first chapter introduces the hydro-physiographic setting of the AHR and outlines initial modelling used to design the sediment augmentation experiment. Chapter 2 then provides a detailed evaluation of longitudinal trends in river response prior to augmentation. This chapter brings together multiple lines of evidence spanning a range of timescales to quantify incisional trends within the AHR. It clearly quantifies and explains the switch from vertical channel incision to lateral channel migration in response to declining bed slope and armouring of the bed of the J2 channel. Chapter 3 extends this analysis into the post-augmentation phase of the experiment (2017-2022), with a particular focus on longitudinal trends of thalweg and mean (active-) bed level responses. The methods used build effectively on those presented in Chapter 2 and focus principally on 1D longitudinal metrics of channel adjustment.

Chapter 4 provides the most refined analysis, drawing the spatially-rich, topobathymetric DEMs of the AHR. These are used to quantify the width-integrated, longitudinal distribution of net geomorphic change. The methods used are generally well-described and insightful. There are key hallmarks of excellence, for example, the use of numerical hydrodynamic simulations to segment geomorphic changes into lateral bank migration and vertical bed adjustment; and use of a discharge weighted normalization to compare changes before and after augmentation under varying flow conditions. As described in the sections that follow, I am sympathetic to the arguments against the application of thresholds to quantify erosion and aggradation volumes explicitly, however, I would argue that the treatment of uncertainties in the lidar models is somewhat superficial. This could be expanded and used to build a more comprehensive treatment of volumetric uncertainty that scales with area and puts the observed longitudinal trends of volumetric change within more robust statistical bounds. This will help to consolidate the broad findings of Chapter 4 and assist in the presentation of information to the key decision makers.

I have no doubt that the authors are well-aware that the current section, table and figure referencing needs to be updated and unified. The new referencing will then need to be married with the Table of Contents (TOC) on pages 4-6 and supported by a lists of tables and figures following the TOC. Aside from this, I found the report to be very well written and presented throughout. The text is succinct, effectively structured. The methods, results and inferences clearly demarcated and discussed critically. The figures are presented to a high standard and where appropriate, maps and spatial data are scaled, orientated and labelled clearly. In a small number of cases, individual captioning could be improved, and this is highlighted in the chapter reviews that follow.

The report draws comprehensively on the literature. This is used to support not just the justification and elaboration of methods but is also used to contextualize the results and substantiate the conclusions. Again, where relevant, additional supporting literature is highlighted below.

The review that follows examines each chapter in sequence and highlights specific questions/comments using the line numbers, table and figure referencing used in the draft document supplied. The executive summary is reviewed last, and my recommendations are presented at the end of this review. In line with the SOW and summary above, I have focused most of my attention on the technical methods and interpretations presented in Chapters 2, 3 and 4.

4. CHAPTER 1

Chapter 1 provides the introductory historical and geographical context, explains and justifies the selection and design of the augmentation experiment. The report reviews a range of studies from the literature that have sought to quantify incision associated with clearwater return flows from the J2 outlet and date back to Williams (1978).

A key focus of this chapter is the discussion of an early model of channel incision associated with clearwater flows published by Murphy et al. (2004). This study predicts the downstream propagation of a wave of incision, that is only arrested when the local rate of sediment transport falls in response to armouring of the bed and decreasing slope. The authors note that this model is based on assumptions of uniform sand and steady flow – factors that as described in Chapter 2 – are likely to explain the difference between this hypothetical model and the slower rate of wave propagation observed in reality. The review then clearly describes how these initial conclusions were refined through the development of the initial (SED-VEG) and then updated (HEC-RAS) numerical simulation tools to predict the annual sediment deficit in the AHR. Details of the modelling are not discussed (having been published elsewhere), but the spatial pattern of the predict sediment deficit is carefully presented, and used to frame the initial 2012-2013 augmentation experiment.

Experience during this trial is discussed and the report builds on this to justify the approach adopted for the large-scale augmentation experiment in 2017-2022. The selection of bulldozing material in late summer/early fall from the reach shown in Figure 0.5 (page 19) is well-presented. This assessment involved a careful appraisal of sediment availability, the development of design surfaces and used a reflexive approach that adapts to flow conditions.

<u>Comments/suggestions:</u>
This is a well-written chapter with few suggested changes or comments. The only points I would raise are:

4.1 Figure 0.5 (page 19) does not have a scale bar and the legend would be better placed at the top of the image rather than obscuring the channel. This is a complicated map, with multiple, overlapping layers – could it be simplified with better rendering? The caption also relies on an acronym and should really be able to stand in isolation (which is does not at present).

4.2 In the discussion of the as-built augmentation volumes (p 21, lines 279-298), while there is reference to adapting the volumes to flow conditions, the role of the Breakthrough Channel as an additional source of material is not discussed. This is reviewed in Chapter 4, but it may be useful to signpost this discussion here.

5. CHAPTER 2

This chapter seeks to create baseline information on the dynamics of channel adjustment across the AHR prior to the start of large-scale sediment augmentation in 2017. A methodology is presented that very effectively draws together multiple lines of evidence and triangulates results to shine a light on the system-scale behaviour. These methods include: the geomorphic interpretation of airborne lidar surveys; integration of these data with historical section surveys; reconstruction of at-a-section bed level change via specific gauge analysis; and an assessment of changing planform geometry. This is an impressive array of techniques that are brought to examine and quantify trends in river behaviour. In concert, they create a robust baseline against which changes during the augmentation experiment can be set.

A central tool to assess current and historical trends in the morphology of the AHR is the development of the Geomorphic Grade Line (GGL) as discussed in Section 1.9 (p. 28 onwards). The method is well described and the production of a Relative Elevation Model (REMs) for 2016 provides a powerful lens on longitudinal patterns. Specifically, this approach enables quantification of the current channel morphology relative to the average elevation of wider valley floor. This is an established tool to interpret channel and floodplain morphology that maximises the rich 3D information in remotely sensed river models (see Brasington et al., 2003 doi.org/10.1016/S0169-555X(02)00320-3; Westaway et al., 2003 doi.org/10.1080/01431160110113070 for some early examples of the approach)

Both thalweg elevation (deepest point in a section) and the mean bed level (averaged over the active channel) are compared to this regional sloping datum. Figure 2.6 shows thalweg elevations relative to the GGL and is a visually compelling summary diagram that captures succinctly the state of the system in 2016. It is an exemplar communication tool. This approach is then extended by integrating thalweg elevations from cross-section survey data in 1989 and 2002, enabling a deeper perspective on the progression of incision over a >25 year period.

Together, the resulting data provides the necessary spatiotemporal information on channel incision that are required to test the forecasts of Murphy et al. (2004) discussed in Chapter 1. The net result is effectively summarized in Figure 2.8. This is a fantastic insight, which clearly demonstrates how the downstream wave of incision appears to have been arrested significantly quicker than forecast. It also reveals how thalweg depths stabilized by 2002, but that mean active bed level continued to decline until the last snapshot analysed in 2016. This difference reflects a change in the mode of channel adjustment, with a shift from vertical incision to lateral migration and the development of a dynamic meandering planform. The result is further supported by the analysis of sinuosity presented in 1.12 (page 47).

Evidence of incision below Overton Bridge is less clear from the lines of evidence discussed above. Thalweg elevations in this reach below the confluence rise gradually (from c. 6 ft at the bridge) downstream before stabilizing (at c. 3-4 ft) at the upper boundary of the Cottonwood Ranch reach. In the absence of repeat sections through this lower reach, the team have used specific gauge analysis also known as constant-discharge analysis to interpret long-term trends in bed level at the two rated sections – Overton Bridge and at the downstream end of Cottonwood Ranch.

This approach aims to infer changes in bathymetry through trends in water surface elevation (stage) at a given discharge. The methods used are well described (albeit not perhaps as well complemented by references to the literature – see comments below), and it is important to recognize that signals of changing stage are not unambiguously tied to a specific morphological change. Compensating changes in width, bed elevation, slope and roughness mean may lead to complex relationships that could be misinterpreted. This point is well-made (p.37, lines 52-57) although could have been expanded using examples from the literature (see for example Criss and Shock, 2001, https://doi.org/10.1130/0091-7613(2001)029<0875:FETFC>2.0.CO;2; Pinter et al., 2006 doi.org/10.1016/j.jhydrol.2006.06.013).

The approach taken here uses three reference discharges, representing an appropriate range of flow conditions from the duration curve, including an index for bankfull discharge. Appropriate statistical models are fitted to stage-discharge relationships around each reference discharge and trends derived for Overton back to 1988 and Cottonwood Ranch back to 2001. The results for Overton are interesting, implying c. 2 ft of incision to 2014, most of which occurred after 2000. At Cottonwood Ranch, the reach appears to have been stable or even net aggradational.

The interpretation of the results is supported by false colour composite imagery for each site, dating back to 1988 and 2004 for Overton and Cottonwood respectively. Interpretation of planform imagery is notorious complex, given the discharge dependency, and it would have been useful to determine and label the estimated discharge associated with these images (Figures 2.9-2.11 and 2.14-2.15). However, when the images are taken together with the specific gauge analysis (again as two complementary lines of evidence), the interpretations are justified, and the value of the gauge analysis to identify of cycles of incision is well made.

<u>Comments/suggestions:</u>
This chapter is very well designed and presented. Multiple approaches are used to detect and decipher trends in the incisional history of the AHR. Combining these results consolidates their interpretation and represents a very high standard of science. There are few specific suggestions I would make, but offer the following comments/suggestions:

5.1 There is no discussion of bed texture/substrate changes in the chapter. As the authors note, the arresting of incision is likely to reflect a combination of factors, including armouring, reduced slope and also exposure of poorly reinforced bank material. Do the authors have any insight into the principal driver of the changing nature of bed adjustment – and specifically – could the revised HEC-RAS sediment routing model discussed in Chapter 1 be used to isolate the significance of the controls?

5.2 The description of specific gauge analysis could benefit from some specific examples from the literature that describe how this approach has been used elsewhere and illustrates the complexities of deciphering bed level change from stage. I have suggested a couple of key examples (above) and there any many more.

5.3 What is perhaps of more interest, is the potential to explore extending this analysis in the future with new data from the Surface Water and Ocean Topography (SWOT) mission. The shortwave radar interferometry that represents the mainstay of this mission has the potential to provide consistent high frequency (7-11 day) measurements of water level, extent and slope at longitudinal spacing equivalent to one channel width. While the scale of the wetted width of the Platte falls at the observational limits of SWOT, these new data may offer the opportunity relax the requirement to focus adjustment solely at the gauged sections and provide an insight into streamwise variations in channel response. See Shaperow et al., (2019 - doi.org/10.1029/2019WR024938) for further discussion.

5.4 As discussed above, labelling the relevant discharge associated with the images presented in Figures 2.9-2.11 and 2.14-2.15 would support their interpretation.

6. CHAPTER 3

This chapter extends elements of the framework used in Chapter 2 to examine longitudinal patterns of channel morphological response through 2016-2021. This epoch represents a one-year lead before the large-scale augmentation began in 2017, but terminates a year before the experiment ended (2022). It's not clear why the period of assessment terminates before the end of the augmentation, but I presume this is when (2022) work on this report commenced? The focus again largely divides the AHR into the J2 reach to Overton Bridge and then Overton to KCD. The toolkit used to assess the pattern of river adjustment relies on multiple lines of evidence as described in Chapter 2, though here there is a greater reliance on formal statistical methods to identify variations in longitudinal response, for example, changepoint analysis (space-series segmentation) and piecemeal modelling of long profiles using a variety of GAMs. The bulk of the analyses are derived from a collection of six topobathymetric lidar surveys of the AHR commissioned to capture the system in late summer/fall each year. This is an impressive dataset and – as highlighted previously – sets this work apart in terms its data rich assessment framework.

The use of changepoint analysis to segment longitudinal patterns of channel adjustment is revealing. This provides an excellent framework to identify discrete, localized trends in incision/aggradation. In addition to direct morphological metrics (thalweg elevations; mean bed levels; local slope), numerical hydrodynamic modelling (HEC-RAS and SRH-2D) is used to predict the effects of changing morphology of downstream wetted width at a reference discharge of 2,000 cfs. This flow represents the operating capacity of the J2 return channel and is sufficient to inundate bedforms and the full channel from Overton to KCD. The analysis integrates results from earlier 1D HEC-RAS modelling (for 2009 and 2012) with SRH-2D models parameterized using topobathy lidar from 2016, 2017 and 2021 (through the augmentation period). The modelling provides insights into the hydro-ecological functioning of the system and is an excellent tool to interpret changes riverscape condition. Intriguingly, the analysis revealed the importance of natural flow events, and particular the role of the high flow event in 2015. This event has clearly played a critical role in reshaping the morphology below the J2 confluence; resulting in a 50% expansion of wetted width at the reference 2000 cfs level. This active width appears to have been sustained through the augmentation period.

The summary conclusions appear to be well justified, with multiple lines of evidence proving an effective means to reinforce the key inferences. A key insight from the analysis is that while there is evidence of an aggradational response within and immediately downstream of the augmentation area, incision and planform change continued to progress downstream in the lower J2 channel during the augmentation

experiment. In this lower reach, significant lateral migration of the channel was evident, resulting in the decline mean bed levels and reduced channel gradient. The rate of progression was, however, relatively slow and, if consistent over time, would imply a decadal timescale for complete transformation to a wandering planform of the J2 channel downstream to Overton Bridge.

The reach below Overton appears to be more resilient to change. In part this reflects sediment inputs from the North Channel, but also evidence that upstream incision in the J2 channel correlates with downstream aggradation below the confluence. This latter pattern appears to be strongly linked to transport capacity, with sediment stored in the J2 reach in dry years and more transported downstream (supporting aggradation) in wet years.

The correlation between channel wetted width and gradient and the inference that a slope threshold of 0.001 is associated with the transition between wandering and braiding planform dynamics is interesting. It is tempting to suggest this threshold is a discriminant index of flow strength, i.e., specific stream power or mean bed shear stress. However, other factors may be just as critical in the maintenance of braiding and could confound this singular interpretation – notably variations in upstream sediment supply (below the North Channel confluence) and bank strength. Additionally, this relationship is presented only for one reference flow condition (2000 cfs). As bed adjustment occurs over a much wider range of flows in sand-bed channels, it may be hazardous consider a single particular flow association. In summary, I would be cautious of using threshold slope relationships in a specific management context.

Overall, the methods used are well described and conclusions justified. The following are a small number of specific comments/questions and/or suggestions for additions or changes:

6.1 The use of terrain based hydrological analysis to derive thalweg depths was not discussed in Chapter 2 in associated with Figures 2.6 and 2.7. Was the same approached used there, in which case it may make more sense to shift the description on page 57 back to Chapter 2. Alternatively, was this approach only used here, and applied as a means to estimate the thalweg alignments and local slope gradients – both of which requiring continuous, spatially contiguous elevation series?

6.2 Figure 3.5 is too small – enhance this plot by presenting as a single page.

6.3 The caption of Figure 3.16 is disconnected from the figure itself (page 72/73).

6.4 It would be useful to plot the timeseries of thalweg alignments around station 70,000 as an accompaniment to Figure 3.18.

6.5 The discharge at the time of capture for the aerial imagery shown for 2016 and 2021 in Figure 3.2 clearly differs. I would be useful to back out the discharge for the two acquisition dates and add this to the images as a label or as a note in the caption

6.6 Treat discriminant slope relationships with caution as per my comments above.

7. CHAPTER 4

In Chapter 4, the assessment of the augmentation experiment is extended to consider net volumetric change over the AHR. The supporting data are measured using DEM differencing methods, making full use of the 3D information in the series of the topobathymetric DEMs. These data offer important insights into the geomorphic processes driving evolution of the AHR and enable direct linkages between the volumes of

material entering the reach via augmentation or natural supply (via the breakthrough channel), and the volumes of stored, eroded and transferred longitudinally.

Overall, I find the methods used here to be well-executed and carefully considered. The conclusions are intriguing, in particular the finding that the bed degradation of the J2 channel decreased by between 45-60% of the pre-augmentation rate. Volumetrically this decline is calculated to be approximately equal to the volume of material transferred downstream from the augmentation zone. This correspondence raises the question of whether the ultimate goal of arresting degradation in the J2 channel could be achieved by (at least) doubling the rate of managed supply. This inference is supported by the apparent downstream connectivity revealed by supply from the augmentation zone and mean bed level adjustment show in Figure 0.11 (p. 102).

This is possibly the most significant conclusion of the entire study from a future management perspective. However, while this appears to offer an attractive forward trajectory, the chapter also reveals the confounding influence of flow and sediment supply through the breakthrough channel during the early years of the experiment. While it's difficult to evaluate the relative roles of these 'natural' and 'anthropogenic' drivers, it does seem likely that continuing attempts to address the sediment deficit in the J2 channel may potentially mitigate the impacts of incision in the lower reach of the channel, and potentially delay or even prevent incision progressing downstream of the North Channel confluence. Uncertainty in predicting future channel adjustment is further complicated by the potential for autogenic stabilization of the lower J2 channel through the process of continuing lateral migration and a concomitant reduction in channel gradient.

While the conclusions of the chapter are, I feel, largely justified and well presented, the scope of this review asked specific questions regarding the methodologies used. There are four components to the work that I feel are important to discuss in this context:

7.1 <u>DEM differencing methods</u>

The approach to DEM differencing applied herein follows the philosophy described in Anderson (2019 – see also Anderson and Pitlick, 2014 - doi:10.1002/2013JF002933). Briefly, Anderson (2019) argues that for the analysis of NET VOLUMETRIC CHANGE it is advisable to neglect the presence of normally distributed random DEM errors as, in summation, these cancel out and contribute negligibly to the overall uncertainty estimation. A caveat, is that this dependency relies on a large sampling interval, and is appropriate only when considering net change integrated over relative large areas – as applied here (where changes are integrated over downstream units equal to c. two-channel widths).

Anderson (ibid.) also argues that the – more conventional – use of detection thresholds to distinguish significant vertical changes from 'random noise' may actually create systematic bias in estimates of volumetric change. This holds true particularly when the vertical magnitude of changes is unevenly distributed about zero. Fluvial processes are an important case in point, as a common observation is that erosional processes are characterized by a signature of localized but deep (high magnitude) scour, whereas aggradation is more typically associated with comparatively shallow (low vertical magnitude) but widely-dispersed sheets of material – see the discussion in Brasington et al., 2003 (referenced above) and also Vericat et al., (2017 – cited within the report). The application of thresholds to distinguish significant changes may, therefore, distort the calculation of net volumetric change by preferentially disregarding shallow – aggradational – changes, while 'retaining' deeper, erosional signatures of bed adjustment. This

point is well made on p. 93, and I concur with this judgement for the estimation of NET volumetric or mean bed level changes.

Following the logic of Anderson (2019) the dominant term in an uncertainty assessment of DEM change is, therefore, the presence of uncorrected systematic errors. Briefly, he demonstrates that these errors will scale linearly with the area over which volumetric changes are integrated. When changes are incrementally summed downstream – for example in the estimation of cumulative volumetric change shown in Figure 0.9 (p. 97) – the uncertainty bounds around this sum will grow in proportion to the integration area scaled by the systematic vertical uncertainty.

Where I might raise an orange flag in the current analysis, is that such systematic errors appear to have been largely neglected on the basis of two actions:

a) Limiting the analysis to an area of interest representing the time-integrated active channel. This is justified as an attempt to focus the calculation of net volumetric change within areas where fluvial processes are dominant. This will eliminate the influence of lidar related artefacts such as differential vegetation penetration, that might otherwise dominate volumetric budgeting over wider areas; and
b) By arguing that common artefacts in lidar-derived DEMs, associated with uncertainties in the POS (position and orientation solution – or trajectory) were eliminated by refined processing undertaken by QSI in which the 2019 surface was used as a datum to calibrate the navigation solution for the remaining surfaces.

With regards to (a) ... restricting the net volume change calculations to a hydrodynamically defined AOI is good practice, and I fully endorse this approach. However, to argue that this will mitigate systematic lidar related uncertainties such as vegetation penetration is to ignore the presence of significant bar top vegetation cover within this AOI – as shown clearly in Figure 0.5 on p. 91. A similar logic applies to the accuracy of bed elevations in the subaerial and subaqueous areas of the survey – which have significantly different vertical accuracy assessments as shown in Table 0.3 on p.93. It is, in my view, highly likely that the ability to detect vertical changes reliably will be strongly conditioned by these surface cover effects (vegetated/unvegetated, wet/dry) and the direction of the surface cover changes. Ignoring these effects, would – following Anderson – only be justified when the units of volumetric integration (the 2-channel width cells) contain comparable proportions of these various cover types and the confusion matrix of change-types is balanced.

With regards to (b) ... calibration of the lidar models to a common reference datum is good practice. This approach seeks to ensure accurate georegistration of the surfaces to a consistent internal measure used throughout the analysis – rather than relying solely on independent geodetic control of each model. The 'flightline-dependent tilts' referred to on page 93 (and also rather loosely by Anderson, 2019) comprise two components. The first of these are global boresight parameters representing the alignment angles, mirror optics and time differences between the laser scanner and inertial navigation unit. The second component represents time-varying uncertainties in the POS that are associated with high frequency GNSS positional errors and the inertial measurement of acceleration and orientation. While it is arguably possible to minimize the global boresight error through co-registration (i.e., reducing the mean difference to zero), the time varying component of the POS uncertainty will always propagate into the estimation of sensor position and attitude. This uncertainty will then be compounded further by errors in the range estimation associated with the laser scanner itself. These effects are usually solved by using information content in

two overlapping point clouds to define time-varying adjustments to the boresight that serve to minimize differences along individual trajectories.

The source of elevation uncertainties does not end here and may be magnified further along the lidar processing and DEM generation chain. Variations in point density, slant angle, classification of the point cloud and ultimately the interpolators used, including the presence/absence of breakline data, all impact the quality and accuracy of a terrain model systematically. To assume that all such systematic errors have been corrected (as asserted on p. 93) without demonstrating this is hard to justify and difficult to believe.

Addressing the above sources of uncertainty is far from straightforward! Previous work has argued for the application of rigorous forward uncertainty estimation of uncertainty to quantify the full error budget on a pulse-by-pulse basis. This approach extends the methods used for the total estimation of uncertainty used in terrestrial laser scanning – see for example Lui et al., 2021 (https://doi.org/10.1016/j.autcon.2021.103673) building on Lichti et al., (2005 DOI.ORG/10.1061/(ASCE)0733-9453(2005)131:4(135)). A similar philosophy is used to support the quality assessment of multibeam echosounder (MBES) data via the calculation of total propagated uncertainty (TPU). Such approaches will not however, account for the effects of variable point density, slant angle, classification (including vegetation penetration) errors and subsequent interpolation.

In practice therefore, these combined effects are more typically assessed empirically. A common approach is through the examination of vertical and horizontal errors associated with what are presumed to be stable, reference surfaces. This assessment should focus on the DEMs produced (cf point clouds) as these integrate all of the above effects.

The analysis reported here could be strengthened by quantifying the magnitude and direction of systematic differences between DEMs, estimated by a computing DEM differences over areas presumed to be unchanged through the analysis period. This could be achieved relatively easily, by selecting a set of hard surfaces (roads, bridge decks), as well as roughened areas and areas under vegetation cover outside the active channel and extracting a large set of elevation differences taken from multiple, (spatially dispersed) independent test areas. The resulting sample errors could then be quantified either as empirical confidence intervals, or under assumptions, in terms of the mean error, SDE and RMSE. These metrics provide a practical insight into the typical magnitude of elevation differences between stable surfaces and offer evidence for systematic directional effects.

This approach is essentially that used in Anderson (2019). We could debate whether this analysis actually quantifies 'systematic' errors, but it is a means to approximate the uncertainty that cannot be eliminated in DEM differencing. Following Anderson, if this error term is then assumed to be spatially uniform (!), it will scale linearly with the integration area over which net change is calculated. The uncertainty used to provide local confidence intervals for each integration area and integrated into the longitudinal plots such as Figure 0.7 (p. 95) and used to derive the cumulative downstream uncertainty for the analysis plotted in Figure 0.9 (p.98) > see for example Anderson (2019) Figure 11.

An alternative approach that may also shed some additional light on systematic errors within a single survey (as opposed to co-registeration errors), would be examine the strip-matching results that QSI should have produced as part of their data deliverables. This metric examines the statistics of elevation variability in the overlapping regions of individual flightlines, and provides a useful indication of the magnitude of unresolved boresight errors and their spatial distribution.

As an indication of what an additional focus on uncertainty might reveal, take the total cumulative change shown (2021-2016) between the augmentation boundary to Overton shown (Figure 0.8 and Table 0.4 p.96/97). This is currently reported as -88,800 yd^3/yr. I don't have the AOI to hand, but let's assume the active channel is 450 ft wide (p. 91) and length of the reach is 32,000 ft (Table 2.1, p. 28). The integration area for the cumulative sum is therefore c. 1,599,998 y^2. If we take a vertical systematic error equal to c. 2 in (0.0555 yd), the total volumetric uncertainty over this area would be +-88,889 yd^2. While these numbers are only indicative, they serve to contextualize the cumulative sums of net volumetric change presented in Table 0.4.

7.2 Net Change Versus Erosion/Deposition Volumes

The approach of Anderson (2019) – neglecting random errors – is appropriate when computing net volumetric change. Net negative or positive volume of change, are not, however, equal to volumes of erosion or deposition. The actual volumes of the negative/positive changes will be larger than any net change, and moreover, should be treated with care to isolate changes that can (in this case) be distinguished from random errors.

I am therefore, a little confused how the values presented in Figure 0.10 have been estimated? This table shows the 'volumes eroded' from the J2 channel within and downstream of the augmentation area – both pre- and post-augmentation. It is a little unclear whether this is a rather 'loose' description and has somehow been derived from the net negative volume change, or whether the actual values of erosion have been estimated here by DEM differencing. If the latter, details of how thresholds have or have not been applied requires discussion, and it obviously opens questions about whether/how this approach to change detection generates net volume changes that differs from the results presented elsewhere. There is a clear potential to 'let the cat of the bag' here and the details of this needs to be clarified – especially given the significance of the erosional volumes relative to the augmentation volume.

7.3 Lateral Erosion versus Bed Aggradation/Incision

The use of numerical hydrodynamic model simulations to segment changes that into patterns of fluvial adjustment that represent lateral channel migration versus vertical bed deformation/transfer is an excellent approach. The discharge ranges used also appear appropriate.

The methods used here aim for a similar outcome to that of Llena et al., (2024 - doi.org/10.1130/B36720.1) in their analysis of channel adjustment and planform change in the Marecchia River, Italy. In their case, however, segmentation was based on geomorphic mapping and assessments of river confinement. That approach is in turn, a simplification of the method used by Wheaton et al., (2013 - doi.org/10.1002/jgrf.20060) to segment changes into specific braiding processes, defined by discrete morphodynamic signatures. These papers could provide a useful discussion of alternative approaches to segmenting DEMs of difference. An important distinction with this adjacent work, however, is their use of DEM differencing thresholding techniques, which permit direct assessment of volumes of erosion and scour and an insight into the geomorphic processes acting.

The only concern I have in this regard, is that the absence of graphical plots of DEM differences for each period precludes a qualitative assessment of the validity of this approach. I can't really see why these plots are not included in this Chapter as they would provide an important visual lens to interpret key changes within the AHR.

7.4 Normalization of Volumetric Change by Discharge

To compare the scale of pre- and post-augmentation volumetric change, the total volumetric change budgets are normalized not by time, but by the cumulative sum of Q^2. This is another excellent approach, as it reflects directly the potential geomorphic work over these two periods. This is an important contribution, that accounts for variability in flow between the two periods and the exponent used in the sum is fair reflection of the driver of transport processes in sand bed rivers.

Beyond these major discussion points, some smaller comments include:

7.5 As highlighted above (6.3) DoDs for the full sequence should be presented in full page map formats, extending the exemplar plot of Figure 0.4;

7.6 Figure 0.1 needs to contain a scale bar and ideally an insight to reveal its location;

7.7 The same is true for Figure 0.5.

7.8 What are the vertical accuracy metrics reported in Table 0.3? Are these empirical confidence intervals or 2 x the standard deviation of error. It would also be normal practice to identify any bias (via a mean error term). If the individual DEMs have been co-registered to 2019 (as discussed on p. 93) then it is unlikely that the mean error (by comparison for RTK GNSS observations) would be zero;

7.9 As discussed in 6.1 above, if a systematic uncertainty term is computed and assumed to scale linearly with the integration area, the models of longitudinal net volumetric change (i.e., Figure 0.7) could be presented as envelope curves, the width of which will largely reflect the area of the AOI within each 900 ft cell.

8. SUMMARY

Overall, this is a very well executed and interpreted analysis of the augmentation experiment. The work has been carefully designed, rigorously completed and draws on a world-class dataset. The methods used reflect the state-of-the-art in fluvial geomorphology and geomorphometry.

I have raised a number of discussion points through this review, the most significant of which relate to the DEM differencing methods employed in Chapter 4 and, in particular, the absence of formal quantification of volumetric change uncertainty. I appreciate the approach taken by the team in this regard but would encourage them to consider the range of options available to compute the residual systematic error and to incorporate this within the presentation of the results.

9. REVIEW SCORES AND RECOMMENDATION

CATEGORY	RATING
Scientific soundness	1
Degree to which conclusions are supported by the data	1
Organization and clarity	1
Cohesiveness of conclusions	1
Conciseness	1
Important to objectives of the Program	1

RECOMMENDATION	
Accept	
Accept with revisions	x
Unacceptable	

Dr Mark W. Smith, University of Leeds, Leeds, UK.

Review of Platte River Recovery Implementation Program (PRRIP) Sediment augmentation data synthesis compilation

The 'Sediment augmentation data synthesis compilation' is a detailed and well-presented body of evidence regarding the efficacy of a long-term sediment augmentation experiment. An impressive dataset has been compiled, detailing a number of lines of evidence which generally support the conclusions made. Evidence is primarily derived from LiDAR surveys, aerial imagery and flow gauge data. Each chapter presents numerous lines of evidence with generally support the overall conclusions of the report. The challenges of a real-world natural experiment are well articulated (P2). The authors correctly note that an experimental control is not possible given the nature of the Big Question articulated. Moreover, while subject to uncertainty, the flow normalization methods (P98) go some way towards correcting for the variable conditions experienced in the pre- and post-augmentation periods.

Incision of the J2 Return Channel since 1984 has been triggered by clearwater hydropower returns. The extent and magnitude of the incision of the J2 Return Channel is clearly and unambiguously presented (P31, P32). The channel degradation is supported further by specific gauge analysis (P41) which also shows that the pronounced incision has not progressed as far downstream as Cottonwood Ranch (P45). Prior to sediment augmentation, incision seems to have given way to lateral migration as the primary mechanism of channel change, especially in the upper portion of the channel (P34, P70). Sinuosity here has also increased (P47). The magnitude of sediment supply via lateral erosion is comparable to that of the sediment augmentation.

Despite extensive sediment augmentation, the incision of the J2 Return Channel has continued to progress downstream (P52, P62), especially in high flow years (P63). Fortunately, there is clear evidence that disturbance has not propagated downstream of Overton bridge (P53, P62). Direct comparison of pre- and during sediment augmentation periods is challenging owing to the different hydrological conditions. There was a decrease in bed erosion by 45-60% upstream of Overton Bridge since sediment augmentation; however, correction for changing conditions for this via flow normalization shows little difference in volume change before and after the augmentation (P98). It is hard to evaluate the specific effect of augmentation on the incision, though there is limited evidence for some positive effect at the upper reach. Whether it can stop the downstream progression of that incision is unclear.

Main Comments

Here, I expand on a few main comments arising from my review of the document. These relate primarily to the evidence presented in the compilation document. I'm mindful that program decision makers will need to make decisions regarding future management strategies based on this evidence. My comments and suggestions are therefore presented with this in mind and aim to maximise the utility of this document.

More specific comments (detailing page and line numbers) are provided at the end of this document.

1. Catchment Perspective and History

History and context of the PRRIP is well detailed in Chapter 1. It would be useful (especially for those of us previously unfamiliar with the catchment) to see a little more context of the broader catchment and its history. For instance, very limited information is provided as to the upstream catchment. This is relevant to the question at hand as it provides information on water and sediment fluxes expected and what may have been experienced in the past. From what I could gather, the water of the J2 Return Channel was taken from the Platte River just downstream of the confluence of the north and south branches near North Platte. It then runs through a series of reservoirs before being returned to the channel as a clearwater flow at the J2 Return Channel. I would be interested to know the approximate magnitude of the abstraction near North Platte.

Also, the historical development of the channel system is important context. Prior to the diversion for reservoirs and HEP, was the J2 Return Channel just a branch of the main Platte River (perhaps the downstream extension of the breakthrough channel that naturally forms)? I did a bit of searching and found a 1951 aerial image that showed a much stronger linkage between the 'North' channel and the J2 Return Channel here. In fact, the North Channel seemed to contain relatively little flow by comparison, though it is hard to tell for sure in the black and white image. The longer-term decline in discharge and channel narrowing since 1865 presented by Williams (1978) (seemingly arising from upstream reservoirs and irrigation) is briefly mentioned in Chapter 1, and also provides useful context with which to interpret measured channel behaviours in response to these clearwater flows.

While none of this information changes any conclusions of the report, these are questions I find myself asking when considering optimum future management plans. A paragraph or two in the 'History and Context' could readily provide this information.

2. Channel Fluxes

I realise there is a lack of information here, but also a general idea of the relative magnitudes of flows in the North Channel and the J2 Return Channel would be helpful up-front. This is most clearly presented on P88 (and the North Channel flows can be estimated by subtraction), but it is useful background through which to assess the findings.

The reason for such information is to get a better idea of the more natural sediment budget here. Are there no direct sediment transport measurements? Are there any estimates of sedimentation rates in the upstream reservoirs? These field measurements would give a better idea of the volume of sediment needed to provide balance to the channel reach. I realise that these data simply may not exist, but it would be useful to identify this data gap somewhere in the document.

3. Field Photographs and Observations

The value of seeing imagery of the river on or close to the ground cannot be underestimated. For example, the image on P22 really helps to set the scene. It was surprising to see so few images of these river systems, aside from aerial imagery. Descriptions often refer to incision (e.g., in the description of the REM on P30). I'd be interested to see these cuts in the field to get a better handle on how the system is changing.

4. Separation of Methods, Results and Discussion

I would encourage a clearer separation of Methods, Results description and Interpretation of data (see also point 6 below on Data collection). The latter two categories are often quite mixed in Chapters 2-4. There is of course an argument to mix these together (it is generally a matter of preference). Likewise, ensure all methods are fully described prior to the results section. The general structure is to provide the Methods/Results of each line of evidence used within each chapter. Perhaps a short Discussion section too would help isolate interpretation from the description?

Generally though, the report structure is clear. The section and figure numbering issue on my copy did confuse it a little, but this will not be an issue in a finalised version.

5. Chapter 3 Rationale

Given the excellent LiDAR dataset assembled, I found myself excited for the identification of areas of erosion and deposition and a volumetric analysis. Prior to this (in Chapter 4), Chapter 3 isolates several key measurements from these LiDAR data and provides a detailed analysis (e.g., thalweg profile, channel width, channel slope, sinuosity). The rationale for reducing the data in this way would be useful to articulate, presumably: (i) for more targeted analysis of hydrologically important variables; and (ii) more direct comparison with pre-augmentation data and trends (Chapter 2).

Many of the same conclusions were, of course, repeated in Chapter 4, as expected given the same underlying LiDAR dataset. Chapter 3 is the longest chapter in the report and the key messages and findings (and how the contribution is different to other chapters) got slightly confused to me on first reading.

6. Data Collection and LiDAR Methods

The LiDAR data sources are described briefly here (P85). It would be informative and transparent to see more specifications of the LiDAR system used, dates of data capture, target flying height, (etc.). Overall, the description of this data collection was rather brief. It also comes quite late in a document (P85) which first presents Relative Elevation Models (REMs) derived from these data from P31 onwards. A section detailing all data collated in the report (e.g., LiDAR, GPS ground control, gauge data) and description of that data collection might be useful earlier in the document.

The project takes the approach of Anderson (2019) and does not threshold DEMs to a minimum Level of Detection (LoD) before calculating erosion/deposition volumes. Given the scale of the systems under evaluation, I have no issue with this approach and, in general, am convinced by the main interpretations of these data.

I would be interested to see more details regarding these systematic errors. Data were post-processed to correct for flightline errors (but not 2009). What were the magnitude of changes arising from this post-processing? Vegetation changes are recognised but not directly considered. Disking of vegetated bars in the Cottonwood ranch is mentioned (P55): did this show up on any DEM of Difference? See comment 7 below.

Generally, it seems that the LiDAR data are accurate to within 3 inches on stable non-vegetated surfaces, perhaps lower where vegetation is present and up to 9 inches in wet areas. Generally, I would expect these to be a reasonable overall error estimate, though in the presence of steep surface features (e.g., banks) the sub-grid variability in elevations might also be of a similar magnitude or even more in isolated grid cells.

The mixture of models and LiDAR data became confusing (e.g., P71). It became hard to identify exactly what output data these models were being used to compute. A flow chart of the method and how LiDAR and models were used to determine key outputs might be informative and add clarity to the overall data processing workflow thereby improving reproducibility.

Perhaps the method that requires the most careful description and cautious interpretation is the calculation of sediment volume changes from the 2009 LiDAR data that had no bathymetric component. I can follow the logic of taking the mean modelled depth and assuming fill below the water surface to give a maximum possible value. However, of all steps this method (P90) seems most open to criticism. Thankfully, ranges are presented and the shortcomings of this approach are acknowledged fully and on reflection I do not believe this method and associated uncertainties change the key conclusions. Still, any attempt to prove the limited impact of this assumption is welcome.

7. Presenting and interpreting DEMs of Difference

The most surprising thing about Chapter 4 was the absence of DEMs of Difference (DoDs). Only one is presented (P90) as part of a demonstration of the method. These data products are extremely useful to evaluate any potential issues arising from systematic errors identified (described above). Moreover, all discussion appears to describe 'net' change in the system, while absolute magnitudes and locations of erosion and deposition are not presented. Presumably, this is because of the need to implement a minimum level of detection to compute such estimates reliably (again though, there is precedent here: Wheaton et al., 2010; Brasington et al., 2012; Smith et al., 2015). A minLoD could be computed from sub-grid elevation variability (if available) or simply the error metrics reported on P93 (though these would be conservative where vegetation is present). To be clear, I don't think calculation of the absolute magnitudes is necessary, but I do think some presentation of the DEMs of Difference adds confidence to the approach, would clearly indicate any systematic errors in the data and gives a richer understanding of the nature and pattern of geomorphic changes in the river (even if this can only be presented by differencing the first and last available survey).

Related to that, I wonder about the style of geomorphic change. Are these big localised changes, or more evenly spread out? A DoD reveals this, as does creation of a histogram of change (plotting the frequency of grid cells that experience different levels of elevation changes) see the citations earlier in this comment. Overall, these analyses yield both greater insight into the processes and greater confidence in the data. I would encourage the addition of these analyses.

8. Summary section

A Summary section would be useful or further specifics could be added to the existing Executive Summary. While the Executive Summary gives an overview of the findings, linking back these findings to specific lines of evidence (e.g., Figures, Tables) makes it easier to see how justified these conclusions are. This is best placed at the end of the document. Such references could equally be added to the Executive Summary (as in P8, L22). While I feel that the evidence compiled in the report supports the key overall conclusions of the document, I note that a few lines in the Executive Summary (e.g., P9, L51-52) push the interpretation a little further than the rest of the document (see my response to General Question 2 below). I would suggest caution here, or at least making these clear links back to the line of evidence from which each statement is derived.

9. Relationship of changes to habitat suitability

To finish on a few broader comments (see also comment 10), the data compilation does a good job of presenting channel changes both before and during sediment augmentation. The stated purpose of this is to preserve habitats of target species. However, the link between these changes and species habitat is only loosely made. What is the relationship between incision and habitat suitability? Likewise, the identification of pronounced channel widening at Cottonwood Ranch (seemingly against a backdrop of long-term channel narrowing presented by Williams, 1978) seemed like it might be good news, though I could not be sure. It seems likely that such interpretation is beyond the scope of this data compilation but seemed lacking given the focus on Extension Big Question #3 (P23).

10. Broader thoughts

Appropriate questions are raised in the Executive Summary. There remain many unknowns here. It does seem that the incision is still migrating downstream despite sediment augmentation, but I agree with the suggestion in the report that this migration rate will likely slow once it reaches the main channel and the impact of floods and variable flows/sediment input buffers the incision.

It leaves the question of what to do about the J2 Return Channel. I recognise the broader management context here too (e.g., the need to refill the meander and so locally steepen the reach to protect the building on the right bank). The suggestions of shifting augmentation location downstream and passive augmentation had been raised in my mind too. The breakthrough channel is something that also keeps raising itself. The longer-term history of the channel seems to suggest that more flows were once routed through the J2 Return Channel as recently as the 1950s (from the aerial photograph I found). The fact that when this breakthrough occurred it "periodically conveyed a substantial volume of flow and sediment" (P86) it does identify itself as a naturally dynamic component of the system and something that would fully convey the augmented sediment. However, the suggestion of the report is that the 2019 breakthrough event was the main driver of increased incision in that year. Given the 15 ft elevation difference that now exists between channels (P32) as a result of degradation, such breakthroughs do seem likely in future.

The focus on the transition between meandering channels and braided sections is also interesting and seems relevant given the focus on habitat protection. While not to be taken exactly (there are many qualifiers and complications), I found myself considering the old Leopold & Wolman (1957) graph that roughly identifies a threshold between meandering and braiding. Again, I wouldn't focus on the specifics here, but even plotting the 'bankfull' discharge level (median annual peak) at Overton (downstream of the confluence) and we are close to/below the threshold line for either the old or new slopes. The 2500 cfs used to estimate the bankfull discharge of the J2 Return Channel (P90) would plot below this and sit firmly in the meandering section. Clearly, the braided reaches of the river (e.g., the onset of braiding at Station 60,000 at the point of high slope

in 2016) show that the Leopold & Wolman (1957) relation is not exactly correct (and there are many examples in the literature where braided sections plot below the threshold line), but it also indicates that braiding might not be sustainable (or stable) in the J2 Return Channel at the present levels of return flow. These observations should be placed into the long-term context of discharge decline in the reach from irrigation, etc. (see comment 1 above).

General Questions

1. **Does the Sediment Augmentation Data Synthesis Compilation adequately address the overall objective – to synthesize multiple lines of evidence from the Program's full-scale sediment augmentation experiment to assess overall results and provide useful information for decision-making related to future sediment augmentation management actions?**
   Yes. This is a well presented and well-compiled report of an interesting management implementation. A data-rich approach is taken and the data are interrogated thoroughly and appropriately. The use of multiple lines of evidence that each confirm a clear outcome adds further support to the conclusions made. My comments above are mainly focused on increasing the clarity and adding further contextual information to support decision-makers, but I endorse the findings made here.

2. **Do the authors draw reasonable and scientifically sound conclusions from the information presented? If not, please identify those that are not and the specifics of each situation.**
   Yes. The data are clearly presented and support the conclusions made. While I have minor suggestions regarding data presentation of the volume change results, these are simply to give further confidence that systematic errors are not problematic. The fact that the evidence presented by the volumetric change analysis confirms previous thalweg-based analysis suggests that none of this would adjust the overall conclusions made.

   The line in the Executive Summary (P9, L52) that half the bed erosion was offset by sediment augmentation might be a little strong given the results from flow normalization (P95-97) and should perhaps be stated more conservatively. At the reach scale to Overton bridge, it may be true since it seems sediment has not been mobilised this far and has been added into the system, but whether or not it is protecting against incision I'm unsure. I'm not 100% clear where the evidence is that the incising channel may be recovering slightly (L51). These few lines are the only such instances I can identify.

3. **Are there any seminal peer-reviewed scientific papers omitted from consideration that would contribute to alternate conclusions that are scientifically sound? Please identify any such papers including citations**.
   I suggest that the overall conclusions are scientifically sound. The citations I present in this document regarding DEM differencing (Wheaton et al., 2010; Brasington et al., 2012; Smith et al., 2015) and sediment budget segregation (Wheaton et al., 2013 in minor comments below) are simply alternative approaches to some of the methodological choices here.

4. **Are the statistical methods and modeling tools used valid and current, and are the associated results presented in a manner useful to Program decision-makers?**
   Yes. There are no issues here. The most complex statistics (GAMMs) are well explained and represented visually. Overall, the report takes a visual approach to representing the data which are also quantified into summary statistics. It is very clear. There were few details on the modelling methods used, though these were often not the core of the analysis, but rather provided a step of the analysis (e.g., to identify a wetted area for given discharge). I was a little unclear as to where in the workflow these models were used and their impact on the final findings (see main comment above), but again I should reiterate that I do not think this has any impact on the conclusions.

5. **Are potential biases, errors, or uncertainties appropriately considered within the methods sections and then discussed in the results and conclusion sections**?
More discussion of these for the volumetric change analysis chapter would be useful (see main comment 6 above). The relative magnitude of different uncertainties relative to the differences observed could be better articulated. I should add that I see no cause for concern, rather it is in the interest of clear reporting to do so. Presenting DEMs of Difference would add further confidence that there exist no outstanding systematic errors in the data.

Technical Questions

6. **LiDAR data was processed to remove non-random error to the degree possible. Accordingly, two- and three-dimensional change analyses did not utilize methods (e.g., thresholding) to quantify and/or account for error. Was that an appropriate approach? Would you suggest an alternative approach to quantifying and addressing error?**
There is a precedent here (Anderson, 2019) which is cited by the report. While it is not my preferred method, I think it is appropriate for the scale of the study. The largest assumptions in the report arise from the calculation of sediment volume changes from the 2009 LiDAR data that had no bathymetric component (P90). The report clearly flags the uncertainty arising from this (e.g., P94) but would likely be the most open to criticism. As described in my comments above, I would like to see more DEMs of Difference to help clarify visually that no residual systematic errors remain.

7. **Is the method used to separate lateral erosion from bed aggradation/degradation appropriate? Would you suggest any alternative method be utilized?**
I can see that this method makes intuitive sense and is defensible. My previous experience was with the method described by Wheaton et al. (2013) of segregating the sediment budget by mechanism. The approach implemented here has the benefit of simplicity and would likely result in the same outcome. As per my minor comment (below) on P85, some visual example of this segregation process would be helpful as the description of the methods here was quite brief.

Peer Review Rating & Recommendation

Category	**Rating**
Scientific soundness	4
Degree to which conclusions are supported by the data	4
Organization and clarity	3
Cohesiveness of conclusions	4
Conciseness	3

I wasn't sure how to interpret the below category. I assume it reflects the degree to which the report addresses the program's objective (though I note it does not address habitat directly)?

Important to objectives of the Program	4

RECOMMENDATION	**(Check One)**
Accept	
Accept with revisions	x
Unacceptable	

Suggested revisions:

My suggested revisions relate to by 'Main Comments' above.

1. **Context**. Comments 1-3 all deal with background context. These were things I felt would enable decision makers to better interpret the evidence provided. Some text on the catchment, flows, history, etc., would be useful, but I recognise that this may not be appropriate for the intended use of the report.
2. **Structure**. I would try to split out result description and interpretation where possible. I realise this is challenging in places. A brief summary section to directly relate key conclusions back to the underlying evidence presented would be helpful here (either at the end of the document or making better use of the existing Executive Summary). The description of the data collection (i.e., LiDAR, flow gauges and related data) could be expanded, separated out from results description and interpretation and moved earlier in the document.
3. **Volumetric changes**. The LiDAR volumetric changes section would benefit from presentation of DEMs of Difference to visually identify any residual systematic errors. Consideration of the relative magnitude of potential errors would also be useful alongside the magnitude of observed topographic changes. See the main comments 6-7 above.
4. **Minor points**. I have a number of minor points below. While these don't all require action, it would be good if they could be considered.

Minor points

Presentation: this was mentioned to me beforehand, but the figure/section numbering of the report is non-sequential. It was also mentioned that the pagination of the pdf I reviewed may be different from other versions. My apologies if my page numbers below are a little off.

P2, L30: 'mining' of sediment invokes some kind of human sand/gravel mining. Is this what is intended here?

P8: there is some issue here with the conversion between tons and cubic yards. The footnote indicates that 1 T = 1.5 yd^3. However, the conversion applied on L7 is different, with seemingly the opposite ratio applied (1.5 T = 1 yd^3). It would be also useful to see the data on the sediment density; however, this does not change the study conclusions.

P8 and P27: presumably this is false colour image (there are several through the document)? Perhaps indicate flow direction. Also, perhaps indicate station 70,000 which is referenced in the Executive Summary.

P9, L25: how far downstream did the 6 ft of incision reach by 2002?

P9, footnote 5: what is NPPD?

P12: why is the map not to scale?

P16: what is the 'proposed flow regime'? Presumably this is a higher flow, relating to the values in the footnote?

P23: two slopes are presented in the graph. What are these?

P28, L81: what is meant by 'pre-development channel'?

P29: clarify that the equation is for the black GGL line. Does this include the J2 return channel and land in-between (I'm just wondering why the convexity is not apparent)? What was Murphy's slope estimate and how exactly does it compare?

P30: are there any field observations of these bank heights (or even measured directly from LiDAR)?

P31: this Figure is very convincing.

P33, L20: it would be good to see these cross-section locations.

P36: the incision measurements are not as severe as modelled by Murphy et al. (2004), though I suppose these new lines would shift down if the same maximum incision depth was used. How would it compare then?

P37, L55: would channel narrowing not *increase* the water elevation for a given discharge?

P41: I assume the presence of triangles (for example) on the graph, where this discharge is well below the triangle stage range, simply reflects the use of the 30-day trailing mean discharge? Perhaps note this in the caption to explain this.

P41, L105: typo 'dischargers'

P41, L116: I deleted an earlier comment about providing a check on channel width using aerial photographs owing to the clearly variable stage. This variability also complicates the interpretation presented here. Overall though, the key point is that the channel is not widening (which I agree with).

P47, L161: 'attempted to'?

P50, L40: four analyses? The following text suggests three, though it is hard to identify exactly what they are.

P55, L12: some of these differences are hard to see in Figure 3.3. I know we are waiting for the subsequent volume chapter, but a difference map of the two models would allow these patterns/changes to be seen more clearly. Likewise, On P56, I cannot see the meander development especially clearly in this comparison of REMs (whereas a DEM of Difference would be very clear).

L58, L75: I need a little bit of clarity on the point being made here. What might the effect be on the calculated slopes? If migration has taken place, would the horizontal distance used in the slope equation be a lot out? Or is this really minor given the migration rates and intervals used (is it 100 ft like the prior analysis)? Without this information, I can't really judge how limiting this assumption is in the slope calculations.

P60: an 'elevation distance from grade line' plot might help discriminate between these two lines a little.

P65: it is interesting to see the slope variability smoothing out between 2016 and 2021 and the onset of braiding at the higher slope location (Station 60,000).

P66, L66: why use two separate discharge levels either side of Overton to clip the transects? Could a more consistent approach be used (e.g., stage when the gage reading was X)? Presumably, the approach taken was simpler to implement, but did it result in a discontinuity at Overton?

P66, L70: a 'single' polygon used for all years. Was this a union of all wetted areas?

P69, L9: is there any deposition taking place on the inside of these migrating meanders?

P71: I found the modelling aspect of the wetted width calculation a little tricky to follow.

P71: the figure really highlights the main problem reach where there are some narrowing or static widths (around 75,000). Surely the increases in width around cottonwood are good? Though it is hard to tell without any identification of impacts on habitats.

P72, L89: the text suggests a nonlinear relationship between slope and width, yet Pearson correlation used. It might be nice to see this relationship. Which direction is the relationship?

P73, L118: "continued to increase"… the real increase from Figure 3.17 seems to be prior to 2012, though following the straightening of the meander, there has been a gradual increase… I doubt this would be statistically significant. Also, is sinuosity not hard to interpret when there is so much braiding. Why was there no braiding index considered? Finally, P72 notes both manual and LiDAR-derived thalwegs… which is shown in the Figure?

P81, L90: there's reason to suspect that the wandering planform migration would slow or cease beyond the confluence.

P85, L62: typo "3three"

P85: some details on the specific LiDAR system used would be informative.

P85, L68: perhaps clarify here that the 1D models were in HEC-RAS.

P85: the partitioning of lateral erosion and bed changes seems clear to me, though an image might help to clarify/demonstrate exactly how this was done. I'm also mindful of mentioning the method of segmenting sediment budgets presented in Wheaton et al. (2013) which could be simplified and applied here. To be clear though, I think the method used would be adequate.

P90, L140: what might be the effect of the disking of vegetation mentioned previously?

P90: the use of 2009 data when no bathymetry was available is slightly challenging. My understanding is that model estimates suggested an average depth of 1 ft and this is used to estimate subsequent sediment aggradation below the water level of the survey. I agree this gives a maximum possible value, but I'd exercise caution in how this value is interpreted. To be fair, this is reflected in the results description (e.g., P94).

P95: exactly how were these uncertainties calculated? I suggest dedicating a short section in the methods to clearly articulating this. No uncertainty in upstream reach? Is it within the thickness of the line?

P98: this flow normalization is good to see. Of course, it is sensitive to the choice of exponent on the *q* divided (P92).

P99, L336: when describing erosion here, is the net erosion? I assume so. Likewise for the Figure on P100.

P103: it would be useful to have a comment about the very different pattern of volume change in 2019/20 in the most upstream portion of the reach. Is this related to the closure of the breakthrough or just the higher flows that year mobilising more of the augmented sediment?

References Cited

Brasington, J., Vericat, D. and Rychkov, I., 2012. Modeling river bed morphology, roughness, and surface sedimentology using high resolution terrestrial laser scanning. *Water Resources Research* 48(11).

Leopold, L.B. and Wolman, M.G. 1957. River channel patterns: braided, meandering and straight. *Geological Survey Professional Paper* 282-B, 39–85.

Smith, M.W. and Vericat, D., 2015. From experimental plots to experimental landscapes: topography, erosion and deposition in sub-humid badlands from structure-from-motion photogrammetry. *Earth Surface Processes and Landforms* 40(12), pp.1656-1671.

Wheaton, J.M., Brasington, J., Darby, S.E., Kasprak, A., Sear, D. and Vericat, D., 2013. Morphodynamic signatures of braiding mechanisms as expressed through change in sediment storage in a gravel-bed river. *Journal of Geophysical Research: Earth Surface* 118(2), pp.759-779.

Wheaton, J.M., Brasington, J., Darby, S.E. and Sear, D.A., 2010. Accounting for uncertainty in DEMs from repeat topographic surveys: improved sediment budgets. *Earth Surface Processes and Landforms* 35(2), pp.136-156.

Williams, G.P. 1978. The case of the shrinking channels: the North Platte and Platte Rivers in Nebraska. M.S. Thesis. University of Wyoming, Laramie. In U.S. Geological Survey Circular 781. 48 pp.

PRRIP Sediment Augmentation Data Synthesis Compilation – Peer Review

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28 March 2024

Introduction

First, I would like to thank the authors of this report ("PRRIP – EDO Review Draft) dated 27 November 2023) for providing a report that has relatively few minor typographic errors and is generally laid out to a high standard. This has eased my review and enabled me to focus primarily upon the scientific methods, results and interpretation. Second, I would like to comment that I consider the work that has been conducted to be well focused upon investigating what is a challenging applied river management problem both for these reaches of the Platte River and for many other global rivers that are characterised by similar pressures due to the consequences of dams. Acquiring system-scale data to monitor changes in river morphology is not straightforward, and neither is the interpretation of the often relatively short timescale (i.e. annual- to decadal-scale) observations that are often characterised by events that cannot be controlled, such as different magnitude high flows. Thus, I would like to comment that I think it is a testament to the authors of this report and the broader Program that the "Big Question" that this report focuses upon is being investigated with the acquisition and interpretation of observational data, with the ultimate objective of maintaining or improving the health of river habitat.

My review, below, is organised into a set of sections. Each section includes my comments on the report's content and recommendations for consideration, throughout this section I include answers to many of the General and Technical Questions. In the subsequent two sections I provide a summary of my answers to the General and Technical Questions (which includes cross-reference back to the detailed thematically organised information), and my rating and recommendation. I am, of course, happy to provide any further details or clarifications as needed; please just get in touch with me.

Specific comments and recommendations on the report

1. Geomorphological overview of the study area

I am not familiar with this river. Whilst the report provides an overview of the history and context of sediment augmentation, in order for me to interpret the results it would have been useful to have further contextual information about the study reach itself. Specifically, I recommend that a contextual section could include information on:

- Topographic context, and natural and anthropogenic confinement: It would be useful to provide a summary of any natural/anthropogenic confinement along the study area. I was particularly interested in the role of natural (e.g. topographic narrowing of the valley bottom) or anthropogenic (e.g. bridge infrastructure) narrowing at Overton Bridge, which may play a significant role in controlling the flux of sediment through this section of river, and thus the degradation/aggradation dynamics (and associated timescales) in the reaches upstream and downstream. It would be useful to provide a detrended (relative elevation model) that extends beyond the active channel (i.e. across the adjacent floodplain) to assist with this contextualisation. This may also help to reveal high bars; from the satellite imagery in the figures it also appears that there are some areas with high, vegetated bars that have previously been geomorphologically worked by the river but are not used for agriculture. It would also be useful to understand the geomorphology of the river downstream from the Kearney Canal Diversion; are there any downstream pressures that may propagate up to the Cottonwood Ranch Reach that may influence the morphodynamics of the study area?
- Grain size: Do you have surface / sub-surface grain size information for different reaches? If so, what are the downstream / bar-scale patterns? This is important since the report refers to bed armouring as a consequence of "hungry water" winnowing fines.
- Vegetation: There are various references to the root systems of vegetation and their relation to bank erosion susceptibility. It would be useful to have a description of the colonisation of bars and banks with vegetation, any seasonal dynamics, and any management interventions.
- Anthropogenic management interventions: In the report there is reference to a (i) meander straightening; (ii) blocking of a "breakthrough" channel; (iii) mechanical augmentation upstream of Overton gage during 2005-2013 (P48 L206); (iv) "disking" of vegetated bars in Cottonwood Ranch in 2018 (P55 L19). I consider these management interventions, which take place prior to or within the timeframe of sediment augmentation, to also be important to the interpretation of the river's morphodynamics in response to management actions. Rather than introducing and referring to these activities in the results sections, I recommend that these (and any other) management activities are described in a contextual section and are carefully interpreted in the overall executive summary. If there are also "historical" management activities that have taken place upstream, within the study area, or immediately downstream, which may have influenced morphodynamics, then I also recommend that these are summarised.

2. Figure quality

Figures are generally produced to a good standard but a number of enhancements could be made to help interpretation, particularly for readers not already familiar with the geographic context of the river:

- Choice of colours on charts for points/lines showing different years. A wide variety of different colours are used on different charts; interpretation would be easier if a consistent palette was used throughout the report, which is also accessible for people with colour-vision deficiencies (Crameri et al., 2020).
- Consistent inclusion of geographic features on maps. Many charts (e.g. Figure 2.6) include a consistent set of geographic features (e.g. JT Return, Augmentation Area, Overton Bridge etc); it would be useful if these features were also consistently shown on all maps; this positioning would help data interpret by enabling easy cross-referencing between figures.
- Add a "flow direction" arrow to all maps.
- Consider using identical extents and scales for similar maps (e.g. why do the extents for Figures 2.1 and 2.2 need to be different – it makes it hard to cross-compare them).

3. Methodological description

Chapters 2, 3 and 4 of the report present analyses that are dependent upon some common datasets but details on the characteristics of these datasets are not always immediately available (e.g. vertical errors associated with dry and wet areas of airborne LiDAR surveys) until a latter section. I recommend organising the report so that a section that provides information on datasets is provided at the outset, before these data are used for analysis. It would also be useful if a hydrograph was presented earlier (e.g. it would be useful to support Section 1.5.2, P20 L240).

4. Review of Chapter 2: Evaluation of trends in incision prior to full-scale sediment augmentation

Below, a review of each of the components of this chapter is provided. The review focuses upon queries or points of clarification. Overall, the material below is intended, for this chapter, to answer the following questions required for the peer review:

- Are statistical methods and modelling tools valid and current?
- Are associated results presented in a manner that is useful to program decision makers?
- Are potential biases, errors or uncertainties appropriately considered with the methods sections and then discussed in the results and conclusions sections?
- Do the authors draw reasonable and scientifically sound conclusions from the information presented?

This chapter uses four approaches (listed below) to evaluate trends in incision prior to full-scale sediment augmentation. The "Discussion" section (which I consider could be more appropriate titled "summary") of this section clearly communicates the overall findings, which are drawn from a generally appropriate set of methods and data analysis (although see comments below). This analysis enables the quantification of: the magnitude of incision immediately downstream of the J2 return (from REM analysis); the timing of changes in the rate of incision along the study area (from cross-section analysis); higher-temporal resolution analysis of degradation/aggradation dynamics at two observation points (Overton; Cottonwood Ranch gages); and analysis of channel sinuosity upstream of Overton Bridge. Based on the data presented, I consider the interpretation of the nature of, and causes

of, planform change to be appropriate (P48 L214-216) but recommend that: (i) there is a comment on on whether the transition is from braided to meandering or whether there is a wandering state, since a wandering planform is referred to in subsequent sections of the report; and (ii) that there is a broader desk-based conceptual interpretation of what a change in planform pattern may mean for differences in the rate of sediment transport and thus supply (with/without augmentation) from this reach to the reach downstream of Overton Bridge (i.e. what are the typical capacities of braided / wandering river types for conveying sediment versus meandering rive types) (see e.g. Church, 2002).

Geomorphic Grade Line and relative elevation model

Use of geomorphic grade line is appropriate for analysis of a continuous length of river where there are no perturbations that may cause "natural" deviations from a linear trend (e.g. influx of sediment from a tributary, a choke point caused by a topographic constriction, adjustment to a sediment wave caused by landsliding upstream, baselevel adjustment propagating from downstream). I have suggested a broader geomorphic contextualisation above but consider that this approach is appropriate overall, subject to these caveats. However, it would be useful if the authors added a sentence or two to more fully justify why this approach is valid for this river (insert on P29 L104).

With respect to the data used for this analysis, my comment above on providing an overview of the LiDAR dataset in an overarching methods section, at an earlier point in the report, is of relevance here. It would also be useful if reference to the 2016 "topography" explicitly states that this topography includes representation of both wet and dry topography (assuming it came from the topo-bathy LiDAR dataset)

Analysis of trends in incision 1989-2016 relative to predictions

I consider the fit of a third order (cubic) regression to be ok to model the trend in the longitudinal thalwegs for 1989 and 2002 but note that there are some potential subsequent cases of potentially overfitting trend curves to data later in the report (see below) rather than letting the data "speak for itself". Whilst there are only two sample years available from the cross-section data, and it is know that there is inter-year variability, I agree with the interpretation of Figure 2.7 that there is a signal of different elevations between stations 50,000 and 90,000 between 1989 and 2002-2016. Shading could be provided around each line to indicate the elevation uncertainty in the different survey techniques, potentially drawing on Bangen et al. (2014) for cross-section surveys and the error analysis for "wet" areas in the 2016 LiDAR survey (presented in Chapter 4). This would enable further interpretation of the point (e.g. at station 50,000 or 60,000 etc) at which the signal is clear from potential survey noise.

I sympathise with the authors with their challenge of reproducing results from a previous study (see data reproducibility recommendation below) and consider that the comparison of modelling and observed datasets (Figure 2.8) is appropriate. I consider that the model predictions from the Murphy et al. (2004) study are rather "crisp" in nature and the summary chart doesn't well present the uncertainties that may be associated with the different predictions (e.g. due to flow variability; uncertainty in grain size; vegetation dynamics etc). I recommend that the comparison (Section 1.10.2 P35) between the observations and predictions could be edited to place less emphasis on the model potentially being "correct" (i.e. the truth) and rather emphasizing what the limitations to the model are in terms of its conceptualisation, assumptions and data inputs relative to the dynamics of the river and what interpretation of the observations allows. I recommend that any reference to a "longer study

period" (P35 L39) explicitly focuses on how further monitoring may allow the river to express its dynamics though interpretation of the observational data rather than any inferences to improving the model-observation comparison.

Gage analysis

This method and data analysis makes good use of two relatively high temporal frequency, long temporal duration gage datasets. I have a number of comments / questions / recommendations for consideration:

- Methodological approach: Why was water surface elevation (i.e. stage height) interpreted and not any bed level information from ratings? Are the latter available. I'd be curious to know whether these gages were included in Slater et al.'s analysis of USGS station data; if so, what do their results show? If not, why couldn't the data be included in Slater et al.'s analysis? This may explain why bed level can't be used here?
- Overall, I found the interpretation of the stage height data for the 10th percentile, median discharge and median annual peak discharge (Figures 2.13 and 2.17) to be more valuable for interpretation of the river's dynamics at the two gage locations than the segmentation of data into four flow stages (e.g. Figure 2.12). However, it was useful to see the full hydrograph in Figure 2.12 (see recommendation about providing more contextual information early in the report, above).
- Can you further expand the reasons for water surface elevation change (P37 L52) to include local change change and roughness change?
- Interpretation of planform change shown in Figures 2.9 to 2.11 and Figures 2.14 to 2.15: I consider that interpretation of planform changes from satellite imagery is very important for the interpretation of river channel change but I think that a clearer presentation of these results is required, which may involve finding imagery from alternative months for each year. What is the source of this imagery; Landsat imagery? Can the caption include detail of why these years are being compared; what is the reason for showing these data (you refer the dynamics that are interpreted on P41 L116-117)? Can you label the figures with your interpretation of changes? Is it worth extending this analysis for the whole study area (it can be done automatically e.g. in Google Earth Engine; Boothroyd et al., 2021) and including this as a separate methods/results section (it's beyond gage analysis), noting that planform change is considered in chapter 4. Would it be useful to digitise low flow wet channel (from image acquisitions with relatively consistent flow, if possible, and active channel (i.e. wet and dry; Boothroyd et al., 2021) extents and make a transparent GIS overlap to compare these data? The wetted extent for each year varies considerably which makes it hard to make any comparisons about the position of the primary anabranch or the extent active channel, can you select images that have similar discharges or, in the very least, label the discharge at the time of image acquisition. At present, I consider that these figures may be a potential source of confusion for decision-makers that may not understand the reasons for the differences.
- Does interpretation of planform change (e.g. switching of anabranches, changes in anabranch widths) in the multi-thread sections (particularly Cottonwood) influence the consistency of interpretations of gage data, particularly for low flows?

Channel sinuosity

This analysis builds on the interpretation of planform change presented within the gage analysis section. I have a few queries on this data analysis:

- Can you provide information on the specific satellite (P47 L164) and the imagery dates (as an appendix?); are the thalweg lines available (digital supplement?)
- How was imagery selected for each year? Similar month, similar discharge?
- Earlier in the report there is an argument presented that there is a shift in river type (broadly from braided to meandering); is it possible to also calculate a braiding index (if imagery with a similar discharge can be selected) to provide further a line of evidence associated with this, and in particular whether the river has transitioned to wandering or meandering (P47 L175-6)?
- Is there value in interpreting sinuosity / braiding intensity of the North Channel as a "control" to compare to the J2 Return Channel?

5. Review of Chapter 3: Evaluation of longitudinal change after sediment augmentation

The review of Chapter 3 takes a similar form to Chapter 2; a review of each of the components of this chapter is provided, focusing upon queries or points of clarification. Overall, the material below is intended, for this chapter, to answer the same four questions outlined above that are required for the peer review.

The objective of this chapter is to examine longitudinal changes between the J2 return and KCD during the first five years of sediment augmentation (2017-2021). In the introduction, I recommend that: (i) explicit reference is made to all LiDAR from 2016 onwards being topo-bathymetric LiDAR that represents both wet and dry topography (assuming this is correct, as described in Chapter 4) and (ii) a more precise/expansive description of "more detailed evaluation" is provided (P51 L43). The suite of methods and associated results shed light on the longitudinal dynamics of the study area. The discussion (P80; section 1.23) is organised well for interpretation by decision makers since it begins by interpreting the results in a downstream sequence. However this section could be improved by considering the following: (i) linking to an overview contextual section that includes details on e.g. the high flow record (to help interpretation of comment on 2019 high flows – P80 L34), breakthrough channel closure (P80 L47-8); (ii) developing of a conceptual/schematic diagram/cartoon to summarise the findings (this could also integrate findings from other chapters and be presented in the executive summary); (iii) conducting an analysis of the longitudinal dynamics of the North Channel, to contextualise the findings for the J2 Return Channel (P40 L92) and improve the certainty associated with the statement about confluence inputs on P81 L92 as these are important for sediment supply downstream of Overton Bridge; and (iv) providing more commentary on the relative merits, scope and feasibility of different future analyses (P81; L97) – I consider that the changes in channel width and slope are already good but this could be enhanced with multi-temporal satellite imagery analysis, lateral thalweg migration quantification could be achieved with various GIS tools, and morphodynamic modelling is potentially financially expensive and subject to considerable uncertainty albeit depending on the questions that are posed.

Relative elevation model

This analysis is both appropriate and rigorous but reporting could be improved by providing clarity on a number of aspects:

- P51 L54. Is “in general” correct; is “For all” more precise?
- P51 L62-3. A channel type change to “wandering planform” is described for the upper end of the J2 Return Channel. Ensure that this is consistent with the meandering to braiding transition noted in the previous chapter; is wandering described here because it is just for the upper end of the J2 Return Channel? That’s factually fine but I suggest that these different transitions are explicitly summarised in the Executive Summary and whenever they are referred to in the report the time range and geographic extent are explicitly noted, to prevent any confusion or mis-interpretation.
- Are elevations associated with the extent of the North Channel excluded from any subsequent quantitative analysis of the dynamics of the J2 Return Channel? The full extent, including both Channels, is shown on Figure 3.1 and I’m concerned that if the North Channel is not excluded then the morphodynamics of that Channel may influence (even bias) any interpretation of data from the J2 Return Channel. It would be worth making a note about this on the figure caption and in the methods.
- Figure 3.3. Interpretation of the differences between these figures gets very close to a map of elevation change (next chapter); it may be worth cross-referencing the subsequent figure that shows elevation change or adding this as another horizontal pane at the bottom of this figure. Text associated with this section actually starts to interpret channel (i.e. volumetric) change. This can be inferred but without a DEM of Difference is this the best place for this interpretation (rather than the next chapter?) Indeed, there is a risk that a decision maker is shown too many plots that are close to showing the same thing, so the relationship between this section of the chapter and the volume analysis should be carefully considered. Nevertheless, comparison of the 2016 and 2021 REMs sheds light on horizontal changes between these times.
- P55 L17-18. There is no data to support this comment on the differing proportions for different categories. Can a histogram be added to the side of each REM in Figure 3.3?
- P55 L19. “Disked” isn’t a term I was previously familiar with, although I accept it may be commonly used in the local area. Perhaps provide a brief explanation. This management intervention is also noted above with respect to providing an overview of management actions.
- Figure 3.4. This is a really nice set of data to work with to investigate this problem; it’s rare to see system-scale topographic-bathymetric datasets of this temporal frequency.

Longitudinal profile of channel thalweg 2016-2021

The position of the thalwegs was automatically detected, then manually corrected in some instances, using a contemporary, and appropriate, GIS topographic analysis tool. Longitudinal slopes were “smoothed” using GAMM. Changepoint analysis was then applied to detect statistically similar segments. I agree that it was appropriate to maintain a common station reference system for the comparison (P58 L75). I have the following comments on this section:

- Consider the logical sequence of the methods section: I think the slope smoothing (i.e. producing the data that will be analysed) should be presented before the changepoint analysis.
- Consider whether some of the differences quoted (e.g. 0.2 ft; P61 L12 / L14) are within the range of uncertainty in the wet area accuracy of the LiDAR (P93 Table 0.3).
- Table 3.1: Consider presenting lower and upper quartiles as well or instead of minima/maxima as minima/maxima can be misleading if there are outliers.

- Figure 3.10: Could a statistical test be used to determine whether there is statistically significant difference between the 2016 and 2021 Thalweg Models? This would help to shed light on my next two comments.
- Reflect upon how the results are phrased and whether some of the small changes in slope are likely a signal or close being similar to the magnitude of noise in the observations. I think that the larger changes in Figure 3.8 are compelling but that interpretation of the small magnitude changes needs to be undertaken carefully.
- Double check that the magnitude of difference between 2016 and 2017 modelled thalweg slopes is correct upstream of Overton Bridge (Figure 3.11). In the caption it would be valuable to concisely explain the channel changes that caused these differences.

Longitudinal profile of mean channel elevation 2016-2021

For this analysis, change in elevation was considered across the whole active river rather than just along the thalweg. This approach to analysis is thus more similar to a volumetric change analysis (Chapter 4) than a thalweg analysis. This is an important component of the analysis since for multi-thread, labile rivers a considerable amount of channel change can occur outwith the primary anabranch and change can also be associated with width and bed variation. I have a number of comments and questions about this analysis:

- Are elevations associated with the North Channel removed from this analysis?
- Why were 2,500 and 5,000 cfs selected? Is there a physical basis for these values? From figure 2.12, I note that these values exclude many Annual Maximum flows. To contextualise and aid interpretation, it would be useful to see a figure showing the inundation area associated with these flows (from 2D model predictions?).
- P66 L76-77. Processes associated with degradation are explained but not those associated with aggradation.
- P67 L81. Is it really convex, or actually predominantly linear?
- Figure 3.13. From c.72,000 ft station to Overton Bridge there is a consistent pattern of average cross-sectional elevation; this is compelling for analysis of the overall trend.
- P69 L24: Is this interpretation of “stable to widening” correct, particularly the “stable” aspect? It would be useful to see a figure similar to Figure 3.13 for the extent downstream of Overtop Bridge; is the average XS difference shown in Figure 3.14 that much different for this reach than for Cottonwood Reach? Greater cross-referencing to the evidence would help here.

Wetted Width

Hydraulic numerical modelling was used to predict inundation area along the study area for selected years. Models before 2016 were constrained by available topographic information and were thus 1D. Models for 2016 and thereafter were 2D as LiDAR data was available. These numerical modelling approaches are appropriate for the nature of the river and the available data. I have a number of comments and questions about this analysis:

- Why was a value of 2,000 cfs used here (compared to 2,500 and 5,000 cfs used in the previous section).
- Were any of the numerical models calibrated / validated with observation data? How well do they perform? A range of metrics are available to assess 2D hydraulic model predictions;

Section 2.51 of Masafu et al. (2024) provides a summary of some of these techniques. If information is available in other reports then a summary of model performance would be valuable here.

- Figure 3.15 shows a large difference between 2012 and 2016. The interpretation (P71 L57-75) focuses upon a large flood event in 2015 causing substantial change downstream of Overton. I think further evidence to clarify the magnitude of this change is warranted. (i) Does analysis of satellite imagery (e.g. Chapter 2 comparisons of imagery) agree with this? (ii) This change is coincident with a change from 1D to 2D numerical modelling and a change in both how flow is numerically simulated and how maps are plotted in GIS. It would be useful to know how sensitive the model predictions are to this change; an appropriate way to evaluate this would be to use the 2016 LiDAR survey to provide topography for a 1D HEC-RAS model to assess how predicted wetted widths compare to the SRH-2D model/
- Figure 3.15: What is the unit on the y axis?
- P71 L66-68. It is good to see this statistical comparison; I encourage this approach elsewhere, where viable.
- Figure 3.16: This is a good summary figure for characterising 2021.

Sinuosity of the J2 Return Channel

This is a comparably short section. I don't have any comments or items needing clarification.

Analysis of incision in the vicinity of Station 70,000

This section includes a detailed analysis of one reach of the study area. Figure 3.19 is nicely interpreted; the overlay on the imagery is clear. I have one query:

- Figure 3.20. I think the discharges for 2016 and 2021 are different. Can the discharges be labelled or imagery with similar discharge be found (see an earlier comment about this for figures in Chapter 2).

6. Review of Chapter 4: Volume change analysis

The review of Chapter 3 takes a similar form to that for Chapters 2 and 3; a review of each of the components of this chapter is provided, focusing upon queries or points of clarification. The material below is intended, for this chapter, to answer the same questions outlined in the introduction to the review of Chapter 2. However, an additional section focuses on answering Technical Questions 6 and 7.

Review

This chapter presents a volumetric change analysis, using two topographic LiDAR surveys obtained in 2009 and 2012, and six topographic-bathymetric LiDAR surveys obtained between 2016 and 2021. The reported dry and wet accuracies of the LiDAR surveys (Table 0.3) are typical of similar surveys reported in academic literature. Systematic error between surveys was minimised by post-processing LiDAR using a common LiDAR flight as a control. This enables varies analysis, the most salient of which I consider to be the longitudinal analysis of volumetric change, presented in Figure 0.8 (P97). This figure also shows cumulative change segmented by the processes of lateral erosion and bed

aggradation/degradation. It is valuable to try to distinguish the causal mechanisms for channel change but I do have some comments and questions associated with this approach:

a. P84 L24. Earlier literature could be cited to refer to the morphological approach. See Vericat et al. (2017), which you already cite, for relevant references.
b. P84 L34-35. Similar to a comment above for Chapter 3, I think it would also be useful to refer to depositional mechanisms here; not all processes in the reach are degradational (as the analysis in this chapter also evidences).
c. P85 L50. Can you refer to the range of actual errors (Table 0.3 P93) rather than typical errors reported in literature? I accept you do this later in this section, and in Table 0.3, but I think these different values could cause confusion to a reader that isn't technically proficient in LiDAR capture/analysis.
d. P85 Section 1.27.1.2 "Hydraulic data". This is an example (together with the LiDAR data) of where there could be cross-referencing through the report and the report could be made more concise by organising common methods into one location (see earlier comments on this). Similarly, I found the material on P87 L97-104 and P88 L109-130 to be rather contextual and more suitable for an introduction rather than methods section.
e. Table 0.2. How were these volumes calculated?
f. P90 L150. Can you provide more information on what hydroflattening means, targeted at a decision maker reader? This is important for the subsequent interpretation, as it is an important source of bias. I think that there is considerable uncertainty associated with the assumption of a constant depth along the river to correct for this (P90 L150-159) and the uncertainties associated with this assumption should be more clearly explained in table and figure captions (rather than just labelling it) and also in the interpretive text that explain results from this assumption.
g. For the LiDAR post-processing, how effective were any filters to remove vegetation or other objects from the DEMs? How many samples points were acquired to assess the accuracies reported in Table 0.3 (P93); what were their distribution? What is the median grain size in the reach and how does this compare to the reported vertical errors (i.e. what is the roughness of the dry, unvegetated topographic surface)? Does the table of wet/dry errors measure errors before or after LiDAR were post-processed to "minimise" systematic error. It would be really useful to see a comparison of the errors before and after this post-processing step, if a suitable number and spatial distribution of "check" points are available.
h. The segmentation of the "lateral erosion" component of the volumetric budget relies upon differencing hydraulic model results. Based on Figure 0.5 it is likely that this method works reasonably where there is bank erosion on either the left or right bank of the active channel (i.e. where banks are relatively high) but I would question how consistent and effective this approach is along anabranches within the active channel where topographic variation is of a lower magnitude. Would manual interpretation of the DEMs of Difference also be valuable (e.g. in a similar manner to Wheaton et al., 2013); lateral erosion usually has a longitudinally extensive "shape" and a relatively large vertical magnitude relative to "patches" of geomorphic change caused by other geomorphic mechanisms. A verification of the hydraulic modelling differencing approach could be conducted by comparing this automated approach to expert manual interpretation.

i. Aside from “lateral erosion”, all other changes are “lumped” together whether they are associated with erosion or deposition. To interpret the dynamics of the study area, I think it would be beneficial to more extensively segregate erosion and deposition within this residual class (e.g. on the bottom panel of Figure 0.8 showing erosion and deposition separately as well as the net).
j. Figure 0.11: The sub-figure is missing vertical and horizontal scales; its presentational quality needs to be improved.
k. Interpretation on P104 L394-395. “Dynamic but stable with no observable signal from augmentation”. Do you know that all sediment generated from augmentation has been deposited in upstream reaches; a cumulative, segmented budget similar to that shown in Figure 5.2 in Vericat et al. (2017) would enable you to quantitatively analyse where sediment is being stored along sub-reaches.

Answers to Technical Questions

LiDAR data was processed to remove non-random error to the degree possible. Accordingly, two and three-dimensional change analyses did not utilize methods (e.g., thresholding) to quantify and/or account for error. Was that an appropriate approach? Would you suggest an alternative approach to quantifying and addressing error?

The reporting of vertical errors before and after post-processing to minimise systematic errors is not sufficiently detailed for me to be able to assess the effectiveness of this correction (see comment g above). It would also be useful to know more about the roughness (i.e. grain size) of the active channel to discern what the influence of roughness may be on the character of the LiDAR derived surface. I also envisage that the presence (and likely removal) of vegetation from the LiDAR derived surfaces (e.g. sparsely vegetated bars) will also contribute uncertainty that is more spatially variable in character than that which has been systematically corrected. In my experience, I have conducted and reviewed very few sediment budgeting analyses that haven’t quantified uncertainties, often although not always using the types of probabilistic thresholding approaches reviewed by Vericat et al. (2017). Although the authors cite Anderson (2019), Anderson still argues that estimated uncertainty bounds should be part of a quantitative estimate of volumetric or mean change (second bullet point in the conclusion e.g. Figure 11 and Table III of Anderson’s (2019) example). I recommend that either Vericat et al.’s (2017) and/or Anderson’s (2019) approaches could be used to characterise uncertainty and enable more rigorous assessment of whether some of the observed changes are indistinguishable from zero within uncertainty.

Is the method used to separate lateral erosion from bed aggradation/degradation appropriate? Would you suggest any alternative method be utilized?

See comment (h) above for my answer to this question. In summary, the approach used may be appropriate but I would like to see a comparison to manual expert interpretation to enable a quantitative assessment of the quality of its ability to segment this type of geomorphic change.

7. **Reproducibility**

It is clear that some modelling results that have been undertaken for previous reports are not accessible and/or not reproducible. Are there plans to ensure that the data and analysis that has been undertaken for this report are long-term digitally accessible?

8. Publication, and space to move and augmentation sustainability

I consider that the results presented in this report would be of interest to both the scientific and applied river management community. I know that the authors are considering dissemination in a journal format. With appropriate contextualisation, I consider that the data and interpretation would be suitable for journals such as *Water Resources Research* or *River Management and Applications*.

Below are some broader comments. These are related to the broader contextualisation of the challenge and/or consequences of the findings. They may be useful here or for the framing of a manuscript (see below).

Space to move: One section of the report notes management interventions to prevent geomorphic change adjacent to a property. Lateral erosion is identified as a key mechanism for sediment generation but this commonly requires space for a river to move. Interesting recent reviews on this topic include Nelson et al. (2024) and Brierley et al. (2023).

Sustainability of augmentation areas: My interpretation of Figure 0.5 is that the 2017-2022 source of sediment from the augmentation areas is material from unvegetated / sparsely vegetated river bars. This material is being "pushed" into the low flow channel. How sustainable is this source (I note that there are other areas where material can be obtained from)? What are the longer-term consequences for "natural" replenishment of these areas and the morphological adjustment of this reach? During a normal (i.e. without dam controls) range of flow variability bars will be inundated and are key sources and stores of sediment; their functioning in this reach will likely be changed as a consequence of topographic lowering albeit within the context of the flows that they are now exposed to. Does this limit how hydropeaking along the J2 Return Channel could, if viable, be used as a possible approach to mobilise sediment in this reach (I note from Figure 0.2, P88 that large flows can be conveyed down this channel e.g. in 2015). The final page of the report discusses dam retrofitting to enable sediment bypassing; this is an interesting idea that could mitigate the challenge of where to obtain material for sediment augmentation.

9. Minor comments

- Section numbering: something has gone wrong and most sections start "0." I use the likely erroneous section numbering that is in the main report document (rather than the contents page numbering, which I think is correct) to refer to particular sections in my comments above.
- Page (P) 8. Section E.1. Consider changing the start of this sentence – it's a big leap for a reader with no prior knowledge to be expected to know what a Big Question is.
- P8 Line (L) 18. Can the caption be expanded to provide more context for the summary.
- P8 L21. More context on the river before hydropower impacts is needed.
- P9 L38-39. Reference to the high flow dynamics during these years would be useful.

- P9 L43. An average is then presented then a range. Need to be consistent or provide greater explanation of these different formats.
- P9 L56. Is the word “specific” needed?
- P16 L140. When will the proposed flow regime be implemented? More context needed.
- P18 L222. What are the “other” geomorphic metrics?
- P27 L48. These places aren’t marked on the maps, or different acronyms are used; see comment above on consistency in geographic reference points on figures.
- P50 L31. This paragraph is relatively general at this stage of the report. The overall aim of the report doesn’t need to be repeated, you can go straight to the chapter objective (next paragraph). Edits such as this will help to make the report more concise.

Answers to general and technical questions

General questions

Does the Sediment Augmentation Data Synthesis Compilation adequately address the overall objective – to synthesize multiple lines of evidence from the Program's full-scale sediment augmentation experiment to assess overall results and provide useful information for decision making related to future sediment augmentation management actions?

Overall, I consider that the report has the correct overall experimental framework to address the Big Science Question: "Is sediment augmentation necessary to create and/or maintain suitable whooping crane habitat?" In particular, it presents a wide ranging set of different lines of evidence. However, I do consider that there are a number of issues that require clarification or additional supporting analyses that could help to support some methodological assumptions that have been made, including the characterisation of uncertainty in the volumetric change estimates. I recommend that the Executive Summary is carefully revisited following any revisions and, in particular, a graphical diagram that summarises the various conclusions for different segments of the river may help to summarise the information for decision makers.

Do the authors draw reasonable and scientifically sound conclusions from the information presented? If not, please identify those that are not and the specifics of each situation.

This question has been addressed in the section titled "Specific comments and recommendations on the report".

Are there any seminal peer-reviewed scientific papers omitted from consideration that would contribute to alternate conclusions that are scientifically sound? Please identify any such paper including citations.

This question has been addressed in the section titled "Specific comments and recommendations on the report" and a reference list to the associated citations is provided below. I note that the report doesn't contextualise the effects of sediment augmentation with other global examples; this would be likely required for publication and the authors may wish to consider broader contextualisation for this report. As a starting point, I would recommend the review by Staentzel et al. (2020).

Are the statistical methods and modeling tools used valid and current, and are the associated result presented in a manner useful to Program decision-makers?

This question has been addressed in the section titled "Specific comments and recommendations on the report".

Are potential biases, errors, or uncertainties appropriately considered within the methods sections and then discussed in the results and conclusion sections?

This question has been addressed in the section titled "Specific comments and recommendations on the report".

Technical Questions:

LiDAR data was processed to remove non-random error to the degree possible. Accordingly, twoand three-dimensional change analyses did not utilize methods (e.g., thresholding) to quantify and/or

account for error. Was that an appropriate approach? Would you suggest an alternative approach to quantifying and addressing error?

Is the method used to separate lateral erosion from bed aggradation/degradation appropriate? Would you suggest any alternative method be utilized?

Both these questions have been addressed in the sub-section on Chapter 4 within the section titled "Specific comments and recommendations on the report"

Rating and recommendation

I note that I don't have a definition for what the different categories mean, so my interpretation of them is thus subjective. However, I hope that the relative differences between my ratings for different sections helps to differentiate where I consider the overall strengths and limitations lie. I have provided brief justification for my ratings.

Scientific soundness = **3**

- An enormous amount of effort has been spent acquiring, processing and interpreting data. However I consider that some clarifications are needed, which would enable this score to be increased.

Degree to which conclusions are supported by data = **3**

- These are generally sound but I consider clarifications are needed to ensure that conclusions are appropriate. Addressing these would enable this score to be increased.

Organisation and clarity = **4**

Cohesiveness of conclusions = **3.5**

Conciseness = **3**

- See comments on presenting a clearer contextualisation, organising methods in one place and using more cross-referencing to avoid repetition.

Important to objectives of the program = **5**

- As noted at the very start of my report, I consider the data that has been acquired and overall monitoring strategy to be excellent and state of the art for this applied management problem.

Recommendation: **Accept with revisions**

References

Anderson, S. W. (2019). Uncertainty in quantitative analyses of topographic change: error propagation and the role of thresholding. *Earth Surface Processes and Landforms*, 44(5), 1015-1033. doi:10.1002/esp.4551

Bangen, S. G., Wheaton, J. M., Bouwes, N., Bouwes, B., & Jordan, C. (2014). A methodological intercomparison of topographic survey techniques for characterizing wadeable streams and rivers. *Geomorphology*, 206, 343-361. doi:10.1016/j.geomorph.2013.10.010

Boothroyd, R. J., Williams, R. D., Hoey, T. B., Barrett, B., & Prasojo, O. A. (2021). Applications of Google Earth Engine in fluvial geomorphology for detecting river channel change. *WIREs Water*, 8(1), e21496. doi:10.1002/wat2.1496

Brierley, G. J., Hikuroa, D., Fuller, I. C., Tunnicliffe, J., Allen, K., Brasington, J., Friedrich, H., Hoyle, J., & Measures, R. (2023). Reanimating the strangled rivers of Aotearoa New Zealand. *WIREs Water*, 10(2), e1624. doi:10.1002/wat2.1624

Church, M. (2002). Geomorphic thresholds in riverine landscapes. *Freshwater Biology*, 47(4), 541-557. doi:https://doi.org/10.1046/j.1365-2427.2002.00919.x

Crameri, F., Shephard, G. E., & Heron, P. J. (2020). The misuse of colour in science communication. *Nature Communications*, 11(1), 5444. doi:10.1038/s41467-020-19160-7

Masafu, C., & Williams, R. (2024). Satellite Video Remote Sensing for Flood Model Validation. *Water Resources Research*, 60(1), e2023WR034545. doi:10.1029/2023WR034545

Nelson, A. D., Collins, V. D., Payne, J. S., & Abbe, T. B. (2024). Proactive river corridor definition: Recommendations for a process-based width optimization approach illustrated in the context of the coastal Pacific Northwest. *WIREs Water*, e1711. doi:10.1002/wat2.1711

Slater, L. J., Singer, M. B., & Kirchner, J. W. (2015). Hydrologic versus geomorphic drivers of trends in flood hazard. *Geophysical Research Letters*, 42(2), 370-376. doi:10.1002/2014GL062482

Staentzel, C., Kondolf, G. M., Schmitt, L., Combroux, I., Barillier, A., & Beisel, J.-N. (2020). Restoring fluvial forms and processes by gravel augmentation or bank erosion below dams: A systematic review of ecological responses. *Science of The Total Environment*, 706, 135743. doi:10.1016/j.scitotenv.2019.135743

Vericat, D., Wheaton, J. M., & Brasington, J. (2017). Revisiting the Morphological Approach: Opportunities and Challenges with Repeat High-Resolution Topography. In D. Tsutsumi & J. Laronne (Eds.), *Gravel-Bed Rivers: Process and Disasters* (pp. 121-158). doi:10.1002/9781118971437.ch5

Wheaton, J. M., Brasington, J., Darby, S. E., Kasprak, A., Sear, D., & Vericat, D. (2013). Morphodynamic signatures of braiding mechanisms as expressed through change in sediment storage in a gravel-bed river. *Journal of Geophysical Research: Earth Surface*, 118(2), 759-779. doi:10.1002/jgrf.20060