SRSL 2013

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Report Title	Scientific Peer-review of monitoring results from New Zealand King Salmon Farms
Client/Customer	Marlborough District Council, Blenheim, New Zealand
SRSL Project Reference	00989_P0657

Document Number	P0657_001

Revision History

Revision	Changes	Date
01	First formal issue	09/08/2013

	Name & Position	Date
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1. Preamble

This report was produced for Marlborough District Council (MDC) by SRSL. Parts A and B are responses to similarly labelled sections of a document provided by MDC entitled "Questions for peer-review of Council's preliminary analysis of consent compliance". A variety of environmental impact reports from the Cawthron Institute (cited below by their 4 digit report number) were provided for information as was a report on the ES methodology. Subsequently, additional questions were provided by MDC and responses to these are given in Section C. This review was also informed by a letter from the Cawthron Institute to The New Zealand King Salmon Co. Ltd (NZKS Ltd). (24/7/13) and by a letter from NZKS Ltd. to MDC together with a "Summary of Comments to the MDC's Preliminary Analysis of Consent Compliance and Findings of Peer review" both 25/7/13, together with comments on an early draft from MDC sent on 5/8/13.

2. Section A

2.1 Waihinau Bay 2012

"The authorisation in force at Waihinau is a Marine Farm Licence (#456), which pre-dated the Resource Management Act (RMA) 1991, but was grand-fathered over to the RMA as a deemed coastal permit in 2006. The permit allows for up to 3000 metric tonnes of feed to be discharged, but has no standards associated with the environmental effects of the discharge. The consent holder is voluntarily applying for a variation to that permit to incorporate monitoring obligations and enrichment scale standards."[1]

This site experienced a fish kill in 2012. In the absence of other explanations and after some efforts to test for disease organisms or other sources of insult, the effect was thought to be caused by chronic exposure to hydrogen sulphide.

As the monitoring report (2274) points out, the sediment sulphide levels were not particularly high when measured in October. The evidence presented in the peer-review brief as bullet points do not immediately lead me to the conclusion that sulphide was the cause (or main cause) of the mortality. It would have been interesting to know if the gills and livers of the dead fish were examined histologically. Although not diagnostic for hydrogen sulphide (Black *et al.*, 1996a; Black *et al.*, 1996b; Black *et al.*, 1994; Kiemer *et al.*, 1995), such examination may at least have been able to rule this hypothesis out.

On the basis of the evidence given to me, I believe that there must remain some doubt as to whether hydrogen sulphide was the cause of the mortality, a view shared with the consent holder who believes that "it is unlikely to be the cause of the mortality onsite". The MPI Information paper (4/7/2012) states in its executive summary: "No cause for the excess mortality has been identified". So, can we be certain that this "adverse effect" was a consequence of the environmental performance of the farm? I do not think so. This was the only evidence given in the brief for the MDC non-compliant determination, so that judgement does not appear to me to be sustained. The consent has no discharge standards for compliance, except a general duty under section 17 of the RMA to avoid, remedy or mitigate adverse effects. If we take the reasonable view that, pending an application by NZKS to vary the consent to put in place enrichment stage (ES) standards, the site can

[1] MDC communication 05/08/13

be assessed using the most recent version of Cawthron's ES scale (#2080) as for the other sites. All of the near-pen sites are ES<5, but the report (page 12) admits that the 50m station at the Zone 1-2 boundary exceeds the equivalent ES for that zone for low flow sites (3.5). Thus, on this basis, the site could reasonably be deemed "non-compliant" if a more modern consent was in place.

Although there is insufficient evidence to attribute the fish kill to an environmental insult caused by the farm itself, a major fish kill is a serious matter and a reasonable condition should be placed on fish farmers such that significant mortality events should be reported immediately to enable detailed analysis while the causes are still present.

MDC states: "It is our view that there is little, if any, resilience left in the benthic environment to assimilate large quantities of organic matter. However, no reductions in the discharge of feed are sought for the 2013 year provided the projected discharge of 429 tonnes is not exceeded."

I entirely agree with the first statement (but see later discussion in section C) although it would be difficult to quantify "large".

"Council is concerned that the benthos may not be able to assimilate maximum monthly feed levels as fish reach adult size. The following questions arise:

a. What is the scientific advice on the sediment enrichment levels prior to harvest under this model?
b. What is the scientific advice on the sustainability of the life-supporting capacity of the ecosystems under the pen under this single age-class model, and the length of fallowing time required?"

a,b. The maximum monthly feeding rate predicted at Waihinau for the period May-13 to Nov-13 is 169 tonnes. Previous feeding rates have been much higher- up to a mean of 390t/M in 2004 (2274, Fig.2). Unfortunately there are no data given for the historic peak feeding rates at Forsyth (Figure 2, 2276) but dividing the 2010 total input (~330t) by 12 months gives a mean monthly feeding rate of 275 t. This is about 15% greater than the maximum rate proposed in table 1 (239t) and so should result in a lower impact than in that year.

Table 1. Feeding plans supplied by NZKS Ltd to MDC on 19 July 2013.

		Grand Total	
		Feed amount in period - modelled	
Site	Closing Month	Forsyth Bay	Waihinau Bay
	May-13	166,399.90	1,229.60
	Jun-13	163,417.30	21,348.60
	Jul-13	161,477.40	38,248.80
	Aug-13	169,684.90	54,951.20
	Sep-13	159,431.40	73,605.70
	Oct-13	75,530.10	109,670.20
	Nov-13	3,197.40	136,998.10
	Dec-13	0.00	159,973.70
	Jan-14	0.00	174,046.90
	Feb-14	0.00	170,947.20
	Mar-14	0.00	210,890.30
	Apr-14	0.00	219,509.20
	May-14	0.00	222,443.00
	Jun-14	0.00	239,066.20
	Jul-14	0.00	165,282.70
	Aug-14	0.00	152,716.20
	Sep-14	0.00	122,042.90
	Oct-14	0.00	60,576.70
	Nov-14	0.00	0.00
Forsyth Bay		739,113.90	2,333,547.40

My personal view, and one which has been largely accepted in Scotland, is that farms should be managed such that at peak feeding rate during the farming cycle they stay well within sediment quality standards. In Scotland, all farms are single year class (the same class for multiple farms in the same water-body) and most are farmed for 22 months with a 2 month empty period to control diseases/parasites. The numbers of fish input at the beginning of the cycle and the harvesting plan are key to managing the impacts in the second year of the cycle when high biomass is maintained through progressive harvesting. In my opinion, farms should be managed at a scale in keeping with the local environmental capacity such that no long site rotation periods are necessary (Pereira *et al.*, 2004). I quote from a review I was involved in a few years ago (Black *et al.*, 2008):

"Fallowing is a term often used for 2 distinct processes: the period of a few weeks between farming cycles when fish are absent from a site after harvesting and before the next restocking – primarily to break disease cycles; and the practice of site rotation where a site may be left empty for one or more years for the sediments to recover. Site rotation has been recommended both by regulators and by scientists (e.g. Carroll et al (2003)) as a method of reducing benthic impacts by allowing time for recovery. However, there is evidence that such site rotation merely allows an otherwise unsustainable site to remain in production on a periodic basis (Pereira *et al.*, 2004). A better solution would be to limit the scale of production at any site such that it does not break EQS's even after repeated farming cycles i.e. within the assimilative capacity of the site."

In order to predict the production level that might be sustainable indefinitely over time in a single year class system, a combination of modelling, monitoring and long-term adaptive management is required to adjust farm size to best fit the environment. A first approximation would to scale the production plan so that the peak feeding rate is no higher than that which has been shown in multi-cycle years to have resulted in compliance.

2.2 Forsyth Bay 2012

MDC states: Consent U040412 is non-compliant with condition 17.

The ES methodology determines ES from a range of weighted groups of parameters. MDC are right to state that ES4 is usually associated with redox values between about zero and +100 mV (2080, Figure 4), whereas the monitoring values are between approximately -100 and zero mV (2276). However, the values measured for sulphide appear to be consistent with ES4 (~500 - ~1200 µM) and sulphide is strongly related to both redox and oxygen dynamics. In any particular determination, it is unsurprising that one parameter is out of the normal range. What ES is attempting to do is to reduce the subjectivity of determinations and to replace this with a more empirical statistical approach.

Significantly, the monitoring was conducted shortly after an 11 month fallow period. It is impossible to know what the redox conditions were through the fallow period, but the fact that they were negative at the end of this period hardly augers well for later in the cycle. I suggest in future that monitoring is done during the worst-case rather than the best case time in order to capture environmental performance during peak stress.

I agree with MDC that the history of the site is not encouraging and that this site is likely to have a long recovery period. As it has so recently had a very long fallow period (2002 – 2009), consideration should be given to revoking the consent for this site and encouraging the company to find an alternative site better suited to its needs. On the other hand, it appears that the company are voluntarily reducing the maximum feeding rate proposed for the site and, given that alternative sites are apparently highly limited, another option is that this new management proposal should be closely monitored for another farming cycle prior to making a more strategic decision about the site.

MDC states after some discussion: "Therefore, Zone 1 conditions are the equivalent of ES 5 in the new model; i.e., peak assimilative capacity."

I agree with the MDC position. It seems to me that there is no doubt that the most appropriate ES classification for Zone 1 is ES5. The methodology paper (2080, page 2) states "At ES5, the benthos is still considered biologically functional, although it is associated with the greatest biomass, and is therefore thought to have the greatest waste assimilation capacity." I think this is broadly correct in the context of the definition and the description of ES5 elsewhere in the document and it certainly does not apply to ES6. However, it perhaps presents a rather rosy picture of what is after all a highly degraded habitat state. The report goes on: "Stages beyond ES5 (i.e. ES 6-7) are characterised by extremely impacted sediments and the collapse of infaunal communities, at which point organic accumulation of waste is thought to greatly increase." It seems to me highly unlikely that the intention of the authors of Zone 1 classification was to permit "extremely impacted sediments and the collapse of infaunal communities". If they really had meant ES6 they would have been perhaps alone amongst environmental regulators and would have had to explain their permissive thinking in detail. Mr Keeley makes the point in his rebuttal (August 2012, para 17.1) that as the only prohibited condition in these older consents is that the sediments are anoxic and azoic, this means that ES6 is a compliant state – although he accepts elsewhere that this is a highly undesirable state. However, as he also accepts that ES7 is unlikely to ever be totally azoic, the implication would be that no ES state could be described as non-compliant. This is an untenable position. What the regulators at that time were certainly attempting to achieve was a minimum standard of ecosystem health. I think everyone agrees that ES6 is not a standard of health but a warning of imminent benthic collapse.

Having said all this, it seems to me that table 1 in this and subsequent reports does not actually assign a level of ES6 to zone 1 - what it actually says is "less than ES6". I understand that this accepts that ES6 is not compliant.

2.3 Otanerau Bay 2012

MDC states: "Consent U040217 is non-compliant" and "Council requires Otanerau to be fallowed with immediate effect."

Given that the benthos at the Pen 2 station had all but collapsed this is completely reasonable and the report (2277) admits that the situation at Pen 2 "exceeded the ES for Zone 1". Interestingly, the redox values are not so different from those measured at Forsyth Bay. However, while at Forsyth the corresponding sulphide concentrations are quite low, at Otanerau bay they are 6000+ µM, very much in the "persistent anoxia" category *sensu* Hargrave et al (2008).

Clearly, immediate fallowing is warranted and I would suggest that the company either find a better site or considerably reduce the production from this farm to bring it into compliance. As mentioned above, I am not really in favour of using fallowing as a management strategy. Better to reduce the size of the farm or relocate permanently.

2.4 Ruakaka Bay

Given that we have already argued that near-pen stations with scores of ES5 but not ES6 are compliant,it is not clear to me on what basis the MDC state that the site is non-compliant. Sulphide concentrations are very high, particularly at the Pen2 station. Interestingly redox is not especially low - I am beginning to doubt whether redox is a particularly good indicator compared to sulphide, see later. The number of animals in the sediment (N) approaches an extremely high level (>400k ind. m^{-2}) only seen elsewhere at Te Pangu and only surpassed at Clay point, see later. However, contrary to the MDC statement, none of the individual samples exceed ES6 with the maximum being ES5.6 at a Pen2 replicate (2279, Appendix 4).

This being said, I entirely endorse all of the other comments made by MDC – this clearly is a site close to the edge. However, requiring a significant discharge decrease for the site for the remainder of 2013 may prove difficult and/or costly to comply with. Perhaps a more strategic consideration, with reduced discharges over a longer time frame, would be preferable both for the environment and for the Company's planning purposes.

2.5 Clay Point 2013

MDC states that the consent is non-compliant owing to breaking the consent limit (4,304 as opposed to 4,000t). This is a simple matter of fact.

Pen 1 and the 90m E stations are the most impacted. The large impact at 90m E is likely caused by the strong current having a residual flow in that direction – but I do not have this information. I would have liked to have known the depth at that station but of the 17 stations sampled, detailed information is only given for 9 (2275m, Table A1.1).

MDC state that the farm is in non-compliance in Zone 1 and Zone 2. I do not see how Zone 1 is in non-compliance. The 70m stations do bring Zone 2 into non-compliance (3.6 instead of <3.5) but this seems to me an artefact of not adjusting the zones to reflect the physical dynamics of the system - as the report's authors argue in their conclusion. The very high sulphide and low redox levels at the 90m E station are not reflected in the benthic information which appears to show a moderate impact.

Regarding the averaging of replicate samples: patchiness is a typical attribute of marine sediments: questions can be asked regarding the appropriate number of replicates to take. To achieve the same error in describing the benthic assemblage, it is likely that the appropriate number of samples would have to change as the diversity of the community changes. There is no *a priori* way of knowing what the optimal level of replication should be. However, if we consider accretion curves *post hoc,* i.e. the number of new species added per additional sample, then we can get a good idea of how well we have captured the assemblage. Three samples seem to me to be quite reasonable and proportionate given sampling protocols in other salmon growing countries (Wilson *et al.*, 2009). Once you have your three samples, for each determinand you can decide whether to consider means or to consider the worst case. I think in general it is better to use all of the data in a mean and report the errors but I can see a case for looking at the worst-case – but this would need to be specified in the ES protocol. The final point to be made here is what is a station? This is important as even small positional changes may yield different results on steep enrichment gradients – yet being moored in a stationary position may involve accusations of pseudo-replication (non-independent replicates). This is an unresolved, non-trivial topic. In any event the ES method uses means but MDC may be within their rights to take maxima into account in their determinations. I am less happy in averaging results from different stations to achieve a mean for a zone as zones may often contain steep gradients of organic enrichment and impact rendering an average across the zone rather meaningless.

MDC recommend that feed levels should be reduced to allow recovery. I agree that feed levels should be reduced to ensure that the farm can be farmed sustainably into the future without non-compliance – this is not really recovery.

Finally, the animal abundance of one replicate at Pen 1 is approximately 1 million animals per square meter – the highest infaunal abundance I have ever heard of!

2.6 Te Pangu Bay 2012

This site has a different type of consent than the others. Whereas other sites must have a maximum ES of <6 in the innermost zone, this site has a maximum ES of <5 as interpreted by the monitoring report's authors (2278, table 1). They also interpret the transition between stages IV and III as being equivalent to <4 and between II and I as ES 2.5. By these measures the site is non-compliant at Pen2, 60m, 200m and 200m-NW. It appears that MDC and Cawthorn dispute the precise transition zones e.g. between zone 2 and zone 3, with MDC taking a more conservative approach. I think that this is splitting hairs and rather than waste time arguing about very slight differences between 2 close points on a continuum (3.5 vs 4), MDC should simply state their interpretation and this should be accepted. In any case the site is still non-compliant at several stations.

As before, the high levels of sulphide in some stations of the main transect are not reflected by particularly low redox values.

MDC requires a reduction in feed discharge to 3500t. Given that the site has had a range of feed inputs in the past it might be possible to use previous environmental audits as the basis of an accurate prediction of the likely future impact of the site to justify the feed consent reduction required.

I accept the idea that at dispersive sites the shape and extent of zones should be adjusted otherwise the unintended consequence of more dispersive sites being disadvantaged occurs. This might bring some stations into compliance but would not affect the overall result i.e. a reduction of feed consent is required.

The peer-review brief at the top of page 10 presents an argument about the nature of continuums. In the paperwork that I have been given I do not see the arguments presented by Cawthron that are referred to in this section. The MDC proposition appears reasonable. All of the wide variety of impact scales that have been produced are more or less arbitrary (but useful) classifications of a continuum which represent a one-dimensional generalisation of multi-dimensional systems. The ES is similar in many ways to the work of Barry Hargrave (Hargrave, 2010; Hargrave *et al.*, 2008) in attempting to use the statistical distributions of the co-variance of some of the wide variety of parameters that are typically measured around fish farms. I would say that it is unlikely that any system will entirely obviate the need for subjective judgement by an expert. These classification systems can only ever hope to explain a proportion of the processes that cause variance in monitoring data. They are none the less very useful at capturing empirical observations.

3. Section B

3.1 i + ii Weighting and TOM

Before discussing weighting, I require a preamble on organic loading. The term organic loading conveys a sense of the amount of organic material (OM) we are adding to the seabed – linked to the idea of the flux or sedimentation rate i.e. the rate at which OM is hitting the seabed. Another concept is the accumulation rate which is the sedimentation rate minus the resuspension rate minus any other process that reduces the amount of OM on the seabed e.g. degradation (remineralisation), dissolution, biological transport. However, what is actually measured in the present ES context is total organic matter by loss on ignition (TOM by LOI). It is not surprising that in some studies TOM has not been shown to be a strong explanatory variable for benthic community structure. This is because what causes changes in structure is a change in the balance between supply and demand of oxygen. Oxygen supply will change with water temperature and current speed. Oxygen demand will depend on the degradation of organic materials with the most labile OM, such as fats and proteins, contributing most. When the demand is great with respect to the supply, the level at which oxygen is present in sediment pore waters rises from the typical few millimetres of coastal sediments to the sediment surface or into the water column – the benthic boundary layer. Sulphide reduction is ubiquitous in soft coastal sediments but its rate rapidly increases as the oxygen consumption rate (mainly from the re-oxidation of reduced intermediates such as iron, manganese and sulphide) increases with increased input of labile OM. The sulphide produced is toxic to animals, which display a range of tolerances. The measurement of TOM in sediments combines the labile OM which causes the impact on benthos plus non-labile OM (slowly degrading biopolymers such as cellulose and lignin). So a high TOM concentration may be composed of a relict and unreactive majority of non-labile OM together with a small amount of reactive OM causing the majority of the impact. Thus in some circumstances TOM concentration does not explain much of the variability in the benthic community. On the other hand, several studies have shown links between TOM by LOI and other commonly measured parameters. Given that this ES has been designed using information at the very farms under consideration here, it would seem plausible to use this parameter in a composite index of impact. This is especially so at the low flow sites where there is a reasonable strong relationship between %TOM and ES, but less so at higher flux sites where the relationship is much less good (Keeley *et al.*, 2012b). Given all this, it is unsurprising in the present context that Keeley et al. (2012a) state that "ES was a more sensitive indicator than %OM alone." Bear in mind that ES in that paper was assigned through Best Professional Judgement and that %OM was measured.

The brief specifically asks about the weights applied to the TOM, other sediment chemistry and benthos and points out that these have been changed to 1:2:7 in the site reports from the 2:3:5 in the methodology paper. The methodology paper states that the weightings have been devised in some way so as to reduce weighting on variables that are less good indicators of ES. I could not find any other information on the weighting process in the methodology paper or in the principal author's published papers on this subject (Keeley *et al.*, 2012a; Keeley *et al.*, 2013a; Keeley *et al.*, 2012b; Keeley *et al.*, 2013b), but I may have missed this. Nor could I find in the farm reports any indication of why the weights had been changed. Given that this is a change to the method, this should have been documented, the reasons for the change justified and the consequences of the change examined.

The peer-review brief asks the following questions:

• Does the current weighting reflect the relative importance of these variables on the biochemical and ecological processes of sediment enrichment and the resilience of the assimilative capacity?

• Should the current weightings be reversed to reflect the relative importance of the relationship between the causative variable (organic loading), response (sediment chemistry) and outcome (infauna composition) variable?

• Are organic loading measures on an appropriate scale with respect to ES scores?

• How closely does the organic loading (observed during monitoring) reflect sediment processes in terms of time lag? In other words, is the enrichment state of the sediments responding to past organic loading?

I should explain that the Scottish system uses a wide range of benthic and chemical indicators but rather than weighting them into an overall environmental quality score, SEPA prefer to set sediment quality criteria (generally 2 criteria, one inside and one outside an AZE which is determined using DEPOMOD having shape and scale dependent on currents and bathymetry) for each indicator. Failure of any of these separate determinations would be regarded as unsatisfactory and trigger management action.

I have some problems with the discussion in the peer-review brief which states that the TOM is the cause, chemistry is the response and the benthos is the outcome. In my view all of these factors are interrelated and are co-outcomes of the sum of all processes natural and anthropogenic that pertain. For example, benthos affects chemistry through bioturbation and bioirrigation. Benthos is determined by chemistry and habitat and larval supply. Chemistry is affected by OM supply from the farm and the environment, OM loss from resuspension etc., water currents supplying oxygen, temperature, availability of electron acceptors, etc.

The question to be asked is what makes a good indicator? I would argue, along with probably every other expert in the field, that it is wise to consider all of the available evidence – as ES attempts to encapsulate in a single number. However, when presented with information on a fish farm site my natural inclination is to look first at the thing that we are trying to protect. What we trying to protect is the bioturbating power of the benthos. So I am interested in the density of animals, the number of taxa and various measures of biodiversity – I like AMBI and ITI which have both proven themselves as good indicators over a range of European fish farm sites (Borja *et al.*, 2009).

I am presently working on a project that is looking at the utility of sulphide as a determinand in Scotland. I had been rather sceptical but am beginning to form the view that sulphide may be a better indictor than redox, notwithstanding its methodological issues (Brown *et al.*, 2011) which relate to what it is actually measuring.

So, I am somewhat happy with sulphide even though I don't quite know what it is measuring, redox I find rather subjective in practice and TOM I don't regard as a very good indicator in principle although it may be in practice at low flow sites. I would therefore tend to favour a composite indicator that gave more weight to the benthic indicators.

I am very interested in the question relating to time lag. At present most models of fish farm impact pay scant regard to time. Via carbon degradation, and sulphide and oxygen dynamics, I am presently attempting to address this within the DEPOMOD context. Fish feed is for the most part made up of highly labile organic material. Once digested and expelled as faeces the majority but not all of this labile material is retained by the fish. The half-life of the labile material is of the order of a few weeks. The chemical response begins to be felt only a few hours after the waste feed or faeces hits the seabed and starts in earnest after a lag as the particle becomes colonised with bacteria. So I would argue that the major benthic response (oxygen demand) is closely tied in time to the input of material. If you switch off the OM supply the oxygen demand will drop off after a few weeks but may not drop back to background levels for some time, being sustained by oxidation of buried sulphides.

I have recently conducted a study on sediment recovery[2] and proposed that recovery time is strongly dependent on the re-oxidation of sulphides temporarily trapped in the FeS_x phase, which may continue to exert oxygen demand at a low levels for several years post cessation of farming. In addition, the relatively non labile fraction of the fish farm OM in the sediment will be supplemented by background supplies of additional OM keeping the sediments in an enriched state with both sulphide reduction and hypoxia retarding the recovery of the benthic community. Differences in background supply and, to some extent, the duration and intensity of previous farming, will determine the recovery rate – although physical factors may dominate at high flow sites.

The argument made in the brief regarding the Te Pangu site is to my mind over-complicated. TOM is not necessarily a good indicator of impact, at high flow sites at least – sulphide is likely to be much better as it is a direct chemical outcome of OM remineralisation. The reason for the observed results is likely not "because previous levels of organic loading have changed the sediment chemistry by inducing anaerobic processes". In even moderately enriched coastal sediments, anaerobic processes will dominate in the sediments below the surface as oxygen generally only penetrates a few millimetres at most.

I do not accept the "solution" proposed for this site. A more appropriate course of action would be to de-weight the TOM component altogether and spread the residue between chemistry and benthos. However, I would strongly urge that a piecemeal approach to weighting will only result in pressure to manipulate the results to obtain the desired result of the manipulator and will make the ES system unworkable. Instead, we should be told more from the model's authors about the statistical basis for weighting and whether there is any statistical merit in having different weightings at high flow sites. [3]

[2] http://www.sarf.org.uk/cms-assets/documents/43892-181648.sarf030.pdf

[3] In a response from Cawthron (24/7/13) point 4 discussed the issue of weighting. This accepts that the change in the weightings should be documented. I accept that the change may have resulted in an increase rather than a decrease in the ES score.

The Te Pangu story is repeated at Clay Point and across the world. High flow sites have greater impacts than models predict (Chamberlain and Stucchi, 2007; Chang, 2012) and relationships between model predictions and observed impact at high flow sites are paradoxically only revealed when resuspension is ignored (Keeley *et al.*, 2013b). This topic is presently the subject of major research programmes in Scotland and Canada. Given that progress in this area is likely in a couple of years or less, I would strongly advise that short-term measures to adapt ES for high flow sites are deferred until the basic science of resuspension around fish farms is better understood.

3. 2 iii Are ES states 5 and 6 sufficiently different ecologically to confidently distinguish them?

Accepting that dissecting a continuum into stages is prone to hazard, and the more the stages the greater the hazard, I do think that ES5 is broadly distinguishable from its neighbours. The peak in opportunist infauna is well documented. I think distinguishing between states more impacted that ES5 is more problematic as azoia is rather hard to define, as has been accepted. Also, azoia is fortunately a state that we rarely observe. I would suggest that an easier boundary to measure is that between ES5 - the peak, and ES6 - the downward slope to ES7. ES6 is characterised by extremes of chemistry – very high sulphide, very low redox, and by reduced abundances of worms (e.g. 2277, Appendix 4). It is not important to distinguish between ES6 and ES7 – both are unacceptable.

So I agree with the model's author that ES5 and ES6 are distinguishable and with MDC that ES6 is closer to ES7 than to ES5.

Regarding the difficulties faced by benthic indicators as sediments become increasingly less diverse with enrichment, this is of academic interest only in my view and does not cause the ecologist any difficulty in identifying a highly perturbed benthic community.

4. Section C

4.1 Council's view that previous compliance regime for consents is equivalent ES 5 or less. The consent holder views it as ES 6 or less. Could you please provide clarification as to, in your view and with reference to the information provided to you, what enrichment score the compliance regime equates to.

Response: I draw the reader's attention to my comments above (137-159). It is my opinion that states of ES6 and above are not or should not be regarded as compliant. ES 5 (to be clear, I include all states between ES5 and 5.9 in this) should be regarded as the maximum impact state consistent with compliancy at any site.

4.2 2 P 6, line 220. Are you aware of other regulators elsewhere that take the worst-case sample station for compliance purposes?

Response: It is worth elaborating on the important distinctions between within-station averaging and between-station averaging. As this section (210-228) attempts to clarify, I think that it is best to consider averages of replicates for the same station. This would be standard practice for very good reasons – for example, if you treat each sample independently you are treating it as an un-replicated station. The way forward should be more replication and not less! However, I think it is perfectly rational to have a rule of the type "no station within a certain Zone may have an average determination that breaks a certain standard". Such a standard would normally be seen as the highest average value for a station that cannot be exceeded anywhere within such a specified zone. To take an average of all such measurements within the zone could lead to very odd outcomes, especially where gradients are steep. For such within-zone averages to work there would have to be a very detailed method of determining station positions that could be reproduced at each farm consistently. I think this is a minefield and would argue that the compliance determination should be per station (averages of replicates) against standards set per zone.

Sediment Quality Criteria in Scotland are shown in Table 2.

Table 2 Sediment quality criteria for benthos and action levels (SEPA Fish Farm Manual, Annex A)

Determinand	**Action Level Within Allowable Zone of effects**	**Action Level Outside Allowable Zone of effects**
Number of taxa	Less than 2 polychaete taxa present (replicates bulked)	Must be at least 50% of reference station value
Number of taxa	Two or more replicates with no taxa present	
Abundance	Organic enrichment polychaetes present in abnormally low densities	Organic enrichment polychaetes must not exceed 200% of reference station value
Shannon -Weiner Diversity	N/A	Must be at least 60 % of reference station value
Infaunal Trophic Index (ITI)	N / A	Must be at least 50% of reference station value
Beggiatoa	N/A	Mats present
Feed Pellets	Accumulations of pellets	Pellets present
Organic Carbon	9%	
Redox potential	Values lower than -150 mV (as a depth average profile) OR Values lower than -125 mV (in surface sediments 0-3 cm)	
Loss on Ignition	27%	

These levels apply at the station rather than zone level. However, many Scottish consents have stations at the cage edge and then straddling the AZE border so there is really little opportunity to average stations.

4.3 How should we view the status of redox potential as a reliable indicator of enrichment in the Cawthron ES model (p 5, line 180)? If negative Eh values are associated with anoxic conditions and reflect anaerobic decomposition processes, yet can be out-of range with other parameters, should the enrichment assessment be based on more limited range of indicators such as benthos alone (as per p 9 lines 350-356)?

Response: Redox is a quick and simple measurement of the conditions in the sediment with respect to key biogeochemical processes. However, it can be rather subjective in use. Sometimes stabilisation is a very long process (up to 60 minutes). Pearson and Stanley (1979) suggested measuring after 60s to bring some consistency to the determination, with immersion in Zobell's solution after each determination. Perhaps when the same electrode is used by the same operator in precisely the same way then the values can be trusted. In many cases I would prefer to look at the shape of the redox curve (Redox Potential Discontinuity, RPD) rather than the absolute values.

Despite these caveats, I will continue to measure redox as it adds valuable additional information that I would rather have than not have. The question is not do we discard it but should we use it in an index of ecosystem state? I think that I would like to hear more debate on that amongst experienced practitioners.

Regarding using the benthos alone: I would not like to be without supporting additional information: sediment appearance, smell, visible fauna, redox, TOM, redox, sulphide – all of these support conclusions that might be drawn from the subsequent benthic analysis. They have the effect of reducing the likelihood of errors in assessing the condition of the seabed – for example it might be useful in assessing whether a sample is low in diversity for stochastic reasons as opposed to being equivalent to ES6. Given that the benthos is the most expensive determinand it makes no economic sense to not measure the other chemical parameters. Again, the question is as to whether we include this is a composite index such as the ES scale or whether these simply support assessments made using other indicators (AMBI, ISI, H, etc). I think this is a matter for expert judgment and I would like to hear more practitioner discussion about this. Personally, I quite like the idea of using such a composite indicator as long as the error bars or confidence intervals are not forgotten. I have a concern that the ES indicator may be being used as if it is a scalpel as opposed to a table knife. I am happy when the data used together produce a result that is ES4 or ES5 but not very comfortable when I see arguments about the decimal points. On the other hand, given the confidence intervals that likely pertain, it is quite reasonable for the regulator to be more concerned by an ES value of SE5.9 than ES5.1, for example. In such a case, while the ES5.9 score might be compliant it might be sufficiently borderline so as to warrant some regulatory action to ensure that the site has a realistic prospect of remaining in compliance in the period until it is next monitored. To facilitate consideration of these data, it might be useful if these could be reported in future by Cawthron with either error bars or confidence intervals as statistically appropriate. It would be useful to hear the pros and cons of such an approach from a range of perspectives.

4.4 In that light, if the biodiversity measures at a station comprise low diversity and low abundance, then is the consent in compliance? It seems that averaging benthos scores with redox (if it is out of range) and sulphides can depress the enrichment score, even when bioturbation is severely limited. To unpack the question, please refer to the Te Pangu Report #2278, page 15, Table 2. Pen 2 has an ES 6 for Infauna composition, yet the organic loading and sediment chemistry pulled the overall ES score lower to 5.3. The concern is that if the infauna composition results are > 6 indicating the benthos is at risk of imminent collapse, that this is subsumed by more favourable redox and sulphides scores. Is this a fair interpretation, or do the redox and sulphides suggest that ES6 for the infauna composition is not confirmed at this station?

Response: Low diversity and low abundance could be indicative of ES6 – a state that is in my view non-compliant. If this is the case then it is likely that sulphide will be very high and redox will usually be quite negative. If so then this would confirm ES6. In the specific case of Pen 2 at the Te Pangu site, it appears to me that the benthos result of ES6.0 is indicative of a highly degraded assemblage. So what is going on with the organic matter, redox and sulphides? I think the first thing to say is that this is a high flow site and globally we are rather weak on what to expect at high flow sites – they do seem to behave differently than expectations based on our (more comprehensive) knowledge of low flow sites. My guess is that the high flow regimes deliver large amounts of oxygen most of the time keeping sediments in a relatively oxic state as well as removing considerable amounts of organic matter by resuspension. However, considerable organic accumulation can still occur. Findlay and Watling (1997) hypothesised that it is the balance between oxygen supply and demand during even relatively short intervals of low current speed that determines macrobenthic structure. It could be that, at that time of monitoring, redox (measured at only 1cm depth) did not reflect conditions during occasional slack water movements. Cawthron should consider this hypothesis in their future development of the method at high flow sites. It could be that these would be better regulated on macrobenthic impact alone. It would be fairly easy to consider the near-bed current record for the site in terms of low-current episodes and again fairly easy to consider how redox and sulphide concentration evolve temporally in cores taken from such sites.

4.5 Can bioturbation potential can be seen as a proxy for ecosystem life-supporting capacity?

Response: Retaining a large bioturbation function could be regarded as an objective method of achieving "ecosystem life-support capacity". It is known that bioturbation increases the rate at which carbon degrades in marine sediments (Heilskov and Holmer, 2001; 2003; Kristensen, 2000; Kristensen and Mikkelsen, 2003) – conversely allowing bioturbation function to stall by over-enrichment will provide a negative feedback which will reduce degradation and retard subsequent recovery. There are methods in existence to estimate bioturbation rates (Black *et al.*, 2012; Nickell *et al.*, 2003) but these may be considered to be more suited to research than to monitoring. In any case, high abundances of macrofauna (ES5) are a symptom of high enrichment but also a sign that the benthos is still functioning. Once abundance starts to collapse (ES6), bioturbation function is lost and the system can be regarded as being highly degraded.

4.6 The consent holder comments that Council appear to be challenging the science underpinning the Cawthron 7-point enrichment scale model. This is not correct; rather some of the assumptions and interpretation are being queried. How does the Cawthron enrichment model compare to monitoring regimes in use in other countries, in terms of its sensitivity and robustness?

Response. I have carried out a review of this (Wilson *et al.*, 2009) but some aspects of this have been superseded . In broad terms, the approach taken is not too much different from that in several other countries. Most countries use a suite of measurements including full benthic analysis. Standards do vary somewhat between countries and are often driven by the wording of laws designed for activities other than fish farming. Norway, uses a rapid assessment method for the area immediately around the cages (Ervik *et al.*, 1997; Hansen *et al.*, 2001; Stigebrandt *et al.*, 2004) which is quite different to that used in other countries. New Brunswick is almost entirely dependent on sediment sulphide measurements (Chang, 2012).

In my opinion the Cawthron method is likely quite robust as it is essentially derived from data collected at the sites that are in question here. Whether it is immediately applicable to other environmental contexts remains to be seen. As a method I cannot see why it cannot be at least as robust as methods used in other countries. The issues are not with the methods but with the standards set in consents. Ideally, all MDC sites can be brought into a common consenting framework with common standards.

4.7 In terms of environmental conditions under pens at Otanerau, Forsyth, Ruakaka, and Waihinau (the "low flow" sites), do we have a sound basis for our assessment that there is little resilience and assimilative capacity at these sites?

Response: On the questions of resilience and assimilative capacity: resilience is the degree to which a system can be (further) stressed before rapid change occurs and is also related to its ability to recover once the stress has been removed. Consider a benthic system in state ES5 where an increase in stress might push the system into ES6 where there is potential for negative feedback and a further descent into ES7. Recovery of function from ES6 and, especially ES7, might be protracted. However, organic enrichment – as opposed to nuclear contamination or persistent organic pollutants - is essentially a natural phenomenon: consider a whale decomposing on the seabed. We can expect the seabed under the farms (or whales) to take a long time to recover, but we can assume that it will eventually recover functionally if there is plenty of undisturbed habitat nearby from which recolonisation can be effected.

It is very difficult to predict how much resilience is left in such systems. Experience is better than theory. The experience at Forsyth, for example, leads me to worry about its resilience Assimilative capacity is closely related to resilience – when you use up the available capacity the system becomes loses resilience. When you go beyond ES5 you are overwhelming the assimilative capacity of the system and further addition of organic material will lead to its accumulation. It is important for stakeholders to consider the underlying reasons for regulation, not least as this will help frame better future regulation. Ultimately we regulate salmon farming primarily on the basis of benthic impact as these are usually the most measurable impacts. It is rational to regulate fish farmers so as to avoid degrading the local environment into an entirely non-resilient, non-functional state. The primary benefactors of this are the fish farmers. Dirty farms often perform badly. Dirty farms also make bad press and this can affect demand.

Otanerau – The site has a station that is ES6+. This is non-compliant. Little resilience left, assimilative capacity exceeded.

Forsyth – at the time of monitoring this site had no station >ES5. However, monitoring was conducted just after restarting the farm after a fallow period and so trouble may be expected given the history of the site. In my view monitoring should be carried during the predicted worst case scenario. I would not be surprised if this site became non-compliant during the course of its production cycle.

Ruakaka – This site has stations that are ES5+. While still compliant there is likely to be little room for further insult.

Waihinau – The near cage stations are compliant (<ES5) but there is an issue at the zone 1/2 boundary. While this is a highly enriched site it is clearly not in such bad shape as the 3 above and may be able to get through the cycle without becoming non-compliant in Zone 1.

4.8 Are adverse effects likely or imminent at the low flow sites if there is no substantive change to the intensity of organic loading?

Response: The company seem to be arguing in the documents sent to you on July 25th that sediment conditions now are not particularly bad in a historical context. This seems a little at odds with the statement in the following question (9) “The consent holder has advised us that they are currently experiencing significant declines in production”.

Clearly, some of these sites have had much higher inputs in the past and much higher environmental impacts. That impacts have been reduced through more efficient feeding and by reduced production (?) is encouraging. Perhaps these could be further improved by installation of new feed monitoring technologies or perhaps all such feasible improvements have already been made. Given the amount to monitoring evidence that exists for these sites it should be possible to make highly informed judgements about how they can be managed to remain in compliance. This is a task for the company in the first instance.

I summarise the intentions of MDC for each of these sites as given in my brief:

Waihinau – no reduction in feed for 2013 (no information about 2014)
Forsyth – the farm should be fallowed
Otanerau – the farm should be fallowed
Ruakaka – a significant reduction in feed.

Each of these farms is operating close to (Waihinau, Ruakaka) or beyond (Otanerau, probably Forsyth) their assimilative capacities. I think it would be reasonable to suggest that inputs to the sediments at the first 2 farms are presently maximal and risks would be reduced if feed inputs were reduced. At the second 2 farms, I think there is little doubt that decreases in feed input are required to reduce existing impacts at Otenerau and are also required at Forsyth to prevent the likely future non-compliance.

4.9 At all sites, except Waihinau, the pens are fixed, and cannot be rotated around the site. The consent holder has advised us that they are currently experiencing significant declines in production. Assuming these sites will remain in production in the long-term (there is a lack of suitable space elsewhere), and with reference to your comments on rotation (pp 3-4), what are the merits of allowing pens to be shifted within the site footprint?

Response: I have to take your assertion that there is no suitable space available elsewhere, however hard this is to understand. But it is my opinion that large scale fish farming has only a limited future in inshore constrained waters globally. Operators are already using bigger sites in more exposed locations to drive costs and impacts down. However, if there really is no alternative then of course rotation can be used provided that the total proportion of the habitat impacted by aquaculture does not become excessive by so doing. If cages, are to be typically moved around a site, especially if the distances are not great, then I think you will need to think very carefully about how you monitor impacts.

4.10 If this was to occur, new consents would be required for each site. Would an appropriate trade-off be to require a lesser enrichment threshold? What other conditions would you recommend?

Response: Important here is to reduce benthic footprint overlap of internally rotated sites to the minimum possible to minimise inputs to the recovering site. A lower enrichment threshold could be considered but I am not sure why this would be a good thing. What we are trying to protect here is as much natural habitat as possible on the one hand (a limit to the scale of impact) and to ensure that the most highly impacted area is still operating within its assimilative capacity (a limit to the degree of impact) on the other. The only reason to reduce the maximum allowable degree of impact where this is not presently exceeding assimilative capacity is to reduce the risk of future non-compliance. This may be laudable where there is a lot of uncertainty about the relationship between inputs and impact. In the present cases there is a lot of historical evidence to guide us in this and it is likely that these sites can be operated a little closer to the wind. I think it is important that the industry is allowed to maximise its production efficiency on as small a footprint as possible but that it should also be monitored more closely to ensure that it never breaks the standards set. To this end I would argue that increased environmental risk should be met not by changing standards but by more frequent monitoring. Given the scale of these operations and their essentially free use of the environment for waste disposal, the small increased costs of such monitoring do not seem disproportionate. The *quid pro quo* of allowing rotation must be that if the farms become non-compliant they must expect and accept immediate sanction.

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