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Referee Report

"Appendix J, Deriving potential fine sediment attribute thresholds for the National Objectives Framework, Franklin et al 2019"

NIWA report for Ministry for the Environment

Overview
My brief was to assess the method described in full in Appendix J of Franklin et al. (2019). It was also suggested that undertaking that job would require general familiarity with the environmental classification system results shown in Appendix D and that I should not require more information on the general context and application of the research than that provided in the executive summary of Franklin et al. (2019). I found the Executive Summary difficult to follow and that it lacked justifications for various aspects of the approach I wished to understand, so I ended up reading much more of the full report.

I was also reluctant to take on this review job because when I first read the Executive Summary I thought that the whole approach taken was problematic. Essentially, I didn't want to review the details of a methodology that I thought was based on a flawed approach in the first place. However, I was encouraged to undertake the review in the spirit of providing useful feedback to the Science and Technical Advisory Group (STAG) and the authors. I have structured my report to document my fundamental problem with the approach followed by a critique of the methodology in Appendix J and a number of other more specific points about parts of the Franklin et al (2019) report. I couldn't adequately review Appendix J without dealing with the other issues mentioned.

Adjusting limits based on spatial variation in a sediment classification
Appendix J (and D) is based on the assumption that it is necessary to adjust fine sediment limits nationally according to landscape variations in fine sediment which result in natural variations in ecological communities. The rationale given in the report is:
"*Because the tolerance of different species to fine sediments varies, it can be expected that biological communities will vary in space concurrently with natural variations in sediment state, if the magnitude of natural variation in sediment exceeds the tolerance range of different species. Consideration must, therefore, be given to accounting for natural spatial variations in ecosystem structure and function associated with natural variations in sediment state when defining sediment attributes.*"

I agree those ecological communities are a good indicator of the impacts fine sediment, that fine sediment varies across the landscape, that macroinvertebrate and fish communities vary in response, but I don't agree that means limits should be adjusted to reflect those variations in natural communities in the way suggested. It is clear that more than high rates of deposited fine sediment cover degrade macroinvertebrate communities (appendix A) and that measures like %EPT are a good indicator of that degradation. In fact, a main conclusion of Appendix A is that, "… EPT metrics were a good indicator of deposited fine sediment effects" (p118). Similarly there is good evideince for effects on fish (Appendix C). However, just because a flat region of the country tends to accumulate more fine sediment, the impact on those ecological values (and life supporting capacity) isn't less! It means a flat area will be *more* prone to degradation, and it certainly doesn't mean the limits set there should be discounted.

Of all the stressors in the NOF, my experience suggests the ecological effects of fine sediment are the most clear and obvious, and the shape of the stressor-response relationship is the best defined. Various authors have called it a 'master stressor'. I think the approach taken is making what should be a comparatively simple process overly complicated. By applying a spatially dependent approach the methodology is also out of line with most other limits set for other NOF attributes. My recent experience at the coal face of stream restoration with managers and farmers also suggests twelve spatially varying sets of limits will be very difficult to implement or for the public to understand. Finally, taking this approach forces a reliance on a modelling approach for justifying limits which is very difficult to grasp and justify (see below). I found Table 1.1 incomprehensible after just reading with Executive Summary, for example.

Methodology in Appendices D & J

Determining a spatially-varying reference state for communities and thereby calculating deviation from that reference state is problematic for a number of reasons:

1. If I'm interpreting the classification and clustering of landscape-dependent fine sediment classes correctly (Appendix D), it is based on data which includes those from already impacted locations. If the classification is to accurately define classes, then it would need to be based on data from non-degraded sites. Thus, I worry that the classification is not a fair representation of the fine sediment state associated with locations because I can't see an attempt to deal with the already degraded state of many of the location used.

2. The absence of actual reference sites for both fish and macroinvertebrate data for a number of spatial classes considerably reduces the rigour because reference state has had to be 'estimated'. I also found it hard to distinguish the rationale for what was regarded as a reference site or to determine what the effect of 'reference site estimation' had on the relationships. It would be useful to see some comparison of sites that were estimated versus those not, and a more comprehensive justification of what constitutes a reference site.

3. There is an assumption (appendix D) that the REC framework represents the underlying processes that govern fine sediment distribution and this has not been adequately justified. I was expecting an analysis of actual fine sediment measures – did I miss it? For example, springs are an example of a habitat which are highly sensitive to fine sediment, but the REC is notoriously poor at identifying them in the landscape.

4. The Community Deviation (Appendix J) method had to 'fill in' missing data in some (many?) cases using adjacent sites. In my experience fine sediment is highly spatially variable within streams and most spatial variability is accounted for by within reach alterations. For example, unpublished analysis of multiple years of fine sediment data from 6 CAREX (www.carex.org.nz) sites in lowland Canterbury indicates that 62% of variation in sediment cover occurs at reach scales and between stream variation accounts for only 28%. This gets worse if local 'hot spots' of sediment are included. Thus, I'm sceptical that extrapolating from adjacent reaches (and certainly not different streams) would provide reliable measures of sediment cover.

5. One community assembly process was considered (competition/predation with trout) when creating models of community structure, but there are many other community assembly processes which are not included in the processes of determining which species might occur at a site. What about flood disturbance which is a main driver of community structure in both fish and invertebrates and we know it is not adequately represented by landscape variables?

6. The fish species modelled to produce the fish limits do not include a non-migratory galaxiid. This is an important gap given that they incorporate the most threated group of freshwater fish in New Zealand. They are also likely to be particularly vulnerable to fine sediment accumulations because they use interstitial spaces as refuges, especially during low flow (see comments on Appendix C below).

Overall, I remain unconvinced about the rigour of the Community Deviation approach. The methodology is complex and difficult to evaluate. My biggest concern is that it appears to rely on layer upon layer of prediction. Each stage seems to involve large amounts of extrapolation or estimation. In that case, one would want to see some verification using data which were actually independently measured rather than training data sets. Statistical measures of uncertainty would also be useful. There are many levels of prediction, but it is hard to evaluate what level of confidence we could have in them.

Comments on the Executive Summary (ES) of Franklin et al 2019
I found the terminology used confusing in places. 'Disturbance' is commonly used to refer to natural alterations to freshwater ecosystem structure (e.g., from floods), so to interpret the NPS-FM values as referring to "structure and function… expected under minimally disturbed conditions" is ambiguous. The term disturbance is being confused with 'altered'.

The 2nd sentence of paragraph 3 does not make grammatical sense, so the statement about the mechanisms leading to accumulation of fine sediment is not clear.

Para 4 – species and life stages of what?

The derivation of the sediment state classification is not explained.

The rationale for adjusting limits based on spatial variation in a sediment classification is not explained. This is an important omission because anybody wanting to quickly understand the derivation of the limits cannot do so based on the ES.

Comments on Appendicies A-C
Appendix A is difficult to evaluate scientifically because not enough results information is provided to allow an evaluation of rigour or outcomes. An important limitation is that it doesn't provide a clear mechanistic basis for proceeding with limit-setting because the mechanistic hypotheses shown in Figure 1-A are not explicitly tested, at least as shown by Table A-2. It also doesn't appear to have used NZ work which has examined the mechanistic drivers of fine sediment effects (e.g., Burdon 2013 PhD thesis, another chapter recently published as Burdon et al 2019).

The lack of helpful mechanistic analysis in Appendix A also makes Appendix B less useful because cause and effect relationships between suspended sediment and macroinvertebrates have not been rigorously determined. This work needed some insightful analysis utilising a smaller number of high quality datasets to determine the *shape* relationships between the stressors and ecological responses.

Appendix C presents a much more useful analysis because it links specific cause and effect mechanisms with both deposited and suspended fine sediment. However, one of the potentially most important relationships, the effects of fine sediment of access to low-flow refuges used by non-migratory galaxiids looks to have been missed. Work by Nicholas Dunn (2003) for alpine and Canterbury galaxias shows that fine sediment accumulation restricts the ability of alpine and Canterbury galaxias to burrow into the substratum intersticies, potentially affecting drought survival. Nicholas may has possibly carried out similar experiments on other threatened galaxiids (e.g., lowland longjaw galaxias). Glenjarman (2017) has also been overlooked.

Refrences
Burdon, F. J., McIntosh, A. R., Harding, Jon S. (2019) Mechanisms of trophic niche compression: evidence from landscape disturbance. Journal of Animal Ecology, doi: 10.1111/1365-2656.13142.

Burdon F.J. (2013). Impacts of sedimentation on the structure and functioning of agricultural stream communities. *Unpublished PhD thesis, University of Canterbury*.

Glenjarman, N. (2017). The impact of suspended and deposited fine inorganic sediment on New Zealand freshwater fishes. *Unpublished MSc thesis, University of Canterbury*.

Dunn, N. (2003). The effects of low flow on Alpine (*Galaxias paucispondylus*) and Canterbury (*G. vulgaris*) galaxiids. *Unpublished MSc (hons) thesis, University of Canterbury*

Angus McIntosh, 10 January 2020